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2009

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# Darwinian Explanations of the Origin of Language



VRIJE UNIVERSITEIT

*Darwinian Explanations of the Origin of Language*

ACADEMISCH PROEFSCHRIFT

ter verkrijging van de graad Doctor aan  
de Vrije Universiteit Amsterdam,  
op gezag van de rector magnificus  
prof.dr. L.M. Bouter,  
in het openbaar te verdedigen  
ten overstaan van de promotiecommissie  
van de faculteit der Wijsbegeerte  
op donderdag 1 oktober 2009 om 15.45 uur  
in de aula van de universiteit,  
De Boelelaan 1105

door

Michel Gerardus Heijdra

geboren te Vlaardingen

promotor: prof.dr. R. van Woudenberg  
copromotor: prof.dr. P.Th. van Reenen

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To the memory of my beloved grandmother M.F. Timmer-Valk (1917-2008)





# Introduction

For those who haven't noticed yet: 2009 is Darwin year, celebrating the 150<sup>th</sup> anniversary of *The Origin of Species* and Darwin's 200<sup>th</sup> birthday. I am proud that in this year my dissertation on the evolutionary origin of language is published and defended.

Darwin's theory is no longer an isolated theory in biology on how (animal) species evolved from earlier ones, as it was 150 years ago. Since its publication Darwin's work has invaded many branches of science. However, those invasions remain a field of hefty debate. This is also the case for evolutionary accounts of the origin of language.

Until quite recently it seemed that the famous ban of the French Linguistic Society of 1866 on papers on the origin of language, due to the contemporary flood of highly speculative papers on this topic, was still being enforced as of today: until the beginning of the 1990s almost no papers on the origin of language were published. Darwinian accounts of the origin of language were even scarcer. This absence of the origin of language as a field of scientific study can partly be explained from the way linguistics as a field of studies enfolded in the 20<sup>th</sup> century, with first structuralism and later generative linguistics gaining dominance in the field. However, in more recent decades new fields of linguistics have emerged and emancipated themselves: from biolinguistics to sociolinguistics, from the study of child language acquisition to pragmatics. These fields will turn out to be crucial in evolutionary explanations of the origin of language. Any explanation of the origin of language has to draw upon the results of many different fields, but for the first time there is such a vast body of empirical literature available that explanations of the origin of language can rise above the level of mere speculation.

Language has always been and is still seen as one of the key features of what makes humans human. Darwin's work, both his *Origin of Species* and especially his *Descent of Man*, implies that all key human features (or 'essential properties' as metaphysicians would like to call them) are not uniquely human or categorically distinct from the features of other animals. There is still a lot of opposition to the extreme implications of this view as humans tend to see themselves, and maybe rightly so, as a special species. This dissertation will shed light on how much of language can be explained in Darwinian fashion. The Darwinian explanation of language is not as straight-forward as that of kidneys or lungs. The reason for this is that the origin of language can and has to be viewed from at least two sides: the origin of *language abilities* (speech organs, neurological mechanisms behind language use etc.) and the origin of (*proto-*)*linguistic forms* (sounds and sound patterns, words, syntactical structure etc.). The former fall more in the realm of biology, the latter in the realm of culture, but any *complete* explanation of the origin of language will have to consider both and their entanglement.

Recently, Darwinian principles have not only been applied to the *evolution of the biological basis of certain key human features* that are usually considered to be part of the realm of culture such as our sense of humor, the prevalence of religious sentiments amongst humans which other animals lack, or our enzymes to break-down the products of agri-culture, but also to *cultural evolution proper* and the evolution of non-biological entities such as the evolution of jokes, religious ideas and tools to work on the land (ploughs, scythes etc.). The best known example of a

Darwinian theory of the evolution of cultural elements is *memology* which was highly popular in the nineties and which *directly* applied all the principles of the well-established theory of genetic evolution to the evolution of cultural elements or “memes”. But there exist numerous other theories of cultural evolution that use Darwinian principles more *indirectly* such as cultural ecology, evolutionary psychology and the dual-inheritance theory. The dual-inheritance theory, which we argue to be the best frame-work to treat any form of cultural evolution from a Darwinian perspective, stresses the entanglement of the evolution of the *biological basis* of cultural elements with the evolution of the *cultural elements* themselves. This dissertation is the first to discuss *in extenso* if and to what extent the problem of the origin of language can be treated within the frame-work of a dual-inheritance theory.

Every chapter of this dissertation has a summary of its own; at the end a summary of the whole dissertation is given. In this introduction we will therefore give but a brief preview of the work as a whole; but before that I take the freedom to elaborate a little on the genesis and context of this dissertation project. In the acknowledgments I thank those without whom this dissertation would not have been completed.

Work on this dissertation started at the end of 2003. I had just returned to Leiden from Berlin where I had finished my master thesis for philosophy, one year after I had finished my masters of physics. During my study of physics I had enjoyed the conceptual side of physics as much as I had disliked working in the lab, and in philosophy I was similarly triggered by conceptual problems and the fundamental ‘what is’ questions (what is life, what is language, what is the mind). The philosophical curriculum back then in Leiden was very broad, covering both continental thinkers and Anglo-Saxon ones, and extending from the classical period to contemporary philosophy. Like most of my peers I was drawn more towards continental philosophy than Anglo-Saxon philosophy, and I hoped to use the philosophical classics to elucidate contemporary problems (‘combining historic and systematic research’ as it was called). The thinkers I felt most akin to were almost all *Idealists* in one sense or another: be they classical rationalist like Descartes and Leibniz, be they German Idealists like Kant and Hegel, be they phenomenologists like Husserl or be they idealistic philosophers of science who stressed the paradigmatic nature of our knowledge like Kuhn. I expected of philosophy a grand rational narrative of the world, our knowledge thereof and our place therein.

After my stay in Berlin my affiliation with Idealism had, however, waned. I had written my thesis on the concept of life in Hegel’s work and Universal Darwinism, and I had become convinced that Hegel’s philosophy of nature was completely outdated; as the philosophy of nature was such an integral part of his whole system, this implied that the whole system had to be considered obsolete.<sup>1</sup> This drew me even more to Universal Darwinism in which I had become interested through lectures by Wouter Oudemans. Oudemans work is hard to summarize, first of all because he would not recognize himself in any proposition about his philosophy as he claimed that his philosophy contained no proposition nor statements. If we nonetheless describe his work, it can best be seen as circling around

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<sup>1</sup> In my thesis I had been unwilling to write this down as bluntly as I feel it right now and I had simply put Hegel and Darwin next to each other as two consistent but completely opposed views on life, the evolution of organisms and the mind that started from two radically different and not further defensible starting points: idealism and materialism.

the work of Wittgenstein and Neodarwinists on the one hand and Heidegger on the other.

As I started my dissertation with Ad Verbrugge as promotor, I intended to outline a sketch of universal Darwinism and to provide a critique upon it from the perspective of continental philosophy, mainly Heidegger and Derrida. There was no formal proposal about the contents of my dissertation (with hindsight I can recommend any promoter to demand such a proposal!) and I was reading the first two years without a very clear goal in mind. In the first two months I read some more about Darwinism, thinking that I knew it already quite well, and in the rest I worked myself through the *Gesamtausgabe* of Heidegger, and the work of the authors he commented upon such as Plato, Aristotle, Descartes, Leibniz etc. However, after 2 years I still felt insecure what my criticism of (Universal) Darwinism would amount to. In parallel, I also read numerous books on linguistics as I barely knew anything about it; I had always felt very uncomfortable when philosophers discussed language and the complex relations of thought, language and reality. I found it difficult to get a grasp on the topic and hoped that studying linguistics would help me. I finally reread Wittgenstein's *Philosophical Investigations*, which I had to read in my first year, but which I only remembered as notoriously hard. Reading Wittgenstein I became more and more convinced (what Oudemans had stressed during his lecture on it) that the philosophical 'what is' questions were better dealt with by science than philosophy, and that philosophy could not lay the ground-work for the house of science as scientists themselves were much more able to do that themselves.

In 2005 I had the opportunity to spend a semester in Princeton; philosophy in Princeton was completely different from what I was used to in Leiden and Berlin. In Princeton, continental philosophy was only taught in the liberal arts department and philosophy was more or less reduced to logic, epistemology and philosophy of language. What I found in Princeton, however, especially in the courses of Bas van Fraassen on quantum information theory, was a type of philosophy which I would like to call 'science based philosophy': it took the results of science to heart, but dared to ask questions that science itself had not yet answered, often by combining results from different sciences (in Bas's course: results from epistemology, information theory and quantum physics). It is this type of philosophy, closely akin to science itself, what I consider this dissertation to be: combining results from the theory of science, biology, paleontology, archeology, sociolinguistics, language acquisition and the philosophy of language. No longer did I intend to criticize Universal Darwinism with the help of continental philosophers, I wanted to analyze to what extent Darwinian explanations could be used to elucidate non-biological evolution processes.

After coming back from Princeton, I started writing a draft of my dissertation; I chose 5 scientific fields in which I wanted to evaluate Darwinian explanations: linguistics, sociology, theory of science, religion and mathematics. Still uncertain about the direction I started with a chapter on the evolution of mathematics. The philosophy of mathematics is the field in which Platonism ('Idealism') is traditionally strongest. If I could show that Darwin's theory could shed light on the 'evolution of mathematics', the use for Darwinian explanations in other areas would probably also be considerable. The chapter I wrote was a hodgepodge of many lines of thought arguing that the biological Darwinian selection process solved

mathematical problems life-forms were subject too,<sup>2</sup> that mathematical problems arise from problems in physical reality, that mathematical theorems are not infallible and that mathematical concepts constantly change; the chapter incorporated results from ethno-mathematics, the history of mathematics, learning mathematics and many other fields. This chapter did not end up in my dissertation, but writing it taught me 2 things: 1) (Universal) Darwinism was not as clear, univocal and strong a position as I had thought it was as defending Universal Darwinism showed me its problems, 2) applying Darwinian principles to a scientific field requires a lot of knowledge of it.

One year later, it became clear that my promotor Ad Verbrugge and I had become estranged: we had a very different stance on how to use the results of science in philosophy, on the status of idealism and the big philosophical systems of the past. I was and am very happy that René van Woudenberg stepped in to become my new promotor. He read the 400 pages I had written thus far in less than one week and he convinced me to focus on the application of Darwin's theory in one area of science only, which became linguistics. I threw away almost all material and started afresh in the 8 months I was left. In the last year I combined my new job as consultant at McKinsey with finishing off the dissertation and the result lies before you.

Preview of its content:

Part 1: Darwinian explanations; different authors from different times mean different things when they speak of a Darwinian explanation. In chapter 1 we discuss what a good Darwinian explanation in biology should look like. This chapter takes more branches of biology into account than any paper we have seen in the literature and provides more precise definitions of the key concepts 'replication', 'variation', 'fitness' and 'selection' than given elsewhere. In chapter 2 we discuss 8 Darwinian models of cultural evolution, 2 ontogenetic and 6 phylogenetic ones. We argue that the dual-inheritance model is the most promising one and on the basis of the definitions of chapter 1 we provide accurate definitions of the key concepts of the dual-inheritance theory, which in the work of its proponents remain rather implicit.

Part 2: Language and its origin; in chapter 3 we define what we regard as language for the purpose of this dissertation using Hockett's design criteria for human language. We further critically assess Chomsky's theory which we see as one of the main obstacles for a Darwinian theory of the origin of language. We discuss a vast body of literature from many scientific fields: physiology, neurology, paleontology, archeology, ethology (animal language research), language acquisition and sociolinguistics, that provides the empirical basis for a Darwinian theory of the origin of language. In chapter 4 we assess the multiple meanings of the phrase 'origin of language' and evaluate to what extent the problem of origin of language can be treated within the frame-work of the dual-inheritance theory.

Michel Heijdra, April 2009

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<sup>2</sup> Due to natural selection the bee hives had evolved with hexagonal cells that optimized area coverage by cells, ants had evolved bio-programs with rules to minimize the collective distance travelled to food sources etc.

# Acknowledgements

This dissertation would not have been possible without a great number of people that have contributed in one way or another to its content and completion.

I owe a lot to Ad Verbrugge who arranged a PhD position for me at the VU after his move from Leiden to Amsterdam, and who has always expressed a sincere confidence in my ability to write an original dissertation. Not only as a teacher has he inspired me to study many classical authors in-depth, but also as a caring tutor he has given his pupil lots of freedom to explore his one path within philosophy. As it has turned out, our paths have followed opposite directions, but maybe one day we encounter each other at one meridian or another.

I am immensely grateful to René van Woudenberg for taking upon him the continuation of this dissertation project, from which he knew it would never look like anything that would have been there if I had started a dissertation with him. Even more so as our ultimate views on the status of Darwinism are probably quite far off. René, you told me once that when you had to switch promotors, also involving a switch from continental philosophy to another type of philosophy, your new promotor Nicholas Wolterstorff said something like: “I don’t judge you on the authorities you quote, but on the nature of your argumentation; not on whether I agree with it contents or views, but on whether I can follow the line of thought even if it does not fit my own position.” These words are as true about your promotor as they are about you. Within one week you were able to read the 400-page semi-popular assemblage of material on Darwin and everything else that my dissertation was before we started. It was quite useless, I see now, but you saw what it could become and gently pushed me to streamline and focus it, even though it is probably still over-sized a factor of 2 to your liking. I hope you have enjoyed working with me as much as I have with you.

Piet van Reenen deserves a big “thank you” too. When I arrived at the VU, I felt a bit isolated, but during the biweekly drink at the philosophy department that you together with Anke almost always visited, I always happily ended up chatting with you, listening to your stories and language anecdotes: how could I have known that you would become my co-promotor! I am grateful to your willingness to immediately invest so much time and energy in my promotion after René asked you, even pushing him in organizing things even quicker. We have very different natural attitudes: whereas you are able to dive into the most complex details, I have the tendency to look for the grand picture: nevertheless despite that, or maybe because of that, we worked together in a real complementary fashion and I am very happy about the end result. I also thank you and Anke for all your support in finishing the dissertation at moments I was quite fed up with it.

Finally, Rob Visser gave detailed input on the biology part; I apologize that you had to read yet another thick volume from my hand, but I am grateful to all the suggestions you gave and the mistakes you prevented me from making. Any residual mistakes are solely due to my purview.

Many others within Academia deserve to be credited. Of course Wouter Oudemans: I think no one had a bigger influence on my thinking than you did. You introduced me to Wittgenstein and his *Philosophical Investigations* (which back then I didn’t appreciate at all) and to *Universal Darwinism*. My liking once for Heidegger (I confess, I used to have Heidegger’s *Gesamtausgabe* in my upstairs bed before Agnes

and I lived together) also started during your lectures, that were my weekly highlight for a couple of years. I think you will be happy with my decision ‘to leave Heidegger alone’ (a sentence you may read against the thread), instead of mixing him up with Darwin and using him in criticizing Darwinism. I am also very happy that I had the opportunity to discuss in the graduate seminar my chapter on Darwin and mathematics.

Herman Philipse in all his British politeness steered me more than once in the right direction. Herman, you gently pushed me to spend more time abroad, far away from “this province” we call the Netherlands; I am thankful that you acted as an ambassador for getting me to visit Princeton and introducing me to Bas van Fraassen. I am also happy that when I was back, you invited this ‘continental philosopher’ to your research seminar on analytic philosophy for graduate students. I have certainly also profited from the less gentle push that I should restrict myself and focus my work more, and I have enjoyed all the debates and the vigorous comments by the participants of the seminar: Remko van der Geest, Victor Gijssbers, Rosja Mastop, Janneke van Lith, Fred Müller and Eric Schliesser.

Eric Schliesser and I turned out to have remarkably similar interests in the history of mechanics, universal Darwinism and the reception of classical philosophy in modern times. Added to that I can say that you acted as a real mentor as you were always willing to read parts of my manuscript, encouraged me to sharpen my ideas and even gave me the floor in one of your conferences. I know you hoped I would pursue a career in philosophy or academia, and who knows what will come: at least I hope they can keep you in the Netherlands so that philosophy in the Netherlands will continue to profit from your career.

As I was struggling through my dissertation, help came from unexpected directions such as from the research group on classical and medieval philosophy of Wouter Gorris which held a meeting discussing in-depth one of the key chapters of my dissertation: I thank the members of the research group and all others present: Wouter Gorris, Suzanne Metselaar, Hein van den Berg, Lieven Decock, Marije Martijn and Loes Derksen for their feedback. Others that have provided invaluable feedback on parts of my dissertation include the members of the research group on political and moral philosophy of the VU: Henk Woldring, Bert Musschenga, Ad Verbrugge, Jan van der Stoep, Govert Buijs, Jos Kole, Anders Schinkel, Ton van Prooijen and Haroon Sheikh, and the members of the similar group at Tilburg university: Paul Cobben, Freek Grootenboer, Herman van Erp, Arthur Kok and Donald Loose. Although my research fitted less and less will within the (wide) boundaries of these groups, I am happy for all the feedback they have given me. Finally, to all the members of the Hegel group of the VU, i.e., Christian Krijnen, Ernst-Otto Onnasch, Peter Koslowski, Sander Griffioen and Ad Verbrugge: even though I am rather pessimistic about the status of Hegel’s philosophy for today’s systematic philosophy, I am very glad to have participated in our discussions.

Many friends also influenced the development of this dissertation. I really enjoyed the *Philosophische Untersuchungen* reading group with my friends Jaap Trouw, Haroon Sheikh and Thomas Brouwer: I hope in 10 years we will still be able to interact by just mentioning the numbers of the sections of the PU to each other. The other reading group, now in existence for almost 10 years, that I must mention is the one run by René de Bakker on continental philosophy. I thank all the members: Lucien ter Beek, Annelies de Bakker, David de Boer, Gijss van Andel and Haroon Sheik for all our many conversations on Parmenides, Plato, Aristotle, Descartes, Leibniz, Nietzsche, Heidegger as well as on *Große Politik und Geschichte*, European

soccer and Eminem's rap music. And finally, the Spengler reading group with Daan Molenaar, Freek Dirkzwager, Martijn den Bleeker, Marijke Breeuwsma, Jorit de Jong, Haroon Sheikh and Ad Verbrugge: Spengler is one of the most consequent cultural relativists I know of and although I am highly critical of his idiosyncratic perspective, I have learnt a lot from discussing the historical and contingent character of almost any phenomenon from mathematics to economics to religion with you.

At the VU, especially in the last 2 years, I have enjoyed the company of great friends and roommates: Gert, Willemien, Suzanne and Hein. Gert Bos had an incredible knowledge of arcane subjects such as Coptic and the early sources of Christianity and I hope he will one day write an improved version of the *Da Vinci Code*. Willemien van Dijk was a hard worker and I hope I haven't kept her from her work by chit-chatting a lot; for sure, we were the biggest sun-lovers of the faculty and we always found some excuse to read or work outside. Suzanne Metselaar completely redecorated the room that like the VU building was rather grey before she entered; she cherished the moods of everyone present and is great to work with; we entertained a semi-continuous discussion on anything from Heidegger to Kate Moss and from Aristotle's metaphysics to plain gossip. The many work breaks with Hein (Hein a cigarette, me a can of Diet Coke) on the idiosyncrasies and pseudo-arguments of German Idealists made us both sharper thinkers and convinced us that seriously studying old thinkers can only have historical value. Even more so, we laughed a lot and had great fun. Finally I thank Mark Okker de Kok for all our trips, for some reason always through Muslim territory, during our dissertations and the many enlightened conversations we held.

During the work on my dissertation I have been president of the Dutch PhD Network (Promovendi Netwerk Nederland). Together with Derek-Jan Fikkers, René van Os, Marijn van Wingerden, Eddy Rijntjes, Marijke Leliveld, Koen van Dam, and Gertjan Tommel, Eric-Jan Smits, Caelesta Poppelaars, Leonie Cramer we fought for the rights of PhDs as employees, a better structure of a PhD program and job chances for PhDs. Apart from the fun we had and the good results we have achieved, our work has helped me significantly in finding the right job after my PhD.

During my dissertation my life situation has changed enormously: from that of an anti-bourgeois student who loved to shock to a man who is expecting his first child and will soon have a mortgage. I don't regret the change as my life has become fuller and richer. I am very grateful to my mother-in-law Truus Penaat who has taken up her old job of editor for this dissertation – this time not using *movable type printing*, but using *Microsoft Word* to make a consistent and visually appealing literature list. I thank my sister-in-law, Elke Penaat, for help in designing the front cover and my Princeton friends Mark and Veronica Rose Alfano for editing the dissertation. I thank my parents for all the support they gave during my years as a student, physically, financially and emotionally. But most of all I thank Agnes Penaat: for the proofreading and consistency check of the dissertation when I was short on time, but a thousand times more so for simply being there, during times I worked hard on the dissertation, during times I was completely fed up with it and especially during times when the dissertation was not at all in our minds.





# **Part I: Darwinian explanations**



# Chapter 1: Darwinian explanations in biology

*Summary:*<sup>3</sup>

*The goal of this chapter and the next of part I is to set up a framework to test the validity and strength of Darwinian explanations outside of biology. This framework comprises 1) a list of precise definitions of the fundamental concepts of Darwinian explanations, 2) some general principles of theory choice, and 3) some characteristics of Darwinian explanations in particular. This list provides us with a picture of what the ideal Darwinian explanation should currently look like (currently, because standards change, often becoming more stringent). Most contemporary Darwinian explanations do not live up to the standards of this ideal, but that need not be a problem as further research often aligns the explanation more to the ideal. The framework will be constructed in such a way, though, that it is strong enough to exclude as invalid pseudo-Darwinian just-so stories and non-explanations that cannot be improved by further research. The framework can be found in §1.4.*

*We arrive at this general framework by first focusing on Darwinian explanations in biology. The framework we find in this chapter will be generalized in chapter 2 to include Darwinian explanations outside of biology. Biology is the native territory of Darwinian explanations in which such explanations have been long accepted, often used, and more or less well-understood (or at least much less disputed than in other areas). In biology also a minimal consensus exists on the fundamental concepts of a Darwinian explanation. These concepts are replication (with inheritance), variation and selection. Biologists further agree that proposed Darwinian explanations should be testable and have empirical consequences if they want to be more than mere speculations. In §1.2 we will look at the different conceptualizations of the fundamental concepts and in §1.3 at ways to operationalize the criterion of empirical testability, by focusing both on general principles of theory choice and on principles governing Darwinian explanations in particular. In §1.1 we start, however, with a little bit of the history of Darwinian explanations, which we do for three reasons: a) to show that Darwinism is not a uniform whole but a collection of subtheories and principles that – although interconnected – have their individual history of empirical testing, validating, and refuting; b) to point out that the success of Darwinism depends not only on the strength of Darwinian theory but also on sociological factors, extra-biological ideas, and results from non-biological sciences; and c) to stress that good Darwinian explanations make extensive use of so-called proximate mechanisms provided by other sciences. We use the results of §1.1 as we operationalize empirical testability in §1.3, but, more importantly, we should realize that Darwinian explanations outside of biology will probably turn out not to be uniform wholes either, and that their success will depend not only on their theoretical strengths but also on general ideas about their object as well as the proximate mechanisms and other results other sciences have already identified.*

*The danger of starting with Darwinian explanations in biology to formulate a more abstract framework for Darwinian explanations in general is that the resulting abstract framework may not be abstracted enough from the specifically biological context. For instance, in Darwinian explanations in biology the distinction between genotype and phenotype often plays a major role. In the process of abstraction towards a more general framework for Darwinian explanations this fact should be reckoned with. Reasons should be adduced why some analogy of*

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<sup>3</sup> Every chapter and subsection of this study starts with a few paragraphs in italics that provide a summary of what is to follow.

*the genotype/phenotype distinction should or should not be included in the general framework. Otherwise, potentially sound Darwinian explanations outside of biology could be rejected because they do not fit the scheme based upon biology. Despite the abstraction problem, the advantage of starting with biology remains that we find in biology paradigm cases of successful Darwinian explanations that give us a firm grip on the structure of Darwinian explanations. We will try to avoid some of the possible mistakes due to insufficient abstraction beforehand by emphasizing the plurality of forms a Darwinian explanation in biology can take. As such, the abstraction will not be based upon only one, highly-specific, form a Darwinian explanation in biology can take.*

*In the final section of this chapter, §1.5, we test our model on the case of sexual selection. Sexual selection is also an important theory in its own right as Darwinian explanations outside of biology often involve the theory of sexual selection. We will distinguish the many senses of the term ‘sexual selection’ and focus on the most controversial usage of sexual selection as ornamental selection. We show that all forms of sexual selection are just subtypes of natural selection but that the irreducibly two-step nature of ornamental selection (turning a cost into an advantage as the cost is a marker of good nature) makes it beforehand harder to test and verify than other instances of natural selection.*

## **§1.1 Introduction: a short history of Darwinism and its use of the principles of other sciences**

Nothing makes sense in biology, except in the light of evolution  
- Theodosius Dobzhansky 1973

*In this introduction we distinguish Darwin’s own work<sup>4</sup> from Darwinism in general, defining Darwinism here, in a first approximation, as adherence to the three fundamental principles of replication, variation, and selection. We tell briefly some episodes of the history of Darwinism to illustrate that, although these principles are necessary, they are not sufficient to account for the possible success of Darwinian explanations. This is important, because the success of Darwinian theories outside of biology need not be guaranteed by the application of the three principles.<sup>5</sup> To account for the actual success of Darwinism in biology we need to consider sociological factors and the discoveries in other sciences as well, so the same will probably hold for Darwinism outside of biology.*

*In our historical sketch we will point out that since the second half of the 20<sup>th</sup> century there has been no serious critique of or alternative to Darwinism in biology, although serious modifications of its fundamental concepts have occurred. The consensus that arose around that time, however limited or how universal historians wish to portray it, was called the Modern Synthesis after the highly influential 1942 book with the same title, The Modern Synthesis,*

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<sup>4</sup> Darwin’s own work is of course also far from homogeneous and has remained in development until his death. See Desmond and Moore 1991.

<sup>5</sup> Or at least should. Non-explanations or otherwise false explanations that call themselves Darwinian can have popular success in society or even the scientific community without adherence to the three principles, but from a normative perspective we nevertheless maintain that this adherence is a minimal requirement for a successful Darwinian explanation.

by Julian Huxley. We argue that Darwinism indeed ‘synthesizes’ the results of the many different branches of biology in two senses of the word ‘synthesis’: a) although the different branches use very different principles the Darwinian principles are shared, i.e., common to all, and b) Darwinian explanations give ultimate instead of proximate explanations of biological phenomena and these ultimate explanations are overarching explanations in the sense that they use proximate explanations within their own explanatory scheme. We argue further, however, that Darwinism does not synthesize the results of these branches in some top-down sense, as in deductive models in general and in Hempel’s deductive-nomological model in particular, where the most general laws and initial conditions suffice to determine the state of the system or the object studied and scientists were thought to be searching for the most general laws synthesizing earlier, less general laws. On the contrary, Darwinism only provides the ultimate framework with which the different branches have to be in conformity, but these branches can have their own special principles as long as these principles are not in conflict with the framework. Moreover, the theories of proximate mechanisms from these branches are fundamental auxiliaries that connect the Darwinian story about a certain phenomenon with empirical results and thereby make Darwinism testable. Analogously, we can expect that Darwinian explanations outside of biology need to be complemented by theories of the proximate mechanisms of other sciences. However, these auxiliary theories may not conflict with the general framework of Darwinism outside of biology.

### **Evolution ancient and modern**

Ideas of the evolution<sup>6</sup> of life are almost as old as mankind. In many pre-scientific cultures we find mythical stories about how animals arose out of matter or proto-animals, and young children when asked where animals come from often advance remarkable ideas of their own. However, despite the naturalness of the idea of evolution, Darwin’s theory as set forth in *The Origin of Species* seems to have arrived late on the stage, only 150 years ago. One reason for this apparent tardiness is that Darwin’s theory is not just some vague intuition about slow transformations of species into each other or out of matter, but a carefully thought-out, empirical theory on the origins of species. We describe here some episodes of the history of the transformation of vague intuition into the empirical theory of Darwinism we know today. We give a list with all the benefactors and obstructors of the success of his theory in his own day, focusing in more detail on how Darwinism overcame classical teleological explanations. We do this because Darwinian explanations outside of biology sometimes reintroduce teleological principles (or related principles such as intentionality) and when this is not done carefully it might create serious tension in the heart of the Darwinian explanation. After the discussion of Darwin’s theory in his own time, we focus on how it was tested and stood up against the competition after Darwin’s death. This investigation will demonstrate that Darwin’s theory was not a uniform whole and cast some light on how it was tested, providing insights which we use in §1.3. Finally we focus on the nature of the modern Darwinian synthesis that resulted in the 1940s.

### **The paradox of mechanical teleology**

In the 18<sup>th</sup> and the beginning of the 19<sup>th</sup> century there arose much controversy about the origin of species and the origin of life. Numerous theories were proposed by naturalists and philosophers alike to explain these origins. To mention just a few: 1) some simply proposed that the original forms of life arose out of fortuitous random

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<sup>6</sup> ‘Evolution’ here in the very broad sense of a long process in which development occurs.

combinations of inorganic matter; 2) others stuck to the idea that God created the first organisms, which later adapted to local circumstances, an idea to which some added the twist that parts of creation had gone extinct in subsequent catastrophes resulting in a succession of different worlds filled with organisms; 3) again others held a cyclical view of development claiming that animal forms evolved all by themselves from simple to complex and sometimes even back again; and 4) still others claimed that there are an original form or *Bauplan* of organisms that was realized (“developed”, literally “e-volved”) to some degree of perfection in the animals around us, 5) without necessarily thinking that this realization process was a chronological process as some pictured this idea as the simultaneous realization of different degrees of the *Bauplan* in coexisting animals.<sup>7</sup> These theories were speculative and suffered from one important problem: the most successful science of that time was Newton’s mechanical theory. This theory criticized Aristotelian forms, especially for their final, teleological causality which pictured the forms as pseudo-agents of goal-directed change in a way that violated linear chronology as the end-product (the form in actuality) and its effects were somehow prior to the undifferentiated matter at the beginning (the form in potentiality). However, without formal and final causality, theories about the origin of life did not seem to get off the ground, because they could not explain the obvious *design* of organisms. Thus it seemed one either had to deny the claims of Newtonianism or give up explaining design. Theories like 1), according to which life arose in a mechanistic way due to some fortuitous aggregation of inorganic parts, were the only ones to escape this dilemma, but they were just too good to be true.

The failure of mechanistic philosophers and naturalists to explain design paved the way for theologians from the tradition of natural theology to use the clearly observable design as *the* argument for the existence of a supremely good creator who planned things this way. The most famous of these theologians today is William Paley, at whose work Darwin directed his attack. Darwin was able to formulate his attack because he saw a way to avoid the paradox; he came up with a way to explain design in a mechanistic universe without invoking formal or final causality; instead, he invoked the principle of selection: selection is a form of mechanical causation that does not violate chronology but that, when accompanied with variation and replication, can explain design or as it could now be called as-if or pseudo-teleology.<sup>8</sup>

By finding a way out of this paradox Darwin satisfied a clear, *long-standing need* to include a theory of the origin of species within the mechanistic worldview. That is an important point to keep in mind, because we can wonder whether Darwinian theories outside of biology also fulfill such a long-standing *need*, or

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<sup>7</sup> Not a single naturalist or philosopher held exactly one of these five views, but elements of these theories can be found in (numbers corresponding to the five theories mentioned): 1) Maupertuis 1745/6; Hume 1779; 2) Buffon 1749, 1778 ; 3) Cuvier 1800-5, 1796; Lamarck 1809, 1802; 4) Geoffrey St. Hilaire 1822, 1818, 5b) (a-chronological) Goethe 1790, Hegel 1830,1818; Richard Owen’s position was somewhat inbetween 4) and 5). See for a general overview of biology in the 18<sup>th</sup> century: Grene and Depew 2004, chapters 2-5 and Ilse Jahn 2004.

<sup>8</sup> The first source in which we found the term ‘pseudo-teleology’ was Huxley 1942, chapter 8. The term ‘as-if teleology’ might go back to Kant’s Third Critique in which he seeks to combine the Newtonian worldview with teleological terminology by prioritizing Newton (as he had done in the First Critique) and reducing teleology to a subjective projection upon the world of an I who seeks unity in his experiences of it. Teleology thus became not a true, direct property of the world as it appears to us, but a secondary projection of a cognitive faculty in us upon it, i.e., teleology became as-if teleology. In §1.23 we discuss the Darwinian notion of teleology and functionality in more detail.

whether they instead are just artificial projections of a successful theory in one domain upon another without a plan behind them about what they need to solve. Be that as it may, our discussion of the paradox has at least shown that one should be careful when reintroducing some form of teleology into a Darwinian explanation outside of biology, because *the rejection of teleology is essential to Darwinian explanation*.

### **Benefactors and obstacles**

Darwin's success is not solely due to his solving the paradox of how to combine mechanistic and (pseudo-)teleological concepts. Other factors were responsible for this success as well. We will first list i) some of the *external* obstacles and benefactors – both sociological and intellectual, in related and compatible branches of science and philosophy – to the success of Darwin's theory and then ii) the factors *internal* to the life and work of Darwin himself. Again, these observations are to remind us of the plethora of factors upon which the success of a Darwinian explanation, in or outside of biology, depends. In this vein, we will list many factors that helped and obstructed the construction of Darwinian explanations regarding the origin of language in chapter 3, before we turn to these theories themselves in chapter 4.

- i) Peter Bowler's book-length account *Darwinism: the history of an idea* (2003) lists several *external* benefactors of and obstacles to the success of Darwinism. The list we present here overlaps partly with Bowler's, but adds some more general elements. We will not spell out why something is an obstacle or a benefactor (often this is self-evident) and to what extent (a task left to the historian).
  - a. Benefactors included: the existence of a scientific research community and all that it presupposes in Northwestern Europe, the criticism of theology in general and of revealed religion and the Bible in particular, the general Enlightenment idea of history as progress, the Newtonian paradigm of science as applied mathematics, the mechanical picture of the world as a causally closed clockwork-like system without formal and final causes, discussions about geological time and the age of the earth in sharp contrast with the 6.000 years the Bible was said to presuppose, the work of Lyell on geological processes and his anti-catastrophistic uniformitarianism supposing that the same principles are operative today as millions of years ago, the ideas of contemporary English economists, especially Adam Smith and the invisible hand metaphor he used to illustrate how those who seek wealth by following their individual self-interest inadvertently stimulate the economy and assist society as a whole, i.e., how selfishness of individual actors can be the basis of a capitalist economy that seemed to function better than any earlier economic system, Malthusian ideas about over-population and its threat to the different strata of society, recognition of the process and the results of artificial selection regarding crop cultivation and animal husbandry, the existence of other theories about the evolution of species and local adaptation, metaphysical ideas about the chain of being or the *scala naturae* and its replacement in the 18<sup>th</sup> century by morphological analysis, more specific biological ideas about a common structure (often called type or form) behind the



apparent diversity of organisms (which Darwin could reinterpret as being due to common ancestry), the discovery of organs which did not or no longer seemed to have a function (i.e., imperfect design), the discovery of fossils and their correct identification as the remnants of the ancestors of contemporary organisms, the discovery of extinct species, the discovery of animals from different continents with radically different properties (including animals from Australia as the platypus and the kangaroo).

- b. Some of the obstacles were: Aristotelian teleology which was still current amongst some biologists, the belief – religiously motivated or not – in the creation of (fixed) typological species, the idea of determinate causation which seemed to leave no role for chance in science, the supposed unique place of man in creation as a rational and moral creature whose existence alone seemed to defy any naturalistic or materialistic ideas about man as a whole, the idea of God who took care of his creation, the absence of a good theory of heredity<sup>9</sup>, the absence of a theory of generation of animals and many gaps in the fossil record.

Without these benefactors on the one hand and the obstacles on which Darwin's theory could test its strength on the other, it would never have had the success it did in fact have.

- ii) Apart from biographical factors,<sup>10</sup> internal factors that explain the success of Darwin's work are mainly two-fold. The first is a) that the task Darwin set himself was limited, and the second is b) that his theory was so rich in empirical detail.

- a. The task Darwin set himself in his most famous book *The Origin of Species* was limited. This can be seen when we realize that the title of Darwin's book is a misnomer – not only because the word 'species' in the title is ambiguous (Darwin gave a new sense to the word 'species' in strong contrast with the traditional meaning of the word denoting fixed eternal species), but also because he did not explain their *origin* as such. Darwin started somewhere in the middle of the history of life, explaining how new species could arise out of earlier

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<sup>9</sup> The theory of heredity prevalent in Darwin's age was blending inheritance. This is an intuitive notion: if you blend a large father with a small mother you will get children of average length. Blending inheritance will thus always tend to conserve an ideal (average) form, whereas Darwin needed a theory of heredity that allowed for significant changes over time.

<sup>10</sup> Biographical factors can hardly ever give a sufficient explanation why of all people this very person made a certain discovery. However, they can make it plausible why Darwin formulated the theory he did. Some of these factors are the pioneering work of his beloved grandfather and freethinker Erasmus Darwin on the origin of species, Darwin's voyage of the Beagle visiting the Galapagos isles and islands where primitive men had never seen English civilization before, his discovery of the different species of finches at the different isles of the Galapagos, his experience of a volcano outbreak near the Andes which convinced him of the enormous events that could take place over geological time, his friendship with many prominent zoologists, his respect for the work of Lyell, his possibility to observe the first orang utan in at display in the London Zoo etc. See Desmond and Moore's definitive Darwin biography of 1992.

- species, and left the question how species as such arose (i.e., the question of the absolute origin of life).<sup>11</sup>
- b. Darwin was not the first to formulate the three principles of his theory, he was well aware. He always generously acknowledged others as co-discoverers of the theory of evolution or natural selection, maybe often too generously as if he wanted a broad consensus for his revolutionary ideas. In an extra chapter in the sixth edition of *Origin*, Darwin mentions 34 of his 'co-discoverers', starting with a suggestion of Empedocles (wrongfully attributed to Aristotle by Darwin) and including the speculations of his own grandfather Erasmus Darwin, the work of Buffon and Lamarck (the leading naturalists of the previous generation), and of course Alfred Russel Wallace whose work enticed him to quickly publish his own findings. Darwin even mentioned two little-known scientists W.C. Wells and P. Matthew who had not only held that species are changeable and that man uses this fact in cultivation, but even clearly formulated the idea of natural selection. Later historians identified even more predecessors.<sup>12</sup> However, the identification of these predecessors by Darwin and others is often rather anachronistic insofar as at least one of the three principles is absent from their work, or the relations among the principles is ill-conceived. Moreover, we just listed the benefactors of the success of Darwin's theory on the basis of which we might argue that the three principles alone (in isolation of other scientific results or sociological processes) do not suffice to account for the success of Darwin's work. What makes Darwin really stand out from his predecessors is in our view that Darwin's book contained more than just a small suggestion. Rather, it constituted "one long argument"<sup>13</sup> backed by *a lot of empirical material*, describing both what natural selection is and how this process could have occurred in actual fact. Only as such this book was able to be the instigator of a scientific revolution.<sup>14</sup>

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<sup>11</sup> At least in his major writings in press. Darwin did famously write in a letter to Joseph Dalton Hooker on February 1<sup>st</sup>, 1871, "It is often said that all the conditions for the first production of a living organism are now present, which could ever have been present. But if (and oh! what a big if!) we could conceive in some warm little pond, with all sorts of ammonia and phosphoric salts, lights, heat, electricity, etc. present, that a protein compound was chemically formed ready to undergo still more complex changes, at the present day such matter would be instantly devoured or absorbed, which would not have been the case before living creatures were formed." Similar observations about the limited scope of Darwin's own theory can be found in Dennett 1995: 42-48.

<sup>12</sup> See for instance Glass 1959, Bowler 2003, 1989, Gould 2002, 1987.

<sup>13</sup> Darwin used this phrase himself to characterize his book *The Origin of Species*. Ernst Mayr (1982) wrote a book with the same title analyzing the *Origin* in detail.

<sup>14</sup> we can completely agree with Dennett as he summarises: "Cute ideas of evolution had been floating around for millennia, but (...) although they did seem to offer a solution of sorts to the problem at hand, they didn't not promise to go any farther, to open up new investigations or generate surprising predictions that could be tested, or explain any facts that they weren't not expressly designed to explain. The evolution revolution had to wait till Charles Darwin saw how to weave an evolutionary hypothesis into an explanatory fabric composed of literally thousands of hard-won and often surprising facts about nature. Darwin neither invented the wonderful idea out of whole cloth all by himself, not understood it in its

Without the limited scope and the huge amount of empirical data Darwin would not have been successful. This is something to remember when we review Darwinian theories outside of biology: a Darwinian story is not enough, it requires a lot of empirical backing, especially regarding proximate mechanisms we will describe at the end of this section.

### **The eclipse and rise of Darwinism**

Despite the fact that there was a clear need Darwin's theory answered and despite the benefactors, the well-chosen scope of the theory and the empirical material adduced in favour of it, Darwin's theory did not gain much currency in the nineteenth century and the beginning of the twentieth century. Darwin's work led to the rapid acceptance of *evolution*, but the central element of his theory: the proposed *mechanism of natural selection* was not widely accepted. Bowler in his 1983 *The eclipse of Darwinism* has documented the critique on Darwinism in this period.<sup>15</sup> Although Darwin found some ardent supporters such as Thomas Huxley, most biologists in this period either supported i) some form of theistic evolution (God's initial plan laying behind evolution or God intervening in the process of evolution), ii) orthogenesis (an innate drive for change towards some future goal state), iii) Neo-Lamarckianism (inheritance of acquired characteristics) or iv) saltationism (sudden large mutations or jumps instead of gradual evolution). Other non-biologists, such as Herbert Spencer in England, Ernst Haeckel in Germany and Teilhard de Chardin in France, included Darwin's theory in their larger work about history as a progressive process. Their work can be seen as early Darwinism outside of biology in the bad sense of the word as they changed Darwin's ideas in the process into pseudo-Lamarckian (Spencer and Haeckel) or spiritualistic orthogenetic ideas (De Chardin). Even Darwin himself did not stick to his theory from the first edition of the *Origin*; in subsequent editions of the *Origin* his position shifted gradually and in other published works even more dramatically in the direction of Lamarckianism, a theory whose validity he never denied.<sup>16</sup>

The reasons Darwin's theory was resisted were numerous. Some of the obstacles we identified above were simply too strong. Furthermore, natural selection with its emphasis on death and competition did not appeal to many naturalists because they felt it was immoral and because it seemed to leave little room for purpose and true progress in the development of life. Others felt that natural selection would be too slow a mechanism given contemporary estimates of the age of the earth and sun, and some – especially paleontologists – thought the predictions about the fossil record implied by Darwinism were not confirmed.

We cannot go into the details why, despite this initial resistance, Darwin's theory finally became virtually unchallenged within biology (see the detailed account in Bowler 2003: 196–253 and Larson 2004: 105–129). We will, however, describe why the competing theories left the stage and how the modern Darwinian synthesis

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entirety even when he had formulated it. But he did such a monumental job of clarifying the idea, and tying it down so it would never again float away, that he deserves the credit if anyone does (Dennett 1995: 33)."<sup>15</sup>

<sup>15</sup> See also Gould 2002 part I, Zimmer 2001, Larson 2004 and a little bit older but still useful: Mayr 1982.

<sup>16</sup> See Gould 2004. The Lamarckian undertone becomes strongest in Darwin's later works, *The Expression of Emotions in Man and Animals* (1872) and *The power of movement in plants* (1882). If one would characterize Darwinism as the denial of the existence of Lamarckianism (amongst other things), one would have to call Darwin's own work anti-Darwinian.

came about, since those stories are essential to our assessment of the strength of Darwinian explanations in §1.3.

i) Theistic evolution gradually disappeared from the scene as scientists became more committed to the idea of methodological naturalism and started to understand that appeals to divine involvement were scientifically unproductive. Theistic evolution lives on in the circles of for instance the supporters of Intelligent Design, but has no following in 21<sup>st</sup>-century mainstream biology.

ii) Orthogenesis was popular amongst paleontologists because they thought they had discovered gradual and constant unidirectional change in the fossil record. Early on the fossil record was too fragmented to support Darwin's theory of non- or at least not-necessarily-uni-directional change as there appeared to be large gaps, but in the 1860s and 1870s many important fossils were discovered – amongst them some that suggested that birds had evolved from reptiles (Larson, 2004: 140). At that time, evolution appeared to be progressive. More fossil discoveries and a more careful analysis of them, however, collapsed the orthogenesis hypothesis because many patterns in the fossil record were non-linear.<sup>17</sup> On the more philosophical front, orthogenesis was also challenged as a form of theistic or teleological evolution in disguise.

iii) Neo-Lamarckianism remained very popular until the end of the nineteenth century, although critics had pointed out that no one had ever produced solid evidence for the inheritance of acquired characteristics. It seemed to rest on the natural idea of human generations inheriting things from their parents in an aristocratic society with little upward mobility, the rich and healthy remaining that way. A serious blow to Neo-Lamarckianism came as the German experimentalist August Weismann came up with his germ plasm theory of inheritance and the idea of what we now call the Weismann barrier preventing any changes that took place in the body after birth from being inherited by the next generation.<sup>18</sup> Today, work (sometimes called Neo-Lamarckian) on epigenetic inheritance and soma-to-germ-line feedback questions the validity of the Weismann barrier, but this work no longer questions Darwin's theory as such since the proposed Neo-Lamarckian mechanisms are parasitic on normal germ line replication (see §1.21). However, Neo-Lamarckianism does come up in evolutionary theories outside of biology and as such is an important competitor of Darwinian theories outside of biology.

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<sup>17</sup> George Gaylord Simpson's work of 1944 *Mode and tempo in evolution* convinced his fellow paleontologists that orthogenesis was untenable. More recently, Van Valen plotted species survivorship curves on an arithmetic scale (number vs. time) finding a linear relationship (Van Valen 1973). This means that the chance that a species will go extinct is independent of its age. Orthogenesis on the other hand would presuppose that later-evolved species have higher chances of survival. Although Van Valen's paper was controversial it inaugurated much more research, which confirmed his thesis. See also Ridley 2005: 637.

<sup>18</sup> Dawkins 1982 cites many foremost biologists who claimed that Lamarckianism is a valid mechanism to explain the evolved design of organisms but that only Weismann showed that Lamarckianism was empirically untenable. Dawkins himself, however, tries to argue a-priori that even if Weismann had not come up with his barrier, Lamarckianism must still be untrue because in contrast with Darwinism this theory of acquired characteristics cannot explain how many functional features of organisms could have evolved (how does training of the eye muscles involve the evolution of an eye lense?) and how they are inherited (how does training my legs influence my gametes?). We think this criticism is valid. We do believe, however, that the general portrayal of Lamarck's experiment by Dawkins is historically inaccurate. Weismann's Lamarckian opponents criticised Weismann's 'artificial' experiments because they thought he only demonstrated that *harmful* mutations acquired by parents could not be inherited by their offspring, but had not shown that the same holds for *beneficial* mutations. Until after the Second World War some strongholds of Lamarckianism existed in continental Europe, especially in France.

iv) Finally, saltationism became less popular as paleontologists filled more gaps in the fossil record. Early geneticists such as Hugo de Vries and William Bateson also relied heavily on saltationism, thinking that evolution was due to discrete mutations in genes (ignoring selection as a normal force shaping forms but only removing monstrous mutations from the population). However, later genetic studies revealed that many mutations had relatively small effects and this meant the end of saltationism.

### **The modern synthesis**

The competing theories were thus slowly defeated. Other factors were beneficial to Darwin's theory too. In this Whiggish history<sup>19</sup> of the success of Darwinism the following factors need mentioning. First of all Mendel's theory of heredity, rediscovered in 1900 by two or three geneticists independently, which answered the need for a non-blending theory of heredity. Second, the work of the biometricians around the turn of the previous century which found a continuous range of variation in various characteristics that they measured in wild populations. Because of this continuous range the biometricians opposed the discrete units of heredity as postulated by Mendel's supporters. Third, the synthesis of the work of Mendel's supporters and the biometricians by the biologist and statistician R. A. Fisher in and around the 1920s which showed that the continuous variation measured by biometricians could be due to the combined action of many discrete genes. Fisher also showed how natural selection could change gene frequencies in a population and thus drive evolution. His work, along with that of J. B. S. Haldane and Sewall Wright (with the latter's appreciation of the importance of stochastic processes in evolution), founded the discipline of population genetics, which integrated Mendelian genetics into the theory of natural selection.

The biologist Theodosius Dobzhansky further bridged the gap between the theoretical and mathematical work on population genetics with the work of field biologists by examining the genetic diversity of wild populations, demonstrating (contrary to the opinion of many population theoreticians) that wild populations had large amounts of genetic diversity with clearly distinguishable differences between sub-populations. Finally, the biologist and taxonomist Ernst Mayr emphasized the importance of *allopatric speciation* in the formation process of new species: according to him new species formed because of geographical isolation which soon resulted in reproductive isolation, a process in which the number of species multiplied (cladogenesis) instead of a process in which one old species transforms gradually into a new one (anagenesis). All these developments led to the emergence of a new overarching, orthodox view, which received its name from a 1942 book by Julian Huxley *Evolution: the modern synthesis*.

We define the modern Darwinian synthesis as a set of ideas from several biological specialties that were brought together to form a unified theory of evolution accepted by the great majority of working biologists. It can be summarized as: genetic

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<sup>19</sup> Whig history presents the past as an inevitable progression towards ever greater liberty and enlightenment, culminating in modern forms of liberal democracy and constitutional monarchy. A Whiggish history is a history from the perspective of the victors, who rewrite their own history in a heroic, hagiographic tone as a progression towards the current state with its proponents as 'good guys' who are on the side of truth (as we now know it) and 'bad guys' who opposed the emergence of these truths because of ignorance or bias. Whiggish history contrasts with true historical research which shows the spectrum of other better or worse competitors and why their attempts to become dominant shipwrecked, accidentally or not.

variation in populations arises by chance through mutation and recombination. *Evolution consists primarily of changes in the frequencies of alleles between one generation and another as a result of genetic drift, gene flow, and natural selection; speciation occurs gradually when populations are reproductively isolated, for example by geographical barriers.* Recent historians have stressed the limited consensus amongst the supporters of the modern synthesis (i.e., Gould 2004), yet at least to the outside world the biological sciences including paleontology, population statistics, (Mendelian) genetics, field biology, taxonomy and ecology seemed to present a unified picture of evolution.

After the Second World War the picture the modern synthesis offered was put on an even firmer basis by the discovery of the *double* pair of chromosomes in every cell and the *double* helix structure of DNA by Watson and Crick in 1953.<sup>20</sup> After that, the biological field moved away from the picture offered by the modern synthesis as more and more heterogeneous elements were added to this picture, such as kin selection and the gene-centred view of selection, new speculations on nearly neutral selection and genetic drift at the molecular level, theories of punctuated equilibrium, new views on viruses and microbes and their evolution, new mechanisms of genetic transfer like horizontal genetic transfer and pseudo-Lamarckian mechanisms, symbiogenesis, evo-devo, the criticism of adaptationism, etc. We will discuss many of these developments as we discuss the understanding of the basic principles in the next section.

Because of these more recent developments the understanding of the basic principles by the scientific community diverged again so that the synthesis was but a node where research fields temporarily converged. Nevertheless, the picture the synthesis offered is still strong enough that different subfields still take it as their starting point even if they argue that many elements of the consensus picture are incorrect and need amending. Before we turn to the details of the modern picture, we concentrate on how Darwin's theory was able to synthesize the various fields of biology.

### **Reductionism and the modern synthesis**

What makes the modern synthesis a synthesis? This question is important because a discussion of the nature of the synthesis here will prevent us later from falling into the traps and misconceptions of theoretical positions that too bluntly claim to have a fundamental theory that unifies multiple domains. As such this discussion can help us decide what sort of synthesis *universal* Darwinism or Darwinism outside of biology is or should be.

We start answering this question by discussing the traditional picture of (theory) reductionism and showing why this picture does not characterize the nature of the modern synthesis; we then use Tinbergen's famous 1963 paper on the methodology of the biological sciences to describe the nature of Darwinian explanations within the domain of biology; finally, we discuss how good Darwinian explanations as ultimate explanations synthesize, in a weak sense of the word, the proximate mechanisms studied by other sciences in their own explanatory scheme.

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<sup>20</sup> Mendel's theory required that for every hereditary element two – possibly different – copies were available within a cell. This was corroborated by the existence of two pairs of chromosomes which both had one so-called allele of a certain trait of the phenotype. During replication the double helix is broken open and both sides of the helix provide in this way a template for a new double-helix of DNA.

## **Ontological and theory reductionism**

In classical philosophy of science the concept of synthesis is closely related to the concept of reductionism (Hempel 1967, cf. Nagel 1961). Reductionism can be divided into ontological and theoretical reductionism. Both types can be subdivided into many subtypes. Ontological reductionism is the idea that everything that exists is made from a small number of basic substances or elements that behave in regular ways, but ontological reductionists quarrel on the exact nature of 'higher order' entities such as organisms or societies and emergent properties (often calling themselves or blaming others for being anti-reductionist); theoretical reductionism is either i) the idea that the *terms* of a theory of science A referring to objects at a higher level of complexity than the objects of science B can be replaced by the terms of science B or ii) the idea that true domain specific theories are reduced to more general overarching theories, an idea usually accompanied by the claim that older theories or explanations are not generally replaced outright by new ones, but that new theories are refinements or reductions of the old theory into more efficacious forms with greater detail and explanatory power. The corpuscular theory of matter of the 17<sup>th</sup> century is an example of ontological reductionism; the idea that all details of chemistry can be described by physics (true or not) or the idea that the gas laws of Boyle and Van der Waals can be described in terms of statistical physics are examples of theoretical reductionism in the first sense; the view that Einstein's theory adsorbed the results of Newton's theory as a proximate case of its own theory that made more accurate predictions is an example of theoretical reductionism in the second sense (whether this simple picture of the relation between Einstein or Newton is completely correct or not). Both versions of theoretical reductionism are at home within the classical Deductive-Nomological (DN)-model of science, which characterizes scientific explanations primarily as deductive arguments with one or more natural law statements among its premises combined with initial conditions. In the DN-model, theoretical reductionism just came down to finding more general laws from whose premises together with initial conditions the older theories, laws and facts as well as new results could be derived (Salmon 1990).

A reduction can have aspects of a synthesis if it integrates two separate, hitherto-unrelated realms into the frame-work of a new theory. A classical example is Newton's law which synthesized the result of Galileo's law of free fall and Kepler's laws of planetary motion into Newton's own reductionistic theory of gravity (again whether this is historically accurate picture or not is moot here).<sup>21</sup>

Given this short elaboration on reductionism and synthesis we can ask what kind of synthesis the modern synthesis was and whether we are dealing with a kind of reductionism or not. Not really with an ontological reduction. True, the modern synthesis is an ontological reduction or even an ontological elimination insofar it eliminated final goals, vitalist forces and big saltationist jumps from biology which respectively orthogenesis, vitalism, and saltationism presupposed. These eliminations are, however, not *at the core of the modern synthesis*, but part of the slow replacement of older biological theories and their theoretical entities and concepts by Darwinism a century earlier. This replacement can thus be called a reduction, but this

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<sup>21</sup> There is an element of ontological reductionism in Newton's synthesis: Kepler's planets (which Kepler thought of as intelligent beings that calculated their perfect orbits) and Galilei's qualitative forms of matter (Galilei believed that earth-matter belong to the earth, Venus-matter to Venus etc.) were reduced to ordinary masses in Newton's theory (one could also call this eliminative reductionism because Newton's theory simply eliminated reference to special forms of matter and planetary intelligences). Primarily, however, Newton's synthesis is a case of theory reductionism.

reduction has no aspects of a synthesis.<sup>22</sup> The modern synthesis as synthesis brought for instance the hitherto un- or hardly-related fields of genetics and ecology together, a process that cannot be understood as an ontological reduction: one might be tempted to view this as a reduction of all biology to *genes* (especially after the 1953 discovery of DNA). However, the modern synthesis was not at all like the strongly reductionist gene-centered view of selection of the seventies, which itself is hard to call a true, successful reduction of biological properties to genes or molecules either since it (still) fails to uniquely map the two notions of genes (i.e., material DNA versus phenotypical properties (see §1.21)). Hence, the modern synthesis is not a synthesis due to a theoretical reduction of multiple realms to one new one, since the principles of genetics, ecology, and taxonomy are not truly *reduced* to those of Darwinism, let alone *deduced* from them.<sup>23</sup>

The traditional DN-model and the picture of synthesis based upon it work to some extent in physics but do not seem to suit biology or Darwinism.<sup>24</sup> First of all because biology *as such* has no laws, or at least none that are as prominent as the ones in physics where the same *fundamental* laws are thought to hold for *all* physical phenomena one ever wants to describe.<sup>25</sup> Biology works not (primarily) with laws,

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<sup>22</sup> One can say that Darwinism has shown that biology needs no extra forces beyond the mechanical forces of physics to explain design and that organic matter is therefore the same as inorganic matter. One might then be tempted to reduce biological theories to physical theories. Dennett 1995 calls this greedy reductionism, a form of reductionism that goes so far that it becomes useless. Yes, Dennett says, one can analyze the difference between an IBM-computer and a MAC in terms of zeroes and ones, yet that is not the appropriate level at which it is meaningful to analyze this difference. Moreover, in §1.23 we argue that the principle of (biological) selection is unique to biology and can not (or at least not readily) be reduced to the principles of physics. For an example of people who held a strong form of reductionism, see the Harvard Dean of the biology faculty Wald as quoted in Mayr 2004: 61 who says that “the whole of biology” is indeed “nothing but molecular biology”.

<sup>23</sup> In early philosophy of biology theoretical reductionism was popular and actively promoted by for instance Schaffner 1969, 1967 and Ruse 1976, 1973, 1971, and more recently and more carefully by Rosenberg 1985 (but see his 2001 on reductionism without laws). Authors like these either try to argue for theory reductionism by reducing biology to physics and chemistry or by reducing higher-level biological theories to lower-level biological theories.

<sup>24</sup> Classical philosophy of science offers two other models for scientific explanation: the inductive-statistical model and the historical model. But the biology of the modern synthesis as a whole is not about statistical inductions, although many biological rules are indeed statistical generalisations. One might say that it fits the historical models better insofar as it is about natural history and it often gives explanations of unique phenomena (the evolution of man etc.), just like ordinary historical explanations. Some Darwinists indeed call Darwinian explanations historical narratives (i.e., Mayr 2004, chapter 2). Yet in normal historical explanations intentions and regularities about behaviour play a key role (Caesar crossed the Rubicon because he wanted to become Emperor (intention), since men generally aspire to be in power (regularity)). However, the explanations of the modern Darwinian use neither intentions to explain natural history nor regularities. I therefore think that explanations in biology are in a league of their own, better not mixed with those of history. See Mayr 2004 *What makes biology unique?: considerations on the autonomy of a scientific discipline*. See also Williams 1981, 1980, Rosenberg 1978, Ruse 1977 and Ayala 1968. See finally §1.23 on the unique role of the principle of selection within biology, absent from (most) other sciences.

<sup>25</sup> The laws of Mendel, the Hardy-Weinberg law, rules about the speed of selection such as given by Simpson or the rules of r-K selection, etc. (see §1.3c) can be called biological “laws”, but they are not laws in the same sense as the fundamental laws of physics. The so-called laws of biology are more like “local generalizations” or rules of thumb: many genetic features obey Mendel’s equations, but many others do not (deviations from the law are for instance used to test assumptions of independent assortment), the Hardy-Weinberg law only works for isolated populations which mix perfectly and in which no selection pressure is active (deviations from the law are used in practice to test the assumptions of no selection and random mating). Moreover, both laws are more like rules of combinatorics applied to the situation of mating and gene distribution than true biological laws holding for all biological phenomena whatsoever as do the fundamental laws in physics for all physical phenomena. Some nevertheless redefine the concept of law to include what we call “local generalizations” (see Lange 1995, Sober 1993), but that is more of a



but with principles<sup>26</sup> and thus the classical picture of reductionism, as based upon physics, does not work in a straightforward way in the case of biology.<sup>27</sup> Moreover, the synthesis of biology is not a case of theory reduction of theories from multiple fields to one new theory, since theories from different biological fields remain relatively autonomous<sup>28</sup> and can at least not be derived from each other or from the three Darwinian principles.

### **Synthesis as consilience, shared principles and Tinbergen's questions**

But if the 'synthetic nature' of the modern synthesis's encompassing the whole of biology is not easily grasped with the tools of classical philosophy of science, how should we understand it? A first, simple, answer could be that the synthesis meant just that people from different fields reached the *consensus* defined above. However, the synthesis was more than just a group of people sharing the same view. The synthetic aspect of the consensus comes from the fact that people from different, hitherto loosely-related disciplines, whose ideas were once in conflict with each other, now reached agreement. One could thus be tempted to characterize the synthesis as *consilience*<sup>29</sup> and we think that is partly right and partly wrong: wrong because the different branches are not completely autonomous (as consilience traditionally demands) since they use the same three Darwinian principles, right because they use principles of their own apart from the Darwinian principles and use them to reach the same conclusion. Our discussion of consilience will pave the way for our discussion of Tinbergen's four famous questions that set up a methodological scheme for the *biological* sciences. Tinbergen's list looks like a rhapsody of questions, but the concept of consilience can be used to characterize its unity. The concept of consilience will also be useful in our discussion of Darwinism outside of biology and

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semantic trick than really pointing out that there are laws in biology. Some grant the three principles of Darwinism itself nomological status (Eigen 1983), but this seems again nothing but a semantic choice (Brandon 1990). On biological laws see also Ruse 1970, Gould 1970, and McIntyre 1997.

<sup>26</sup> Laws are propositions or statements that hold for all phenomena at all time (for instance  $F=ma$  or the entropy  $S$  cannot decrease, i.e.,  $dS/dt \geq 0$  ( $dS/dt$  is bigger or equal to 0)), Principles are statements but fundamental causes or causal mechanisms such as replication, variation, and selection that can be substantiated in many different ways.

<sup>27</sup> More extensive refutations of the usefulness of the DN-model of theoretical reductionism to describe theory development in biology are given by Kincaid 1990, Kitcher 1984, and Hull 1974.

<sup>28</sup> The two uses of the concept gene by molecular biologists and population biologists are independent insofar as there do not exist "bridge principles" between the two uses of the word gene in the two different disciplines (Hull 1974). Because of this the two disciplines remain autonomous. See also §1.21 e) genes.

<sup>29</sup> The word consilience was first used by one of the pioneers of the philosophy of science, William Whewell in *The Philosophy of the Inductive Sciences* of 1840. In this book Whewell saw consilience as the correspondence of two inductions based on different classes of observations: "The Consilience of Inductions takes place when an induction, obtained from one class of facts, coincides with an Induction obtained from another different class. Thus Consilience is a test of the truth of the Theory in which it occurs." Whewell was a child of his time and the tradition of English empiricism in characterising scientific knowledge acquisition as a process of induction. That may be an outdated idea but the concept of consilience as a correspondence between two realms of science is still useful. The biologist and philosopher E.O Wilson revived the notion of consilience in his 1998 book with the title *Consilience, the unity of knowledge*. In his view consilience is not so much about true (exact) correspondences of results of two different realms, for him mere consistency between the results of realms was sufficient to use the term consilience. We use the word consilience in a sense akin to Wilson's, but with more emphasis on the process of interaction between scientists in the different fields in achieving a mutually consistent story. Consilience as consistency can never be the sole hallmark of truth, though, and neither can it be the sole criterion for theory choice. See §1.3b.

universal Darwinism, because here issues of consistency between and independency of (the results of) different branches of science are prominent too.

Consilience exists when two independent sciences or branches of science come to the same conclusion, for instance when both morphological comparison and radioactive analysis date a fossil in the same period. In the case of the modern synthesis, genetic analysis, population theory, and field biology reached the consilient conclusion that natural selection working on gene frequency variation present in natural population could account for the evolution of species. However, the modern synthesis is not such a crystal clear case of consilience in its traditional sense since we do not really have *more or less independent* scientific realms that reach the same conclusion: the example of radioactive analysis and morphological comparison is independent insofar as they use different principles (identification of the time era by proximity of form versus calculations of radioactive decay) and different observations (morphological parameters versus levels of radioactivity), but in the modern synthesis the different realms share many observations (replication rates etc.) and principles (functional view of organs etc.). Most importantly, *although the disciplines of the synthesis have their own principles, they all ascribe to the same three fundamental Darwinian principles.*

If this is true, the modern synthesis should be considered a *shared framework* consisting in the three principles of Darwinism. And indeed if one reads the work of the people from the different disciplines of the synthesis or the later Darwinians amending the received view of the synthesis one always finds a clear formulation of the three principles. This is true whether one reads Julian Huxley<sup>30</sup>, the man who coined the term ‘modern synthesis’, or the two population theorists John Haldane<sup>31</sup> and Ronald Fisher<sup>32</sup>, whether one studies the more practically orientated zoologist and taxonomist Ernst Mayr<sup>33</sup>, the field biologist Theodosius Dobzhansky<sup>34</sup>, or the paleontologist George Gaylord Simpson.<sup>35</sup> It is also true of later work which challenged and supplemented details of the synthesis such as the work of molecular biologist and Noble prize-winner Jacques Monod,<sup>36</sup> the complex mathematical work of John Maynard Smith<sup>37</sup> and William D. Hamilton<sup>38</sup>, the popularized version of Hamilton’s work by Richard Dawkins<sup>39</sup>, and their strong criticaster Stephen J. Gould<sup>40</sup>. It also holds for embryology where until recently Darwinian principles were

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<sup>30</sup> See Julian Huxley, *The Living Thoughts of Darwin* (1939), *The New Systematics* (1940) and especially *Evolution: the Modern Synthesis* (1942), all in many reprints.

<sup>31</sup> See John Haldane *The causes of Evolution* (1932), especially chapters 2 and 3.

<sup>32</sup> See R.A. Fisher *The Genetical Theory of Natural Selection* (1930).

<sup>33</sup> See Ernst Mayr *Systematics and the Origin of Species* (1942), *Animal Species and Evolution* (1963), *Populations, Species and Evolution* (1970).

<sup>34</sup> See Theodosius Dobzhansky, *Genetics and the Origin of Species, Evolution, Genetics, & Man* (1955) and *Genetics of the Evolutionary Process* (1970).

<sup>35</sup> See George Gaylord Simpson *Tempo and mode of evolution* (1944).

<sup>36</sup> See Jacques Monod *Le hasard et la nécessité* (1970).

<sup>37</sup> See John Maynard Smith *The Theory of Evolution* (1958), *Evolutionary Genetics* (1989) or (coauthored with Eörs Szathmáry) *The Major Transitions in Evolution* (1995).

<sup>38</sup> See William Hamilton’s collected papers, *Narrow Roads of Gene Land*, volume 1 (1996), 2 (2002) and 3 (2005).

<sup>39</sup> See Richard Dawkins, *The selfish Gene* (1976), *The Blind Watchmaker* (1986) and *Climbing Mount improbable* (1996).

<sup>40</sup> See Stephen Jay Gould *The structure of evolutionary theory* (2004).

notably absent, but now the Darwinian-inspired discipline of EVO-DEVO is taking over the field.<sup>41</sup>

Thus *common, shared* principles in different disciplines are an important first sense in which the modern synthesis can be called a synthesis. However, the synthesis consisted not just in the formation of a group of people with the *same* fundamental ideas or the *same* background perspective on their field: this characterization of the synthesis is far too weak since there have existed many groups that started to share the same ideas but whose formation and ideas we would not call a synthesis. The more that is needed is that originally divergent, or hardly related ideas were brought together into a consistent story.

We thus think the nature of the synthesis can, ironically, be best described by looking at the *different* principles the scientists of disciplines entering the synthesis uphold *apart* from the three Darwinian principles, as well as at the consilient *interaction* between these two groups of principles. Let us compare Darwinian molecular genetics, Darwinian population biology, and Darwinian paleontology. All use Darwin's three principles but have principles of their own too: these partly determine how they use Darwinian principles, whereas the Darwinian principles often suggest what questions would be interesting to pursue for the different fields using their own principles. We give examples of the methodology and research topics of the three fields and put between square brackets the Darwinian principle operationalised in these examples. The field of molecular genetics uses the principles of (bio)chemistry and it has a lot of interest in neutral mutations and drift [zero selection], whole insertions of strings of DNA [forms of variation], as well as in the complex interaction of DNA, RNA, and proteins, the replication of non-nuclear DNA such as mitochondrial DNA and the replication of non-coding DNA which jumps from chromosome to chromosome [replication]. The field of population biology uses principles of combinatorics, Mendelian genetics, and game theory, and it looks for instance at deviations of the Hardy-Weinberg laws [a measure of selection], the competition between sub-populations [group level selection], and the diffusion of mutations in a population [variation and replication]. The field of paleontology uses principles of geology, nuclear physics, and morphological analysis to date and interpret the fossil record, and it looks for instance at extinction rates [selection], taxonomical distributions [variation], and lineages in the fossil record [replication chains].

Thus it seems the Darwinian principles and the field-specific principles mutually constrain each other. The Darwinian principles are substantiated by the field specific principles, while the use of the field's specific principles (the type of questions on which they are applied) is often guided by Darwinian principles.

Note that this does not imply that there is no tension between the results of the different fields using Darwinian principles: population dynamics often assumes a naïve Mendelian picture of mutation and discrete alleles, whereas molecular biologists know all about the complexities of mutation and the many intermediate levels

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<sup>41</sup> In the 19<sup>th</sup> century Ernst Haeckel tried to integrate embryology into Darwinism with his famous recapitulation theory stating that ontogeny recapitulates phylogeny, i.e., that an embryo goes in its development through the stages of the ancestors of his species. This hypothesis has been rejected, although the field of evolutionary development (Evo-devo) has shown how master- or Hox-genes vary little between major cladistic lineages, suggesting common developmental processes behind the embryonic development of widely different species. See for the history of the recapitulation theory Stephan Jay Gould's *Ontogeny and Phylogeny* (1980). A good account of Evo-devo is that of the embryologist and molecular biologist Sean B. Carroll entitled *Endless forms most beautiful* (2005).

between genes (alleles) and phenotypes; future research can, however, try to *align* these fields into a more *consilient* picture.<sup>42</sup> This consilience is possible because of the autonomy of the field-specific principles of the different branches of biology.

If we now look more closely at the nature of the mutual constraints, it might seem like a weak form of constraint: the three common principles of Darwinism constrain the different branches of biology so little that they can have very different *extra* principles. Sometimes it seems even that Darwinian principles are hardly used at all in a discipline, as anyone following a lecture on the many details of the *actual* workings of the cell will recognize. On the other hand Darwinian principles play such a fundamental role in biology that they are important for *all* actual subdisciplines of biology and *even constrain the nature of future* disciplines beforehand (as they did retrospectively, for instance, for sociobiology and EVO-DEVO). From this retrospective point of view the constraints are so strong that Dobzhansky could famously say that “*Nothing in biology makes sense, except in the light of evolution*” (1973).

The two examples of the actual workings of the cell and Dobzhansky’s quotation reveal something about the nature of the mutual constraint: many fields of biology describe the actual workings (we could call them *proximate* mechanisms) of biological processes, whereas Darwinism describes their origin *and* sustainment by so-called *ultimate* mechanisms. Whereas proximate mechanisms describe *how* something works, Darwinian ultimate mechanisms explain *why* they are there, i.e., because of what function they originated and are being maintained (this is Dobzhansky’s “why they make sense”). Since for almost every *current* or *actual* how-mechanism in biology one can ask why it is there *historically*<sup>43</sup>, a proximate mechanism usually has a corresponding ultimate mechanism. This seems to explain why Darwinian explanations penetrate all branches of biology: Darwinian mechanisms explain (the origin of the use of) the proximate mechanisms found in these branches.

However, things are not so neat: Darwinian why-mechanisms cannot be isolated completely from proximate mechanisms as why-mechanisms work through proximate mechanisms so that why-questions in biology are usually answered through the description of the how-mechanisms involved.<sup>44</sup> On the other hand,

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<sup>42</sup> It is unfortunate that the two major textbooks on evolutionary biology: Douglas J. Futuyma, *Evolutionary Biology* (2003<sup>4</sup>) and Mark Ridley *Evolution* (2005<sup>3</sup>) both start with the naïve view of population theory. Given the prominent role of population theory in the modern synthesis which shaped our ideas about evolution, this is understandable, yet starting with cutting-edge genetics would be good for modern academic introduction into evolution.

<sup>43</sup> It is hard to draw a clear boundary between a historical and an actual mechanism in a world that changes continuously as does the biological. The scheme in the next § will choose to distinguish them by single life ‘histories’ (actual mechanisms) from replicator lineages (historical mechanisms).

<sup>44</sup> A simple example: why meiosis distorter genes exist is explained by describing how these distorters copy themselves through meiosis. See also our discussion of fitness in §1.23 where it is explained how evolutionary fitness should be measured using proximate mechanisms. Philosophers have written a lot on why- and how-questions, especially regarding physics, which we cannot comment upon here. It is assumed here that all answers to why-questions in physics, both the more fundamental and more applied why-questions (‘why do stones fall?’ versus ‘why does the rainbow look as it does?’), are nothing but how-descriptions, as illuminating or surprising as they can be. However, in biology things are different. We will see in §1.23 that Darwin’s third principle of selection presupposes the concept of function which in turn presupposes as-if teleological terminology. Because of that, one cannot eliminate answers to why-questions from Darwinism. One can describe how an adaptive trait evolved (by proximate mechanisms), but this description of the evolution process is incomplete or meaningless (one can describe any random ‘evolution’ whatsoever in the fossil record) as it is not described from the perspective of a function that is selected for. The question: ‘why does function X exist?’ is thus ambiguous. It can mean: a) describe the proximate

actual how-mechanisms are sometimes explained through considerations of why they are there.<sup>45</sup> Niko Tinbergen's famous 1963 paper, based upon work by Konrad Lorenz<sup>46</sup>, on the methodology of ethology seeks to disentangle, at least conceptually, the two types of mechanisms.

### **Tinbergen's four questions**

Tinbergen distinguished four questions in his paper, which he divided into two pairs of questions – those about proximate mechanisms and those about ultimate mechanisms.

#### *Proximate mechanisms:*

1. Causation: how do morphological and behavioural traits function on the molecular, physiological, neuro-ethological, cognitive, and social level, and what do the relations between the levels look like?
2. Development (Ontogeny): how do the morphological and ethological traits change with age? Which developmental steps *and* which environmental factors play when what role in evolution?

#### *Ultimate mechanisms (sometimes called evolutionary mechanism):*

3. Evolution (Phylogeny): how does a trait compare with similar traits in related species, and how might it have arisen through the process of phylogeny? Why did the resulting trait evolve in this manner and not otherwise?
4. Function (Adaptation): how does the trait impact on the animal's chances of survival and reproduction?

The difference between question 3) and 4) rests on the fact that evolutionary explanations of animal traits are not exhausted by functional or adaptive explanations (as in 4). There may be numerous reasons why natural selection may fail to achieve optimal design (see §1.23 on the concepts of adaptation and function). The reconstruction of phylogeny thus focuses on non-adaptive elements of behaviour resulting from the phylogenetic history of a species, whereas adaptation focuses on the functional design of organisms.

Tinbergen's scheme for ethology was generalised for biology as a whole by evolutionary psychologist R.M. Nesse (2000) whose scheme, with modifications, we reproduce below as table 1.1 (see also Mayr 1982).

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mechanisms behind the evolution of function X, and b) because of Y, the thing function X functions for exists. Question b) cannot be reduced to a question of the form a).

<sup>45</sup> Example: actual types of social behaviour are described or modeled by evolutionarily stable strategies (i.e., current how-mechanisms are described by why-mechanisms that explain their sustainment).

<sup>46</sup> The author of the book that gave the Modern synthesis its name wrote similar methodological considerations as early as 1942. See the long quote of Julian Huxley in footnote 193.

The four areas of biology		Two different objects of Explanation	
		Developmental / Historical	Single Form / Ahistorical
Two different kind of questions	<b>Proximate</b> Explain how organisms work by describing their structures and mechanisms and their ontogeny	<i>Ontogeny</i> Description of an organism's development, from DNA code to the forms of different life stages  <i>Developmental explanations</i> for sequential changes in individuals across the lifespan	<i>Mechanism</i> Description of an organism's structure and how its mechanisms work  <i>Mechanistic explanations</i> for what an organism's structures are like and how they work
	<b>Ultimate/Evolutionary</b> Explains why organisms are the way they are by describing how selection shaped current forms and their phylogeny	<i>Phylogeny</i> Description of the history of a species as reconstructed from its fossil precursors and DNA evidence  <i>Phylogenetic explanations</i> for sequential changes in a species across time	<i>Adaptation</i> Explanation for the characteristics of a species based on how they give a selective advantage  <i>Evolutionary explanations</i> for <b>why</b> and organism is the way it is.

Table 1.1 The four areas of biology

On the basis of this table we can say that Darwinism synthesizes the different subdisciplines of biology by specifying the ultimate mechanisms behind the proximate mechanisms the different branches describe.

The terms 'ultimate' and 'proximate' suggest that ultimate mechanisms are more fundamental; however, proximate mechanisms can often be described without any ultimate mechanism, not seldom without even using typical biological principles and only using physical or chemical laws.<sup>47</sup> On the other hand, true adaptive explanations require alternative scenarios to be considered (walking upright can be an adaptation to move faster, to speak more clearly, to free the hand, etc.) and this requires a detailed analysis of the proximate mechanisms involved (see §1.3b). Hence, proximate mechanisms *can* be described without appeal to ultimate mechanisms, but not the other way round, in contrast with the idea that ultimate mechanisms are more fundamental. That is not shocking news, since we know that physics normally does not need the results of biology (except maybe sometimes in biophysics), whereas

<sup>47</sup> Insofar as proximate mechanisms are based upon chemistry and physics we may expect that theoretical reductionism in biology is occasionally feasible. See also Mayr 2004:79.

biology often uses results from physics.<sup>48</sup> Nevertheless, the descriptions of a proximate mechanism can often be more illuminating when they are buttressed by an explanation of the origin of the mechanism given by an ultimate explanation, and the perspective of ultimate explanations opens up a horizon from which proximate mechanisms are worth studying. Moreover, the ultimate explanation itself cannot be replaced by a description of proximate mechanisms alone (see §1.2.3 on *adaptive selection*, a category unique to biology). In that sense ultimate explanations are fundamental (i.e., non-reducible to other mechanisms).

The above characterisation of the nature of the synthesis using modified versions of Tinbergen's questions is important for our later discussion of Darwinian explanations outside of biology for two reasons: it would have to presuppose a) that disciplines synthesized by Darwinism can have their own principles and proximate mechanism, but b) that ultimate explanations of the adaptive function of proximate mechanism origins are to come from Darwinism *if* Darwinism outside of biology is to work analogously to normal Darwinism in biology. We now turn to a description of the three principles in more detail.

## §1.2 Three fundamental principles

*We will first give a short description of the three closely interrelated principles and then turn to each one of them in much more detail. Summaries can be found at the beginning of each paragraph. We try to give examples of the uses of the three principles in many different domains because we use these examples to abstract towards a more general framework in the next chapter which we do not want to be based upon the highly domain-specific type of Darwinian explanations in biology.*

In a famous passage at the end of chapter 4 of the *Origin of Species* Darwin himself summarizes the nature of these three principles and their relation to each other in biology (Darwin 1859: 127):

“If during the long course of ages and under varying conditions of life, organic beings vary at all in the several parts of their organisation, and I think this cannot be disputed; if there be, owing to the high geometrical powers of increase of each species, at some age, season, or year, a severe struggle for life, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all

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<sup>48</sup> See also Rosenberg 2001:162 who concludes “Insofar as science seeks to complete real explanation for historical events and patterns on this planet, it needs to pursue a reductionistic research program. That is, biology can nowhere remain satisfied with how-possibly ultimate explanations, it must seek why-necessary proximate mechanisms, and it must seek these explanations in the interaction of macro-molecules.” Note the inversion in the use of how and why: for Rosenberg ultimate explanations are how (possible) explanations and proximate explanations why-necessary explanations, whereas we called proximate explanations how-explanations (descriptive reformulations) and ultimate explanations why-explanations. This is because Rosenberg thinks physics gives true why-explanations (the laws are the why's for him), whereas we held that physics gives only how-descriptions, and in his view biology and its weak how-explanations should ideally be reduced to physics. We agree with Rosenberg on the importance of finding proximate mechanisms and of rooting ultimate mechanisms in physics and chemistry, but disagree insofar as Rosenberg seems to think that ultimate mechanism (i.e., explanations based upon adaptive selection) can be replaced completely by proximate mechanisms.

organic beings to each other and to their conditions of existence, causing an *infinite diversity in structure, constitution, and habits*, to be advantageous to them, I think it would be a most extraordinary fact if no variation ever had occurred useful to each being's own welfare, in the same way as so many variations have occurred useful to man. But if variations useful to any organic being do occur, assuredly individuals thus characterised will have the best chance of being preserved in the struggle for life; and from the *strong principle of inheritance* they will tend to produce offspring similarly characterised. This *principle* of preservation, I have called, for the sake of brevity, *Natural Selection* [our italics – MH].”

For short: animals breed more offspring than can survive given the limited means for existence; animals of the same species vary a little from each other, these two things combined result in a struggle for means of *existence* like food and means of *reproduction* like sex partners, in which only the fittest, or best (i.e., the animals that are best *adapted* to their world) will survive and reproduce; their offspring will inherit their well-adapted constitution.

Although the mechanism described works for all organic beings, the exemplary object to which Darwin applied his theory was sexually-reproducing multi-cellular creatures, organisms fairly close to us humans on the evolutionary scale.<sup>49</sup> If we want a more general perspective on Darwinism in biology we need to be aware of the fact that the exemplary object of application might influence our understanding of the three principles. This is an extra reason why we do not use Darwin's own theory, but the work of more modern theoreticians to discuss the three principles in more detail.<sup>50</sup>

## §1.21 Replication

*In this section we will restrict ourselves to a definition of replication in biology for the purpose of Darwinian explanations. We will not attempt to define the notion of replication in more general terms, which is left to §2.21. We show that, although biologists rarely define the notion of replication, such a definition is badly needed to distinguish real replication from*

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<sup>49</sup> David Hull already pointed this out: “The paradigm of an organism is an adult vertebrate, preferably a mammal. Unfortunately, these paradigmatic organisms are at the tail end of several important distributions. The vast majority of organisms that have ever lived have been small, unicellular, and asexual. According to recent estimates, systematists have described nearly 1.7 million species of organisms. Of these, about 751.000 are insects, 250.000 are flowering plants, and only 47.000 are vertebrates. But nearly all vertebrate species have been described, while most species of insects remain undescribed. According to one estimate, 30 million insect species are probably extant. But even that number shows a bias, because it includes only extant organisms when easily 99 percent of the species that have ever lived are extinct. Roughly 3.5 billion years ago, life evolved here on Earth. Not until 1.3 billion years ago did eukaryotes evolve. None of these were large, multicellular organism, nor did they reproduce sexually. Multicellularity and sexuality evolved only 650 million years ago, during the Precambrian era. Hence, it seems strange to pick even insects as the paradigmatic organism, let alone vertebrates. The most common organisms that ever existed are blue-green algae. (...) [Y]et nearly all the literature of evolutionary biology concerns large, multicellular organisms that reproduce sexually (Hull 2001: 15).”

<sup>50</sup> Our method to describe the current state of Darwinian explanations in biology has a lot in common with Gould's 1400+ page magnum opus entitled *The structure of evolutionary theory* (2002). In this book Gould compares Darwinism to a firm limb which branches into three branchlets that in turn branch into even smaller twigs. The firm limb symbolizes the success of Darwin's theory, the three branchlets the three fundamental principles of this theory and the twigs the more detailed versions of these three principles.



pseudo-replication and thus real applications of Darwinism to pseudo-applications. We describe a couple of replication mechanisms found in biology. On the basis of this list of examples we define Darwinian replication in biology as the temporal process in which one or more initial relatively complex but stable tokens of certain kinds (called replicators) become two or more other either identical or highly similar tokens of the same kinds (and their intermediate forms) that at least in principle can themselves be the subject of replication too in such a way that i) they form univocal (usually vertical but possibly sometimes also partly-horizontal) lineages and sometimes branches of generations of replicators, and that ii) the replicators causally determine, either directly or indirectly, their own replication process in a systematic way.

We explain the elements of this definition and focus on the notions of inheritance and of reproductive lineages. After that we are in a good position to analyze the influential view that genes are the true replicators by distinguishing between units of replication and the locus of selection. Finally, we will briefly discuss the Darwinian rule that replicators that share the same replication channel co-adapt.

Replication is hardly ever defined explicitly in biology. The two standard text books on evolutionary biology by Ridley (2004) and Futuyma (2005) do not define it, and neither do two prominent biologists and philosophers who have sought to introduce new terminology into biology in order to clarify issues around replication, viz., Richard Dawkins with his concepts replicator and vehicle<sup>51</sup> and David Hull (1980b) with his concepts of replicator and interactor (although Hull comes closest to such a thing as a definition in his general account of selection processes in Hull et al. 2001 by mentioning *iteration* and *information* as the necessary and possibly sufficient conditions for replication).<sup>52</sup> Darden and Cain (1989: 110) even forego a definition of replication altogether by demoting the principle of replication (iteration) to an ancillary feature of the principle of selection. In their work replication is almost equivalent to ‘that which can be selected for’. Although in Darwinian explanations replication always figures in a bigger process of selection and although we believe that a definition of replication in Darwinism should reflect that fact, we nevertheless think that we should define replication as best as possible without reference to the other two principles.

We do not know why biologists normally do not define replication – maybe because they think it is a primitive concept or because they think it is so trivial that it

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<sup>51</sup> In his book *The selfish gene*, in which he coins the word ‘replicator’, Dawkins writes: “At some point a particularly remarkable molecule was formed by accident. We will call it the Replicator. It may not necessarily have been the biggest or the most complex molecule around, but it had the extraordinary property of being able to create copies of itself (1976: 15).” He adds as characteristics of replicators: longevity (in the form of copies), fecundity, and (copying) fidelity. In his famous paper on *Universal Darwinism* written a little later (1982), in which he seeks to demonstrate that the principles of Darwinism are the only principles that can account for design in the natural world, Dawkins does not define replication either. Note that in *The selfish gene* (1976) the word vehicle does not occur; Dawkins coined it in 1978 as he realized that the individual organism did have some role in the selection process, even though still a minor one (see Dawkins 1978, 1994).

<sup>52</sup> Hull et al. 2001: 514 “Replication contains two elements; iteration (or repetition or recursion, depending on one’s terminological preferences) and information.” In the same paper Hull et al. refer to replication as “inherently a copying process” adding that “all that is required is [not just] heritability, but genealogical inheritance.” In Hull 1981: 33 he defines replicators as “entities which pass on their structure directly in replication.” This 1981 definition is meant as a critique and a firmer grounding of Dawkins’s notion of replicator: Hull does not wish to restrict the notion of replicator to genes (although he finds the gene perspective important). Because there is no recombination, Hull argues for instance that in asexual reproduction the organism itself is the replicator because it replicates its structure in a fairly direct and accurate manner.

needs no further clarification. In the case of Darwin's own theory with its exemplary object, sexually reproducing creatures, this might indeed have been trivial: replication was in this case equivalent to *reproduction* of clearly distinguishable individual creatures like mammals, birds, and reptiles.<sup>53</sup> The details of the mechanism of reproduction may have been unknown as some of them still are today, but in the time of Darwin and long before him people of course knew and could observe that animals simply beget offspring and thus replication needed no definition.

However, the details of the reproduction mechanism, the extension of Darwinism to plants, microbes, and other kingdoms, the discovery of the genetic code, and more recently the rise of epigenetics (i.e., ideas about extended phenotypes and symbiogenesis etc.) force us to rethink what replication is. We demonstrate this by discussing the replication mechanism of some biological properties in more detail. Our definition will be based upon this discussion. The resulting definition will be a definition of replication in biology which focuses primarily on the replication of living beings<sup>54</sup>, although the definition is not exclusively applicable to this; we will speak of replication in general in section §2.12.

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<sup>53</sup> We do not use the word 'reproduction' here instead of 'replication' as some biologists prefer (see for instance Mayr 2004: 87), because the word 'reproduction' is too tightly linked to acts of sex and the replication of animals that engage in sex to be of use in a general account of Darwinism in biology.

<sup>54</sup> We will not give a definition of a living being. We regard it a family resemblance concept, a network of related, overlapping uses that does not admit of a full definition. Some oft-mentioned characteristics of life are homeostasis, organization, metabolism, growth, adaptation over time, response to stimuli, reproduction, etc. However, many philosophers have complained that many organisms lack all or most of these characteristics: think of fungi and bacteria, especially the ones that depend for their growth and replication on eukaryotic host cells such as the Chlamydia bacteria. (See for critique of a definition of life or a living being along similar lines: Bonner 1974, Dawkins 1982ab, Hull 2001.)

One could of course try to weed out characteristics until one finds what the different kingdoms of life have in common: they are carbon- and water-based cellular forms with complex organization and heritable genetic information, but this does not cover what is so special about life (i.e., it is like calling a man a biped without wings) and it excludes for instance viruses and other non-cellular forms of life such as prions. One could be tempted to identify the essence of living being solely with that the capacity to reproduce as the metaphysical tradition did from Plato till Hegel (thus today potentially including viruses etc.), but this idea is wanting insofar as this essence would not be unique (i.e., to speak metaphysically, it would not name a *differentia specifica*): many non-living things such as crystals can be said to replicate whereas many living things such as sterile casts of ants in principle do not replicate even in principle. A non-metaphysical definition that is equally wanting identifies a living being with that which has the capacity for descent with modification (i.e., that which is subject to the principles of Darwinism), but we cannot use this definition here since we are defining the principles of Darwinism themselves.

Finally one could choose to define life using non-biological principles. This is often done by physicists who take a reductionist stance towards biological categories such as Bernal (1967, 1951) and Avery (2003). In the tradition of Schrödinger's famous 1944 account *What is life?* (who was not the first himself either) they define life as a member of the class of phenomena which are open or continuous systems able to decrease their internal entropy at the expense of substances or free energy taken in from the environment and subsequently rejected in a degraded form. However, there are many non-living systems which decrease their internal entropy by taking in energy from the environment: think of gas accidentally trapped in glacial ice, the formation of crystals, etc. Thus their definition may give a necessary, but certainly not a sufficient, condition. However, even a local decrease in entropy may not even be necessary as one has to define a system in which entropy decreases. Viruses or bacteria may do irreparable damage to the body and thus increase the entropy of the body system. The production of new viruses may cause a local decrease in entropy, but the net result within the living body is an increase in entropy. Thus the physical definition of life, which does not have any other resources to identify a living system than to look at a local system in which internal entropy decreases, may misidentify life and as such the definition is not sufficient to characterize life.

Because of the problems with explicitly defining life we choose to call the concept of a living being a family resemblance concept. The borders of this concept are vague as they are for any family resemblance concept. We will use it here rather straightforwardly to exclude replicating whirlwinds, crystals, computer viruses, and artificial lego-robots that can build a copy of themselves etc., but to include

We describe first a representative sample of replication-mechanisms (from small to big) that have played a role in the philosophy of biology. We use them to abstract towards a definition of replication in biology that *can be used in Darwinism and is not restricted to a single domain of life*. If we just look at things that seem to ‘replicate’ *without further qualification*, we can say (from small to big) that nucleotides, cistrons, longer pieces of DNA, RNA and proteins, whole chromosomes, cell organelles, whole cells like muscle cells, germ line cells and heart cells, tissues, organs, organ clusters, organisms, their parasites and viruses and symbiotic bacteria, the behavioral patterns of organisms, the artifacts these organisms make such as nests, webs and dams, groups of organisms from families to herds, and maybe even species or higher taxa *all replicate*. We look at a couple of the replication processes behind them in more detail. It is not our goal to give *detailed* descriptions of these processes, because we leave that to the biologists; our goal is to show the *wide variety* of forms of replication in biology, emphasizing the *mechanism* behind it. Our descriptions will be relatively long because we use the same examples to discuss variation and selection and to compare replication mechanisms outside of biology with types we encounter here.

We describe shortly the replication of a) chemical replicators, b) viroids, virusoids and prions, c) viruses, d) non-coding and jumping DNA, e) DNA, f) mitochondrial DNA, g) chromosomes, h) epigenetic settings, i) cells of the immune system, j) bacteria, k) plants, l) animals, m) bees and bee hives, n) kin, o) behavioural patterns, p) extended phenotypes, q) populations, r) species and higher taxa. When we print a letter **bold** in this chapter, we refer back to these examples. We print in *italics* the most important observations about their replication.

### **Eighteen examples of replication mechanisms:**

**a) chemical replicators:** there is a vast literature of more or less well-informed speculations on the origins of life.<sup>55</sup> These speculations almost invariably start with simple chemical molecules which either *individually or collectively have the ability to replicate themselves*. Self-replicating molecules are not rare: almost all salt molecules form crystal structures in which layer upon layer of the same basic structure is repeated. However, we will not consider salt crystals to be replicators in the Darwinian sense since, for short, their replication *excludes the possibility of variation*. The *ionic bonds* of salt crystals are so strong and the mechanism of replication is so strict (a salt crystal forms a *highly specific electric potential pit in which the exact same molecules as the crystal itself is composed of fit*) that they hardly allow for variation (of course accidentally wrong salt ions are included in crystals as impurities, but these impurities are not copied along). Moreover, many *ionic* bonds dissolve in water and they hardly ever link more than 4 molecules as the cations (the positive particles) and anions (the negative particles) themselves consist of only a few molecules.<sup>56</sup> A careful first conclusion about Darwinian replicators is therefore that they need to be *relatively large* and that their *replication must allow for variation*.

The most popular model of the origin of life using chemical replicators is the RNA-world hypothesis claiming that naked, unprotected *covalently bound* RNA

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(being more inclusive than some biologists) viruses, bacteria, archaea, protista, fungi, plants, animals, etc. Chemical replicators before the advent of cell-walls are a clear border-case and so are viroids, virusoids, and prions. See also Rybicki 1990.

<sup>55</sup> See for instance the book length accounts of Hazen 2005, Maynard Smith and Szathmari 1995 and De Duve 1996.

<sup>56</sup> The largest cation and the largest anion are: pyridinium  $C_5H_5NH^+$  and acetate ( $CH_3COO^-$ ).

molecules of hundreds of bases existed before cellular organisms evolved with DNA coding for their phenotype. The main reason for this hypothesis is that RNA can both *store information like DNA* and *catalyze reactions like proteins*.<sup>57</sup> The models of RNA-world have two steps: from the well-understood RNA-hypercycle which is responsible for the evolution of fast replicating RNA-molecules (Eigen and Schuster 1979) to the speculative step of how proto-cells evolved around these molecules with RNA or DNA coding for their coil. The RNA-hypercycle happens when a *group of RNA molecules is accidentally formed that is capable of catalyzing its own continuing replication (autocatalysis)*. Due to replication failures RNA-molecules then evolve that are either faster at replicating or better at catalyzing (i.e., faster or used by many RNA-molecules) and thus competition and selection (Fernando and Rowe 2007)<sup>58</sup> exists amongst different *types* of RNA-molecules, often called quasi-species (Eigen et al. 1989). The replication mechanism of the RNA-replicators in the world before proto-cells is thus such that they do not store information coding for a phenotype, but are immediately *causally involved* in replicating molecules of their own type or that of other types that help their own type in turn. Only much later do (the more stable) DNA take on a central place in the picture of RNA-world evolution. This DNA only codes for and does not catalyze its own replication.

**b) viroids, virusoids and prions:** *viroids* are plant pathogens consisting of small, highly-complementary, *circular*, single-stranded RNA-strings without the protein coat that is typical for viruses. They get *transmitted by seed and pollen* and although they *do not code* for proteins, they can disturb the growth of a plant by RNA silencing (see **f**) on epigenetics) and they replicate via a double-stranded *RNA-intermediate* and are then cut by a dicer, although not all details of the replication and transmitting process are well understood. *Virusoids* are virus-like particles, also called satellites because they require an *assistant virus* which harbors the virusoid and is required for successful infection. *Viroids* code for nothing but themselves, *they are parasitic on other viruses and replicate just like viruses using the host cell's machinery*<sup>59</sup>; prions are infectious agents composed of only proteins and are believed to infect and propagate by refolding abnormally into a structure which is able to *convert normal molecules of the protein into the abnormally structured prion form*.<sup>60</sup> Prions thus *use the normal protein replication machinery of the cell for their own replication, converting normal proteins into themselves*.

**c) viruses:** viruses are infectious agents that are unable to grow or reproduce outside a host cell; they consist of genetic material and a protective protein coat called capsid encoded by the virus<sup>61</sup> and sometimes also of a lipid 'envelope' which derives from the host cell membrane; once they infect a host cell *they use its machinery to make more genetic material and protective coat proteins*. Viruses are classified by the type of genetic material they contain and the intermediate stages of their reproduction process: viruses can contain double-stranded DNA or RNA or single-

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<sup>57</sup> RNA still fulfills both roles in cells, it stores information like DNA as messenger RNA and it catalyzes reactions like proteins as for instance ribosomal or transfer RNA.

<sup>58</sup> Another important factor of competition is stability and replicating accuracy of the individual molecules.

<sup>59</sup> See <http://www2.oakland.edu/biology/chaudhry/pics/Viroids%5B1%5D.pdf> retrieved at 13 November 2007.

<sup>60</sup> See Collinge 2001. All known prions induce the formation of an amyloid fold, in which the protein polymerizes into a fiber with a core consisting of tightly packed beta sheets.

<sup>61</sup> Some of the capsids are self-assembling protomers, but often the genetic material of the virus codes for proteins that help in constructing the capsid. Some viruses, such as the famous mimi-virus contain even more cell machinery. See Prescott 1993.

stranded DNA or RNA with in the latter case either positive or negative orientation, and two extra categories: double-stranded DNA with RNA intermediate in the life cycle and single-stranded (+)sense RNA with DNA intermediate in the life cycle. This last group is also known as retro-viruses that reverse transcribe their genome from RNA into DNA, which can then be integrated into the host's genome *so that the virus replicates as part of the cell's DNA* (HIV works like this). A final important replication/variation mechanism of viruses is the reassortment process called viral sex which happens when different strains of the same virus, such as the bird and the human influenza virus, shuffle and mix each other's genetic code producing progeny viruses (offspring) that have unique characteristics (see Goudsmit 1998). In sum: in all processes of viral replication viruses *use the normal cell machinery for their own replication, but they do code for their own capsid*.

**d) selfish DNA:** selfish DNA refers to those sequences of DNA which, in their purest form, have two distinct properties: (1) the DNA sequence spreads *by forming additional copies of itself within the genome*; and (2) it makes no specific contribution to the reproductive success of its host organism. Examples are transposons, also called jumping genes, sequences of DNA that can move or replicate themselves to different positions within the genome of a single cell in spite of often being detrimental to the rest of the genome. They thus use the normal cell machinery for their own purposes.<sup>62</sup>

**e) DNA:** book-length accounts have appeared about the evolution of DNA (Alberts et al. 2002), so our discussion here can be brief. We make a non-standard distinction between a) the *short* cycle of DNA-replication and b) the *long* cycle of DNA-replication (our terminology). This distinction rests upon the distinction of DNA as a sequence of nucleotides and DNA as genes coding for phenotypic properties.<sup>63</sup> The result of the short cycle are new strings of DNA, the result of the long cycle (ultimately) new organisms (algae, plants, animals etc.) with DNA coding for a phenotype that collects food and attracts mates so that replication ultimately continues.

The *short cycle* is well-studied and concerns what happens during meiosis/mitosis in which, roughly speaking, the two strands are separated and each strand's complementary DNA sequence is recreated by an enzyme called DNA polymerase. This enzyme creates the complementary strand by finding the correct base through complementary base pairing and binding it onto the original strand.<sup>64</sup> Scientists use the enzyme polymerase in the famous polymerase chain reaction to *amplify* (i.e., *artificially replicate*) a short piece of DNA *in vitro* (the scientists adding base nucleotides), and thus the short-cycle replication seems to need but little of the cell machinery.

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<sup>62</sup> See Orgel and Crick 1980. Selfish DNA is closely related to non-coding DNA: DNA that has accumulated during evolution and may have coded for phenotypic properties in the past, but no longer today. Selfish DNA, however, does code for proteins, although these only help in its own replication. Sometimes selfish DNA and non-coding DNA are collectively called junk-DNA, although junk might be a misnomer insofar as this DNA may still play a role in evolution as a variation-generating device or in gene regulation. See Gibbs 2003.

<sup>63</sup> A further function of DNA in chromosome chains is to give support to the chain in regions prey to mutation near the centromeres or the telomeres. This function of DNA is an exaptation or secondary effect based upon the molecular form of DNA and not on its coding properties. As such DNA is just like the chemical replicators not only informationally, but also because it is causal mechanistically involved in its own replication process.

<sup>64</sup> Note that this process is somewhat different in prokaryotic cells and eukaryotic cells. See Alberts et al. 2002.

The *big or long cycle* is species-dependent and is not completely known until the proteomics project for every single (type) of organism is deciphered; it describes, roughly speaking, how from a zygote an organism is formed which matures and reproduces, i.e., how DNA codes for proteins and an organism's phenotype which can ultimately be used for attracting resources and building the nucleotides of the DNA for its own cells and the gametes of the next generation as well as for attracting a mate so that its gametes are actually transmitted to the next generation.

Note that the distinction between the short and the long cycle is not a clear-cut one. In eukaryotic cells part of the DNA process of replication is *recombination* during meiosis as two chromosomes of the same pair of chromosomes are cross-linked and exchange DNA. Is recombination part of the short or the long cycle? (We would argue, if pressed, that it is part of the long cycle as recombination does not occur in prokaryotes or in *in vitro* replication and requires complex cell machinery). Due to recombination, however, DNA is reshuffled all the time so that *exact strings of DNA* (in eukaryotic cells) cannot be seen as stable replicators since they last only for a few generations; the longer they are the shorter they last. This problem can be overcome if we allow ourselves to speak of strings of DNA as replicators regardless of their chromosome location, but that is – when we allow for mutation – often not really workable in practice.

As we saw: *DNA all of itself never copies itself spontaneously*, it requires either (in the short-cycle) enzymes and building-blocks or (in the long-cycle) phenotypic cells and all of the cell machinery to extract building blocks from the environment and to assist its replication. We thus have a chicken-and-egg situation: genes code for their cells, but these cells are already needed for DNA to be transcribed and to be replicated. We come back to this chicken-and-egg problem below as we give our definition of replication and discuss gene selectionism. It is important to see that this chicken-and-egg problem is central to biological replication as *no isolated replicator copies itself spontaneously without other things present that are replicated as well* (except, perhaps, for very simple chemical replicators such as crystals which require only environmental interaction to copy themselves but which we denied to be true Darwinian replicators, see **a**).

**f) cytoplasmic DNA:** cytoplasmic DNA is non-chromosomal DNA in organelles in the cytoplasm such as mitochondrial and plasmid DNA. These cell organelles, which originated as independent unicellular (prokaryotic) bacteria with their own DNA, were symbiotically encapsulated into the cells of another to perform special tasks for the resulting cell. The resulting cells are called eukaryotic and in the course of evolution the encapsulated cell organelles became dependent on the cells that encapsulated them for providing them with nucleotides for their replication (in accordance with the Darwinian rule that elements that share the same replicating channel co-adapt, see below). The DNA inside the cell organelles *replicates just like normal chromosomal DNA in the nucleus of a cell, but it has some tricks to distort the replicating process to its own advantage*. Animals generally inherit cytoplasmic elements exclusively from the female gamete; from the viewpoint of mitochondrial DNA, it is therefore wise to make sure a woman begets as many daughters and as few sons as possible, because sons are dead ends for cytoplasmic DNA. And indeed cytoplasmic DNA has evolved a number of mechanisms to increase the production of female

descendants and eliminate offspring not containing them, such as male (embryo) killing, male sterilization, and parthenogenetic killing to name but a few.<sup>65</sup>

**g) chromosomes:** chromosomes are the organized form of DNA consisting of long strands of DNA of which some code for genes and regulatory elements; prokaryotes most often have circular chromosomes, while eukaryotes have linear chromosomes; sexually reproducing creatures have two pairs, although sometimes more (such as in polyploid plants and animals). Normally the number of chromosomes the cells of individuals of sexually reproducing species have are the same<sup>66</sup>, although the number of chromosomes species have, is highly variable: maize contains 20, tobacco 48 and the Adder's tongue fern 1248; a fruitfly has 8, a snail 24, a human 46 and a goldfish 100-104. Chromosomes often *split* or *fuse* during evolution, which is often not a real problem since the split or combined set of chromosomes contains just as much genetic material as before. What is remarkable about the replication of chromosomes is i) hitch-hiking and ii) recombination.

i) Since one chromosome codes for many genes it can happen that when a gene is changing at one locus over time, it can also effect change in related genes at linked loci, a process called hitch-hiking (see Ridley 2005: 210), i.e., when one gene on a chromosome *increases in frequency* due to selection, it may cause other genes on the same chromosome that are linked to it by replication to increase as well.

ii) recombination is the process of cross-over during meiosis by which a strand of DNA is broken and then joined to the end of a different DNA molecule so that genetic material is reshuffled over the two chromosomes joined during meiosis. Due to meiosis the chromosomes of sexually reproducing creatures are *never exact replicas of the previous generation's* (see **e**). It is believed that recombination leads to more variation (Felsenstein 1974, Muller 1964) and provides a way to overcome Muller's ratchet (Muller 1932), i.e., the irreversible accumulation of deleterious mutations.<sup>67</sup> It can break down the genetic hitchhiking we just described because it can break the bonds between linked genes on the same chromosomes; the further apart the linked genes are, the more easily this happens.<sup>68</sup> Some genes are located closer to the centromere and have a *higher chance of recombination during replication*, others lie further away from it. This may be due to adaptation as some genes profit from more mutations, see §1.22 on variation mechanisms. Recombination nevertheless questions whether we should regard chromosomes as stable replicators (even though individual strings of DNA in eukaryotes are replicated within chromosomes).

**h) epigenetic settings:** epigenetics<sup>69</sup> is a new, flourishing area in biology that sometimes involves pseudo-Lamarckian evolution. Epigenetics refers to features

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<sup>65</sup> See Burt and Trivers 2005. Parthenogenetic killing can be caused by cytoplasmic bacteria in species where males have half the chromosomes of females; the bacteria forces the cell to duplicate its chromosomes thus feminizing the males.

<sup>66</sup> But see Ridley 2005: 82 and 360 on variations of the number (!) of chromosomes in local populations of the same species.

<sup>67</sup> Two negative mutations on different chromosomes can be shuffled in such a way that one gamete contains a chromosome with zero negative mutations, and another two; the first lead to highly viable offspring, the second to instant death or at least much less viability and thus the two negative mutations are eliminated from the gene pool whereas the good genes are fixed. See also Maynard Smith and Szathmari 1995.

<sup>68</sup> See also Ridley 2005: 211.

<sup>69</sup> The word 'epigenetics' is much older than its current use. Historically, epigenetics was the theory that opposed preformationism (the idea that in every embryo a mini-form of the full animal was present) by claiming that the cells of an embryo could split and diversify to create a full-grown animal.

such as chromatin and DNA modifications that *are stable over rounds of cell division*, but that *do not involve changes in the underlying DNA sequence of the organism*. Epigenetics is generally thought to be about mechanisms involved in the differentiation of cells: some cells of a mammal become muscle, other heart, liver, kidney cells, etc. This requires different genes activated in different cells and these ‘settings’ need to be inherited so that old kidney cells beget new kidney cells and not heart cells etc. (only stem cells keep the possibility to differentiate into any cell); proposed epigenetic mechanisms for this kind of ‘cell memory’ include amongst others: DNA methylation, paramutation, bookmarking, imprinting, gene silencing, X chromosome inactivation, the progress of carcinogenesis, many effects of teratogens, regulation of histone modifications, etc. (Allis et al. 2007, Jablonka and Lamb 2005). Normally the epigenetic settings ‘reset’ upon replication of an organism. However, some epigenetic features *show so-called transgenerational inheritance* which means that they are inherited from one generation to the next; exactly how this takes place is uncertain. Since epigenetic settings can be changed due to environmental conditions, these settings are acquired during the lifetime of the organism and thus in the case of transgenerational inheritance of epigenetic settings we have a case of Lamarckian evolution (see for instance the well-documented case of paramutation observed in maize in Chandler 2007 and Ridley 2003). Most transgenerational epigenetic properties are lost in a few generations; thus it is suggested that epigenetic features may play a role in short-term adaptation of species by allowing for *reversible phenotypic variability*, leaving the genetic code unchanged; be that as it may, epigenetic settings question in any case the dominant view of the modern synthesis which considers evolution as a process involving only changes in gene frequencies.

**i) cells of the immune-system:** the immune-system is a collective noun for mechanisms within an organism that protect it against disease by identifying and killing pathogens. The main distinction in mechanisms is between the innate immune system and the adaptive immune system. The former is non-specific, responds immediately and maximally to exposure, has no ‘memory’ and is present in almost all forms of cellular life; the latter is specific; its responses lag behind exposure, although the exposure leads to immunological memory and it can only be found in jawed vertebrates. The complete innate system is genetically coded, the *details* of the adaptive system not. We will focus here on the adaptive system because of the unique form of the replication of its cells.

The adaptive system can recognize and remember specific pathogens, an ability which it uses to mount stronger attacks each time the pathogen is encountered. The mechanism is far too complex to describe here (Janeway et al. 2001). Its key elements are somatic hypermutation and V(D)J recombination. Somatic hypermutation involves a programmed process of mutation affecting the variable regions of immunoglobulin genes inside human cells (and only those cells); V(D)J recombinations are irreversible recombinations of genes coding for specific antigen receptors. Together, hypermutation and V(D)J recombination form a *shuffling mechanism that allows a small number of genes to be able to generate a vast number of different antigen receptors*. The progeny of the cells whose genes have thus been affected will inherit these genes. Because of that, immunity once gained can ideally last for a whole life-time. *However, the immunity is not transmitted to generational offspring and thus the immune cells are all in principle dead-end replicators*. Two elements of this system are worth emphasizing: a) babies do receive antibodies from their mother through the placenta and breast-milk; however, this results only in *passive* immunity because the fetus does not actually make any memory cells or antibodies, it only



borrowed them and hence does not inherit them; b) in the adaptive system once a pathogen is recognized a complex mechanism ensures that cells that secrete vast amounts of the right antibodies start dividing; we have here a case of what we would call *directed replication* or *amplification*.

**j) bacteria;** bacteria are prokaryotic micro-organisms that are ubiquitous in every habitat on earth. They come in many shapes and guises. In the human body there are approximately 10 times as many bacterial cells as human cells, with many bacteria on our skin and in our gut system.<sup>70</sup> Many bacteria, although unicellular, live in dense aggregations called biofilms. In these biofilms extra-cellular structures develop such as networks of channels for better *diffusion*. Certain bacteria also produce so-called endospores: dormant structures (i.e., structures with no detectable metabolism) containing DNA that are highly resistant and can remain viable for millions of years surviving even vacuum conditions and radiation in space. In bacteria *reproduction and cell growth are tightly linked*: bacteria grow to a fixed size, replicate their DNA and then reproduce through *binary fission* which creates two clones. Apart from the endospores<sup>71</sup>, a few things about the bacterial replication process are worth mentioning: bacterial cells sometimes form more complex extra-cellular structures that facilitate the dispersal of newly formed daughter cells such as fruiting bodies containing up to 100.000 new cells. Some bacteria also transfer genetic material between cells, either when they take up exogenous DNA from the environment or when a bacteriophage introduces foreign DNA in their chromosome. Most famous, however, is the process of bacterial conjugation when DNA is exchanged through direct cell contact. This is a *process of horizontal instead of vertical, generational gene transfer*.<sup>72</sup> Finally, bacteria can depend on other organisms for their replication, either of their own kind (as in biofilms) or of another kind. The most extreme example of the latter case are the encapsulated bacteria that now live as the cell organelles of eukaryotes, but human gut bacteria and skin bacteria are others beautiful examples of a symbiotic relationship.<sup>73</sup> Babies in the uterus do not have gut bacteria but directly after vaginal delivery, babies have bacterial strains in the upper gastrointestinal tract derived from the mothers' feces and later through breast-feeding; the skin bacteria are transferred through kissing, caressing, etc. Feces, milk, and skin contact thus act as *replication channels through which a real sample of the bacteria is transferred* that starts replicating immediately afterwards by itself.

**k) plants:** plants come in many sizes, shapes, and sorts; here we only focus on some aspects of their replication (Raven et al. 2005). Their cellular replication is that as described under **e)** DNA where we distinguished between the short and long reproductive cycle. With respect to the short cycle in plants, it is worth remarking that plants like bacteria sometimes *exchange DNA* and that polyploidy of **f)** chromosomes is often an important step in the speciation of plants. With respect to the long reproductive cycle in plants we have to distinguish between asexual and

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<sup>70</sup> This is due to the size of bacterial cells; all their dimensions are typically 10 times as small as a human cell's; hence, in a fixed volume 1000 as many bacteria fit as human cells. See Sears 2005.

<sup>71</sup> The production of endospores is primarily not a reproductive process (i.e., the number of endospores per cell does not exceed one), but only a process to protect the core of the bacterial cell. However, some bacteria such as the *Anaerobacter* can make up to seven endospores per cell and in these cases endospores can be seen as a replication mechanism. See Stunov et al. 1999.

<sup>72</sup> It is uncertain whether the exchanged pieces are replicated beforehand or whether a piece of a cell's own DNA is exchanged. In the former case, we could speak of horizontal pseudo-replication, in the latter of horizontal transfer or exchange.

<sup>73</sup> Of course not all bacteria that need a host to reproduce have a symbiotic or mutualistic relation with their host; other possible relations are parasitic or possibly commensalistic relations.

sexual replication; *asexual replication and growth are virtually the same in plants*: pieces that fall off a tree can grow into new trees (i.e., a process used in plant cutting)<sup>74</sup>; sexual reproduction involves the production of flowers, fruits, and seeds; plants try to prevent self-fertilisation; they use other organisms (that often co-evolved with them) to cross pollinate themselves and *to spread their seeds to replicate in other places*. Many plants have a seasonal growth pattern: they live, reproduce and die within one or two seasons; during a year, plants may thus not be present in one of their forms.

**l) animals:** animals reproduce sexually and this process is the exemplary case we unconsciously think of when discussing replication. Some animals reproduce, however, in a special way as in the case of *parthenogenesis* (salamanders and even Komodo dragons) or *clonal fragmentation* (as in the case of worms or starfish).<sup>75</sup> In animal reproduction we can again distinguish the short and long cycle of replication. During the long replication cycle of animals *metamorphosis* can occur. Metamorphosis is a not uncommon biological process by which an animal physically develops after birth or hatching, involving a conspicuous and relatively abrupt change in the animal's form or structure through cell growth and differentiation, which is usually (but not always) accompanied by a change of habitat or behaviour. Examples of metamorphosis can be found amongst insects, amphibians, molluscs, crustaceans, cnidarians, echinoderms, and tunicates. Probably the best known examples are frogs and butterflies. Two types of metamorphosis are distinguished: hemimetabolism and holometabolism: in hemimetabolism, the development of larva often proceeds in repeated stages of growth and ecdysis (moulting) and the juvenile forms closely resemble adults, but are smaller; in holometabolism larvae differ markedly from the adults. The borderline between the two types is vague. It is also not the case that animals undergoing metamorphosis *need to go through every stage before they can reproduce* as some species of salamander only metamorphose under certain environmental stresses and are sexually mature long before. Metamorphosis underlies the same short cycle of DNA replication as normal animal embryology; only the long cycle has *an extra intermediate form*.

**m) bees and beehives:** the replication system of bees and other hymenoptera such as wasps and ants is well-studied. It became a topic well-known to wider audiences through the work of E.O. Wilson *Sociobiology, the new synthesis* (1975), because it seemed to open new ways to study altruistic behaviour in biology. The work of Wilson relied on earlier theoretical work by W.D. Hamilton on kin selection (1964). This paper sought to explain how organisms could increase the fitness of their own genes by aiding their close relatives. Hamilton's rule stated that a costly (potential altruistic action) would be reasonable from an evolutionary point of view if  $C < R \text{ times } B$ , with C the cost of the action, R the degree of relatedness<sup>76</sup> and B the benefits conferred unto the recipient of the actions.

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<sup>74</sup> Some plants even grow adventitious plantlets on their leaves with the explicit 'purpose' of dropping them off to form independent plants, e.g., *Tolmiea menziesii* and *Kalanchoe daigremontiana*.

<sup>75</sup> See Hull 1981 who explicitly regards for instance the dividing of a paramecium into two a form of replication.

<sup>76</sup> In many examples in which Hamilton's theory is applied in for instance evolutionary psychology it is said that the (genetic) relatedness of two brothers is 50%. That is of course nonsense; the relatedness is much higher: two brothers have more than 99% of their genome in common (and they share more than 90% of their genome with apes etc.). What people should mean is that the genetic relatedness to one's brother regarding the specific gene or piece of DNA that codes for the altruistic behaviour towards kin is at least 50% (and probably higher if the father and mother were homozygous for the trait which is here supposed to be dominant). See for instance Dawkins (1976) who provides an example of a runt that does not fight till it dies, but that let's itself be eaten by its litter-mates: "That is to say, a gene that gives the

Bees and other Hymenoptera became the centre of attention of the discussion of the evolution of eusociality because of their unusual *haplodiploid sex-determination system*.<sup>77</sup> In this system females are (often) more closely related to their sisters than to their own (potential) offspring (i.e.,  $R_{\text{sisters}} > R_{\text{offspring}}$ ). Thus, Wilson reasoned, for a female worker a ‘costly action C’ would be spent best in helping to raise her sisters, rather than reproducing herself (although female workers are capable of laying their own eggs). That is why normally the bee queen lays all eggs. Out of this collection of eggs a whole new colony of bees grows that builds its own hive again.

It is sometimes said, looking at the fact that *the whole replication of the hive goes through the bottle-neck of the queen laying the eggs*, that the true organism is the hive as a whole, of which the individual bees are but the organs.<sup>78</sup> We think that this terminology is on the one hand confusing and on the other elucidating. The word ‘organism’ seems to imply that the beehive is a *living being*. And indeed the beehive grows, its temperature is kept constant, and it ‘uses’ material from its surrounding ‘excreting’ its useless parts etc. which are all characteristics of life (see footnote 54). However, to call a beehive alive seems but one of the family resemblance uses of the concept of life, which lies nearer to the boundaries of this concept than to its standard uses: few people would call a snail’s house alive instead of non-living, although beehives and snail houses are rather similar. Individual bees seem to deserve the predicate alive much more than the beehive as a whole. That is why we will not use the metaphorical super-organism talk and use the terminology of **o**) extended phenotypes to characterize artifacts built by animals such as snails’ houses and bees’ hives. However, the terminology of super-organism is also elucidating if the word organism is understood as “having organisational structure”. Individual bees can only survive within such as organisational super-structure: although the ancestors of bees were solitary, modern bee life is fully integrated into colony life. The replication bottleneck of the queen ensures that colony life will be the living style of future bees. For our discussion of replication mechanisms we should note that *whole populations can again and again be reproduced through exemplary individuals*.

**n) kin:** kin are genetically related family members. In our discussion of **m**) beehives we already discussed Hamilton’s rule for kin selection. We saw that in kin selection the *replication chances* depend on the caring behaviour of related kin. In this case the replication of an organism is thus not the replication of an isolated individual confronting the outside world and his competitors, but a process in which other individual organisms play a role as well.

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instruction ‘Body if you are very much smaller than your litter-mates, give up the struggle and die’, could be successful in the gene pool, because it has a 50 per cent chance of being in the body of each brother and sister saved, and its chances of surviving in the body of the runt are very small anyway (Dawkins 1976, 2006, p. 130).” Dawkins consequently speaks of the one gene of sacrificing behaviour that has a 50% survival chance and not of the 50% relatedness with its brother genome as a whole. (Dawkins’s only fault is that he should have written at least 50%.) A fictional example that makes the application of Hamilton’s rule even clearer is that of the gene for helping people with a green beard which codes for both a green beard of one’s own and the altruistic attitude towards people having a green beard too.

<sup>77</sup> In a haplodiploid sex-determination system such as that of the bees, the bee queen has 32 chromosomes, the male drones 16 (from unfertilized eggs) and the female workers 32 (from fertilized eggs). The female workers can lie unfertilized eggs, but they share only 50% of their genes with them (half of the chromosome), whereas they share 75% of their sister’s genome (half of the queen = 16 + half of the male drone with whom her mother mated to beget her and her sister = 8).

<sup>78</sup> See Seeley 1989 on the honey bee colony as a “super-organism”.

**o) extended phenotypes:** extended phenotype is the term coined by Richard Dawkins in his book *The extended phenotype* (1982, with modifications 1999) subtitled *The long reach of the gene*. In this book Dawkins argues that although genes directly control only the synthesis of proteins and have but an indirect effect on phenotypes, that yet to restrict the phenotypic expression of genes to an organism's body is arbitrary. Dawkins proposes to regard beaver dams and spider webs, which he calls extended phenotypes, to be part of organisms' phenotypes just as organisms' bodies since they can be said to be under *indirect* control of the genes. We think this terminology is justified as long as one keeps in mind that the extended phenotype is under even less direct control than the normal phenotype.

There is feedback between the extended phenotype and the normal phenotype: beavers with longer teeth make (replicate) better beaver dams, which feeds back on the replication chances of beavers themselves (one could call this co-evolution). If one treats the beaver and the beaver dam as equals from the bare point of view of replication, this can lead to funny reversals, such as a "bird being the nest's way of making another nest."<sup>79</sup> The bird-joke involves the same kind of inversion we saw in discussing the long cycle of DNA replication with DNA replication dependent on the replication of cells, so that we could say that DNA is a cell's way of making another cell. This reversal will be avoided if we look at the *stability* of replicators and the *heritability (and causation) of changes*: mutations in genes can affect the extended phenotype but not (or hardly ever) the other way round. We discuss this issue further below under 'the gene as replicator'.

**p) behavioral patterns:** behaviour can be i) genetically programmed, ii) learned, or iii) a combination of both (genetic predisposition but fine-tuning by practice). There are Darwinian explanations of learned behaviour that have to determine how behaviour is replicated, but we leave them out of the discussion<sup>80</sup> and focus on i) and iii).

i) Genetically determined behaviour seems to replicate just like any other genetically determined morphological trait like an organ; however, there are behavioural traits such as homosexuality which seem detrimental to their practioners from the point of view of their replication but which are nonetheless suspected to be a partly genetic preference arisen by natural selection, as homosexuality is recorded in hundreds of species<sup>81</sup> and thus replicates successfully. It is hard to understand the replication mechanism behind this trait, especially when dealing with *strict* homosexuality. One proposed mechanism is that homosexual brothers help sisters raise the children these sisters have with other men; sisters have at least a 50% chance to share the gene for homosexuality (which includes caring behaviour toward's one's sister and her children) and thus the sons of their sister have a 25% chance of being homosexual too; when sisters that are helped by brothers (especially in polygamous societies) raise substantially more sons than competitors, this gene may thus survive. It is unimportant for our discussion here, whether the proposed replication mechanism is true. What is important is that *a genetically determined behavioral pattern can replicate without being involved in the act of replication in the direct sense*. It is in this vein that evolutionary psychologist Thompson (1994: 638) observes with respect to natural

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<sup>79</sup> Patrick Bateson made this joke in a review of Dawkins's book. See also his later reflections on this issue in which he tries to remove confusion (Bateson 2006).

<sup>80</sup> See chapter 2.1 under the subheading Evolutionary epistemology.

<sup>81</sup> See <http://www.news-medical.net/?id=20718> retrieved at 15 november 2007: 1,500 animal species practice homosexuality.

selection “it does not require (...) *direct* descendants; all it requires is the presence of a configuration of elements in one generation making more likely the presence of the same configuration in the next generation” (our italics). Thompson’s criterion is too loose as he would treat birds (or their DNA) and bird’s nests on an equal footing (see **o**); however, Thompson’s point about the indirectness of trait lineages in descendants is well taken.

iii). Many behavioural patterns result from a genetic predisposition combined with fine-tuning by practice.<sup>82</sup> These patterns replicate just like normal genetic traits with the extra requirement that generation after generation there are contexts in which a) parents or other care-takers correct their children or b) new-born animals have ‘spare-time’ to do trial-and-error-learning. *In the former case replication of the pattern comes to a halt when children grow up without parents; in the latter case this need not be the case as any animal born can reinvent the trait by fine-tuning, unless developing the trait is a prerequisite for replication.*

There is a controversial mechanism which relates fine-tuning and genetic predisposition the other way round: the so-called Baldwin effect named after the American psychologist Mark Baldwin who proposed it in 1896. The mechanism is so controversial because it looks like Lamarckian evolution of acquired traits, although it is not, since it involves only the inheritance of the *tendency* to acquire certain traits. The Baldwin effect (or the Baldwin mechanism as we prefer to call it) postulates selection upon (*general*) *learning* mechanisms so that animals can learn to respond to changing environments; animals that are better at learning to respond to changed environments have better survival chances and in the end of the selection process the learned behaviour becomes so good that it almost resembles an *instinct*. Supposed examples are the learned response to a new predator, man’s plastic brain, and man’s genetic tolerance for lactose.<sup>83</sup>

These examples show that despite the description of the mechanism very different things fall under the phrase heading of the Baldwin effect. We italicized three words in the description of the mechanism: a) general, b) learning, c) instinct. a) general: animals can have highly specific learning abilities such as that of the little duckling that quickly learns to follow his mother (by priming) or a little more general abilities such as that of a little colt that quickly learns to walk, as well as more general learning abilities such as a monkey’s ability to solve little puzzles or man’s ability to reason. It is unclear what level of generality the Baldwin effect refers to. b) learning: the concept of learning in biology normally involves *brain* processes that are used to overcome environmental challenges, but the example of lactose tolerance shows that,

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<sup>82</sup> See for examples David Moore’s *The Dependent Gene: The Fallacy of Nature vs. Nurture* (2001).

<sup>83</sup> Lactose intolerance increases with age until adulthood and is quite common around the world. Although as low as 2% of the Swedish in adulthood have this condition, 75% of African-Americans and as many as 98% of Thais suffer from it. That is why Thai curry dishes are based on coconut milk instead of normal milk. See Norman Kretchmer, Lactose and Lactase, *Scientific American*, October, 1972. The reason for the high European tolerance is that the Indo-Europeans were dairy breeders who drank the milk of their livestock. Children who could not digest this milk died (at least in times when milk was the only available food) and so the genes for lactose tolerance spread in populations of dairy breeders. Note that in a population that tolerates lactose better and better, the collective rewards of dairy breeding become higher and higher. Thus there is a positive feedback between the percentage of lactose tolerance in a population and the scale of its dairy culture. But not only the human genetic make-up became adapted to the milk; the genes for milk producing of cows became adapted to human taste. Horse, pig, and even gaur and bison milk is much less adapted to human stomachs. This is not an isolated coincidence: it is a general law of Darwinism that things that go through the same replication channel become more and more adapted to each other. We would call this process co-evolution instead of the Baldwin effect insofar as we are skeptical about the exact definition and workings of the Baldwin effect. On co-evolution, see §1.23.

for the Baldwin effect, learning just means responding to an environmental challenge using *any organ* (such as the stomach that “learns” to tolerate lactose). However, as such learning denotes almost nothing more than environmental interaction which is quite a vague and broad concept, c) instinct: the occurrence of the Baldwin effect involves two phases: first animals are said to learn by trial and error and *general* learning mechanisms, then their children are said to learn by *specific* imitation (due to a genetic tendency to acquire this *specific* form of behaviour) so that in the end the learned behaviour becomes like instinct. What is problematic, though, is that in the first phase animals are said to have been selected for general learning abilities and thus morphological plasticity in response to the environment, whereas the resultant ability in the second phase is highly specific and not plastic at all (which might not be very beneficial as environments keep changing). Moreover, the genetics of the abilities in the two phases are likely to be unrelated, which makes it hard to speak of a true transition mechanism and to draw a clear lineage of the trait(s). Due to the vagueness regarding these three points we prefer to forego talk of the Baldwin effect (see also Bateson 2004 and Sterelny 2004) and analyze the mechanisms subsumed under this heading by one using concepts such as co-evolution, extended phenotypes, etc.

**q) populations (intra-demic groups):**<sup>84</sup> many species of organism live together, either because they have little means of transporting themselves (bacteria, plants), or because it confers benefits upon them to do so (schools of fish, herds of mammals) or because of a combination of both. Within such a population i) sometimes individuals sacrifice themselves for others as for instance parents do for their children and ii) sometimes animals cooperate reciprocally.

i) In the early literature sacrifices were explained by group selection or as we call it by naïve species selection: an individual sacrifices itself because that is good for the species as a whole (Wynne-Edwards 1962): e.g., a bird makes alarm sounds to warn its fellows even though this focuses the attention of the predator towards itself, because the warning is good for the species as whole. This idea of naïve species selection was firmly criticized by J. Maynard Smith (1964) and G.C. Williams (1972) using Hamilton’s rule and the idea of kin selection (see **m**) bees and beehives), as well as by Robert Trivers with his idea of maximizing the altruistic *parental investment* towards genetically related children (1972/3/4). E.O. Wilson (1975) and Dawkins (1976) made this critique on *naïve* species selection famous.

However, more recently the balance has shifted again a little in favor<sup>85</sup> of group selection, now called *multi-level selection*. David Sloan Wilson and Elliott Sober (1994, 1998) claimed that the case against group selection has been overstated, arguing that groups can have functional organization in the same way individuals do and, consequently, that groups can also be the object of selection with cooperating

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<sup>84</sup> Biologists distinguish intrademic groups from interademic groups. In the case of intrademic groups, groups are formed during part of the organismic life-cycle as is for instance the case for most mammals; in the case of interademic groups, the groups are formed at the moment of birth and remain the same during the life cycle as for instance with bee colonies. In this paragraph we restrict ourselves to the former. For an example intrademic groups see **m**) **on bees and beehives**.

<sup>85</sup> E.O. Wilson, the father of sociobiology, recently argued some 30 years since his influential book on sociobiology (Wilson 1975) appeared that kin selection could no longer be seen as the mechanism underlying extreme eusociality. E.O. Wilson’s early work had relied heavily upon his study of the hymenoptera such as bees and wasps with their remarkable haploid-diploid replication mechanism which buttressed the kin selection model. More recent work shows that eusociality is not constrained to the hymenoptera but that a large number of other lesser-known species of insects cooperate as well, although their replication mechanism is not haploid-diploid. That is partly why E.O. Wilson changed his views. See his foreword in D.T. Costa’s *The other insect societies* (2006).

groups out-competing non-cooperative groups. This can only happen, however, when the selection pressure on the group as a whole is stronger than the selection on individuals within a group to exploit the group at their own expense. That is why modern group selection is called multi-level selection, taking into account both selection on the group versus other groups and of individuals in groups with other animals within this group.

We will not go into the details of the literature on multi-level selection; it is an empirical question whether good examples of it can be found. The only problem we have with the literature on multi-level selection is that it is sometimes claimed that *groups replicate*. However, groups can't be said to give rise *directly* to offspring of groups; i.e., *groups do not form clear lineages of reproduction (ancestry)* due to interbreeding of parts of groups with other groups.<sup>86</sup> Thus groups *reoccur* but do not *reproduce* as we would put it, *despite the fact that selection may act on groups (or animals within groups) instead of individual animals*. We come back to the issue of group selection below as we distinguish *units of replication and units (or loci) of selection*. An important difference between reoccurrence and reproduction is that there is no reason why the *number of the reoccurring things* would change over time, whereas the concept of reproduction includes necessarily *that the number of replicated entities can increase* (or decrease).

ii) Reciprocal altruism has been studied a lot since Trivers's work (1971); reciprocal altruism can occur between both genetically related *and* genetically unrelated individuals. It has no consequences for the concept of replication except that it explains how species might co-evolve (and co-reproduce/co-replicate).

**r) species or higher taxa:** naïve species selection (see **q**) is nowadays ruled out; multi-level selection, though still controversial, is often accepted, with populations at the highest level of the hierarchy. Some people nevertheless want to allow for selection at higher levels than the population level such as species or even higher taxa. The famous biologist Stephan Jay Gould (2004) for instance defended the view that there are macroevolutionary processes that shape evolution at and above the level of species, which are not driven by the microevolutionary mechanisms at the basis of the Modern Synthesis. If one views species as individuals<sup>87</sup> that replicate (by cladogenetic speciation) and die (go extinct), then it seems species could be subject to selection too so that their occurrence changes over geological time, much as heritable selected-for traits change in frequency over the generations. Indeed, we can find differential persistence of 'species' in the fossil record (just like Darwin found that the number of finch species must have increased from one original species to almost as many species as exist in the Galapagos Islands). However it is nearly impossible to demonstrate whether in these cases species-intrinsic properties (instead of properties of genes, cells, individuals or populations) have been the object of selection. Moreover, defining what exactly counts as a species forms another challenge (see §1.22). Further, can we really equate speciation with a form of replication? Speciation happens when two species are split and no longer interbreed; in reality this is often not a discrete one-time event: groups interbreed but at some time two extremes of the group (geographical separated extremes of an interbreeding

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<sup>86</sup> Of course we can study populations at time  $t_1$  and at time  $t_2$  and we can observe that the offspring of some of the individuals of group  $x$  at time  $t_1$  make up group  $y$  at  $t_2$ , but we think we cannot say that the group  $y$  itself is the offspring of group  $x$  insofar as members of group  $x$  may still be members of group  $y$  and even beget children with members from group  $y$  or another group  $z$ , etc.

<sup>87</sup> The view that species are individuals has been popular since Ghiselin postulated this view as a radical solution to the species problem (Ghiselin 1974). The word 'individual' as applied to species is in our view, however, a very loose, misleading way of speaking.

continuum, or morphological extremes fixed by sexual selection, etc.) are no longer able to interbreed.

The process of speciation is often a continuous process with two groups within a species being less and less able to replicate rather than a one time off cut of the possibility of interbreeding (see §1.22). The non-discreteness of the process does not distort replication lineages as much as in the case of intra-demic populations: intra-demic populations could not be said to give rise to unique and clear lineages, whereas speciation does seem to give rise to two clear lineages of new species. However, upon closer inspection, the non-discreteness of the speciation makes it hard to speak of *replication* too. Speciation can better be described as *splitting* instead of *replication*. Finally, one clear form of replication is cloning, and to speak of replication it is at least required that the resultant replicated entities resemble the original beings. In speciation, however, the pseudo-replication gives rise to two *very distinct* new replicators. I.e., the replication mechanism of speciation is more of a splitting than a true replicating mechanism in which parents (here: parent-species) and offspring (here: offspring-species) do not form lineages but *only* branches at each generation (speciation event). Darwinian replication can better be restricted in such a way that it necessarily involves *both* lineages and branches and thus excludes speciation as a pseudo-instance of replication.

### **A definition of Darwinian replication in biology**

We have now seen a lot of replication mechanisms. Using them, we can abstract towards a formal definition of replication. After explaining the elements of this definition we turn to the problem of the unit of replication (gene versus something else).

We define *Darwinian replication in biology* as the temporal process in which one or more initial, relatively complex but stable, tokens of certain kinds (called replicators) become two or more either identical or highly similar tokens of the same kinds (and their intermediate forms) that at least in principle can themselves be the subject of replication too in such a way that i) they form univocal (usually vertical but possibly sometimes also partly horizontal) lineages and sometimes branches of generations of replicators, and that ii) the replicators causally determine, either directly or indirectly, their own replication process in a systematic way.

An explanation of the details of this definition:

- *temporal*: replication is a real process in actual space-time; in a multiplication process one entity becomes two or more, but in the mathematical notion of multiplication the temporal side is absent; that is why we include the word temporal in our definition of replication.
- *process*: we use process instead of act to distinguish the process of replication from the act of replication, the latter is part of the former but not necessarily the other way round. The act of replication, moreover, is ambiguous: it can mean having sex or the actual moment of copying of a replicator. However, most replicators do not have sex to be replicated and the actual moment of replication is vague: at what moment during the many phases of cell division is DNA exactly copied?<sup>88</sup> To include non-sexual replication and to get rid of this vagueness we speak of the *process* of replication. Finally, the word 'act' sounds *active* as if the replicators are *directly* involved in their own replication, but some replicators are rather *passive* during their

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<sup>88</sup> That is why we also prefer to use replication process instead of replication event as the word event seems to require that we can clearly point to a unique moment when the replication is actually done.



replication (for instance, prions, viroids, epigenetic settings, etc).<sup>89</sup> Furthermore some replicators are not *directly* involved in their replication in the sense that they do not really replicate themselves as the case of homosexual behaviour through the children of one's sister showed. That is why we speak of the process instead of the act of replication as we want to include all these cases under our concept of replication in biology.

- *one or more*: in some replication process, the process only starts when two replicators are copresent and replicate at the same time: think of the RNA-world in which multiple RNA-strings replicate in each others presence. Think also of sexual replication in which the chromosomes of two individuals are mixed into the chromosomes of a new individual/new individuals.

- *relatively complex*: this is to exclude chemical replicators such as crystals with their ionic bonds that are simple two- or few-atom combinations; as we spoke of chemical replicators we said that replicators need to have at least a certain size. Size can be used to characterize chains of atoms but if a replicator is not made up of atoms in any direct sense of the word (the epigenetic settings, homosexual behaviour), we need to use the more general vocabulary of *complexity*; we will not place a lower bound on the degree of complexity and we will not give a mathematical expression of the complexity involved. Complexity is here taken as a relatively intuitive notion; this will become clearer as we discuss the notion of information under the heading "of the same kind". This vague notion of complexity is needed in the definition of Darwinian replication because too simple structures do not allow for much (heritable) variation and the process of Darwinian evolution requires that the replicators allow for variation. Implicitly we thus smuggle variation into the definition of replication with the word complexity, but we think in an innocent way as we only say that what is replicated should be able to be subject to (heritable) variation.

- *relatively stable*: this is to ensure that replicators can be identified in multiple generations as the same; part of the meaning of the word 'stable' is also covered in our definition of replication under the heading "univocal lineages", part of it under "of the same kind", yet one can have univocal lineages without stability and one can have replicators of the same kind (phenotypes or even extended phenotypes) that are not really stable or whose stability is due to the stability of another unit. Dawkins (1978) understands stability ultimately not as the stability of a traditional substance but as chances to be copied: individual replicators need not be very stable structures if only they get faithfully copied on average before they breakdown; this kind of stability is implicitly meant in our definition.

- *either, or*: this 'or' has to be read as follows: the outcome of a replication process can be similar but at least sometimes has to be a little dissimilar. Darwinian replication can lead to absolutely similar replicas as in the case of cloning; most of the time due to mutations, replication errors, recombination, etc. replicators do not look exactly like

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<sup>89</sup> The distinction between passive and active replication, between being replicated and replicating oneself may be partly due to a linguistic convention, without a real difference in nature between replicating oneself and being replicated. Evidence of the linguistic character of this distinction is the fact that for instance the German verb 'to replicate' is reflexive, i.e., 'sich replizieren', a verb that can be used for both 'active' and 'passive' forms of replication (thus for replicating oneself and for being replicated). As the German language makes no difference, but the English does, why not suppose that English has a grammatical distinction that is of no use in describing reality? On the other hand, the fact that we tend to say that genes replicate themselves, whereas genetic settings are being replicated, may have something to do with causal primacy: genes are seen as true replicators and epigenetic settings as depending upon them (although genes are not be copied when epigenetic are not set). See below on the gene as replicator.

their parents. For a replicator to be a Darwinian replicator, however, it is necessary that it is at least sometimes heritably different from its parents because otherwise there could be no evolution (as in the case of crystals). Again we smuggle ‘being possibly subject to *variation*’ into our definition of replication.

– *of the same kinds*: the words ‘same’ and ‘kind’ need elucidation. The word ‘kind’ will be analyzed in §1.22 as we discuss the species problem in biology in general; the word ‘same’ is our attempt to speak of heritability in more general terms. Without heritability or sameness the traits selected for by natural selection could not be retained and hence there would be no evolution towards increasing complexity. It would be theoretically helpful if the notion of complexity and sameness/heritability could be made more precise. One could think of the notion ‘information’, which is a mathematically precise notion since the work of Shannon on information theory (1948, 1949)<sup>90</sup> and which is nowadays used a lot in physics to characterize the information content in systems, as for instance in quantum information theory which exploits the discreteness of quantum states in physics to define physical information. The most obvious replicators bearing information in biology are the linear sequences of bases in molecules of DNA. And indeed (see Amos 2005), in DNA-computing DNA is used as a bearer of information by an *in vitro* computer that exploits the recombining properties of DNA (Adenine binds to Thymine, Guanine to Cytosine etc.). However, DNA-computing only uses the formal recombining properties of DNA as present in the short cycle of replication (see **e**) and does not use the ‘information’ DNA contains about the proteins and ultimately the phenotype it codes for (see also Küppers 1990). In normal replication, however, the true information or hereditary traits consist in DNA *as it is interpreted* in protein synthesis. More DNA (think of junk DNA, see **f**) alone does not mean more information or complexity: under **g**) we already mentioned that whereas humans have 46 chromosomes, the generally-considered less-complex goldfish has 100-104 chromosomes and an Adder’s tongue fern even 1248.

If we look more carefully at the notion of information, this becomes clear. Shannon’s notion of information is only a quantitative characterization of the relevant (‘uncompressable’) bits of data a source broadcasts and does not tell anything about the qualitative nature of this information. For instance the digital email-message: 100110010000000010000000111111101 could mean the useless spam information: *buy Viagra!* or the highly relevant and informational: *you are fired!* Shannon, however, is only interested in the pattern 100110010000000010000000111111101 as a *quantitative* pattern and how it can be compressed given the fact that it is not a completely random pattern.<sup>91</sup> DNA-computing only manipulates the quantitative

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<sup>90</sup> Shannon was a technician at Bell labs and was working on data encryption. He worked on telephone lines and tried to optimize the number of messages that could be sent through a single telephone line. One should not forget this background as one discusses Shannon’s information theory.

<sup>91</sup> The Shannon information is at a max when a pattern of bits is maximally random. The above pattern 100110010000000010000000111111101 is not maximally random as it can be compressed to for instance 10(2)110(2)10(8)10(7)111111101, with the 0(x) with x the number of zeroes after each other. More formally, Shannon defined the notion information entropy (the measure of information) as the average number of bits per symbol needed to encode a message. One thus needs a key to decipher the encrypted code back into symbols. This key can be rather complex and the more complex the key, the more messages can be shortened. The key itself is a characterization of the source and it is nonsense to speak of information in Shannon’s sense without a source and a key. It is thus wrong to speak of the Shannon information of a system as such, as some physicists do; Shannon information is the information *of messages given a certain source*. The field of data encryption is concerned with finding optimal keys to encrypt messages from different sources (i.e., keys as simple as possible to get messages as short as possible).

information contained in a string of DNA like 'ATTCGAATTT' etc. and the way DNA recombines, without looking at the qualitative impact a string of DNA has in the big cycle of replication in a certain organism through its coding for proteins and a phenotype. As such, the notion of Shannon information does not help us to characterize heredity as the heredity in gene-based selection is the heredity of phenotypical traits.

Thus if it does not work to characterize heredity and sameness by the notion of information for the clearest case in biology, it will certainly not work for other cases (see also Griffiths 2001). That is why we stick to this imprecise, intuitive definition of 'sameness' in our definition of replication. As said, in §1.22 on the species problem we do analyze the species problem and the notion kind; this will elucidate what counts as 'of the same *kind*' by giving us some pragmatic criteria. One very important one will be *sameness by genealogical descent* as cladism uses to distinguish kinds.

- *at least in principle can be the subject of replication too*: most replicators never replicate. A beautiful example is the egg of the cod. A female cod lays approximately 5 million eggs and since the population of cods is (used to be) stable (thus each reproducing cod has on average two children that reproduce as well), we can infer that at least 4.999.998 eggs/juvenile cods die before reproducing (Ridley 2004: 72). Nevertheless, we need to include in our definition of replication that replicators can at least in principle be the subject of replication too, otherwise replicators can in principle go extinct. By including *in principle* we thus exclude the cells of the immune system as true objects of Darwinian replication as they are in principle dead-end replicators: they replicate for a few generations but their lineage in principle ends with the death of their host.

- *and their intermediate forms*: to allow for metamorphosis. Some forms may be absent from some generations, as some life forms that undergo metamorphosis can reproduce before they have reached their final form.

- *univocal lineages*: this is to exclude populations and species as replicators, i.e., to exclude re-occurrences and splitting events from true replication. The univocal lineages also help in distinguishing which replicators are of the same kind and which not.

- *(usually vertical but possibly sometimes also partly horizontal)*: usually the lineages are vertical or generational. However, to allow for cases such as viral sex, bacterial DNA transfer and DNA exchange in plants we need to allow for the possibility of horizontal (inter-generational) lines in the lineages of descent.

- *causally determining, either directly or indirectly*: two things are worth elucidating: a) 'causally determining' 'in a systematic way' and b) 'either directly or indirectly'. We add b) 'either directly or indirectly' to include such things as genetically determined homosexual behaviour which was said to be copied indirectly through the genes of a sister and we add a) 'causally determining in a systematic way' to exclude extended phenotypes as true replicators. At first we wanted to write causally *involved* but the extended phenotype is causally involved in the replication of the beaver and its genes, that is why we chose causally *determining*. In one sense of the word 'determining' extended phenotypes do determine the replication of DNA (determination as involvement), but not in the sense meant here. By 'causally determining' we mean that changes (mutations in genes) *heritably* affect extended phenotypes, whereas random changes in the extended phenotype (beaver dams destroyed by the wind, spiderwebs perforated by animals walking through them) feed back on the replicating

chances of individual beavers but do not change the genes themselves in such a way that the mutation in the beaver dam is heritable.<sup>92</sup>

Finally: the above definition of replication is *circular* since the definition of replication contains the word ‘replicator’ and ‘which can themselves be the subject of replication too’. This circularity is, however, not problematic. First of all: circular definitions can be true, but one normally worries that they are uninformative; however, this definition is clearly not uninformative since it contains criteria for calling something a replicator. Moreover, since the circularity involved is actually *iteration* the circularity involved in the definition as replication is not a full-blown circularity. Iterative definitions are a type of implicit definition that are rather unproblematic, as can be seen from mathematics and computer science in which they often play a role. The only condition on an iterative definition is that an *initial condition* or a *null state* is specified: in our case a first replicator which itself did not arise by replication. Although we can only give a speculative answer to the question of the first replicator (see **a**) on chemical replicators and the RNA-world) we know that in principle this initial condition can be satisfied. That is why the supposed circularity in our definition is unproblematic.

### **The gene as replicator**

The above definition of replication allows us to look more closely at the question of the true replicator. The camps in the debate are strongly divided between Williams (1966), Dawkins (1976 (2006), 1978, 1982), Dennett (1995), Hull (1980a, 2001) and Kitcher (Kitcher/Sterelny 1988) on the one hand, who regard genes as true replicators and phenotypes as vehicles, interactors, or flesh robots, and Sober and Lewontin (1982, 1983), Sober (1984), Mayr (2004: 152) and Gould (1980, 2004) on the other hand, who see genes just as ‘bookkeeping units’ and hold that individual organisms (or even species) are the real entities in the Darwinian process of differential reproduction. The discussion was much more lively in the 1980s than today, although elements of it live on in more recent discussion around multi-level group selection and Darwinian explanations of ethics (for instance Sober and Wilson 1998). In our summary of the discussion we follow a number of authors who make a distinction between units of replication and the loci of selection (or the units of selection as they are more commonly known). This distinction quieted down the debate as one started to realize that the replicators and the loci (unit) of selection *do not necessarily coincide*.

We summarize the debate, updated with new developments since the 1980s that add some new perspectives, for three reasons:

a) outside of biology we will encounter the problem of the units (loci) of selection again and a discussion here in the field in which the problem arose will help us to get a grip on the problem outside of biology,

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<sup>92</sup> Changes in genes coding for dam building behaviour do lead to changes in beaver dams which can in turn feed back on the replication chances of beavers and their dam-building genes. The causality comes ultimately from changes in the dam-building genes and not from changes in the dam itself. Only those changes are heritable and lasting. See also the irritated denunciation by Dawkins of Bateson’s joking idea to call the extended phenotype a true replicators of its own: “No! Bateson was not right, he was not even close to being right (2004: 171).” This is easy to understand in the light of Dawkins’s theory of selfish genes. Dawkins continues: “(...) Bateson’s nest joke and others of this kind are not funny. There may be back arrows in all sorts of other senses but, in the sense that specifically matters for Darwinian evolution, the causal arrow of biological development from genotype to phenotype really is a one way arrow.”

b) outside of biology we will often find claims about a (human) ‘gene for x’: a gene for math called the math gene, a gene for belief in God called the God gene, etc. A discussion of the gene as replicator will be of real use in getting an evaluative grip on these proposals, and

c) outside of biology (in for instance cultural evolution) issues of altruism and group selection pop up again and again and since for historical and factual matters discussions about altruism became intertwined with talk of genes as selfish replicators, a discussion of selfish genes will shed light on the issues of Darwinian altruism as well.

We start our discussion of the replicator/locus of selection debate by discussing the two meanings of the word ‘gene’; then we list the arguments pro and contra the view that the gene is the fundamental replicator; finally, we make a distinction between replicators and the loci of selection and we summarize the multiple forms of replication discussed above (a t/m r) using these concepts.

### **The ambiguity in the concept ‘gene’**

It is first of all important to remind ourselves of the ambiguity in the word ‘gene’. In short: on the one hand, a gene is ‘just a string of DNA on a chromosome’ (or a piece of RNA if we speak of virus genes); on the other hand, a gene is a ‘gene *for* a certain phenotypical trait’ such as a gene *for* blue eyes, a gene *for* strong teeth, etc. Both notions of ‘gene’ play a role in the replication process; the small replication process is concerned with the replication of strings of DNA, the large cycle of replication takes the phenotypical traits that help directly or indirectly to replicate these strings of DNA and the traits into account. As is well known, the two notions of genes cannot be mapped easily: the Mendelian presupposition of one gene (one piece of DNA/RNA), one trait (one gene for something) broke down a long time ago as the relation between genotype and phenotype was investigated in more detail.

The unique correspondence broke down from both sides:

a) from the side of DNA: the same piece of DNA can code for a protein that fulfills a role in many processes, one piece of DNA can thus be responsible for many phenotypical properties; worse still, some pieces of DNA are over-coded: they are part of two overlapping ‘genes’ transcribed during protein synthesis, just as in the spoken sentence (written down pseudo-phonologically) ‘Humeets John’ [i.e., ‘Hume meets John’] the letters [‘me’ for Hume and *meets*] are over-coded;

b) from the side of the phenotype: for a phenotypical property many genes are required. There is not just one gene for blue eyes. This gene presupposes the genes for face formation, eyeball formation, lens formation, cornea formation etc., but even if we excluded these, the gene for blue eyes presupposes genes responsible for creating a blue pigment, the genes for attaching this pigment to the iris, as well as all the genes coding for the enzymes used in these processes to catalyze these possibly slow reactions, etc. Hence what is the true gene for blue eyes? The more complex a phenotypical trait (think of such a complex phenomenon as religion) the more complex (and less easy to understand) the link with DNA.

Some have tried to solve the lack of a true one-to-one correspondence between genes as DNA and genes as genes-for-phenotypical-properties by defining a gene as that piece of DNA without which this property would not develop or be maintained. But then the proteins responsible for eyeball formation would count as the gene for blue eye formation too. Fixing the definition by defining a gene as that ‘piece of DNA without which *only* this property would not develop’, will not help either for two reasons: 1) the over-coding of certain pieces of DNA: the absence of DNA would also cause other properties not to develop, 2) the vagueness of what

counts as ‘everything is the same, only the eyes are not blue’? Does that mean that the eyes are brown, or green, or do not have any colour at all, or does it mean that the iris (the body with the eye colour) is not present, or that only the pigment did not attach itself? The simple conclusion should be that the relation between DNA and phenotype is very complex; through many highly intertwined and interdependent, intermediate stages of gene transcription, protein synthesis and enzymatic catalysis the two things called ‘gene’ are linked. As yet nobody knows the exact ‘key’ to this complex labyrinth of intermediate stages.<sup>93</sup>

An extra problem in finding the key is that the second definition of a gene as a gene *for* something is ambiguous too: some genes are genes for something because they were selected for this function (a true adaptation), whereas other genes are genes for something as a byproduct of the selection process. We can use our fingers to pick our noses and in some sense our genes are responsible for the fact that we can pick our noses, but hardly anyone would speak of a gene for nose-picking; we do speak of a gene for thumb formation and a gene for grasping though. However, since we wish to postpone a discussion of the *criteria* whether something is a true adaptation to section §1.23 on selection, we will not distinguish the two senses of the word ‘gene for’ in the remaining part of this section.

Despite the difficulties in relating the two notions of the term ‘gene’, nobody doubts that in some sense pieces of DNA (genes) are responsible for phenotypical traits (genes for something). Dennett (1995) uses the metaphor of a recipe to understand the relation of DNA to its phenotype: a recipe can be called the *cause* of a dish insofar as it contains the *instructions* for preparing and serving it, although of course the person instructed and the causal relations of the ingredients are just as much causes that determine the outcome of the dish; in a similar vein DNA contains the instructions for ‘building and maintaining’ an organism, but the cells instructed (the proteins prescribed or coded for) and the causal relations between the building blocks needed are just as much real causes that determine the outcome. In sum: just as there is no dish without ingredients, there is no organism without the cells and building blocks (food) it assimilates, yet recipe and DNA are fundamental in *shaping* the dish/organism.<sup>94</sup>

### **Genes as replicators, pros and cons**

With this preliminary elucidation of the concept of gene we can look at the controversy over whether DNA or organisms (or other entities) are the true replicators. We will see that this issue has relatively little to do with replication, but more with selection. We will first mention the advantages and then the disadvantages of seeing genes as the fundamental replicators.

The advantages of seeing DNA as the true replicator have to do with: a) parsimony, b) selfish DNA, c) altruism (kin selection), d) copying fidelity, e) the Weisman barrier, and f) distinguishing replicating things from things that just reoccur.

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<sup>93</sup> The code of human DNA has been cracked by the genomics project in 2001 (although a lot of details are still to be filled in). The project to unravel the labyrinth of the connection of DNA to its phenotype is called *proteomics* and is still in its infancy. This project is many, many times more difficult.

<sup>94</sup> The analogy should not be extended too far. It is useful in providing an image of how to understand the causality between genes and phenotypes which is one of instructions and outcome; however, there are also non-instructional causal arrows between DNA and phenotypes (which are absent in the case of a normal recipe) insofar as the structure of DNA in chromosome chains provides support to the chain in regions prey to mutation as near the centromeres or the telomeres. See also footnote 63.

**a) parsimony** (“less is better”): with the help of the gene view of replication many forms of replication can be reduced to the special case of gene replication, from the above list all cases except **h**, **i**) and **r**) are dealt with from the gene point of view by Dawkins (1976, 1982); he also explicitly speaks of the positive parsimonious character of his explanations (and so do Kitcher and Sterelny 1988). It is a general principle of science to prefer more parsimonious explanations. However, not all replicators, even those inside a genotypic-phenotypic environment, can be dealt with from the gene’s point of view as case **h**) about epigenetic settings showed. Thus the gene’s point of view is not an all-encompassing solution to all cases of replication inside biology.

But let us bracket **h**) for the moment and consider what would actually be won if all evolution could be described from the perspective of the gene. “Nothing,” according to Sober and Wilson (1994) who hold that the claim of the gene as sole true replicator is either “false” or “vacuous”. It is vacuous if the claim means that genes are the units of heredity and that as such every Darwinian evolutionary process can be ‘represented’ by discussing genes, a claim with which it is hard to disagree but which is hardly informative; it is false if it is meant that the gene is the *causal nexus* in the evolution of traits: the supposed parsimonious view of gene selection has to argue *how* in each case a gene is involved (which will lead to very unnatural stories about causality), whereas an explanation that postulates other or even multiple levels at which selection occurs is often much *simpler* and *more readily applicable* to new situations in which the relation of a gene to a higher class in the hierarchy is as yet unknown (see below on screening off). Thus gene-selectionism can be argued to be less parsimonious. Finally, next to the principle of parsimony stands that of methodological pluralism: why restrict oneself *a priori* to one point of view if others are available? Even Dawkins (1982), especially in the first and the final chapter, seems to admit the usefulness of a plurality of viewpoints as he speaks about two perspectives on evolution (the gene view and the organismal view) that are like the different faces of a “necker-cube” even admitting that there is no “empirical” test to demonstrate the truth of his “new perspective.”<sup>95</sup> Its ‘truth’ could then only be demonstrated by the ‘fruitfulness’ of the domain of ‘research questions’ it opens up.

Nevertheless, to the defense of gene selectionism one might point out that unless one defines parsimony and plurality strictly (class of phenomena to be explained, scope, etc.) discussion of which explanations are more parsimonious is sterile wordplay; as two theories differ in many respects and as they explain the ‘same’ group of phenomena differently they are sometimes more complex and sometimes simpler than their competitors. If, however, one defines these concepts too strictly the answer to the question *which explanation is more parsimonious?* becomes trivial (simply because the restrictions favour one of the competitors).

Putting meta-scientific evaluations aside, the only simple critique against the *parsimonious* nature of gene based selection (with vehicles) is thus the first remark above that gene-based selection is *not parsimonious enough*, since some replicators cannot be interpreted from the gene perspective.

**b) selfish DNA** (see also **d**) above on selfish DNA): Doolittle and Sapienza (1980) and Orgel and Crick (1980) first discussed selfish DNA, which implied that the gene’s perspective had to be taken seriously: the spread of selfish DNA is detrimental to its host, the organism, and thus it can only be explained from the perspective of DNA, not that of the organism or the cells inside which it replicates.

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<sup>95</sup> See also the new introduction by Dennett to the second edition (Dawkins 1992).

However, opponents argue that the replication of selfish DNA is either almost-neutral (which, however, seems unlikely given the fact that there are mechanisms to “unmask cheating genes” during meiosis (Crow 1999), to remove over-abundant copies, and to correct miscopies during mitosis) or the result of accidental non-adaptive replication, i.e., replication of entities that are not selected to copy themselves but are accidentally copied. However, being accidentally better at replication (in a *systematic* way as is the case here) is an important criterion to be recognized as an adaptation (see §1.23); that is why the few opponents of the idea of selfish DNA need to make a much stronger argument before they can defeat the idea of selfish DNA.

**c) altruism (kin selection):** ever since Dawkins’s famous book *The selfish gene* (1976), discussion of altruism in biology and the gene’s perspective have been entangled. In this book Dawkins reinterpreted Hamilton’s work on kin selection (1964). Hamilton formulated his theory in terms of *organisms* that are related to each other (i.e., in terms of kin), but Dawkins reformulated Hamilton’s results from the perspective of an organism’s *genes*. Dawkins gave many examples of behaviour that is detrimental to an organism, but profitable for its genes/DNA.<sup>96</sup> Such behaviour can be called “un-selfish” from the perspective of the organism, although “selfish” (i.e., evolutionary advantageous) from the perspective of its genes. Hence, the name *selfish genes*.

It is important to distinguish between the highly technical notion of evolutionary selfishness/altruism which is strictly related to fitness effects of actions from psychological selfishness/altruism which is based upon motives and moral standards behind actions (Sober and Wilson 1998). If this distinction is taken into account the claims of Dawkins and other sociobiologists either turn out to be false or appear much less controversial than they seemed beforehand, although we forego a discussion of evolutionary ethics here to §2.1.<sup>97</sup>

What is important is Dawkins’ reinterpretation of Hamilton’s work are its advantages: although one can formulate kin selection in terms of kin, it is in practice hard to ensure you have all the relevant kin included in the equations of kin selection (for instance in cases of the evolution of homosexuality, see **p**i above).<sup>98</sup> If one takes the gene’s perspective the problem of all relevant kin is easily overcome because one focuses on the process of transmission of a trait from one generation to the next (a

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<sup>96</sup> A particular blunt example by Dawkins 2006 (originally 1976): 125ff: “Runts constitute a particular example [of gene-based selection]. We can make some (...) general predictions about how a mother’s tendency to invest in a child might be affected by his age. If she has a straight choice between saving the life of one child or saving the life of another, and if the one she does not save is bound to die, she should prefer the older one. This is because she stands to lose a higher proportion of her life’s parental investment if he dies than if his little brother dies. (...) On the other hand, if her choice is not such a stark life and death choice, her best bet might be to prefer the younger one. For instance, [since] (...) the big one is more likely to find his food unaided (...). There [always] comes a time in the life of a child when it pays the mother to divert investment from him into future children. When this moment comes, she will want to wean him (...). [129] Sometimes, one member of a litter is a runt, much smaller than the rest. He is unable to fight for food as strongly as the rest, and runts often die. (...) We might suppose intuitively that the runt himself should go on struggling to the last, but the theory does not necessarily predict this. As soon as a runt becomes so small and weak that his expectation of life is reduced to the point where benefit to him due to parental investment is less than half of the benefit that the same benefit could potentially confer on the other babies, the runt should die gracefully and willingly. He can benefit his genes most by doing so (...) and let himself be eaten by his litter-mates or parents.”

<sup>97</sup> But see §2.1 under evolutionary ethics and n).

<sup>98</sup> Sterelny and Kitcher 1988 make a similar point providing many examples of well-known biologists making mistakes when calculating kin selection using the perspective of the organism.



homosexual brother helping his polygamous sister to raise her offspring etc.). That is a good methodological reason to prefer the gene's perspective.

**d) copying fidelity:** Dawkins (1982/1996) characterizes replicators by their copying fidelity and argues that phenotypes or extended phenotypes are not copied exactly enough to be seen as replicators; however, the notion of exactness is left undefined and thus there seems to be no reason to disqualify phenotypes as replicators on the basis of copying fidelity: maybe they are "less faithfully copied" than genes, but "still faithfully enough" to be recognized through multiple generations (Sober and Wilson 1994). Moreover, chemical replicators (like RNA) were copied really erroneous, whereas phenotypes seem to be rather stable; according to Dawkins one should thus deny chemical replicators the role of replicator, which seems to go against the spirit of his work. However, Dawkins could argue oppositely that the exactness of phenotypic replication is solely due to the even higher copying fidelity of DNA, which brings us to the next point.

**e) the Weisman barrier:** this barrier separates the germ line cells from the mutations in other body cells and thus prevents any direct influence or "causal back-arrows" of the phenotype on the replication DNA. For Dawkins this barrier is one of the main motivations for his view, since differences in genes determine differences in phenotype and not the other way round (Dawkins 1996, 1982a: 85, 1982b: 51). However, we saw that the Weisman barrier is breached by **b)** retro-viruses which reinscribe themselves into the DNA of germ-line cells and by **h)** epigenetic rules. Retro-viruses are not problematic from the gene perspective as their RNA is modifying chromosomal DNA, just as selfish DNA does and the existence of the latter has proved to be one of the argument's *for* the gene perspective, but the epigenetic settings cannot be covered by the gene-based view of evolution.

**f) distinguishing replicating things from things that just reoccur:** it is terminologically useful to distinguish things that can be said to replicate themselves and thus form clear lineages from things that merely reoccur. See above **p)** and **q)** on populations and species. When one takes the gene's perspective, replication and reoccurrences are most naturally distinguished (especially when one disregards chromosomal recombination, see **b)** below).

The problems of seeing the gene as the main replicator are a) heterozygote superiority, b) recombination, c) shielding-off, and d) book-keeping.

**a) heterozygote superiority:** Sober and Lewontin (1982, 1983) and Sober (1984) argue against the gene-based perspective from the case of heterozygote superiority: as is well known, heterozygote superiority occurs when organisms have a higher fitness in case they have a copy of both the dominant allele 'A' and the recessive allele 'a' of a certain gene<sup>99</sup> compared to organisms having either 'AA' or 'aa' as their genotype (see Ridley 2004: 123-7). The most famous case of heterozygote superiority is that related to sickle cell anemia; the 'Aa' genotype is in this case believed to protect its host to some extent against malaria, but the 'aa' genotype causes sickle cell anemia which can lead to deadly strokes and serious damage to organs. The replication chances of the 'A' gene thus depend on the version of the

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<sup>99</sup> Due to inbreeding deleterious mutations will become fixed in sexually isolated populations; when these isolated populations come into sexual contact again after being separated for some time their offspring will have less deleterious mutations; this is a case of *global* heterozygote advantage. Here we are, however, concerned with heterozygote advantage *at a certain locus on the chromosome* (the allele).

similar gene on the other chromosome present ('A' or 'a') and thus, it is argued, (single) gene selection is not a correct description of natural selection.

However, as Kitcher and Sterelny (1988) argue, this is not a real objection to the gene perspective when one argues from this perspective all the way long. Just as the survival and replication chances of individual organisms sometimes depend on the population in which they live and the frequency of certain types of other individuals within a population (think of the famous Hawk-Dove model, see §1.23), so the survival and replication chances of a gene depend on the other genes present (part of the selection environment of individual organisms is made up out of other individuals, part of the selection environment of genes out of other genes).

The proponents of organismal selection may then, in line with their earlier critique, switch to multi-level selection and claim that just as for the replication of genes the organism is the level of selection, so for the replication of individuals the population is the level of selection. The gene theoretician might then counter that all the levels of the hierarchy depend ultimately on gene replication. However, we forego a detailed discussion of multi-level selection here till c).

Proponents of the gene perspective may plead that heterozygote superiority is not fairly common (although not everyone agrees on this).<sup>100</sup> At least some biologists believe that heterozygote superiority originates when a favourable mutation has recently arisen in a population on one of the members of a pair of chromosomes (and not on the other). In the course of evolution due to linear shifts and whole insertions of DNA sequences during DNA replication the two supplementary alleles will become located on one and the same chromosome as one longer genetic sequence; if this happens the longer gene will become fixed as a homozygote in the population.<sup>101</sup> This will happen relatively quickly and that is why heterozygoteness is rare. As a positive side effect of the fixing of the new longer gene, the earlier disadvantages homozygotes will go extinct. If this story about the origin of heterozygoteness is true, it is told from the perspective of the genes and thus for the discussion of heterozygote advantages the gene perspective might actually be something helpful instead of a challenge.

**b) recombination:** in sexually reproducing creatures DNA is recombined during meiosis by the crosslinking of chromosomes of the same type; this process eventually breaks down any long strings of DNA, the longer the faster. If one regards longevity as a characteristic of a replicator, and if one defines a gene as any arbitrarily long piece of DNA that can be identified through multiple generations (as Dawkins did in 1976), then the gene cannot easily be regarded as a replicator since recombination breaks down long pieces of DNA in but a few generations: the phenotype is much more stable cross-generationally and should thus be seen as the

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<sup>100</sup> Just from a mathematical point of view: if there were i) no mutation and zero selection pressure and ii) we can assume random mating evolution (which tends to fix homozygoteness), the rate at which heterozygotes decrease in population of  $2N$  gametes is  $1/2N$  per generation. See Ridley 2005: 148 for the details of the mathematical calculation. When the above conditions do not apply, heterozygotes will remain present in a population for a longer time, but not that much longer unless there is strong non-random mating and the mutation rate from homozygotous into the heterozygotous is non-small (which is usually not the case). This means that in the case of heterozygote superiority there must be a strong selection pressure because of the evolutionary advantage heterozygotous advantage offers, and the mathematical demonstration shows that this advantage needs to be quite substantial. Otherwise, the heterozygotous genotype decreases at the fast rate of  $1/2N$  per generation.

<sup>101</sup> This change may need to be accompanied by changes in regulatory genes that determine when genes are transcribed since the double set of chromosomes containing each long new genetic sequence may lead to overdosage of certain proteins.

true replicator.<sup>102</sup> However, the genetic material present remains the same during recombination (apart from the possible mutation in DNA coding for proteins due to recombination) and thus if the proponent of the gene perspective includes the clause that a piece of DNA may be spread out over the whole chromosome, the case of recombination is not a serious objection to this perspective.

**c) bookkeeping:** some, like Gould (2004), argue that genes are causally (almost) inert and their role is just to keep a record of the information needed to build a cell, whereas natural selection is a process that requires the causal interaction between one entity and the environment; if anything could take the role of this interacting entity it is the organism. In the chicken-and-egg situation of genes requiring cells and the other way around, Gould would thus put the emphasis on the role of cells. Against this idea we already used the metaphor of a recipe to describe the causality involved between a gene and its phenotype. This involves a form of causality which is far removed from billiard ball ‘intermediate’ causation but the pseudo-informational form of causality is a real form of causality too.<sup>103</sup>

**d) screening off:** screening off used to be one of the biggest arguments against gene-based selection and was made explicitly by for instance Brandon (1984), although it had been expressed earlier in a metaphorical form by Gould (1980a): “selection acts on phenotypes” and Mayr (1963: 184): “selection favours (or discriminates against) phenotypes, not genes or genotypes.” The notion of screening off originates in the work of Salmon (1971), and without all the math can be expressed as ‘if A renders B statistically irrelevant with respect to outcome E, but not vice versa, then A is a better causal explainer of E than B.’ Or, put differently: proximate causes screen off remote causes from their effects. Take ‘A’ to be the phenotype, ‘B’ the genotype, and ‘E’ the evolution of a trait. Since there is no direct correlation between A and B<sup>104</sup>, Brandon argues that fitness depends on the phenotype (the big cycle of replication) and that selection acts at the level of the phenotype.

### **Multi-level selection, replicators and the locus of selection**

In 1988 Brandon expanded the notion of screening off to explain multiple-level selection, distinguishing like Hull between replicators and interactors<sup>105</sup>, with the former being screened off by the latter in the aforementioned causal sense. This model is useful but still too monolithic. Sober, who as we saw was highly critical of gene-selection, showed that different traits of a living being could be selected for at different levels: an organism’s altruistic drive at the level of the group as a whole and an organism’s selfish drive at the level of the individual or sub-groups such as kin (and out of these opposing selection forces a meta-stable balance between

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<sup>102</sup> See footnotes 52 and 75, and Hull 1981 who in similar vein but oppositely argues on the basis of the absence of recombination in asexual organisms that in asexual organisms (in strong contrast with sexual organisms) the organism is the replicator.

<sup>103</sup> Moreover, under **a)** chemical replicators we described the RNA-world in which RNA was both the informational barrier and the direct causal interactor and how within this world DNA has supposedly evolved. This evolution offers an immediate causal story of how DNA grew into the role of an informational carrier that is only causally active at a distance. It clearly demonstrates that DNA is or at least has not always been a causally inert bookkeeping device.

<sup>104</sup> An animal can have a gene but the phenotype fails to be realized due to interference of other genes or environmental condition, or the other way round: a certain phenotype may be induced judiciously without the gene present.

<sup>105</sup> We call interactors the ‘loci of selection’. Usually the locus of selection is called unit of selection, but we refrain from that terminology because the word ‘unit’ is too strongly associated with corpuscular ideas.

cooperation and cheating can arise). Sober defines “Objects at level X were units of selection in the evolution of trait T iff one of the factors that influenced T’s evolution was that T conferred a benefit on objects at level X (Sober and Wilson 1994: 436).” That is a very useful definition, because it opens the possibility of an entity’s being shaped by forces at different levels because of selection on its *traits*.

After introducing multiple levels of selection in this way, Sober as far as we know refrains from replicator-talk. We do not know the reason for this, but maybe he thinks that if one understands his definition of units of selection, talk of replicators is either confusing or superfluous: he has brought genes back to earth by regarding them as nothing but bookkeepers of hereditary information. Organisms replicate just in the normal way ethologists describe and the (replicative) subsistence of traits is due to selection at the level of the relevant trait.

We will opt for a combination of Brandon’s and Sober’s views: We do think that replicators are important for reasons outlined above (parsimony, copying fidelity, clearer view of some sorts of kin selection, distinguishing replicating things from things that just reoccur), but we also want to allow for the possibility that the same replicators are shaped by selection at multiple levels. An important element of this distinction is that there are no direct causal back-arrows between replicators and interactors (unless they coincide), just as due to the Weismann barrier the changes in phenotype were not supposed to be reflected back into the genotype. We know that the evolution of epigenetic settings breaches the traditional Weismann barrier, but the evolution of epigenetic settings itself can nevertheless be modelled by the more general scheme of an unchanging replicator and a (variable) locus of selection (see table below).

As mentioned above, the replicator and the locus of selection can coincide as in the case of selfish DNA and chemical replicators. That they often do not coincide in biology is due to the distinction between genotype and phenotype that fundamentally shaped biological evolution. However, outside of biology, *we cannot always expect to find an analogous distinction between genotype and phenotype when we are considering extra-biological entities shaped by (pseudo-)Darwinian processes. We might find that replicators and the locus of selection here usually coincide. Even then, though, it is useful to distinguish an entity as a replicator and as being shaped by selection.*

If we now look again at the examples of replication mechanisms above we can use the terminology to analyze these mechanism in terms of replicators (units of heredity) and the (primary) loci of selection (table 1.2). When there is no unique locus of selection associated with the replicator, we will point that out. This approach removes the conflict between the two camps in the gene-selectionist debate; it combines the central role of genes or DNA as replicator and bearer of heredity (a perspective from which evolution can almost always be looked at) with the view that causal (non-informational) evolutionary forces most often act on higher levels than DNA or genes.

<b>entity</b>	<b>units of replication/heredity</b>	<b>(primary) loci of selection</b>
a) chemical replicator (RNA-world)	RNA	RNA
b) - viroids - virusoids - prions	- single stranded RNA - DNA (or RNA?) - prion (protein)	- viroid capsid - virusoid capsid (+ virus combination?) - prion (protein)
c) viruses	- single/double stranded RNA or DNA	virus capsid
d) selfish DNA	DNA	DNA
e) DNA	DNA	phenotype (big cycle of replication)
f) cytoplasmic DNA	DNA	phenotype (big cycle of replication) but in case of meiosis distorting: gametes
g) chromosomes	DNA	phenotype (big cycle of replication)
h) epigenetic settings	proteins/other particles that offset DNA-reading	phenotype (big cycle of replication)
i) cells immunesystem	not replicators according to our own definition	n/a
j) bacteria	genes and or parts of DNA in exchange	phenotype (big cycle of replication) or networks (populations of cells); in case of parts of DNA in exchange possibly also DNA
k) plants	Sexual: genes Asexual: genome	phenotype or parts of the phenotype (cuttings of plants)
l) animals	Sexual: genes Asexual: genome	a) phenotype or b) parts of the phenotype (splitted starfish) or c) the normal phenotype + extended phenotype or d) populations
m) bees and beehives (inter-demic groups)	genes	beehive as a whole
n) kin	genes	kin group
o) extended phenotypes	genes	co-evolved phenotype and extended-phenotype
p) behavioural patterns - genetic - non-genetic	- genes - see §2.2	- phenotype - see §2.2
q) populations (intra-demic groups)	not replicators according to our own definition	n/a
r) species or higher taxa	idem	n/a

*Table 1.2 Unit of replication versus locus of selection, inspired by Brandon 1988*

Conclusion: the gene perspective is a useful tool (as in kin-selection) and sometimes the only correct perspective to describe things (as in the case of selfish DNA); it emphasizes the importance of copying-fidelity and causal barriers preventing back-arrows. The challenges against it can be met, especially if one distinguishes between a replicator and the locus of selection and considers the fact that in actual problem-cases issues around the locus of selection are more important than those around replicators. This is also apparent from the fact that not all forms of evolution in biology can be described from the gene's perspective as the evolution of epigenetic settings, but this type of evolution could be described by distinguishing between a replicator and a locus of selection. The distinction between a replicator and a locus of selection will be important in analyzing levels of selection outside of biology.

### **Sharing a replication channel**

We conclude our discussion of the concept of replication with a general rule in Darwinism: *two replicators that share the same replication channel become slowly more and more adapted towards each other*. This rule can be illustrated by a non-biological example. Currently almost all computers run on Microsoft Windows and almost every new computer bought has Windows pre-installed. Personal computers and the software program Windows thus go through the same sale channel. And what do we see? Computers and Windows become more and more adapted towards each other as computers come with Plug-and-play devices that Windows (but not other operating systems) recognizes immediately and with keyboards with the special Windows keys that did not exist some 10 years ago.

In the previous §§ we have already seen numerous examples of this rule: for instance, mitochondria and prokaryotic cells became so well adapted that nowadays the mitochondria are encapsulated in eukaryotic cells in such a way that neither can survive without the other. An equivalent expression for the rule of the same replication channel is the idea of 'common faith'. One can wonder why almost all body cells of an organism specialize and let themselves be subsumed as part of the organism instead of replicating for themselves like cancer cells do. The answer is: because of common faith. The specialized body cells better fulfill their task and by doing so allow cells containing (highly) similar DNA as theirs to become gametes, because in this way they maximize their future replication: when they turn into cancerous growth (as they sometimes do due to mutation) they ultimately destroy their own replication chances by destroying the body as a whole. In other words: the specialized body cells and the gametes have a common faith and because of that the former can specialize so that the replication chances of DNA similar to theirs are as high as possible.

## §1.22 Variation

Evolution is a change from a no-howish untalkaboutable all-alikeness by continuous sticktogetherations and somethingelsefications.

-William James 1880 (as quoted by Dennett 1995: 147)

*Ever since biometricians started measuring all sorts of properties of organisms in the lab we have known that a lot of variation exists on all sorts of levels in organisms. This variation was also found to be present within natural populations of organisms. We define variation in biological evolution preliminarily as a random (blind) difference of a certain biological property between two or more individuals of the same kind. This variation is due to environmental or genetic reasons. We describe some variation-mechanisms and emphasize that an understanding of these mechanisms is required to make out whether we are dealing with variation in the Darwinian sense of random variation. This variation is the chaotic element that brings novelty in the world of the replicators. We discuss the concept of emergence (defined as the unforeseeable arisal in evolutionary history of novel design features) to explain this novelty and criticize philosophers who have found the concept of emergence wanting. Finally, we emphasize that the product of nature is not the Jamesian no-howish untalkaboutable all-alikeness: nature comes in 'lumps' and 'clumps': clusters of things more alike than others. We shortly discuss the species problem in order to find a way to talk about these clusters. We will see that we cannot give one unique criterion to talk about a species, but that they nevertheless exist. This discussion is important because we need principles or criteria to characterize the 'lumps' and 'clumps' of objects subject to variation outside of biology. After the discussion we formally define variation as: variation of a trait in biological evolution is continuous or discrete yet small difference of a certain biological property between two or more individuals of the same kind that is mechanically caused by a difference in the replicator or the environment, whereby usually only the former is heritable (and thus the true object of adaptive selection) and whereby a kind is determined by a pragmatic combination of cladism and some typological parameters.*

### **Variation before Darwin**

Darwin's theory requires a lot of variation. People before Darwin knew that animals differ in many of their characteristics and some even interpreted this variation as due to the adaptation of animals to local environmental conditions. Apart from that, ideas existed about a *continuous chain* of beings on which the different species could be located in a hierarchical way. However, each species was seen as a fixed point on this chain and the different inter-species variations were regarded as aberrations of an ideal species. The pre-Darwinian theory of heredity buttressed this idea as it saw heredity as blending so that deviations from the average ideal were lessened by the mixing of inner-species extremes.

What Darwin needed for his theory was variation that transcends the traditional species boundaries. Insofar as a defining element of what species were used to be formed by these boundaries, the species concept had to be transformed. We will first focus on variation, then on the so-called species problem. We will use this discussion to come up with a definition of variation that can also be used outside of biology. A lot of emphasis will be put upon the variation-mechanism.

## Variation present in nature

As already mentioned, Darwinism requires a lot of variation, and indeed a lot of variation on almost all levels was found, even within the same species. On the level of DNA (many neutral mutations of base-pairs coding for the same protein, but also non-neutral mutations), on the level of genes and whole chromosomes (even in number of chromosomes), on the level of cells and phenotypic appearance, on the level of behaviour, etc. Biologists know that many traits are plastic and variable, a fact used by farmers for thousands of years – consciously or unconsciously – in crop selection and artificial breeding to profit from better crops and animals.

We define variation in biological evolution preliminary as a random (blind)<sup>106</sup> difference of a certain biological property between two or more individuals of the same kind. This variation can be a) continuous or discrete and b) due to genetics or environmental interactions, which thus gives in principle a 2-by-2 grid of possibilities.

**a) continuous or discrete variation:**<sup>107</sup> at the beginning of the previous century biologists were fighting over whether the variation required by evolution was discrete (jump-like) or continuous. In one camp were the geneticists who had just rediscovered Mendel's laws and who claimed on the basis of the discrete traits of classical Mendelian genetics that the variation required for evolution was also discrete, proceeding by macro-mutations. In the other camp were the so-called biometricians who measured all sorts of characteristics of humans and other animals in the lab and found that variation was continuous. This dispute was resolved as it was shown i) how *multiple* Mendelian alleles coding for the same trait could result in semi-continuous variation (for example only three genes coding for growth could already result in  $2^3=8$  different outcomes, see Ridley 2004: 226f) and ii) that in wild populations (instead of the highly selected populations in the lab of the biometricians) a lot of variation was present too. In the modern synthesis of the thirties and forties macro-mutations were ruled out<sup>108</sup> as variation was seen as something semi-

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<sup>106</sup> We extensively discuss what 'blind' means below.

<sup>107</sup> In the literature the discussion about variation's being continuous or discrete (or better: about the speed of evolution being always the same, or being faster at one time, slower at another, etc.) has been hijacked by discussion of the theory of punctuated equilibrium, which holds "that the great majority of species, as evidenced by their anatomical and geographical histories in the fossil record, originate in geological moments (punctuations) [such as the Cambrian explosion which gave rise to arthropods - MH] and then persist in stasis throughout their long durations." See the 700+ page second half of Gould (2003) which is entirely devoted to it.

The idea of punctuated equilibrium seemed like swearing in the church of Darwinian orthodoxy, since Darwin had always insisted on slow continuous accumulation of changes over vast amounts of time because he wanted to model the evolution of life after the *Principles of geology* by the father of modern geology Lyell, who had fought 'mystical' catastrophism in geology all his life by postulating the (in his eyes) more scientifically and methodologically correct uniformitarianism: the idea that the same causes have been operating in the past as now so that only small accumulations of causal effects are to be invoked to explain the formation of such huge structures as mountains, cliffs, and ocean beds. Equivalently, Darwin held that only normal causes still observable today (not radical breaks such as Noachian floods) could account for the evolution of life-forms. Here, however, we will bracket a discussion about whether in evolution punctuated equilibria occur or not. From our perspective in this section the variation punctuated equilibrium theorists postulate is just as continuous as the variation Darwin assumed to be present: both are variation on the huge geological time scale, the former still gradual but only slightly less gradual and less constant than the orthodox Darwinian view holds.

<sup>108</sup> Macro-mutations do seem to offer an explanation of how certain phenomena arose, and we see time and time again that biologists and other scientists (especially the latter) introduce macro-mutations to account for certain phenomena. However, upon a little reflection one can easily see that macro-mutations are not explanatory at all. If one wants to explain how a Boeing 747 was formed one could say that the



continuous: on the smallest scale variation is discrete, but on a larger scale variation is (as good as) continuous.

This picture used to be a tempting one, especially at the time the structure of DNA was unravelled in 1953: on the smallest scale of DNA the variation is discrete, but on the phenotypical level due to gene effects averaging out and especially due to non-discrete environmental interaction the variation is continuous.

In this picture of evolution, genetic variation is thus discrete<sup>109</sup>, environmental interaction non-discrete (the amount of sun-shine, food, temperature etc.). However, more recent developments have destroyed this simple picture: *much genetic variation may be much less discrete than some versions of variation due to environmental interaction*. We will mention three extreme examples: i) neoteny, ii) endosymbiosis, and iii) viruses and viroids.

i) neoteny is the retention, by adults in a species, of traits previously seen only in juveniles. Well-known examples are the sparse body hair and enlarged heads of adult humans reminiscent of baby chimpanzee primates (Desmond Morris 1967, 2004; Gould 1980, 2002), but neoteny is much more widely spread in nature. It is common amongst domesticated species such as dogs, cats, pigs, cows<sup>110</sup>, etc. and its occurrence is seen as a byproduct of man's artificial selection for docile or friendly juvenile characteristics, but it can also occur due to natural selection. It is suggested that the phylum of the chordates (including all vertebrates) arose as their ancestors, the tunicates, became sexually mature before metamorphosis. This could be due to mutation (and at least one genetic mutation is needed for sexual maturation of otherwise neotenus, i.e., non-sexually mature, creatures to beget offspring); however, since the onset of metamorphosis depends upon environmental stimuli, it could also be the case that permanently changed environmental circumstances have made metamorphosis impossible, leading to a different neotenus phenotype.

ii) endosymbiosis is the process in which one organism lives inside the body or cells of another; we already saw the examples of the mitochondria and gut bacteria. The origin of endosymbiosis is the environmental process in which one organism ends up in the body or cells of another; this *environmental* process can lead to *radical* retransformations (the whole realm of the eukaryotes arose due to this process), although this environmental process need not be sudden and it can be accompanied by genetic mutations.<sup>111</sup>

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materials were arranged by chance into the right combination (i.e., by some macro-mutation of the arrangement of the constituent parts). This explanation is not satisfactory at all as it explains nothing: it takes the end-product and says that by chance the parts were suddenly rightly arranged. This explanation of the design of something thus works a-priori and always, irrespective of the phenomenon to be accounted for. As such its explanatory value is empty and void. Daniel Dennett (1995) baptizes macromutations as hopeful monsters and emphasizes that they indeed are monstrous non-explanations. A Darwinian explanation requires one to postulate many small variational steps leading from state A to state B. Empirical discovery of fossils of intermediate states or empirical models of proximate mechanisms can test whether the postulated variational steps were the right ones.

<sup>109</sup> We distinguished two notions of genes: genes as pieces of DNA and genes as genes-for-a-certain-trait. In both senses the variation be discrete: it is clear that variation in base pairs is normally discrete (from Adenine to Cytosine, or Thymine to Guanine etc.), whereas a non-neutral mutation in a gene can lead to a very different protein being coded for which in turn can lead to major (discrete) phenotypical changes.

<sup>110</sup> See Trut 1999 on a Russian study of pedomorphosis in a 40-year breeding program to domesticate red foxes.

<sup>111</sup> I.e., it needs not be the case that one organism enters the body and cells of another and that forever after they replicate together (a macro-shift); it can be the case that many organisms of a certain species for some time during their life-time happen to live inside that of the bodies or cells of organisms of another species until they become so co-adapted that they live inseparable henceforth. In the process of co-

iii) the origin of viruses and viroids: in the previous section we described how viruses (see **c**) and viroids (see **b**) replicated using other cells. There are two competing types of theories about the origin of their replication processes: some see viruses as the descendants of the chemical replicators that existed before DNA-based geno-phenotype replication (see **a**)), others as parasites that arose as freely-floating pieces of DNA or RNA (which could arise through copying errors and breakings of DNA-chains) that became copied autonomously by the cell machinery of eukaryotic cells. In the former case the evolution of viruses was *slow* (gradually they adapted themselves to use the cell-machinery of other cells, whereas they originally used each other to catalyze their replication; if the latter theory is true, however, the origin of viruses was due to a large (variational) step. In sum: although macro-mutations are ruled out in favour of semi-continuous variation, some major shifts can occur, both due to genetical 'variation' (including breaking/splitting) and environmental interaction. These major shifts put pressure on our definition of variation as a *small* difference of a certain biological property between two or more individuals of the *same kind*: can we still speak of the *same kind* when major shifts happen? The discrete incorporation of endosymbiotic bacteria inside unicellular organisms did not just give rise to a new species, it gave rise to a whole new order at the top of the taxonomical hierarchy (the eukaryotes versus the earlier prokaryotes). Nevertheless, we can speak of the *same* bacteria with and without cell organelles and there have probably existed in-between cases before the co-evolution became a common faith: it is likely that initially the cell organelles (being still independent cells) spent only part of their life time in their host until they became fully integrated.

**b) variation due to genetics or environmental interactions:** under a) we already encountered examples of difference between variation due to genetics and due to environmental variables. This distinction is important because the former variation is supposed to be *heritable*, but the latter not (but see our discussion of neoteny above). Only *heritable* variation can be the object of adaptive selection.

This distinction between heritable and non-heritable variation is much more fundamental than that between genetic and environmental interactions, as this distinction can almost only be used for entities in biology when there is a clear distinction between genotype and phenotype. It can already not be used for **a**) chemical replicators, **b**) virusoids, **c**) viruses, **d**) selfish DNA, and **h**) epigenetic settings. Outside of biology the distinction between genetic and environmental factors is of even less use.

If we nevertheless focus upon genetic and environmental factors we know that they can in principle be distinguished in artificial selection experiments in which environmental variables are held constant or varied for two different or equal populations. In practice, however, experiments are hard i) as variation can be due to many different environmental variables that all need to be taken into account and ii) because often the exact genetics responsible for the phenotype are unknown (although less and less in this age of genomics). Traditional quantitative genetics has developed techniques to disentangle the different components of the supposed

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adaptation the species may undergo genetic mutations and the earlier cohabitation of the two species might be due to a mutation in the host (that allows the guest to enter) or the guest (that knows how to hijack the host).

genetic variation on the *phenotype*.<sup>112</sup> As selection often tends to stabilize optimal characters, selection reduces the genetic variability of a character in a population, so that the observed variation can be used as a measure of selection. In a stable situation new variants keep popping up due to mutations so that in a natural stable population a mutation/selection balance may account for the observed variation in natural populations, although the actual observed variation is still imperfectly understood. Modern quantitative genetics with its concentration on genetic sequences further complicates the traditional picture as it has found many neutral mutations and different rates of variation in different species over geological time, possibly due to selection or different repair mechanisms.

Modern quantitative genetics further needed ways to distinguish the number of mutations to get from one sequence to another (the sequence ATCG could have resulted from CTAG by at least three ways: by *one* inversion of the first three bases, by *two* substitutions or by *one deletion* of the front AT accompanied by an *one insertion* of TA between C and G).<sup>113</sup> Modern genetics thus made it even more important to understand the variation mechanism, to which we turn now.

### **The variation mechanism**

Only since the rise of modern genetics do we know about the details of the variation mechanism. Darwin himself did not know about it, nor did he need it to formulate his theory: for his theory i) the observation that organisms varied in numerous heritable ways (although his pangenesis theory of heredity was of course wanting) and ii) the postulate that the variation was '*blind*' or '*random*' (and not directional as in orthogenesis) sufficed. We first concentrate on the nature of this blindness or randomness and then discuss some variation mechanisms responsible for it.

Variation is sometimes regarded as part of the selection process and included in the definition of selection (Darden and Cain 1989). However, we think that for the purpose of clarity it is better to give a stand-alone definition of variation, all the while admitting that Darwinian variation is not a stand-alone process but part of the evolution process. We thus take variation as a precondition for selection to act (see also Hull 1980).

We agree further with Hull et al. 2001 that "the characterization of the variation that functions in selection processes has been one of the most contentious topics in literature. (...) It seems that no adjective exists in the English language that accurately reflects the sort of variation that occurs in selection processes. Is this variation blind, chance, random, non-prescient, nondirected, nonteleological, unforeseen, or what? (2001: 513)."

The terms 'blind', 'non-prescient', and 'non-teleological' are not completely satisfying as they seem to be used metaphorically: nobody believes that nature can actually see or know something. 'Random' and 'chance'<sup>114</sup> are unfortunate because both words are well-defined concepts from mathematics, but the requirements mathematics puts on these concepts is so strict that no biological variational phenomenon can be characterised by them. An example: DNA consists of four base pairs but the chemical bonds of these base pairs with the sugar chain of the double

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<sup>112</sup> See Ridley 2005 chapter 9 on quantitative genetics. The standard introduction is Falconer and Mackay 1996.

<sup>113</sup> These three versions correspond to so-called genetic variation mechanisms of point mutations, string inversions, whole insertions, and deletions.

<sup>114</sup> Of course there are other uses of the concept of 'chance' that are non-mathematical. We come back to them as we discuss the concept of emergence below.

helix is not equally strong. That is why some mutations occur more often than others: i.e., replacements of cytosine by thymine occur more often than guanine by thymine etc. (Hartwell et al. 2006). Thus physical (chemical) constraints can be responsible for deviations from random mutations on the level of the genes.

Other examples of departures from true randomness demonstrate why ‘non-directed’ and ‘non-teleological’ are infelicitous as well: i) on certain chromosomes hotspots exist that exhibit extremely high rates of mutation (often near the centromere); some genes coding for the immune system lie in this region, mutating thousands of times faster than other parts where genes for traits avoiding mutation lie (Pennisi 1998); ii) sex probably arose to create more mutations to fight fast-evolving parasites, so sex is a *functional* mechanism explicitly in place to cause mutations, or more specifically, to *direct* the number of mutations; iii) we saw how **h**) epigenetic settings mutate and can be used by an organism (or its genes) to adapt quickly to changing environments for a few generations without changing the underlying genes; iv) we saw how some **c**) viruses use RNA to store their hereditary code instead of DNA, and this has a good reason: 1 out of every  $10^4$  RNA bases mutates, whereas 1 out of every  $10^9$  DNA bases mutates during replication; as such they can evolve more quickly; and v) we just mentioned that some base-pair mutations occur more often than others. Three nucleotide bases code together for 1 amino acid (amino acids are the building blocks of the proteins). We know that the  $4^3=64$  different combinations of three base pairs called codons give rise to only 20 amino acids, some amino acids are thus over-coded. For example: as many as 6 different base pair combinations code for the amino acid *Leucine*, whereas only 1 codes for the amino acid *Tryptophan* (Hartwell et al. 2006). It is not known whether a functional explanation can be given for the non-equal degree of over-coding, thus compensating for different mutation rates, but it is theoretically possible.<sup>115</sup>

Hence, variation can sometimes be non-random and functional (as-if-teleological or directed) and this seems to leave us with no predicate to uniquely qualify variation in Darwinism.<sup>116</sup> However, we think that these predicates can still be useful if we disentangle proximate mechanisms from ultimate mechanisms. Ultimately a certain variation mechanism may exist for some function, to quickly adopt to parasites for instance. As such variation may seem as-if teleological, directed, or non-random in the sense of non-arbitrary. Proximately, the variation itself is *mechanically* caused and random in the sense that the variation mechanism does not determine what the individual *token* mutations look like (the *type* of mutations may be highly constrained though, although never absolutely; think of endosymbiosis, whole insertions into the genome, etc.). The fact that some token mutations of a certain type occur more often than other token mutations of the same or another type can be explained mechanically too by some physical probability model. The sense in which something can be mechanically caused and at the same time random is discussed below as we discuss emergence.

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<sup>115</sup> See Hartwell et al. 2006. See also Ridley 2005: 89 “Although mutation is random and undirected with respect to the direction of improved adaptation, that does not exclude the possibility that mutations are non-random at the molecular level. For example, the two-nucleotide sequence CG tends to mutate, when it has been methylated, to TG. (The DNA in a cell is sometimes methylated, for reasons that do not matter here.) After replication a complementary pair CG on the one strand and GC on the other will then have produced TG and AC. Species with high amounts of DNA methylation have (perhaps for this reason) low amounts of CG in their DNA.”

<sup>116</sup> See also Benson 1997, Lenski and Mittler 1993 and Peck and Eyre-Walker 1998.

Hull et al. admit that the minimal predicate applicable to variation is “caused – *totally* caused. No one writing in this literature feels inclined to introduce miracles in their description of variation (id.)” Based upon the above discussion and §1.1 we would like to be more specific and say that Darwinism requires *mechanically caused variation*, i.e., *variation that can be analyzed by proximate physical and chemical mechanisms (even though variation may be induced for a function)*. We therefore place a lot of emphasis on the actual variation mechanism. The above characterization clearly excludes genuine *mental, intentional causation* by an agent if that exists at all (and if so, if it cannot be completely reduced to physical mechanisms).<sup>117</sup> Excluding this type of causality was the real meaning of using predicates like ‘blind’ to characterize variation.

### **The species problem**

In our preliminary definition of variation the word ‘kind’ occurs, which forces us to consider the so-called species problem that revolves around the question of what a species is. It is sometimes said that Darwin’s theory made this problem obsolete since his theory destroyed the idea of the existence of (atemporal) species. There is no species problem, it is claimed, because species do not exist: there are only individuals that can or cannot replicate. The title of Darwin’s famous book would then be a clear misnomer: the origin of species would be an origin of nothing. Nevertheless, our world is not like the continuous flux of the Jamesian “no-howish untalkaboutable all-alikeness”: the world comes in relatively discrete “humps, lumps and clumps” (Millikan 2006), at least from a synchronic perspective. It is important to have a way to talk about these discrete humps, lumps and clumps. We will see that there is one unique way to characterize all living organisms in the tree of life, and that we thus have to use different criteria for different branches of this tree to name the relatively discrete lumps and clumps in which nature presents itself. We will be realists regarding the existence of these lumps and clumps: the lumps and clumps usually do not exist merely because of our perspective or our name-giving, but because they exist out there in the world.<sup>118</sup>

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<sup>117</sup> One may object that we implicitly defined mechanical causation as the opposite of genuine mental, intentional causation and that therefore this definition of variation is no better than the definition that claimed variation was blind or non-prescient, but we disagree. Mechanical causation is not (directly) defined by reference to its opposite (the mental) but as the general name for the causality involved in processes described by the laws of physics.

<sup>118</sup> Of course we might use certain typological features that catch our human eyes to identify these lumps and clumps, but that turns out to be useless as we find new organisms and intermediary cases (as traditional taxonomy was upset by the discovery of Australia’s fauna with its remarkable kangaroos and platypuses), but then we start to use different criteria and update our view on the lumps and clumps that exist *out there*, not in our minds.

In this section we will first discuss the state of current taxonomy<sup>119</sup> and then opt for a pluralistic species concept which relies on cladism and other typological criteria relevant for the domain of life discussed: we further emphasize the importance of an exemplar. The concept we propose is not new, although the syncretic and pragmatic combination of typological criteria is something not often found in the literature. An advantage of our idea of species (as a combination of cladistic descent and typological criteria) is that it is readily transportable outside of biology where lines of descent and typological criteria will be our guide in naming the humps and lumps of the non-biological world.

Traditional taxonomy confined itself to the naming of sexually reproducing animals (and plants), either because other organisms were unknown or because of the anthropomorphic interest in creatures relatively similar to us in the tree of life. In this domain there was often agreement whether something counted as a species: the species Aristotle distinguished in his *Historia animalium* are *grosso modo* still considered as species and the classification system of the pre-Darwinian, Neoaristotelian biologist Linnaeus is still used with minor modifications today, despite the fact that we now know that Aristotle and Linnaeus were wrong in thinking that species were immutable.

The study of viruses and bacteria, as well as the empirical discoveries in the areas of botany and paleontology, made distinguishing species harder, although in practice biologists still have little problem in speaking of species. Nevertheless, philosophers and theoretical biologists have written thousands of papers on the species problem. A recent systematic paper distinguished as many as 22 different species concepts that are said to be needed to organize the realm of nature (Mayden 1997 in the book length anthology on species by Cladrigde et al. 1997), and the number of species concepts can easily be multiplied if we look at once-popular but now-discarded concepts of species and the competitors to Mayden's ideas. It might then seem that the species problem is just a philosophical problem in the pejorative

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<sup>119</sup> Our goal in discussing the species problem is to find a way to cut biological nature into discrete, recognizable units, because we can then use this cutting method outside of biology. Thus our interest in the species problem is that of taxonomy. A lot of the literature on the species problem is, however, interested in explaining *why* there are species: why there are these discrete humps and clumps, why there is this unique category in taxonomy (under genus, family, realm etc. and above races, subpopulations etc.) that is somehow supposed to be "more real" than all the others (Mayr 2004). Only secondarily do they touch upon the problem of taxonomical analysis in *identifying* species. Because of our interest in taxonomy, our discussion of the species concepts in the literature may be biased beforehand. However, this bias is not too damaging as even those interested in why there are species agree that their species concepts should be at the basis of an ideal taxonomy. Mayden (1999) distinguishes between conceptual and operational species concepts; conceptual species concepts explain why a species is a species, operational concepts how we can distinguish species (in taxonomy). Mayden calls the former ontological and the second epistemological and according to him most species concepts are epistemological instead of ontological, but real species concepts should be ontological concepts. Unfortunately, Mayden does not argue why this should be: he only argues why epistemological concepts are wanting (for instance because they focus on arbitrary traits like Linnaean taxonomy), but not how one should determine an ontological species concept and why it would not suffer from arbitrariness. We do believe that organisms come in clusters – to that extent this implies an ontological realist regarding species – but we do not think that these vague clusters have any existence apart from the individual organisms. Species concepts should be used to name these clusters; biology should give us reasons why organisms come in clusters (by analyzing speciation), but the *concept* of clusters need not include the reasons *why* there are such clusters (which are many because speciation comes in many forms (Ridley 2005: 382-419 discussed three independent dimensions by which speciation can be analysed a) the type of environmental process: allopatric, parapatric, sympatric, b) the nature of the reproductive isolation: by pre- and post-zygotic isolation, and c) the selection forces operative: natural or sexual selection or a combination of both etc.; these three dimensions alone already give 18 possibilities, which would give rise to 18 different species concepts).

sense of empty word-play, as the number of species concepts multiplies exponentially without any fruitful empirical progress; that is why many biologists have shown little interest in the species problem, although others do claim that species concepts are important empirically.<sup>120</sup> Whether there are empirical consequences of the species problem or not is not primarily important for us here; we are interested in the reasons why biologists can so easily *identify* species with remarkable agreement so that the concepts behind such identifications might be used to characterize the world of non-biological entities evolving by Darwinian selection in which much less agreement is to be expected.

Our account will differ from that of many others insofar as we do not believe in one or a few unique criteria, principles, or guidelines that can be used to identify all species as many philosophers or much more recent thinkers have wanted.<sup>121</sup> We will be content when we can identify a few pragmatic criteria that hold for some branches of the tree of life. We will, however, take care to look at the species problem in many different branches so that our approach to the species problem will be as general as possible. This general result can then be used for the (pseudo-)species problem outside of biology.

An important first distinction is that between *synchronic* and *diachronic* species concepts. The former deals with species frozen at a certain moment in time, the latter with species as they evolve over time. This distinction was meaningless at the time when species were still considered as atemporal, immutable forms as in the Aristotelian-Linnean tradition which gave rise to the typological species concept. The first fundamental revision of the species concept Darwinism inaugurated was therefore the possibility of this distinction: that species are *temporal* units.

The most famous synchronic species concept today is the authoritatively known *Biological Species Concept* or BSC as developed and defended by Ernst Mayr, the spokesman for taxonomy among the architects of synthesis. It is defined as 'groups of interbreeding natural populations that are reproductively isolated from other such groups', where interbreeding indicates a 'propensity'.<sup>122</sup> The advantage of this definition is that it can easily deal with male and female organisms of a certain species, although they can often be very different: think of male and female peacocks. A classification system that distinguishes different species by listing properties (such as the typological species concept) might put males and females of the same species in different species categories;<sup>123</sup> the BSC averts this. Another advantage is that the BSC

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<sup>120</sup> These empirical problems usually arise when one expects the species concept to explain *why* there are species instead of taxonomical indicators (see previous footnote) and when one looks at highly specific species concepts. Examples of empirical problems are the discussion between defenders of the BSC and the ecological species concepts whether the absence of reproductive gene flow or allopatric selection is the primary cause of speciation and thus of the existence of species.

<sup>121</sup> Even Mayden (1999, 1997) who seems a pluralist because he acknowledges 22 different species concepts, with different concepts for different branches of the phylogenetic tree, is ultimately a monist insofar as he places the Evolutionary Species Concept as the all-embracing concept on top. An early paper by Mishler and Donoghue (1982) *Species Concepts: a case for pluralism* comes close to our true pluralistic position.

<sup>122</sup> See, Mayr 2000, p. 17 and Mayr 1970, 1942. See for the interpretation of this propensity §1.23 on fitness as a propensity to beget a certain number of offspring.

<sup>123</sup> One might say that the typological species concept could easily avoid this trap by always distinguishing the features of a male and a female of sexual species X with its two unique members. However, in quite a few species more than one morphological distinct version of a gender exists (in some fish species there are for instance three morphologically distinct males: one big fighter, another the smaller helper of the fighter who attracts females and mates after the big fighter, and finally the small inconspicuous male who lives alone and drops his semen on the remaining unfertilised eggs when nobody looks).

can automatically distinguish sibling species: “pairs of species that differ reproductively but not [or better: almost not] morphologically (Ridley 2005: 368).”<sup>124</sup> Another advantage, at least in the eyes of its formulator Ernst Mayr is, that it provides a *mechanism* for the genesis of species called (cladogenetic) speciation: when groups no longer interbreed and there is no longer a *gene flow* between them, they become different species.<sup>125</sup> In the absence of speciation the BSC also explains why the members of a species resemble one another, because the interbreeding guarantees that the members have the same genes. The BSC is the most “widely accepted species concept today, at least among zoologists (Ridley 2005: 351).”

The problems with the BSC are, however, many. We mention three. First of all, it does not give clear-cut distinctions. Dennett puts it like this:

“The criterion of reproductive isolation is vague at the edges. Do organisms belong to different species when they *can't* interbreed, or when they just *don't* interbreed? Wolves and coyotes and dogs are considered to be different species, and yet interbreeding does occur, and – unlike mules, the offspring of horse and donkey – their offspring are not in general sterile. Dachshunds and Irish wolfhounds are deemed to be of the same species, but unless their owners provide some distinctly unnatural arrangement, they are about as reproductively isolated as bats are from dolphins (1995: 45).”<sup>126</sup>

In paleontology where we usually cannot determine which creatures have interbred at all the problems with the BSC are even bigger than Dennett sketches.

A still bigger blow to the BSC comes from the discovery of so-called ring species (species being an unfortunate word in this case!), such as the herring gull whose territory is formed by a broad ring around the north pole.<sup>127</sup>

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<sup>124</sup> The classic example is the species pair *Drosophila persimilis* and *Drosophila pseudoobscura*, which do not interbreed but are almost indistinguishable.

<sup>125</sup> This idea: that gene flow is responsible for keeping boundaries in sexually reproducing species intact has been criticized a lot, especially by botanists (note that plants reproduce sexually as well of course). Already in 1969 Ehrlich and Raven pointed out that the extent of gene flow seems to be very limited in many organisms and may not account for the apparent integrity of the morphological units we observe in nature. Bradshaw (1972: 42) joked that “effective population size in plants is to be measured in meters and not in kilometers.” Levin 1979: 383 stated: “The idea that plant species are Mendelian populations wedded by the bonds of mating is most difficult to justify given our knowledge about gene flow. Indeed a contrary viewpoint is supported. Populations separated by several kilometers may rarely, if ever, exchange genes.” Lande 1980: 467 has stressed that “of the major forces conserving phenotypic uniformity in time and space stabilizing selection [instead of gene transfer] is by far the most powerful.” Finally, Grant (1980: 167) suggested that “the homogeneity of species is due more to descent from a common ancestor than to gene exchange across significant parts of the species area.” See also the review by Mishler and Donoghue (1982). Mishler and Donoghue cite studies that suggest the existence of disruptive selection even in the face of gene flow, thus “morphological divergence and the attainment of means of reproductive isolation can be uncoupled events in space and time.” Ridley (2005: 369–72) gives a more recent update on the still inconclusive research on the relation of morphological divergence and the attainment of means of reproductive isolation. Note that the concept of reproductive isolation is almost as vague as the species concept itself. Dobzhansky 1970 lists as many as 6 types of prezygotic and as many as 3 types of postzygotic reproductive isolation, such as (pre-)ecological isolation, seasonal or temporal isolation, sexual low attraction, mechanical isolation, pollinator isolation, and gametic isolation, and then (post-)hybrid inviability, hybrid sterility in the first generation, and hybrid breakdown in the second generation.

<sup>126</sup> Paterson seems to realize some of these problems for the BSC as he defines species more narrowly on the basis of a shared specific mate recognition system, a sensory system by which organisms recognize potential mates. However, there are degrees in recognition and one of the mates may recognize the other who does not recognize the other in mating, etc., so that the vagueness is back. See Paterson 1993.

<sup>127</sup> Recently Liebers et al. 2004 have shown by genetic analysis that the case of the herring gull is slightly more complicated. Better examples of ring species might be the *Ensatina salamanders*, which form a ring round the Central Valley in California, and the Greenish Warbler which forms a ring around the Himalayas.



“As we look at the herring gull, moving westward from Great Britain to North America, we see gulls that are recognizably herring gulls, although they are a little different from the British form. We can follow them, as their appearance gradually changes, as far as Siberia. At about this point in the continuum, the gull looks more like the form that in Great Britain is called the lesser black-backed gull. From Siberia, across Russia, to northern Europe, the gull gradually changes to look more and more like the British lesser black-backed gull. Finally, in Europe, the ring is complete; the two geographically extreme forms meet, to form two perfectly good species: the herring and the lesser black-backed gull can be both distinguished by their appearance and do not naturally interbreed (Ridley 1985: 5).”

However, many ordinary concepts are vague but their vagueness does not restrain us from using them: often one can take just the endpoints as exemplars and then the problem is gone.

A third and still bigger problem is that the definition works only for sexually reproducing creatures, whereas many organisms are asexual. This need not be a problem if one, as quite a few philosophers have done, simply denies that asexual organisms come in species!<sup>128</sup> That seems like ivory tower philosophy that takes its own definitions more serious than reality. It is clear that there are many different groups (“humps and lumps”) of asexual organisms with distinct properties, groups that should definitely be called species and that are indeed distinguished as such by taxonomy. However we do agree that the actual formulation of a taxonomy of asexual organisms such as non-flowering plants and certain bacteria is much harder than one for sexually reproducing creatures: plants do not have fixed or average sizes, cannot as easily be seen as individuals (is one mushroom or a fairy ring the individual?), etc. Moreover, since completely unrelated bacteria and even plants exchange pieces of DNA they do not seem to come in entities that are completely reproductively isolated. Nevertheless, we would argue that to speak of “completely unrelated bacteria exchanging DNA” already involves distinguishing species, be it in a different way than in the case of truly reproductively isolated sexually reproducing creatures. The upshot is that although the Biological Species Concept can be of use as a first attempt to categorize sexually reproducing creatures (and only those) it fails for the above reasons. Most other species concepts are diachronic concepts, to which we turn now.<sup>129</sup>

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<sup>128</sup> For instance Ghiselin 1987 and references therein.

<sup>129</sup> Another synchronous species concept is the ecological species concept which just as the BSC seeks to explain *why* there are species. According to the ecological species concept species are species because the ecological factors such as parasites present, the food niches available etc. naturally tend to produce clusters within species that after some time turn into new species. In practice the BSC and the ecological species concept often recognize the same species in nature. Another once famous species concept on the boundary of synchronic and diachronic concepts is the view first advocated by Michael Ghiselin as “a radical solution to the species problem” (Ghiselin 1974) and later worked out by David Hull (Hull 1976), which holds that species are individuals with parts rather than classes with members. Ghiselin and Hull thought of classes as the a-temporal abstracta of mathematics, and of individuals as full-blooded historical entities. They hold that species are better described as individuals because in strong contrast with (mathematical) classes species come into existence and go extinct, species are spatially localizable and ‘species can be the units of selection’, just like ‘normal’ individuals. An important counter-argument to this loose analogy between ‘normal’ individuals and species is, however, that species are not the integrated, cohesive wholes one thinks of when normally speaking of individuals (see also Van Valen 1982 and Holsinger 1984 proposing the ecological species concepts). Of course groups of the ‘same’ organisms or even taxa of organisms are historical entities – just as individual organisms – only on a longer time-scale, and it is likely that there are selective pressures on whole groups and not just on individuals as we discussed in §1.21 **p** on populations. Yet, being an historical entity and being subject to selection does not turn species into individuals, unless in a very non-common and empty sense of the ambiguous word ‘individual’ as every group can be called an ‘individual’ from a higher ‘perspective’. Despite this obvious criticism prominent species theoreticians such as Mayden 1999, 1997 and Wheeler and Meier 2000 still think of species as individuals.

Most diachronic species concepts are based on cladism. Cladism uses the branches or clades of the evolutionary tree for distinguishing species. Cladism holds that at each node of the tree one species becomes two. Cladism thus uses evolutionary descent.<sup>130</sup> A problem for cladism is that it cannot easily distinguish higher groups, such as families and orders. “We are very closely allied to chimpanzees, for example, sharing with them 97 percent of our genetic endowment. Yet our life style is so remote from theirs that we seem to constitute not only a different species among the *Hominidae*, but even a separate family (Green/Depew 2004: 304) [Note that current taxonomy of course claims the opposite – MH].” Another problem for cladism is polyphyly, when a certain trait – for instance through cross-hybridization of plants – arises multiple times in evolutionary history; cladism would then suggest that each time a new species has arisen. Worse still is that the actual evolutionary tree is not a tree which branches in an unambiguous way, from a few big initial branches to many end-branchlets: the tree of life “grows often in upon itself” as the cases of endosymbiosis and bacterial sex showed<sup>131</sup>, so that the tree of life can better be described as the *bush of life* with some more or less clearly distinguishable branchlets at its remote corners where sexually reproducing animals are located. Thus, cladism alone does not seem to be enough to characterize species. Put in another way: in the bush of life evolution does not just take place because of vertical (cladistic, historical) separation of lineages, but also because of pseudo-horizontal (synchronic) diffusion and exchange between lineages.

The solution to the problems for cladism seems to us to fall back on the old typological or phyletic species concept, which characterizes species by their overt properties. The typological species lies at the basis of the system of Linnaeus.<sup>132</sup> Linnaeus’ contemporaries often complained about the arbitrariness of the traits he selected for his hierarchical taxonomy (Ereshefsky 2001). One could use different traits and either get the same taxonomy (in case the traits of the second group are each exactly co-extensive with some trait of the first group) or a different taxonomy.

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<sup>130</sup> Cladism originates in the work of Willi Hennig on “phylogenetic systematics” (Hennig 1966, 1950) and to a lesser extent in that of Gaylord Simpson (Simpson 1961). The supporters of cladism have split into multiple schools. Wheeler and Meier 2000 distinguish four different schools of cladism. The differences among these schools are not important for the goals of our discussion.

<sup>131</sup> As Lynn Margulis 1998: 52 puts it: “In reality the tree of life often grows in on itself. Species come together, fuse, and make new beings, who start again. Biologists call the coming together of branches – whether blood vessels, roots or fungal threads – anastomosis. (...) Anastomosis, although less frequent, is as important as branching. Symbiosis, like sex, brings previously evolved beings together into new partnerships.” An even better example of ingrowth is offered by retroviruses (see §1.21 c). These viruses inscribe themselves into the DNA of widely different organisms: the same virus could inscribe itself in the genome of cats and humans without being present in the genome of their most recent common ancestors, nor in the other lineages that have evolved from this common ancestor.

<sup>132</sup> In Linnaean taxonomy a researcher had to follow the steps outlined in the Linnaean tables and at the end he would know the species name of the specimen he studied. The steps in this table worked top down from kingdoms, to classes, orders and finally genera and species. For instance, when it was determined that a certain specimen was an animal (kingdom) that cared for his offspring (order: mammals), the researcher had to count the number of toes to determine whether the mammal belonged to the order of the perissodactyla (like horses, zebras, rhinoceroses etc.) or the artiodactyla (like pigs, goats, cows and camels etc.). Further steps singled out the genera of horse like animals from rhinoceros and finally the species of zebra from the species of horse etc. Linnaeus introduced the binomial nomenclature for species that assigns to each species its genus name and a specific descriptor into biology. Philosophers will immediately recognize that Linnaeus uses Aristotle’s definition of a definition according to which a definition should state the *genus proximam* and the *differentia specificam*. In the Aristotelian tradition one can even find precursors to Linnaean classification tables. It is no wonder people often speak of the Aristotelian–Linnaean system of classification.

The question is: do the chosen traits really cut ‘nature at its joints’? Does this procedure really single out the true natural species out there? This question seems only to make sense if one believes in the existence of unique essences as the pre-Darwinian Linnean biologists of course did.

In an otherwise excellent textbook on the philosophy of biology Elliot Sober characterizes the procedure of typological or phyletic taxonomy as assuming “that each natural kind can be defined in properties that are possessed by all and the only members of that kind (2000: 148).” But this caricature is against the spirit of the tradition of Aristotelian taxonomy. As an example, for Cuvier the essence is determined by the “total conditions of life”, the unique style of this kind of organism’s existence: its morphology, physiology, behavior, relationship to other organisms, and to its ecological niche. “No little list of properties would suffice to capture it (Grene and Depew 2004: 293).” Most Aristotelian taxonomists would probably agree with Cuvier; only for reasons of economy the smallest number of traits were chosen to distinguish the biggest possible slice of the realm of species, and it was obvious for them that species differed in many other, often much more important, aspects from each other.

We can inaugurate the typological species concept by avoiding the traditional objections to it: we do not take the type to be atemporal and we do not think a short or even finite list of properties can (uniquely) characterize all individuals of a type: some individuals of a species may lack quite a few of the properties but still count as members of this species, and neither do we think that the type exists independently from the individuals nor that every individual can always be unambiguously accommodated under one type.

A practical procedure based on the typological species concept would be to take a historical exemplar of X (or in cases of sexually reproducing creatures 2 exemplars of a pair) and claim that anything that shares most (for instance >95%) of the properties of this exemplar is also an X.<sup>133</sup> This used to be the practice in many branches of taxonomy like zoology and is still the practice in some branches of botany, where the first botanist who describes a species just sticks an exemplar of the plant on a piece of paper.

However, one cannot easily copy exemplars and that is why for practical communicative purposes one still needs a *list* of properties that are important (but neither necessary nor sufficient) to characterize a species. On these list can be

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<sup>133</sup> An of-voiced criticism of typological thinking on the basis of an exemplar is that it chooses one individual which is seen as somehow a “better example” than other individuals of that species (see for instance Mayr 1976). Exemplars are then easily seen as more real or more representative (think of all fantasies of ideal types in Platonic heavens or the mind of God); this is a bad thing because it downplays the huge natural variation present in a population. One could remedy this by taking multiple exemplars and averaging all their properties (which is sometimes hard when one exemplar lacks the property), but even this does not remedy the situation since the choice of the group of exemplars is again arbitrary (although in taxonomical practice one just chooses such as group). Equating a species with the synchronic population of all individuals that can interbreed, as Mayr seems to do, does not work either, because this discriminates present individuals against past and future ones. Some include the past and future ones also in their definition in an attempt to synthesize the BSC and cladism as the phylogenetic approach of Wiley 1981. However, since it is almost impossible to determine whether past organisms could interbreed with present ones (how can we determine what the last human-primate ancestor was with whom we could no longer interbreed), even Wiley has to rely on typological criteria in determining species. It seems to us that there is simply no way around using types in taxonomy to identify species.

properties like: i) cladistic descent<sup>134</sup>, ii) sexual interbreeding, and iii) genetic markers (specific DNA strings), but even these three combined will not be enough to uniquely characterize *all* species, and extra typological properties about for instance morphology, behaviour, and (developmental) physiology etc. can always be added pragmatically.

Cladism and sexual interbreeding are not enough for reasons outlined above, but even though DNA-analysis is used more and more often in modern taxonomy it has some problems of its own: DNA varies a lot inside populations of the same species, usually of course less than it differs from the DNA of other species, but not necessarily so. Think of two groups A and B of initially the same species, group A with little variation, group B with a lot of variation (group A can for instance be a smaller group with a lot of interbreeding or a group that went through a bottleneck); group A may now due to a mutation split (“speciate”) into the now-reproductively-isolated species (and populations) A’ and A’’, whereby only A’’ can still interbreed with B. Genetically, individuals of A’ and A’’ might still be much more akin to each other than they are to B, although individuals from A’ are in a different species than those of A’’ and B. Moreover (a variant of a problem of cladism), taxonomy based upon genetic variation identifies genes with DNA or RNA, which is the first meaning of the concept gene (see §1.21),<sup>135</sup> whereas for the real appearance of an organism and the typological concept of species the notion of a gene as a gene coding for some trait is much more important. In real DNA-analysis of the differences between species this is consciously or unconsciously admitted: one does not start with unidentified DNA-strings to set up a taxonomy of species: one starts with some *exemplars* of species and then looks for differences in DNA, which sometimes has the surprising result that some species are seen as more closely related (cladistically) than previously thought, or the other way round. In choosing an exemplar, however, one already relies on other (morphological) traits than cladistic descent and sexual interbreeding.

This pragmatic biological species concept seems readily exportable to non-biological worlds of pseudo-Darwinian replicators: using a combination of cladism and other typological traits one can name the humps and clumps out there (the criterion of sexual interbreeding can of course not be used).

## Emergence

Variation can be called the chaotic component that brings novelty and innovation into the world of design. Without variation there would be nothing to be selected for and thus no evolution. Due to variation more complex entities can arise with whole new properties, a process called emergence. It is not the case that Darwinian evolution is necessarily a process towards complexity (as orthogonalists such as Spencer, Haecker and Teilhard de Chardin held, see §1.1); no, it is the other way round: complexity can only arise *by accident* when something a little less complex has previously arisen (by accident as well). Dennett with this point in mind says that Darwinism denies the existence of “sky-hooks”: hooks hanging freely in the air

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<sup>134</sup> Cladists and supporters of the BSC may object and say we offer nothing new when we use their concept as a typological trait; they are right. We synthetically and pragmatically use their criteria without the burden of claiming that these criteria can be used individually to characterize all species.

<sup>135</sup> Better: it relies on genes as sequences of DNA/RNA and cladistic method to minimize the number of mutations that get from one sequence to another, although we cannot be sure that the mutation resulted from this process.

raising things upward; Darwinism requires only a “smaller crane” on earth to lift a “bigger crane”, nothing more and nothing less (Dennett 1995).

What has worried some philosophers are the notion of emergence and the more fundamental notion of accident, which are both used in a sense which is slightly more technical than the common sense notions. The Darwinian sense of the word ‘accidental’ combines two of its common sense uses: i) the sense of *without purpose* and ii) the sense of *unpredictable*.

i) The sense of *without purpose* or *blind* was discussed above as we discussed the nature of variation: ‘accidental’ means in the context of Darwinism the same as *being mechanically caused*.

ii) The sense of *unpredictable* is nowadays less of a problem with many people basically familiar with chaos theory (the “butterfly effect”). People were once worried by the implication that evolutionary theory demanded that a completely deterministic physical world allowed for true novelty. This emergence of novelty was pictured as a break in the determinist worldview, or the new properties were seen as mystically supervening on the primary deterministic worldview. There were many philosophical attempts to reconcile Darwin’s theory with this picture.<sup>136</sup> Since chaos theory rose to public prominence, however, the problem of unpredictability within a deterministic worldview has disappeared: even within physics,<sup>137</sup> the pillar of the deterministic worldview, it was found that knowledge of the exact laws and the initial conditions up until an arbitrary level of accuracy was not enough to describe a physical system. That is because arbitrarily small changes in the initial conditions could lead to infinitely many different outcomes, even though the laws are deterministic. Thus chaos theory puts serious constraints upon our predictions. Darwinism uses ‘accidental’ in the same sense as chaos theory: Darwinists do not doubt that everything is mechanically caused (and not magically supervenient), but this does not mean that they can always predict the outcomes.<sup>138</sup> This is not mysterious, but just the same as holds for chaos theory

A related question concerns the issue whether evolution would proceed in the ‘same way’ if it started again on our planet with slightly different initial conditions. This discussion has recently been most vehemently fought between Stephen Jay Gould and Simon Conway Morris. Gould held that evolution would proceed radically differently given different initial conditions (something plausible given the butterfly effect) whereas Morris speculated in *Inevitable humans in a lonely universe* (2003) that humans are more or less the logical outcome on evolution on earth with its highly specific conditions. Whereas the former stresses contingency, the latter stresses all sorts of factors (mechanical, developmental, social/organizational)

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<sup>136</sup> Some for instance tried to see the biological variation as the result of indeterministic quantum fluctuations (a typical example of using a hard-to-understand theory such as quantum mechanics to explain a hardly related but difficult problem in its own right by thinking that hard problems get solved by hard theories).

<sup>137</sup> The description of chaos theory that follows is sloppy, although its basic idea is sound. For a popular yet more technical accurate account see James Gleick, *Chaos: Making a New Science* (1988).

<sup>138</sup> One might argue that the cases of Chaos theory and Darwinism are not equivalent. In Chaos theory the unknown “new” outcomes are of the same type: ultimately elementary particles interacting, whereas the new outcomes of Darwinism are the emergent properties of complex living organisms. However, behind the evolution of these complex organisms lie just the same processes as the other processes involving elementary particles. The only difference is that at the basis of organisms are accidental structures that could retain their structure (heredity) through replication allowing them to become complex by small changes over geological time.

that have constrained evolution beforehand (see also Morris 1998).<sup>139</sup> It is an empirical question how strong these constraints are; we will return to it very briefly in §1.23 on selection and adaptation, where this discussion is better handled. The fact that more or less the same constraints have given rise to such vastly different creatures (if we look at the fossil record and current biodiversity) and the fact that nobody has even remotely shown how a set of physical, chemical, and social constraints leads necessarily to a certain creature (human or not), casts strong doubt on the idea that these constraints are so strong that they lead to “inevitable humans”.

### **The definition of variation**

Given this initial discussion we can now give our formal definition of variation: *variation of a trait in biological evolution consists in ‘continuous’ or ‘discrete yet small’ differences of a certain biological property between two or more individuals of the same kind, that are mechanically caused by differences in the replicators or the environment. While using this definition, two things have to be taken into account: 1.) usually only the differences in the replicators are heritable (and are thus the true object of adaptive selection), 2) the ‘kind’ that the definitions speaks of is determined by a pragmatic combination of cladism and some typological parameters.* Knowledge of the variation mechanism is needed to determine in controversial cases whether we are dealing with Darwinian variation or not.

Note finally that with this definition we exclude other entities from Darwinian evolution than our definition of replication did: the definition of replication seems more restrictive in the sense that it excludes the cells of the immune system, which would be an acceptable version of variation, constrained and partly pre-programmed as it is, and importantly: mechanically caused. However, the definition of variation could exclude cases the definition of replication does not when for instance replicators undergo mutations too big to be called Darwinian variation: the cases of the mitochondrial organelles already put stress upon our definition; entities undergoing even bigger mutations (as happen outside of biology and in some proposed theories) could be seen as perfect replicators but nevertheless as clearly not Darwinian from the perspective of variation.

## **§1.23 Selection (Fitness)**

*The concepts of fitness and selection are the hardest of the three fundamental concepts; whereas we found little work on defining replication and variation, a lot of work in the philosophy of biology concentrates on fitness and selection and the related notions of adaptation, function, and the even more general notion of teleology. With our extensive definitions of replication and variation and the distinction of a replicator and the locus of selection (see §1.21) we are now in a good position to formulate definitions of fitness and selection. We will first delineate the non-Darwinian uses of the terms ‘fitness’ and ‘selection’. We then note the notorious ambiguity in the definitions of adaptation, fitness, and the products of selection which either i) equate them with (possible) offspring or ii) identify them with a property of an entity that allows it to do something (better). After that we discuss many elements that influence the fitness of organisms. Then we will discuss the Panglossian paradigm and the many pitfalls one has to avoid to*

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<sup>139</sup> Note that Gould has always stressed the importance of constraints too in his famous critique on the Panglossian adaptationist fantasies (Gould and Lewontin 1979). However, these constraints leave enough room for indefinite variation given different initial conditions.

determine in practice whether something is an adaptation. After these warnings we define the expression “a trait A is fitter than trait B” as “the trait A confers upon its bearer(s) at level X a net heritable advantage expressible in trait-dependent currencies usually given by the proximate mechanisms involved, an advantage which is ultimately thought to be related to the expected number of offspring or a representable measure thereof over a number of generations or within a fixed amount of time and in a ‘fixed’ abiotic and biotic environment, whereby the representable measure of offspring, the number of generations or the timeperiod and the environment are chosen in a trait-dependent relevant way. We further define selection as: repeated cycles of replication, variation, and environmental interaction so structured that environmental interaction causes the fitness of traits to be increased (and thus replication to be differential) or acquired fitness to be maintained. We finally define adaptations in terms of natural selection as its products; we define function in a two-fold way following the Von Wright-Cummins discussion, whereby function I is defined in terms of selection as Von Wright does and function II, although for its existence dependent on the existence of function I, as Cummins does; finally we define Darwinian teleology also in terms of selection as teleonomy: the quality of apparent purposefulness and of goal-directedness of structures and functions in living organisms that derive from their evolutionary history and adaptation for reproductive success.

The third principle of Darwinism is the hardest to analyze, but also the most fundamental. Darwin found this principle so important that the original title he had in mind for the *Origin of Species* was simply ‘natural selection’, which now only appears in the sub-title. Later he became a little uncomfortable with this name and considered Spencer’s word ‘fitness’ and ‘survival of the fittest’ a better phrase to express his ideas, because this phrase did not seem to suffer from the anthropomorphism inherent in the notion of natural selection:<sup>140</sup> selection was something farmers, dairy breeders, and crop cultivators did *intentionally*, and now this human intentional action was attributed to nature, which was supposed to do the ‘same’ thing, but now unintentionally.<sup>141</sup> We agree with Darwin: although for heuristical purposes the notion of selection was useful when he first presented his

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<sup>140</sup> See Spencer 1864: 444 “This survival of the fittest, which I have here sought to express in mechanical terms, is that which Mr. Darwin has called ‘natural selection’, or the preservation of favoured races in the struggle for life.” In the later editions of the *Origin* Darwin does even use the notion of survival of the fittest, fully crediting Spencer: “I have called this principle, by which each slight variation, if useful, is preserved, by the term Natural Selection, in order to mark its relation to man’s power of selection. But the expression often used by Mr. Herbert Spencer of the Survival of the Fittest is more accurate, and is sometimes equally convenient (chapter 3).” This expression is more accurate, because – as is clear from the rest of chapter 3 – it avoids the hidden anthropomorphism in the word ‘selection’. Darwin introduced the notion of selection (man’s selection) in earlier chapters to demonstrate how man in a very short time could adapt organisms to his own taste as a hint to the powers of natural selection: “man by selection can certainly produce great results, and can adapt organic beings to his own uses, through the accumulation of slight but useful variations, given to him by the hand of Nature. But Natural Selection, as we shall hereafter see, is a power incessantly ready for action, and is as immeasurably superior to man’s feeble efforts, as the works of Nature are to those of Art (idem).”

<sup>141</sup> Of course nature in selecting on the lactating abilities of cows does something different from what man does: artificial selection works much faster than natural selection since a farmer can intentionally force a well-lactating cow to mate with a bull whose female children also lactate well, whereas in nature the mating is much more random so that the ability to lactate well spreads much more slowly through the population. Moreover, a farmer can almost exclusively breed for lactation because he supplies his animals in his warm stables with food, medicine, etc., whereas nature has to select for cows that have to do much more than produce milk (the modern farmer’s cow might not survive outside the stables). Thus nature and man do not do exactly the same thing, but the result is more or less equivalent and both profit from natural variation.

ideas, the term 'fitness' is less anthropomorphic, and when we define selection we have to take care to remove any notion of intentionality. Since the word fitness is used most often in philosophical literature on selection/adaptation etc. we choose to start focusing on this notion.

We will first describe some non-Darwinian uses of the terms 'fitness' and 'selection'. These non-Darwinian uses will turn out to be non-functional notions. After that we describe the systematic ambiguity *in the context of Darwinism* in the notions of adaptation, fitness, and selection, where definitions sometimes directly relate these notions to the number of offspring and sometimes relate them to some kind of that which is indirectly related to reproduction. Then we give our own preliminary definition.

### **Non-Darwinian uses of selection and fitness**

Dennett (1995: 163) borrows an example from Küppers (1990) of a game with a checkerboard and differently coloured marbles to illustrate non-Darwinian selection (a game which is equivalent to genetic drift): "Start by randomly placing the marbles on all the squares creating the initial confetti effect [i.e., a potpourri of colours on the squares – MH]. Now throw two (eight-sided) dice to determine a square (column 5, row 7 for instance) on which to act. Remove the marble on that square. Throw the dice again; go to the square they name and check the colour of the marble [i.e. replace or when empty put a marble in this spot – MH] with the colour of the marble on the just-vacant square ('reproduction' of that marble). Repeat the process, over and over. Eventually, it has the effect of unrandomizing the initial distribution of colours, so that one colour ends up 'winning' but for no reason at all – just historical luck. He [Küppers] calls this "non-Darwinian selection" because it is selection in the absence of a biasing cause. Selection without adaptation would be the more familiar term." The existence of this type of selection is not a refutation of the claims of Darwinism; it is only a process that Darwin does not describe.

A simpler example of non-Darwinian, non-adaptive selection is a natural sieve or filter: clay soil is not letting certain particles through. One could also call this non-Darwinian fitness as the selected particles have the property of *literally* fitting through the sieve or not.<sup>142</sup> These two examples show that the selection Darwinism has in mind is *adaptive* selection: *the property or trait of an entity that is selected upon as such should increase its [the entity's] chances of replication*; otherwise the selection cannot be called Darwinian. A marble's being red as such does not influence its chances of (pseudo-) replication, a particle's size fitting through the clay sieve does not influence its chances of replication (because the particle probably does not replicate in the first place.) With that in mind, we can turn to the Darwinian uses of the term 'selection', which should display a clear relation with functionality.

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<sup>142</sup> Note that Ariew and Lewontin 2004: 347 argue that the sieve-metaphor (or in their terminology the metaphorical extension of the "key-and-lock" sense of the word fitness) is guiding Darwin as he speaks of fitness: "[Darwin] introduced fitness as an inexact metaphor. (...) The natural properties of individual organisms varied from individual to individual within a species. (...) At the same time, the organisms lived in an external world – the environment – whose structure was causally independent of those organisms. Different individual members of a species, then, 'fit' into the environment to different degrees as a consequence of their variant natural properties, and those that made the best 'fit' would survive and reproduce their kind better than those whose 'fit' was poorer. The word 'fit' ('fittest', 'fitness') is a metaphorical extension of its everyday English meaning as the degree to which an object (the organism) matches a pattern that is pre-existent and independently determined (the environment). This metaphorical lock-and-key fitting of the organism into the environment is reflected in the modern concept in ecology of the environmental or ecological 'niche' that species are said to 'occupy'."



## The ambiguity of the Darwinian use of the terms ‘adaptation’, ‘fitness’ and ‘selection’

Numerous definitions of adaptation, fitness, and selection exist; tons of papers have been written about them. Papers on adaptation and selection tend to focus on how authentic adaptive selection (i.e., normal natural selection) can be distinguished from non-adaptive selection; papers on fitness tend to focus on how to define and to measure it. In this huge set of definitions two basic types can be distinguished: the first relate these notions directly to the number of offspring (type 1 definitions), the second to some form of causal functionality which *indirectly* influences replication chances (type 2 definitions). In the former group we find definitions of adaptation as that which causes differential replication,<sup>143</sup> of fitness as the number of offspring, and of selection as the force which causes differential replication.<sup>144</sup> In the latter group we find definitions of adaptations as traits to do/have X, of fitness as being fit to do/have X, and of selection as a causal force operating on traits to do/have X, where X is a behavioural or morphological feature. Since we seek a definition and the literature that tries to define one of these notions concentrates on the notion of fitness, we will start with this notion.

Of the two types of definitions, the former has been more popular – so popular, in fact, that the second is often ignored or forgotten – for reasons we will discuss shortly. One particular definition, however became popular by reintroducing some elements of a type 2 definition in a type 1 definition of fitness: the so-called propensity definition of fitness. The propensity definition saw fitness as the *disposition* or the *potential* to beget a certain number of offspring (Mills and Beatty 1979). This definition avoids the problem of chance: when two identical twins or clones live in the same environment but one by chance gets killed, the naïve definition of fitness<sub>1</sub> as the number of offspring would entail that two twins have a different fitness, whereas common sense would say that their fitness is equal. Insofar as the definition mentions the number of offspring, one could call the propensity definition a type 1 definition; insofar as it mentions a *disposition* it has an element of fitness<sub>2</sub>. In our own definition of fitness we will more fully integrate fitness<sub>1</sub> and fitness<sub>2</sub>.

We will first describe definitions of fitness<sub>1</sub> and of fitness<sub>2</sub>, then we give our own definition; after that we turn to the notions of selection and adaptation. The discussion is further complicated by terminological issues: fitness<sub>1</sub> is sometimes called Darwinian fitness, whereas Darwin himself used the word fitness more in the sense of fitness<sub>2</sub>, although traces of what is now called Darwinian fitness (fitness<sub>1</sub>) can be found in Darwin’s Malthusian ideas on *exponential population* growth.<sup>145</sup>

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<sup>143</sup> This definition of adaptation is rare, as many associate adaptation most naturally with functionality (fitness<sub>2</sub>). However, examples of this type of definition can be found in Mayr 1988 where (general) adaptation denotes “the propensity to survive and reproduce” and Reeve and Sherman 1993 where an adaptation is defined as “that form of a character, among a set of variants, that has the highest fitness,” where fitness is equated with the number of offspring.

<sup>144</sup> See for example Crow and Kimura 1970: 173 “selection occurs when one genotype leaves a different number of progeny than another.”

<sup>145</sup> See Krimbas 2004: 186 who also provides further references “Although Darwin barely ever used the word fitness in *The origin of species*, he clearly had the concept of it. He took it to capture a property of an individual, viz., a physical property of the organism accommodating to its way of living, and thus a cause which explains the success of individuals subjected to the process of natural selection.” See also Ariew and Lewontin 2004: 349 “In the original Darwinian structure, fitness is a derived property of the natural properties of individuals living in a particular juxtaposition to the structure of the environment. The characteristic Darwinian adaptive explanation [using fitness] is a kind of engineering analysis in which particular natural properties of individual organisms were shown to lead to greater expected reproduction

Fitness<sub>1</sub> in its most naïve form equates the fitness of X (usually a population or an individual) with X's number of offspring. This definition arose in the work of Fisher (1930) in the field he can be said to have founded called *population genetics*. He defined fitness (baptized by him Malthusian fitness) as the ratio of the number of individuals of one generation ( $N_{t+1}$ ) to that of its parental generation ( $N_t$ ), which according to him is equal to the exponential growth of 'm', the Malthusian parameter (a heritable factor), defined as the difference between the percentage 'b' of the individuals that give birth to one individual during a time interval  $\Delta t$  and the percentage 'd' of those dying in the same period, i.e., as a formula (with 'e' of course the base of the natural logarithm):

$$N_{t+1}/N = e^m = e^{(b-d)}$$

The fitness was thus a measure of the *growth rate* of a *population*. However, "in this approach all individuals of a population may be considered identical, and thus the fitness concept refers to a common individual property no less than it characterizes the whole population" (Krimbas 2002: 186).

Fisher's definition seems simple and intuitive: the organisms with most offspring will survive, and they are the object of Darwinian explanations. On the basis of this definition Fisher formulated his fundamental theorem of natural selection that states that natural selection tends to maximize fitness: "The rate of increase in fitness of any organism at any time is equal to its genetic variance in fitness at that time." Since variations are always positive, the change will always be in the direction of an increase in fitness.<sup>146</sup> As we will see, Fisher's definition has serious problems, foremost that it does not work for sexual populations as it disregards Mendelian segregation.

The classic competing model to Fisher's was formulated by the two other cofounders of population genetics, Wright and Haldane, and is called the basic viability model that can be found in any text on population genetics (Gillespie 2004, Hartl and Clark 2006). In this model in its most basic form a population is represented as the relative frequencies of two alternative alleles at one locus so that absolute numbers of *individuals* do not enter the model. Other allelic loci are supposed to be the same, the reproduction is thought of as completely sexually and

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by those individuals in particular environments." Michod 1986: 290 can thus write "The meaning of "fitness" in Darwin's phrase ["survival of the fittest"] is similar to that of the term "adaptedness". By "survival" Darwin was often referring to survival over evolutionary time, or evolutionary success – a measure of which is fitness in Fisher's sense [fitness<sub>1</sub>]. Thus Darwin's phrase makes the falsifiable claim that the actual rate of increase of a type, its fitness [fitness<sub>1</sub>] (Darwin's "survival"), is determined by its adaptedness ["fitness<sub>2</sub>"] (Darwin's "fitness")."

<sup>146</sup> Fisher himself saw this law as an analogous to the second law of thermodynamics and fitness as analogous to entropy. It is this law that provided easy ammunition to the critics of Panglossian adaptationism, but this is partly due to misunderstanding based on Fisher's feud on adaptive landscapes with Sewall Wright. For forty years people read it as saying that the average fitness of a population would always increase, and models showed this not to be the case. However, Fisher's theorem – rightly understood – is correct, but not very interesting: the theorem gives a formula for part of the change in gene frequency, and not for all of it: the part due to natural selection as Price 1972 made clear. Fisher's concept of fitness thus does not explain natural selection, but presupposes it since all it does is redescribe its outcome. More recent work on inclusive fitness by Alan Grafen (Grafen 2006, 2003, 2002, 2000), the world's leading mathematical biologist, and others try to improve Fisher's work by including other factors in the definition of fitness to explain the whole change in genes. This work is good for modelling some cases, but we hope to show that since it does not include elements of fitness<sub>2</sub> it will not provide us with a general solution to defining fitness.

successive generations are discrete with no overlap so that all reproduction can be seen as simultaneous. In this model one is interested in the differences in frequency of certain alleles between generations (measured from gamete to gamete). Schematically:<sup>147</sup>

Generation $t_1$			
gametes	$A_1$	$A_2$	
frequencies	$p_t$	$1 - p_t$	
<i>mating</i>			
zygotes at birth	$A_1 A_1$	$A_1 A_2$	$A_2 A_2$
frequencies	$R$	$H$	$D$
<i>survivorship</i>			
adults	$A_1 A_1$	$A_1 A_2$	$A_2 A_2$
frequencies	$R' = RV_1/V$	$H' = HV_2/V$	$D' = DV_3/V$
Generation $t_2$			
gametes	$A_1$	$A_2$	
frequencies	$p_{t+1} = R' + H'/2$	$1 - p_t + 1 = D' + H'/2$	

Table 1.3: replication and survival in the population model of Haldane and Wright

Selection is now said to occur when the frequency  $p_t$  changes and the genotype that increases in frequency is said to be fitter than the decreasing one. Finally on the basis of this system and the frequencies of types, the population fitness can be defined as the expected mean number of progeny of every individual of the parental population (calculated by the frequencies times the absolute number of individuals in a population and the values given to the specific fitnesses of the various genotypes)

Although this model can thus accommodate Mendelian segregation, it suffers from many of the same problems as Fisher's model. We will briefly review these problems, as most of them can be of importance outside of biology as well. Note that behind the model stands the ideal of attributing a *single scalar fitness value*<sup>148</sup> to an individual or a population. The critique will show that this ideal is untenable.

The problems are as follows:

**Circularity:** the fitness<sub>1</sub> definition is said to be tautologous, for more than one reason. For short: the fittest are defined in the fitness<sub>1</sub> definition as those that survive and reproduce; the term central to Darwinism "survival of the fittest" then becomes "survival of those who survive and reproduce", a clear tautology. We discuss the many different allegations of circularity in the next §1.3, where we will show that although fitness<sub>1</sub> is indeed circular, the fitness<sub>2</sub> definition removes these allegations.

**Fitness as cause:** closely related to the circularity charge is the problem that both the Fisherian and the Wrightian/Haldanian proposals face: they intend to regard fitness as the *cause* of differential selection (fitter animals will be selected for):

<sup>147</sup>  $V_i$  is the probability of survivorship into reproductive adulthood of type  $i$  given  $V$  (mean population viability).

<sup>148</sup> Or as Rosenberg 1983: 461 puts it: "Fitness levels must be measured in a coin common to many biological systems if it is to play its explanatory function in the theory of natural selection." See also Krimbas 2004 and Ariew and Lewontin 2004 criticizing the original intention of population genetics that they can represent the fitness of an individual by just one unique scalar.

“but since the estimation of fitness is performed a posteriori, that is after the occurrence of selection and on the basis of selective results, it might be taken that causes and effects are confounded” (Krimbas 2004: 186). The proponent of population genetics might respond that many causes are measured on the basis of their effects: a thermometer measures the temperature of a water basin (which is the cause of it becoming warm) by the effect the water has on it (see Sober 1984, who spells out this analogy in more details). However, the temperature of the water basin can be measured by many means (many types of thermometers, by many derived thermodynamic parameters, etc.), but fitness<sub>1</sub> can *only* be measured after the selection by counting offspring. As such the circularity charge of confusing causes and effects (results) has a strong case.<sup>149</sup>

**Sexual reproduction/Mendelian segregation and heterozygote superiority:** as mentioned above, Fisher’s model cannot accommodate sexual reproduction as it cannot accommodate Mendelian segregation. Fisher simply equates the ratio of a certain genotype  $f$  in two successive generations:  $f_{t+1}/f_t$  to its fitness, but as many have noted, for instance Maynard Smith (1991), this leads to odd consequences in the case of heterozygote superiority such as sickle cell anemia in which homozygotes  $aa$  have no offspring. These homozygotes arise from matings between two heterozygotes  $Aa$ . If these heterozygotes increase in frequency (due to the prevalence of malaria, to which they are resistant),  $aa$  will automatically increase in frequency too according to Fisher, although the  $aa$  genotype has zero offspring and thus zero fitness!

**Asexual and atypical reproduction:** asexual reproduction on the other hand cannot be accommodated by the Wright-Haldane model so that no general model exists for both sexual and asexual reproduction. Moreover, the Wright-Haldane model can only accommodate sexual reproduction that works *perfectly according to the rules of Mendelian segregation* and thus does not work in case of genes for Mendelian distortion such as meiotic drive, etc. It also does not work for many of the replication mechanisms we discussed above, as those of chemical replicators (**a**), prions etc. and viruses (**b** and **c**), selfish DNA (**d**), cytoplasmic DNA (**f**), and epigenetic settings (**h**) which each require different models that take care of their unique form of replication.<sup>150</sup> Finally, although the Wright-Haldane model captures some aspects of sexual Mendelian reproduction, the model is hard to work with in practice as keeping track of genes in sexually reproducing communities is hard due to

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<sup>149</sup> See also Ariew and Lewontin 2004 who distinguish between fitness as cause- and fitness as outcome-models, and Henle 1991: 97f who discusses authors who on the basis of the problems of regarding fitness as cause have used fitness only as a “description” of natural selection. Authors differ on whether this description has still “explanatory value”; Dunbar 1982 denies this, Ender 1986 opposed this view, and Krimbas 1984 took a middle position. We will argue below that the fitness<sub>1</sub> definition indeed has no explanatory value, but the fitness<sub>2</sub> definition has, because it includes the functional trait that is selected for.

<sup>150</sup> Michod, following Eigen 1979, 1971 who came up with the idea of the RNA-hypercycle in a world of precellular “naked” replicators (see **a**) chemical replicators), distinguishes Malthusian evolution from non-Malthusian evolution. According to Michod, Darwin is a Malthusian insofar as he believed that “better-adapted [i.e., fitter] types should increase, even if rare (Michod 1986: 293).” The work of Eigen that in some circumstances, such as that of the RNA-world in which replicators interacted cooperatively for their own replication, a rare type cannot increase regardless of the adaptive capacities it might have (stability, the force to attract nucleotide bases, etc.). Supporters of the fitness<sub>1</sub> definition would of course say that this rare type then simply has zero fitness, but Michod understands “fitness as a function of functional components” like our definition of fitness<sub>2</sub> and then this statement of adaptive fitness without reproduction makes sense. Even the supporters of the fitness<sub>1</sub> definition have to agree that non-Malthusian effects complicate their own simple picture and that their definition cannot do justice to the interdependent replication of Eigen’s chemical replicators.

recombination, etc. Nevertheless, including the fitness effect of sexual reproduction is essential to determining the fitness of sexually reproducing creatures, as we will see below when we discuss pleiotropy, inclusive fitness, population density, etc.<sup>151</sup>

**Finding a representative measure of reproduction:** as we discussed the different reproduction mechanism above, we saw that some organisms like plants (but also bacteria and animals see **k**, **l**, **m**) can reproduce both sexually and asexually, i.e. in the case of plants by producing flowers, pollen, and seeds which are dispersed over wide areas (sexual reproduction) as well as by cloning themselves through stems, and for instance underground connections that pop up in the neighbourhood of the mother plant with which they remain connected (as mushrooms and violets do). How should we now express the total offspring of a plant in one scalar unity, as we have to combine two completely different kinds of offspring?<sup>152</sup> This problem is known as the “book-keeping problem”. It shows that the formulators of the fitness<sub>1</sub> definition had animals or humans in mind where offspring consist of discrete individuals that can easily be counted. Proposed solutions to the book-keeping problem are attempts to build population genetics on the basis of energy fluxes (Margaleff 1968) or to use biomass as a measure of population fitness (Dobzhansky and Pavlovsky 1961, Van Valen 1976). Neither model gained much currency in the literature. Measuring or determining energy fluxes is hard and both biomass and energy fluxes obscure the details of the different replication modes and their future fitness (5 kilograms of seeds dispersed over a wide area may be fitter than a 5 kg plant in bad condition) and thus these suggestions do not solve the problem. Interestingly enough, by finding alternative representations for individuals the fitness<sub>1</sub> definitions come a bit closer to the functional fitness<sub>2</sub> definitions as they make implicit that fitness is correlated with other pseudo-functional parameters because they make implicit that being fit means *converting* as much biomass into your kind or controlling as much energy as possible. The suggested parameters can be demographical (to which we turn next) or non-demographical such as dominance status, territory size, mating ability, health, etc.<sup>153</sup>

**Pleiotropy (and genetic linkage, epistasis):** as we discussed the ambiguity of the term ‘gene’ in §1.21 we already noted that one gene (as a piece of

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<sup>151</sup> Keller argues explicitly against the mistaken tendency in much evolutionary theory to assume “reproductive autonomy of the individual organism” thus failing to recognize that “the fitness of a particular female (or male) reproducing sexually always depends on the availability, and second on the fertility of males (or females) in the breeding group. (...) For sexually reproducing creatures, fitness is in general not an individual property but a composite of the entire breeding population (Keller 1987: 393).” See also Byerly and Michod 1991: 10.

<sup>152</sup> See also Krimbas 2004: 196, and Ariew and Lewontin 2002: 359f, who spell out this problem in a lot of detail: “Violets reproduce in two ways. A flower may set seed and these seeds, having dispersed, may each give rise to a new plant. Alternatively, a violet stem may send out underground runners which, at points along their lengths, put up flowering stalks. A plant produced from seed is a genet while one produced above ground and asexually from a runner is a ramet. The problem that has plagued evolutionists who deal with organisms that have both sexual and vegetative reproduction is how to count ramets and genets in assigning reproductive fitness. Do all the ramets of a single original stem count as belonging to a single individual, or is each to be counted as a separate individual? It might be argued that since the ramets are all connected as a single body, they are collectively one individual. But is the occurrence of a break in the underground stem sufficient to produce a new individual for accounting purposes? (...) [Or] suppose an entire field is occupied by hundreds of ramets from a single original plant, except that in one spot there is a single plant that is a genet of a different type. Are the two types to be counted as equally frequent?” Another example with both sexual and asexual forms of replication are aphids, which reproduce parthenogenically in summer for many generations and then switch to sexual reproduction in autumn.

<sup>153</sup> Cooper 1984 calls them “derived” or “indirect measures of fitness” and discusses some of their aspects. See also Krimbas 1984 and Henle 1991.

DNA) can code for more than one phenotypical trait (*gene as trait*), a phenomenon called pleiotropy; the fitness models use the 'gene' primarily only in the first meaning as *piece of DNA* and forget thus that a gene (as a piece of DNA) may become fixed although it codes for a functional trait<sup>154</sup> that might even be harmful to total fitness (as long as the other functional trait it codes for outbalances this effect). Genetic linkage (when two genes lie close together on the same chromosome so that it takes a long time for them to be separated by recombination) and epistasis (when the phenotypic effect of one gene is modified by one or more other genes) provide other instances of genes' being selected not just for their own fitness, but due to that of others.

**Delayed gene action or postponed reproductive fitness effects:** the effect of some genes crops up after more than one generation (think of mules), but other interesting examples are the famous grandchildless mutant genes of *Drosophila melanogaster*: female homozygotes for the mutant allele produce sterile offspring (regardless of the genotype of the male parent or that of the offspring). The reason for this is that early in the development of the children of this homozygous mother the polar cells (from which gametes are derived) do not form. "The phenotype of the individual is thus not determined by its own genotype, but reflects the genotype of its mother (not necessarily from its mother's phenotype, which is determined by its grandmother's genotype). In these cases offspring counting for fitness estimation should be delayed by one generation: grandchildren are to be counted."<sup>155</sup> Do not think, however, that by counting grandchildren instead of children all problems will be solved: the effects of some genes are even further delayed. Think of the effects of methylation we discussed under the reproduction of epigenetic settings which can be set for many generations (**h**). Ahmed and Hodgkin (2000) have further shown how complete sterility may be produced by mortal germline mutants in some strains of worms after *four* to even *sixteen* generations (due to this mutant the chromosomes of these worms exhibit progressive telomere shortening until they become completely sterile). One could also think of forms of antagonistic evolution such as displayed in arms races and in the well known Red-queen process, also called run-away sexual selection in which due to female preference genes males start to display more and more costly traits to be successful at replication until the traits are so costly that the population could ultimately go extinct (see Ridley 2005, p. 639, Burian and Richardson 1992: 356 speak of "evolutionary dead-ends"). Thus it seems we have to measure many, many generations although it is unclear where a *general* definition of fitness which covers all cases would have to put the limit, and not only because of practical problems:<sup>156</sup> if one goes back more and more generations and if one assumes random mating, after not that many generations an animal is the ancestor of *all* the animals of his kind and if one goes back a little bit more all animals are the ancestors

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<sup>154</sup> To understand pleiotropy we have to introduce the concept of function in the discussion of fitness and thus we automatically come into the realm of type 2 definitions of fitness.

<sup>155</sup> See Krimbas 2004: 191, who also gives references to many similar mutant genes in other fly species and mentions other interesting delayed effects. Zebra finch females deposit on the basis of their attraction to a certain male a varying amount of testosterone in eggs, which plays an important role in the fitness of individuals. A life history count based on an egg count misses this fitness effect which is dependent on the parent male's phenotype instead of their own genotype.

<sup>156</sup> Some authors, such as Cooper 1984, have indeed included in their definition of fitness that we have to look at a long or even an indefinitely long period to measure fitness. An indefinite time would make measuring fitness impossible.

of all animals (see Dawkins 2004, chapter 3). The concept of one animal's being fitter than another then becomes meaningless.<sup>157</sup>

**Inclusive fitness:** a related problem to the above is that of inclusive fitness; I can have many children but if I have so many children that many suffer from starvation before they reach adulthood and replicate themselves I am worse off than a parent that invests more in fewer children.<sup>158</sup> In my own reproductive success the reproductive success of my children should be included, and actually also the reproductive success of my children's children, etc. This again brings up the problem of where to cut the arbitrary number of generations we should consider, but with the *added* problem of assigning fitness components of offspring to parent fitnesses: since the fitness<sub>1</sub> definition assumes *discrete* generations, we have to find some way to accommodate inter-generational costs and benefits. Wolf and Wade (2001) formulated such a model and faced the difficulty of assigning the fitness of a parental trait's contributing to offspring's survival to either the fitness of the children or to that of the progenitors (provided it is of course not assigned to both). Either option is not attractive in practice: if the fitness is assigned to the offspring one easily misses actions undertaken by the parents for their offspring which do not have an immediate visibility in survival chances of offspring, and if one assigns the component to the parents' fitness (because it increases the chances of their genes to be present in the next generation) this looks strange insofar as, from the perspective of the parent, the actions are fitness-decreasing (i.e., the actions decrease survival chances).

**Variance of average number of offspring (fertility):** not only the mean number of offspring, but also the variance of the number of offspring is important. Sober (2001) explains this in an easy way. Compare two types: type A always produces two offspring, whereas type B produces one individual in some generations and three in others, with equal probability. The expected reproductive rate is thus the same for both types, namely two. However the expected relative frequencies of the two types over two generations is not the same: suppose we begin with two animals of each type: in the first generation there will be four type A's and either two or six type B's (with equal probability); this gives for the expected relative frequency in the total population of the two types in the next generation: Type A =  $(0.5) (4/6 + 4/10) = 0.535$  and Type B =  $(0.5) (2/6 + 6/10) = 0.465$ .<sup>159</sup> Should we

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<sup>157</sup> An example: there are 6 billion people. If we assume random mating and that every parent begets two children (rather unrealistic, but acceptable acceptable for the model), we only need to go back 23 generations to find a person who is an ancestor of all humans alive. 23 generations may sound a lot, but on the time scales of evolution this is of course nothing. 23 generations brings us only as far back as the Renaissance or the end of the Medieval period. We have to go only a few more generations back before every human we encounter is an ancestor of every human now alive (Dawkins 2004 calculates 31 generations). Note that for Dawkins this argument is an extra reason to think of evolution as gene instead of individual based. We have to go many more generations back before we find the most recent common ancestor of a gene since only half the genes of each parent are transmitted to each new generation and different genes on the chromosome can have different common ancestors. See §1.21 on gene-selection.

<sup>158</sup> This problem becomes even more virulent in species with so-called telescoping generations such as aphids whose remarkable reproduction system we already discussed at the end of footnote 152: the parthenogenetic, viviparous female aphid will have a daughter within her who is already parthenogenetically producing its own daughter at the same time. This leads to the situation where the diet of a female aphid can have inter-generational effects on the body size and birth rate of aphids. In other words, what the aphid eats can directly change the size and fertility of the aphid's daughters and granddaughters (Nevo and Coll 2001, Jahn et al. 2005).

<sup>159</sup> Note that the lowest variance is thus advantageous in the long run. One can subtract  $1/Ns^2$  from the actual mean of the expected number of progeny (where  $s$  is the variance in number of offspring and  $n$  the population size) to correct for this.

therefore use the simple mean reproductive rates or take the variances also into account (which makes things soon incredibly hard)? Both give different answers for our scalar value of fitness.

**Average age at reproduction:** the Fisher and Wright-Haldane model base themselves on the average number of offspring but they do not take into account the *age* at which individuals reproduce. This is an important parameter too: organisms that reproduce sooner will replace their fellow organisms that reproduce later (in case the total population size is *constant* and we can ignore parental care). The simple explanation is that growth is exponential and that the difference between two exponential growth rates is still exponential, so that the types that reproduce on average earlier replace types that reproduce on average later (classical reference: Thoday 1953). In the presence of a major predator, selection on early reproduction has indeed been found (see Kaplan and Cooper 1984, Reznick, Bryga and Endler 1990).

**Variance of average time (place) of reproduction:** not only the average age of reproduction, also the variance of the ages at which reproduction happens plays a major role (the standard references are the papers by Gillespie 1973/4/5/7). The argument is parallel to that of the variance of the average number of individuals produced above. Parallel arguments can be made about variation in spatial location in the case of sexually reproducing creatures of certain types.

**Population density:** the fitness of some genotypes can depend on their frequency as some genotypes only have a chance of survival when they are present in large numbers in a population. Darwin thought this impossible as he held a so-called “Malthusian view of evolution” (Michod 1986: 293) stating that fitter types<sup>160</sup> should increase.<sup>161</sup> However, as Michod noted, some forms are *only* going to increase whenever they are available in sufficient quantities, for instance Eigen’s replicators (see **a) chemical replicators**) and, more importantly, sexually reproducing creatures: since the reproductive rate of any particular sexually reproducing creature depends on the density of suitable mates over a wide range, invading sexual species “that are better adapted can be suppressed while they are in minority (idem, p. 297).”<sup>162</sup>

**Population size reduction or growth:** we already noted some corrections of fitness in which the population size played a role, such as the variance of the mean number of offspring. But population size can influence the measure of fitness in other ways, at least in the Haldane-Wright model which uses a relative instead of an absolute measure of fitness. Demetrius (1992) for instance showed the dependence of genotype dynamics on the demography and growth rate of the population. If the population as a whole is increasing in size, the first, precocious,

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<sup>160</sup> Darwin meant by fitter types better adapted types. See for Darwin’s concept of fitness footnote 145.

<sup>161</sup> See Darwin 1859: 174 “Each new form will tend in a fully-stocked country to take the place of, and finally to exterminate, its own less improved parent or other less-favoured forms with which it comes into competition.”

<sup>162</sup> We doubt, though, whether the evolution of sexually reproducing creatures is truly Malthusian: just as a single, better-adapted asexual organism may not reproduce by chance, so a single or a couple of better-adapted sexual organism(s) may not reproduce since they cannot find a mate. Mutations will reintroduce the favoured mutant in the asexual population, just as they will with the favoured sexual mutant and eventually it will mate (although these chances may be lower than that of the asexual mutant’s replicating). Because of that we think both types of evolution are Malthusian, although it cannot be denied that density effects play a role in fitness determination (see also above: variance of place of reproduction).



genotype increases in frequency, while if the population size is decreasing, it is the second genotype that increases relative to the first.<sup>163</sup>

**Frequency-dependent selection:** sometimes the fitness of a certain genetic type depends on its frequency in a population (standard reference: Kojima 1971). The best example of this is given by the famous Hawk-Dove model, first presented by biologists and game-theorists John Maynard Smith and George Price in their 1973 *Nature* paper whereby hawks choose to fight over resources, which is costly, and doves choose to avoid fights and search for resources, which is less costly. A population of only hawks is unstable and a population of only doves is soon exploited by hawks: thus there is an optimum ratio of doves to hawks in a population to which the population fluctuations will always return (the so-called Evolutionary Stable Strategies ESS).<sup>164</sup> Any deviation from this stable strategy will reduce or increase the fitness of the types (fewer hawks than at the stability point increases the fitness of the hawks; more hawks lowers their fitness etc.). In the case of the Hawk-Dove model, the fitness of an *individual* hawk or dove is thus dependent on the frequency of their comrades and enemies in the *whole* population. Another example of frequency-dependent results is the famous result by Fisher that the sex ratio in populations of sexually reproducing creatures (ignoring inclusive fitness effects) tends to be exactly  $\frac{1}{2}$ , which means that if females are more abundant than males in a population their fitness tends to be lower than that of males and the other way round.<sup>165</sup> The fitness of a single genotype in that case thus depend on the constitution of the whole population.

**Co-adaptation:** co-adaptation arises by co-evolution (Ridley 2005: 616), which happens when two natural types (species, genders etc.) that are dependent for their existence upon each other (be it symbiotic or parasitic) become gradually adapted towards each other, either in a mutualistic way (if both adapt at the same time towards each other), or in an antagonistic way<sup>166</sup> (when changes in one of the types causes changes in the other as in an arms race) (see Thompson 1994). Co-adaptation is far from uncommon since all forms of sexual selection (understood as ornamental selection) that are widely spread in nature are cases of co-adaptation. Since sexual selection can help “the expansion of the co-adapted phenotypes when they are in minority (Michod 1986: 307),” it should thus not be overlooked when calculating fitness<sub>1</sub> of types in a population (assuming for the moment that fitness<sub>1</sub> can be measured). Other forms of co-evolution include insect-plant co-evolution (Ehrlich and Raven 1964) in which taxa of insects become almost one by one adapted to taxa of plant species in so-called mirror-phylogenies and the parasite-host

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<sup>163</sup> This result holds independently of the reason for the increase or the decrease of the population as long as they influence all types equally; it may be due to environmental factors (constant catastrophies, depletion of the environment, etc.) or due to changes in the adaptive properties of all the organisms in the increasing/decreasing population (which represents in itself a change in fitness<sub>2</sub>). See also Charlesworth and Kiesel 1972.

<sup>164</sup> See for instance Maynard Smith *Evolution and the Theory of Games* (1982).

<sup>165</sup> See for more on frequency-dependent selection also Ridley 2005: 127f. For a better explanation of Fisher's results and deviations from it, Ridley 2005: 337f.

<sup>166</sup> The antagonistic selection can lead to a static outcome or to a dynamic process that is never finished. An example of stable antagonistic selection provides the just-described Hawk-Dove model with its ESS; an example of the dynamic process is the indefinite adaptation of parasites and hosts mentioned below. This indefinite evolution is sometimes called the Red Queen effect since Van Valen 1973 postulated it after the Red Queen in *Alice and Wonderland* who says to Alice (as a variation on Achilles and the turtle) that she can never outrun her, because everytime Alice reaches her previous location, she (the queen) will already have gone one step further. See also the popular monograph by Matt Ridley, *The red Queen* (1993).

co-evolution for immunity versus virulence (Ridley 2005 chapter 22). These forms of coevolution too can seriously affect the fitness<sub>1</sub> of types in a population and thus need to be taken into account when calculating it.

**Defining life-histories:** many of the previous points may be summarized as the problem of defining life-histories. We need to know an enormous number of things about the lives of *individual* organisms to truly calculate their fitness<sub>1</sub>. As Ariew and Lewontin put it: “The minimum dynamic model for predicting genetic changes requires the complete specification of the age-specific rates as calculating devices [Ariew and Lewontin think of the well-known model of Charlesworth and Giesel 1972]. Indeed, even the complete relative age-specific rate schedules are not sufficient. It is also necessary to know whether the population as a whole is increasing or decreasing in size, which requires both the *absolute* age-specific mortality and fertility schedules and the age distribution of individuals in the population (2004: 356).” Measurements of all these parameters can be done and have been done by for instance Newton and coworkers (1989), who spent many, many years observing certain birds for their longitudinal life-history data. There are many shortcuts to measuring life-history data<sup>167</sup>, but it is clear given the above considerations that, ideally, the life-history of an individual has to be measured “from zygote to zygote” which may be hard since this would often require observations of the internal fertilization.<sup>168</sup> But apart from these practical problems and the enormous amount of work in collecting all the life-history data, this still would not do for a fitness<sub>1</sub> measurement, since fitness measurements of life-history data on a zygote to zygote basis still miss inclusive fitness effects, delayed gene effects, and to a lesser extent: frequency dependent selection, co-adaptive selection (of individuals, but also pleiotropy etc. regarding genes) and as we will see environmental effects.

**Stable versus variable environments:** fitnesses are fitnesses in a certain environment and fitness in one environment does not mean anything for fitness in another (for instance Brandon 1990). An easy solution is to make fitness a “relative property” within a stable environment (Rosenberg 1983: 458),<sup>169</sup> although this would replace the single scalar quantity of fitness of an individual by a whole set of environment dependent scalars. However, some environments either vary spatially or change periodically (or randomly but regularly), and some organisms have adapted to

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<sup>167</sup> See Stearns 1992 and De Jong 1994 who discuss various means to measure individual fitness in populations with age-structured life histories. Proposed shortcuts are population growth rate (Futuyma 1986, Ricklefs 1990), copulatory success (Arnold and Wade 1984b), survival to maturity (Schluter and Nychka 1994), the product of fertility and survival (Ehrlich and Roughgarden 1987), the number of offspring reaching sexual maturity ‘p’ defined as  $p = N(1-q)$  where N is the number produced, and q offspring mortality between birth and maturity (Henle 1991: 99) or even simply lifetime reproductive success (Newton 1989). Ridley’s authoritative textbook on evolution (2005: 109f) only mentions estimating fitnesses using the rate of changes in gene frequencies and the survivorship of the different genotypes in mark-recapture experiments.

<sup>168</sup> See Krimbas 2004: 189 who is highly critical of using life-time histories to measure fitness: “We may at first consider as a measure of a certain genotype the mean number of offspring it produces. The only permissible way to perform this counting is to start counting the zygotes produced by individuals of this genotype from the beginning of its life as a zygote till its death. Counting from zygote till zygote may be a difficult or a problematic operation, since in different animals internal fertilization may render these observations impossible. To bypass this difficulty, it was suggested to transport the counting period starting at a convenient age after fertilization and ending it at the same age in the next generation. This is a mistaken suggestion since it excludes a part of life history of a genotype from consideration but instead it includes the corresponding period of possible different genotypes, since Mendelian segregation does not insure that offspring share the same genotype with their parent.”

<sup>169</sup> See also Rosenberg 1985: 155 who states clearly “Fitness is a relational property, reflecting the interaction of an organism and its environment.”

live in exactly these environments (compare what we wrote on epigenetic settings **h** as a device to cope with changing environments).<sup>170</sup> This seriously complicates measuring fitness, even if it is admitted that fitness is a relative notion (relative to a certain environment), because one needs to specify exactly what spatially/periodically variable environment one considers.

**Individual fitness versus group fitness and the fitness of other entities:** the models upon which  $Fitness_1$  is based were formulated by population theorists and meant to be about populations. However, we remarked above that they seem to be able to hold just as well for individuals. In practice, only few people are willing to speak about individual fitnesses (for instance Brandon 1990).<sup>171</sup> This is due to the statistical nature of evolutionary theory. The life of an individual is affected by chance processes and for the  $fitness_1$  values to have any basis in reality we should attribute them to types (as the averages of individuals) instead of to individuals as the traditional interpretation of  $fitness_1$  holds (but see below as we discuss the propensity definition which seems to have another way to deal with change processes and individual processes). We think the traditional definition makes sense; what worries us, though, is the ascription of fitness to types based on *individual organisms*, the entities Fisher, Haldane and Wright following Darwin held to be the objects of evolution as they formulated their theories in the 1930s. Above in §1.21 we discussed multi-level selection and the distinction between replicators and the locus of selection and embraced the definition of Sober that “Objects at level X were units of selection in the evolution of trait T iff one of the factors that influenced T’s evolution was that T conferred a benefit on objects at level X (Sober and Wilson 1994: 436).” Sober’s definition would allow us to define  $fitness_1$  for objects at level X<sup>172</sup>, but we will not do this;  $fitness_1$  can only be well-defined for *true replicators*, not for entities at the level of selection, because only true replicators (see our definition) come with *univocal replication lineages* whereas non-replicators such as intra-demic groups (which can be the object of selection) only reoccur and do not replicate (see **p**). The reoccurrence cannot in any straight-forward sense be modelled by an exponential growth as in Fisher’s model, and the Haldane-Wright model also works only for generations that are *autonomous*.<sup>173</sup> However, since we do not think the  $fitness_1$  model is adequate at all we do not seek to apply it to objects at level X as an extension to the  $fitness_1$  model. We will instead choose to attribute fitness to *functional traits T* (as coded for or determined by replicators); by allowing that these traits are selected at multiple levels we include multiple selection in our  $fitness_2$  definition.

**Excluding chance from fitness and the propensity definition:** Above we saw that although the equation of population genetics holds for both individuals

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<sup>170</sup> Tuljapurkar 1990 describes estimating fitness in temporally fluctuating environments and Houston and McNamara 1992 do the same thing for spatially varying environments. Houston and McNamara add to that a description of fitness in case of dispersal of offspring among different environments. The classical reference is Richard Levins 1968.

<sup>171</sup> See also McGraw and Caswell 1996, who present a model for the estimation of individual fitness from life-history data in which they try to find “the population growth rate of the individual”.

<sup>172</sup> Rosenberg 1983: 458 as an early proponent of some version of universal darwinism explicitly speaks of the fitness of objects at level X “Actually, the differences in fitness level may be predicated of organisms, genomes, gametes or other types of items that constitute a biosphere, a line of descent. These items need not satisfy ordinary intuitions about their organic constitution, or methods of reproduction. Indeed they had better not be so restricted if the theory of natural selection, which species their mechanism of evolution – or non-evolution – is to be nomologically universal.”

<sup>173</sup> It was already hard to include intragenerational benefits and costs of inclusive fitness in the Wright-Haldane model; intragenerational mixing would completely upset the model.

and groups or types, the orthodox interpretation speaks only of the fitness<sub>i</sub> of types. This was to exclude change in attributing fitness<sub>i</sub> to entities in nature that are subjected to the statistical principles of natural selection. This can best be understood by considering twins or other forms of clones living in the same environment of which one by chance dies, whereas the other leaves four offspring.<sup>174</sup> If one equates fitness with the (absolute) *actual* number of progeny the fitness of the first twin is zero, whereas that of the second twin is four. However, if fitness is to have any explanatory value we would expect that both twins had the same intrinsic fitness value.<sup>175</sup> This is where the propensity definition, which equates fitness not with the actual number of offspring but with the “expected number” of offspring or the “propensity<sup>176</sup> to leave a certain number of offspring” (Mills and Beatty 1979) comes in. This definition has been defended by many proponents (Burian 1983; Brandon and Beatty 1984, Sober 1984a, 1984b, Richardson and Burian 1992), and many of them see in it also a way to answer the allegations of circularity, because propensity fitness is seen as the *cause* of differential reproduction.

One would expect that Mills and Beatty had applied these propensities to types, but strikingly they do not: they speak of individual fitness propensities and ask us to think of an “imaginary animal” with the propensity of 0.5 to leave two offspring and of 0.5 to leave four offspring, and another animal with the propensity of 0.05 to leave one offspring, of 0.05 to leave two offspring, of 0.6 to leave three offspring, and 0.3 to leave four offspring. On the basis of these individual propensities Mills and Beatty then define the fitness of types (which is a simple average) and relative fitness of types as fitness of a type/max fitness (where the max fitness is simply the fitness of the most successful type).

It is unclear how one can determine these individual propensities. If one could overcome all the problems described above about life-histories, inclusive fitness, etc., one would have a measure of *type* fitnesses and one could use this to attribute to an individual animal the number of offspring one would expect from it and on the basis of its actual number of offspring and one could calculate how much an individual deviates from the average propensities, but that is a far cry from measuring (and empirically testing) the propensities of an individual animal to leave 1,2,3,4 etc. offspring. As such, the propensity definition does not solve anything and skeptics can easily hold that the idea that the propensities are the *cause* of differential reproduction is of no explanatory value, since in principle they cannot be measured

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<sup>174</sup> Twins have played an important role in furthering our understanding of “explanation and prediction in evolutionary theory” since Scriven’s 1959 paper. The twin example is included in the work on fitness by Beatty and Mills 1979, Sober 1984, Rosenberg 1983 and many others. Some theoretical studies have even defined fitness as “population growth rate of a clone” (Stearns and Crandall 1981). McGraw and Caswell 1996: 51 complain that empirical studies of fitness have seldom taken advantage of clonality “despite the fact that many species naturally, or with prodding, can produce such clones.”

<sup>175</sup> The orthodox theorist could say that talking about the fitness of individuals is nonsense and that fitness is only attributable to groups or types, but then we could compare two twin populations in identical environments where one environment is hit by a catastrophe so that none survive. In that case we have the same problem the propensity definition provides a solution for, now in the case of populations instead of individuals.

<sup>176</sup> The word propensity indicates an objective yet statistical “physical disposition” or “ability” (Mills and Beatty 1979: 270) that is hidden from direct measurement. This concept originates in the work of Popper and others on statistical properties in nature as philosophers were looking for a word to describe for instance the objective yet statistical property of an atom to decay after a certain time. See Popper 1959, 1990.

individually (not even by their supposed effects: the actual number of offspring of either types or individuals).<sup>177</sup>

The old allegation against the fitness<sub>1</sub> definition that it confounded causes and effects (see above *fitness as cause*), a problem the propensity was said to solve, still stands too. Mills and Beatty (270) compare the relation of propensity fitness as cause to actual reproductive output with the “solubility of a salt” as the cause of its actual “dissolving”, understanding solubility as a “non-relational property of a substance” and dissolving “as a relational process” (Byerly 1986: 499). However, unless one gives a very good account of the micro-structure of a salt and how it causes the salt to be dissolved, the explanation of dissolving by solubility (just as that of actual fitness<sub>1</sub> by a propensity) is like Molière’s famous *virtus dormitiva* as the cause of why opium makes people fall asleep.<sup>178</sup>

Mills and Beatty are ambiguous about further analyzing the propensity from a *causal* point of view. On the one hand, they interpret the propensities as hidden (but objective) variables that cannot be further analyzed (see footnote 176), and thus they circularly identify propensities explicitly with potential chances of actual offspring number. But on the other hand they relate the fitness propensities to “survival and reproduction”,<sup>179</sup> and they even say that it is tempting to define fitness in “terms of functional traits”, although they reject this for reasons of complexity.<sup>180</sup> We will show, however, that the only non-circular way of speaking about fitness is by incorporating functional elements in the definition.

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<sup>177</sup> Rosenberg does not go into as much detail about individual and type propensities and simply writes: “it is obvious that there are no resources within the theory either to identify or to count these propensities. Moreover, (...) the process of identification moves in the reverse direction. That is, it is only by reflecting on evolutionary considerations that we can assess the bearing of raw data about actual reproduction rates on theoretical claims about propensities to do so. In short, Mills and Beatty’s proposal is operationally sterile as a definition of fitness (1982: 270).”

<sup>178</sup> Rosenberg 1982: 272 makes a similar point: “Interpolating a disposition between fitness and actual survival and reproduction [actual survival and reproduction are the components Mills and Beatty try to split fitness into] severs the direct logical connection between these three notions. But it does so only by introducing a fourth term, the disposition, which is itself unaccounted for in the theory, and so opens up again the prospects for charges of circularity it was meant to forestall. (...) The propensity is the cause of the differences in actual rates, is transmitted from ancestors with the same propensity, and is intra- and interspecifically variable. How can we know all this about the propensity? Through its causes and effects reflected in the actual retrospective and prospective rates of reproduction. But this is the circularity problem with which Mills and Beatty begin.”

<sup>179</sup> See Mills and Beatty 1979: 270 “fitness is somehow connected with success at survival and reproduction, although it cannot be defined in terms of actual survival and reproductive success.” See also Richardson and Burian 1992: 351 “It is important to discriminate different propensities, distinguishing a propensity for reproduction from, an example, a propensity for survival, or within the former, distinguishing mating success from viability and fecundity.” See also Arnold and Wade 1984ab.

<sup>180</sup> See Mills and Beatty 1979: 268f “Since an organism’s traits are obviously important in determining its fitness, it is tempting to suggest that fitness be defined entirely independently of survival and reproduction, as some function of traits. (...) This suggestion derives *prima facie* support from the fact that given such a definition, explanations of differential offspring contribution which appeal to differences in fitness are noncircular. However, no one has seriously proposed such a definition, and it is easy to see why. The features of organisms which contribute to their survival and reproductive success are endlessly varied and context dependent.” Note that Beatty elsewhere explicitly relates fitness to optimum design models (Beatty 1980). Byerly 1986 and Michod 1986 actually understand Mills and Beatty in such a way that their propensity definition can be equated with a functional definition, although this functional definition cannot be spelled out explicitly. See also Burian and Richardson: “To summarize, the best interpretations of fitness as a propensity make fitness a causal consequence of the features of an organism in a relevant range or distribution of (expectable) environments (1992: 357).”

This brings our discussion of fitness<sub>1</sub> to a double conclusion. 1) We saw that the original intention of the founders of population genetics to have a *single* scalar representing the fitness of a type cannot be upheld, because in the light of the above discussion it becomes clear that one has to make some choices before one can determine the fitness values and there is not always a reason why one choice is better than another, thus giving rise to multiple values for the fitness of types by different researchers. Of course, on some choices consensus is possible: it can be argued that one should fix the population density and generally take the variance of the actual number into account etc.<sup>181</sup> However, the choice of the number of generations (for delayed gene effects), the environment, etc. would still be arbitrary.<sup>182</sup> Therefore, it seems that unqualifiedly speaking of the fitness of an individual is useless and that if we still wish to calculate fitnesses these will be measures qualified by different aspects of the life history of animals, its own constitution and that of its biotic and abiotic environment. 2) Which brings us to our second point: in discussing aspects of fitness in the sense of gene frequencies, time and time again functional and phenotypical elements (for which the gene itself codes) interfering processes turned up that played a role in the evolution of the gene frequencies, which could not simply be accounted for by a simple scalar fitness values. Examples are our discussion of reproduction modes and their measure, pleiotropy, delayed functional effects, inclusive functional effects, etc. The fitness<sub>2</sub> definitions will include these aspects.

### **Fitness<sub>2</sub> definitions including functionality:**

The fitness<sub>2</sub> definitions were overshadowed in the literature by fitness<sub>1</sub> definitions due to the success of the Fisher and Wright-Haldane model that canonised the latter within the framework of the modern synthesis, and these mathematical models gave evolutionary theory a truly scientific flavor. Darwin's own use of the term was much more like a fitness<sub>2</sub> definition. Ariew and Lewontin (2004: 349, 351) place two explanatory models involving fitness next to each other, that of Darwin and used by empirical ecologists such as Kettlewell and Ford in their famous studies of the melanic form of the moth *Biston betularia* (see De Roode 2007, Majerus 1997 and Kettlewell 1973), the second is that of the mathematical theoreticians:<sup>183</sup>

Model 1: natural properties → fitness to environment → reproductive rates → changes in representation in the population → mutation/change environment → consequences for natural properties etc.

Model 2: reproductive rates → reproductive fitness → change in representation of types → mutation → reproductive rates etc.

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<sup>181</sup> Even then one would not succeed in finding a single scalar value for fitness as the mean and the variance of a statistical distribution are independent in the sense that they cannot be calculated from each other so that even if one would have consensus on taking variances into account the fitness would have to be expressed by a pair of numbers fitness (mean, variance).

<sup>182</sup> Krimbas 2004 tries to argue in principle against finding a single scalar measure of fitness by identifying three criteria that any definition fitness has to satisfy and then shows that these criteria are in contradiction with each other. Unfortunately, his choice of criteria (such as fitness over 1 generation, why not multiple?) are themselves arbitrary. Similar reasoning can be found in Ariew and Lewontin 2004.

<sup>183</sup> We slightly modified Ariew and Lewontin's models to make the evolutionary processes described more obviously iterative.

As is clear from the description of the models, the first includes (functional) natural properties, and these determine fitness as they are the “mediating natural properties” (Lewontin and Ariew 2004: 349). We will argue that these natural properties and their causal interactions with the environment are responsible for the non-circularity of the fitness definition in §1.4. We will follow Michod and Byerly (1991), whose work comes closest to our position.<sup>184</sup> Michod and Byerly distinguish in their 1991 paper adaptedness as the *sum* of all adaptive properties with which both had equated fitness in their earlier work [also called A-fitness] from the fitness of individual traits [also called F-fitness], thus a distinction between “general adaptedness versus specific adaptations” (Michod 1986: 290). Our own definition brings out the relation with individual traits even more strongly as it restricts the scope of fitness attributions to individual traits.

Consider first the following simple observations: animals with enzyme X are *fitter* than animals without it, because the former can digest many more types of food easily. This statement is empirically testable and uncontroversial; being fitter is related to a natural function at which one type of organism is better than others, and this advantage (in a complex, not easily quantifiable way) influences its replication chances. The fitness<sub>2</sub> definitions try to capture this intuition.

Byerly in his *earlier* 1986 article “fitness as function” writes:

$$f_i = (dN_i/dt)/(N_i) \text{ [F-fitness]} = F(N_i, A_i, E) \text{ [A-fitness]},$$

whereby  $f_i$  is the increase of a genotype  $i$  as a function of the number of organisms  $N$  with genotype  $i$ , the adaptive properties of the genotype  $A_i$ , and environmental variables  $E$  (see also Michod 1986’s definition of fitness by its “causal components”). In this mathematical function two definitions of fitness are equated: “I shall call fitness as relative rate of increase of a genotype F-fitness (after Fisher); fitness as representing the totality of intrinsic adaptive properties of organisms is designated A-fitness (adaptedness).” In Byerly’s definition, the original metaphorical sense of Darwin’s concept of fitness as a key fitting a lock comes back insofar as fitness is a function of the adaptive properties of organisms (the key) and the environment (the lock). “However, there is no simple property of organisms analogous to shape which corresponds to the sum of the contribution of adaptive characteristics of organisms to fitness” (*id.*, p. 497). The identification of adaptive characteristics is done by considering the “design properties of an organism”; that is why Burian (1983) calls A-fitness “engineering fitness”.

This idea of A-fitness is criticized for three main reasons: a) the distinction it postulates between organism and environment, b) the “myopic” character of design arguments (Rosenberg 1982), and c) its non-mathematical, variable, and complex nature.

**a) distinction between organism and environment:** it is common to insist that fitness is a relational concept, as we saw above. Fitness depends on environmental properties rather than simply on properties of the organism. However, the question is whether we can distinguish adaptive properties from the environment as the function suggests, because actual, realized adaptations depend strongly on for instance the right type of food’s being present in an environment (without certain food children may suffer from reduced brain capacities), etc. We would then seem to

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<sup>184</sup> See also the more empirical work by Lande and Arnold 1983 and Arnold and Wade 1984ab who quantify the selection gradient as relating the trait(s) and fitness. See also Rauscher 1992.

have:  $F(N_i, A_i(E), E)$  with the adaptive properties as an element of the environment and since animals change the environment (e.g., beehives, but also by just eating and exploiting resources etc.) we can also say that the environment is a function of the adaptive properties  $E(A_i)$ . Some see this as a problem<sup>185</sup>, since “we do not want the adaptive properties to be in turn relations between the organism and its environment” (Byerly 1986: 499), because “this would threaten the kind of problem Russell and Moore saw for absolute idealists: if to know the reference of the relata ‘a’ and ‘b’ of a relation  $R_{ab}$  we must first understand the relation  $R_{ab}$ , how can we state the relation as holding between definite entities.” However, we think this problem is not really a problem in practice as we can define (sometimes more easily than other times) the ‘original’ (more or less stable) adaptive properties  $A_{i0}$  and the ‘original’ environment (more or less stable)  $E_0$ . Rewriting the function  $F$  this would give us  $F(N_i, A_i(E + A_{i0}), E(A_i + E_0))$ . Of course the *realization* of an adaptive property  $A_i$  depends on the environment<sup>186</sup>, but part of the adaptive properties themselves are simply hard-coded in the genome of an animal and this part is not or almost not environment-dependent (at least for relatively long periods of time), just as the environment is changed by the adaptive properties of animals but not completely (the capacity of the sun is not changed although actual *access* to the sun can be limited), and this independent part of the environment is what we refer to by  $E_0$ . Although in some cases in practice it might be hard to distinguish  $A_{i0} + E_0$ , the new function answers Byerly’s worries which only hold if  $A_i + E$  are *completely* interdependent, instead of only partly, as seems reasonable.

**b) the “myopic” character of design arguments:** the use of the concept of design in evolutionary biology has been scrutinized by some philosophers who hold that i) ‘design’ is a term intimately connected to the history of natural theology, wrongfully implying an intentional plan behind evolution; others found the word a wrong analogy to human design since evolution proceeds by “tinkering” (Jacob 1977) instead of a plan, ii) and still others such as strong reductionists opposed any pseudo-functional terminology in the description of the causally-closed universe. These allegations have been answered effectively: i) the facts about complex design in the biological world that natural theologians, such as the famous William Paley (1802) used to argue for the existence of God, are exactly the facts Darwinism explains without God (Dawkins 1983). ii) Darwinians admit of course that natural and human design are different (although the latter also involves more tinkering in the form of trial-and-error-learning than one might think at first), but

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<sup>185</sup> Ollason has pursued this line of reasoning against the A-definition of fitness and tries to convince his reader to return to the Fisherian F-fitness seen as a property of animals: “The environment in which an animal evolves can be defined only with respect to the properties of the phenotype of the animal and that it is therefore not illuminating to try to explain the phenotypic properties of the animal in terms of adaptation to an environment that is defined by those very properties. Furthermore, since there is no way that the environment can be defined independently of the presence of an animal there is no way that the quality of an animal can be assessed [as A-fitness definitions try] I propose that fitness is nothing more than the production of offspring, that is a phenotypic property like the others (1991: 81).” Ollason’s work falls prey to all the objections to the fitness<sub>1</sub> definitions given above, including the charge of “circularity” which he instead blames upon the A-definition: “it is impossible to account for fitness [differential reproduction] in terms of the other phenotypic arguments except by circular argument.” We argue against the claim that  $A_i + E$  are interdependent by postulating  $F(N_i, A_i(E + A_{i0}), E(A_i + E_0))$  in which  $A_i + E$  are partly interdependent, but also partly independent. The charge of non-circularity is answered in §1.4.

<sup>186</sup> To overcome the problem of the environment-dependency of adaptive properties Michod suggests measuring adaptive capacities “in the presence of abundant resources (Michod 1986: 301)”, but then you are unable to measure adaptive properties for scarcity or non-homogeneity of resources and many adaptations are probably adaptations for these properties.



functional/design terminology should not worry reductionists if only they would not claim, like “greedy reductionists”, as Dennett (1995) calls them, that the difference between IBMs and Macs (two completely mechanical devices) are zeros and ones instead of functional criteria.<sup>187</sup>

However, in the discussion around the concept of fitness, design arguments (often in the form of optimality models, see the classical paper of Maynard Smith 1978a) are of a different kind: design arguments are said to be myopic insofar as they forget to calculate the costs for having a trait so that having a trait seems more worthwhile than it actually is, or they forget that the optimal design (from a human perspective) is not the design found in nature due to (developmental/historical evolutionary) constraints, or they forget that the actual design in nature can be the result of a compromise between multiple competing adaptations (for instance of muscles for strength, flexibility, cost of resources in developing and maintaining the trait, etc.).<sup>188</sup>

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<sup>187</sup> Dennett 1995 tries to explain functional terminology by postulating an “intentional stance”, a perspective from which the world can be described (subjectively?) as functional whereas the world itself is just mechanical. This intentional stance seems to have a lot in common with Kant’s position in his Third Critique in which he analyzes functional terminology (which is not an integral part of the Newtonian worldview on which he bases his analysis of perception and reason in the first critique) as a subjective synthesis of objective perceptions which is being projected upon them (not on the things themselves but on the already synthesized objects of perception, i.e., objects as they appear to us). The constitution and the actions of organisms are thus not truly functional (teleological), but only appear to our judgment as if they were such. Dennett calls this as-if intentionality, thus making the concept of design parasitic on the notion of intentionality (human or divine) which is denied to exist in its full-bloodedness. Dennett’s move is almost like explaining humans as non-divine beings and then denying that divine beings exist without telling exactly what the divine beings would look like if they were to exist.

We dislike the Kantian turn in explaining design, which is unnecessary if one understands Darwin correctly. Dawkins 1986 explains design as a property of objects themselves and not of our way of viewing them. He analyzes design as that which cannot be the result of chance all at once, that which is complex yet orderly. Dawkins says that when we find a perfect platina cube on a remote planet, we know that it is highly plausible that this cube was designed as no single natural process is known that creates those cubes. Just the same holds for animals: they are just too complex to be the product of chance. He then starts to argue that the only plausible process that can account for design in nature is natural selection and he compares the evolution of a Boeing 747 with that of a stretched DC-8: it is virtually impossible for a Boeing 747 to arise by chance from just the molecules present, but it is easy imaginable how a stretched DC-8 evolves out of the design of a normal DC-8 by small steps. Dawkins then argues against even the possibility of the competitors of natural selection such as saltationism, theistic (goal-directed) evolution and Lamarckianism, to account for design. Especially the latter claim, that Lamarckianism which posits the use and disuse of acquired characteristics, cannot account for design on the basis of its postulations is controversial; Dawkins says that the use and disuse are simply too crude and imprecise to account for the precise adaptations and that many of the acquired characteristics are bad or deleterious and that Lamarckianism thus lacks an account of how good characteristics are herited and bad characteristics are eliminated. I am sympathetic towards Dawkins’s account of the origin of design but am critical of three points: 1) Dawkins has not shown that natural selection is the only theory (of all possible theories) that can give a scientifically credible account of design, he has only shown that its current and past competitors cannot (similar logical point Sober 1996: 73); 2) Dawkins leaves out the possibility of other processes that can account for design that once evolved by natural selection but whose actual workings are nowadays no longer described well by orthodox Darwinism, think of pseudo-Lamarckian evolution of epigenetic settings in cells that evolved by natural selection and of human artifact creation on the basis of an (intentional) mind that was created by natural selection; 3) Dawkins explains natural design just by that which cannot be directly the product of chance. We will argue below that although this is an important element of design, it can better be analysed in terms of natural functions. Lewontin (1990) also criticizes Dawkins and the like by pointing out that there are (mathematically) complex things like waterfalls and highly orderly things like crystals that are not the result of adaptive selection.

<sup>188</sup> See for instance Rosenberg 1982: 461 “We can tell that a particular property – like mimicry, or disease resistance – makes for fitness by seeing that it contributes to optimum or satisfactory design. To recognize that a particular trait is suitable for attaining a certain end in a given environment we need not enquire into

Classical opponents of strong adaptationism (baptized by them as the Panglossian paradigm after master Pangloss from Voltaire's *Candide*, who held that our world is the best of all possible worlds) like Gould and Lewontin (1978) have criticized design arguments in even greater detail for holding that any *functional* trait of an organism is an adaptation and thus increases fitness (in our sense of fitness<sub>1</sub> of leading to more offspring and in the sense of fitness<sub>2</sub> insofar as not every *functional* trait is a *fitness*-increasing trait as the trait of nose-picking is a functional byproduct of using hands but not one which increases fitness). These objections against adaptations and design arguments, however, do not threaten our definition, but only strengthen it: as a critique of fitness<sub>1</sub> by stressing neutral drift and population effects it buttresses our observation that an increase in genes or other entities is not always due to an adaptation, and as a critique of *naïve* fitness<sub>2</sub> for uncoupling the concepts of function in general from adaptation (adaptive functions), since there are biological functions that are not the product of evolution. Below we discuss the concept of adaptation in more detail.

**c) The non-mathematical, variable, and complex nature of adaptations:** the qualitative (non-mathematical), variable, and complex nature of adaptations is said to render the definition either non-explicit or the instantiation/use of the definition case-dependent. For instance, Rosenberg arguing against functional definitions of fitness writes: “The theory of natural selection, however, neither can nor should take sides on which are the indefinitely many causally relevant features of a situation that determine the fitness of an organism in that situation (Rosenberg 1982: 271).”<sup>189</sup> And in similar vein Mills and Beatty write: “Since an organism's traits are obviously important in determining its fitness, it is tempting to suggest that fitness be defined entirely independently of survival and reproduction, as some function of traits.” Hull (1974) hints at the desirability of such a definition. This suggestion derives *prima facie* support from the fact that given such a definition, explanations of differential offspring contribution that appeal to differences in fitness are noncircular.

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reproductive success; instead we may employ theoretical or engineering information to identify alternative strategies for meeting a design-requirement, and to grade the efficiency of those alternatives in the light of environmental and organic constraints. Pursuing this approach enables us to identify some of the determinants of fitness in particular cases, and so grounds restricted judgments of comparative fitness on its causes instead of its effects. But design criteria are often myopic. They can easily lead to misidentifying the more fit as the less, and vice versa; thus a parasite more efficient at wasting its host may be wrongly identified as fitter than one which survives it just because the survivor is less efficient and therefore does not destroy its host. At best, employment of design criteria may enable us to make restricted qualitative non-fungible comparisons of fitness, with respect to a small number of properties, and a restricted class of environments. Optimum design cannot play the systematic quantitative role required of a general measure of fitness that the theory requires. Fitness levels must be measured in a coin common to many biological systems if it is to play its explanatory function in the theory of natural selection.” We agree with Rosenberg until his remark on the myopic character. The fact that he can identify the pros and cons of a trait makes clear that design arguments are not myopic, and some of the considerations one should take into account can even be thematised systematically as one should always look for fitness<sub>1</sub> effects on the first versus effects on multiple generations etc. Making the distinction between A-fitness and fitness of a trait solves Rosenberg's worry that only a limited number of traits can be used to assess the fitness<sub>1</sub> of an organism.

<sup>189</sup> See also Rosenberg 1983: 454 “[Definitions of fitness] will not take the form of universal general statements about fitness that are both true and manageably short. Because of the number and the heterogeneity of causal forces that determine fitness and because of our ignorance of the general theories that describe them and their interactions, we cannot expect anything like the interpretation of temperature, as mean kinetic energy, involved in the reduction of the ideal gas law to the kinetic theory,” and Rosenberg 1985: 163 “[the functional concept of fitness has] no manageably specifiable base in occurrent properties.” See also the previous footnote on the common coin in which fitness should be measured according to Rosenberg.

However, no one has seriously proposed such a definition, and it is easy to see why. The features of organisms that contribute to their survival and reproductive success are endlessly varied and context-dependent. What do the fittest germ, the fittest geranium, and the fittest chimpanzee have in common? It cannot be any concretely characterized physical property, given that one and the same physical trait can be helpful in one environment and harmful in another. This is not to say that it is impossible that some as yet unsuspected (no doubt abstractly characterized) feature of organisms may be found which correlates with reproductive success. Rather, it is just to say that we need not, and should not, wait for the discovery of such a feature in order to give the definition of “fitness” (Mills and Beatty 1979: 268f).

This point is well-taken; indeed there can be no single measure of fitness if one looks at functional and causal components, and indeed it is impossible to determine a total fitness score from all the individual adaptations and very hard to decide how big the increase in offspring is due to one single adaptation (although experiments with clones seem to make this experimentally testable). However, these observations do not forestall the possibility of fitness values (plural) for individual traits that are expressed in the “custom-made” physical quantity of relevance, such as energy consumed per use cycle of an organ, the amount of carbohydrates taken up into the bloodstream, the speed and accuracy of observing a predator etc.

If we take these criticism on A-fitness into account we can formulate a stronger fitness<sub>2</sub> definition. We propose to define fitness as follows: *fitness is fitness of a trait*. We define the expression ‘the trait A is fitter than B’ as ‘*the trait A confers upon its bearer(s) at level X a net heritable advantage expressible in trait-dependent ‘currencies’ usually given by the proximate mechanisms involved, an advantage which is ultimately thought to be related to the expected number of offspring or a representable measure thereof over a number of generations or within a fixed amount of time and in a ‘fixed’ abiotic and biotic environment, whereby the representable measure of offspring, the number of generations, or the timeperiod and the environment are chosen in a trait-dependent, relevant way.*’ Fitness is thus a relative measure which means that it only makes sense to speak of trait A being fitter than B and not of absolute fitness. As an abbreviating way of talking one may speak of the fitness of A, although this should always be understood as the fitness of A relative to B’s, C’s etc. This definition includes our view of multiple-level selection.

Explanations of the elements of this definition:

*its bearer at level X*: traits are traits of bearers (chromosomes, individual phenotypes, groups of organisms, etc.) at level X and selection acts on the level X, in conformity with the definition of Sober and Wilson used in multiple-level selection; note that the same *trait* can have different fitness values at different levels

*net advantage*: a trait can have benefits and costs; in a true fitness determination these should be taken into account. Sometimes the currency of the costs and the benefits differs: living with longer legs can mean eating 20% more leaves, against the cost of falling down 5% more often. The exact list of costs and benefits can be long or incomplete, just as a balance of a new research or engineering project only mentions the main costs and benefits; just as financial balances are nevertheless useful, so are design balances of evolutionary design. Nevertheless, just as in the case of making a balance of an engineering project, one should not be myopic and should look for hidden costs and shared benefits (co-adaptations).

*heritable advantage*: only advantages that are heritable contribute to fitness<sub>2</sub>, not accidental advantages that fitness<sub>1</sub> definitions would include. Since replicators are the true carriers of hereditary information, *replicators* are thus indirectly included in the definition of fitness<sub>2</sub>.

*trait-dependent currencies*: fitness as fitness of traits can only be measured on the basis of physical measures (the ‘currencies’) that characterize the costs and benefits of a new trait.

*proximate mechanisms*: scientific measures of the advantages and costs of traits can often be given in multiple ways: exclusive access to 5 extra trees, 20% extra leaves eaten, x % extra carbon intake, etc. The best measures are often those in more general physical terms.

*an advantage which is... relevant way*: this long qualification serves to indicate the relation of a trait to increased offspring. Often the exact relation a trait has in relation to offspring is unknown<sup>190</sup>, although in studies of clones in asexual organisms or sexual organisms that can be forced to replicate parthenogenetically one can measure it, and in such cases it is acceptable to express the fitness of a trait not only in terms of physical measures of the proximate mechanisms but also in terms of extra offspring. When it is unknown how a trait affects offspring rates (and also when they are known), one can express the advantage in terms of physical measures of the proximate mechanisms involved.

*ultimately related*: traits exist ultimately because of the reproductive advantages they confer on their bearers, ultimately in the sense of the ultimate mechanisms of Tinbergen (see §1.2).

*thought to be related*: *thought* because often it is impossible to give an exact quantitative measure of the number of extra offspring due to a certain trait, although an analysis of the proximate mechanisms gives one every reason to believe the trait is related to increased numbers of offspring.

*expected number*: expected to exclude chance just as the propensity definition; one calculates the expected number just classically from the *actual* reproductive success of types defined by the number of animals involved.

*number of offspring or a representable measure thereof (chosen in a trait-dependent relevant way)*: a *representable* measure to include a) *multiple* modes of replication, trait dependent because a trait can only influence one mode of replication (chosen in a trait-dependent relevant way), and to include b) sexual/asexual and atypical methods of replication that are dependent on the specific replication machinery behind the trait under discussion (think of heterozygous advantage/meiosis distorters etc.)

*a fixed number of generations or a fixed amount of time (chosen in a trait-dependent relevant way)*: some traits have advantages or costs over a number of generations (the grandchildless gene etc.) and some traits are time-dependent, such as early birth, spread of birth over life-time etc., and some traits are both (inclusive fitness effects). When one considers the trait itself or the mechanism behind it the measure one should use often lies at hand (time in case of early birth etc.). One should use both normal means and variances in the rare cases when trait fitnesses are measured in offspring numbers.

*‘fixed’ abiotic and biotic environment (chosen in a trait-dependent relevant way)*: fixed biotic environment includes stable relative and absolute number of cogenitors and predators/prey, again these are trait-dependent. By fixing these parameters, non-adaptive fitness effects (i.e., relatively more offspring on the basis of non-adaptive trait effect such as those based on density effects) are excluded along with frequency-dependent effects (of course the definition can explain why a rare type increases

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<sup>190</sup> The proponents of the A-fitness definition had this problem too, but they had the extra problem of finding a complete sum of all the effects of traits on replication rates; our definition is free from the problem of completeness.

*initially*, i.e., at the very moment the population is ‘fixed’). ‘fixed’ is between quotes to indicate that pseudostability such as present in periodically varying environments or constantly, randomly changing environments can be used as well (in their character they are fixed); again, when one considers the trait itself the environment one should use often lies at hand.

With this definition of fitness we can now easily formulate a definition of selection. Above, we already separated the completely non-Darwinian uses of the word selection from the proposed Darwinian concepts of selection<sub>1</sub> (selection as the process that operates when one type *systematically* leaves more offspring than another) and selection<sub>2</sub> the process that operates when the fitness<sub>2</sub> of some types of offspring increases [or is maintained].<sup>191</sup> It is the functional definition of fitness that adds the flesh to the bones of the word systematically in the selection<sub>1</sub> definition.

This basically suffices as a definition of fitness, but we will shortly compare our definition with those in the literature and elaborate a little bit on our definition, especially on the distinction between stabilizing and directional selection.

The definition of fitness: academic textbooks on evolutionary biology usually do not give an explicit definition of selection, just as they did not give definitions for replication and variation.<sup>192</sup> For more explicit definitions we have to turn to other sources, but full explicit definitions are scarce as most people tend to define fitness instead. The definitions that we find are often elliptic or include a reference to the two other fundamental concepts of Darwinism: replication and variation. We already discussed Sober’s definition of units of selection at multiple levels and included this element in our definition of fitness. The philosopher that comes closest to defining selection is David Hull who has spent the last two decades applying Darwinian theory outside of biology (see his 2001 *Science and selection: Essays on Biological Evolution and the Philosophy of Science* and Hull et al. 2001 and Hull 1988):

“Several authors have attempted to characterize selection in as brief a fashion as possible. For example, Campbell (1974b) describes selection as a function of blind variation and selective retention, while Plotkin (1982, p. 84) characterizes it as a matter of generation, testing and regeneration. The trouble with these characterizations is that they are too brief. If one wants to understand selection, a sentence or two, no matter how succinct, will not do. Understanding space and time requires more than looking up these terms in a dictionary or in a physics text. Similarly, anyone who wants a deep understanding of selection has to study this phenomenon. Just inspection of a brief characterization of the process will not do. This much being said, we define selection as *repeated cycles of replication, variation and environmental interaction so structured that environmental interaction causes replication to be differential*. The net effect is the evolution of lineages produced by this process. Each word in this definition needs careful explication. The message is not to be found in the preceding brief characterization of selection but in the ensuing discussion (Hull et al. 2001: 513).”

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<sup>191</sup> Sober 1984a: 184 distinguishes these as selection *of* (selection<sub>1</sub>) versus selection *for* (selection<sub>2</sub>).

<sup>192</sup> The definition is absent from both Ridley 2005 and Futuyma 2003, the two currently most widely used textbooks.

This beautiful definition of Hull's is almost an explanation of the whole logic of the process of evolution (natural selection); it is brief and dependent on the concepts of replication and variation, but so is our definition of fitness. The advantage of our definition of fitness is, however, that it includes an explanation of the phrase "so structured" that "differential reproduction" results by focusing on the *traits*. A further drawback of David Hull's definition is that it is only applicable to directional instead of stabilizing selection. Repairing these two problems gives us the following definition of selection as *repeated cycles of replication, variation, and environmental interaction so structured that environmental interaction causes the fitness of traits to be increased (and thus replication to be differential), or acquired fitness is maintained*. We do agree with Hull though that a true understanding of this definition requires a "study of this phenomenon" in its various aspects, to which we turn now.

Discussion of the concept of selection: Ridley does distinguish between a) directional selection that happens when (the extreme forms of) some trait are actively selected for, b) stabilizing selection "in nature the more common" (76), and c) drift, which is sometimes misnamed neutral selection.

a) A special case of directional selection is disruptive selection when a population is split into two extremes, when the extremes are each individually fitter for a certain task (such as getting a certain food) than the average, who can perform both tasks but with less perfection. In that case speciation can happen, the splitting of one species into two (see footnote 119).

b) Stabilizing selection is selection that weeds out deleterious mutants and averages out extremes (especially when sexual mixing is involved) so that organisms can be described as fixed types during short-to-medium time intervals over geological time. Since mutations will often arise recurrently (most of which are unmitigatedly bad) and since selection may not weed out deleterious mutations in one generation (especially when they are not that deleterious) a calculable equilibrium frequency that describes the amount of less fit genotypes present is reached when the speed of new mutations' occurring and the speed of the removal of these genotypes by selection is equal.

c) Drift or "neutral selection": the rediscoverer of Mendel's genetics at the beginning of the twentieth century, De Vries, along with many other early supporters of Mendelian genetics and other supporters of (discrete) saltationism, held that selection is but a minor force shaping organisms: it only serves to wipe out older species when a new favourable saltus (evolutionary jump) has occurred (see §1.1). The new synthesis of the 1930s and 1940s gave a prominent place to the force of selection. Three developments in the 1970s challenged the hegemony of the orthodox view on selection: i) the work of the molecular biologist Kimura *cum suis* (1968, 1983) on neutral drift in molecular evolution, ii) Gould and Lewontin's attack on the Panglossian paradigm, and iii) renewed ideas about catastrophism.

i) Kimura argued along three lines against natural selection: "1. the absolute rate of molecular evolution and degree of polymorphism, both of which have been argued to be too high to be explained by natural selection. 2. the constancy of molecular evolution, which has been argued to be inconsistent with natural selection [because the rate of morphological change during evolution, think of the evolution of a wing, fluctuates between fast (at first) and slow]. 3. The observation that functionally less constrained parts of molecules evolve at a higher rate, which has been argued to be the opposite of what the theory of natural selection would predict (Ridley 2005: 159)." All three observations against natural selection are false but started interesting research programs in biology on molecular evolution. It was found

that the exact number of molecular mutations (due to multiple processes that could account for molecular substitutions/changes) was hard to measure and usually lower than expected due to double counting; it was found that most substitutions involved changes in DNA that had no influence on the amino acids they coded for (synonymous changes) or substitutions that had little influence on the proteins they coded for (substitutions of amino acids for functionally less-constrained parts of molecules), and it was found that the molecular clock (the rate of nucleotide base substitutions) varies from species to species, although it is much more constant (especially when one corrects for the number of generations and the average time before they beget offspring) than morphological evolution. This is all perfectly sound from the perspective of natural selection, especially when one makes a distinction between replicators and their (morphological) locus of selection: the molecules of replicators (DNA) can vary a lot as long as this leads to non-morphological changes (i.e., DNA changes in non-coding sequences, synonymous changes, and changes in functionally less-constrained parts), natural selection is neutral against them and it is an empirical question how much of molecular evolution falls into this category; this drift, however, cannot completely explain morphological evolution, which requires molecular changes that lead to morphological changes upon which natural selection can act (see also Sober 2005b). Only in cases where a replicator and the locus of selection coincide (as in the case of non-coding DNA) can the term ‘drift’ or ‘neutral selection’ legitimately be used to name the evolutionary process observed. However, since some nucleotide bases are stronger than others, one could still call this *natural selection* for stability. Finally, one positive role drift can play in evolutionary theory is one Kimura did not mention: in *small* local populations where, due to inbreeding and size effects (bad) mutations accumulate, drift is a factor in explaining the dynamics of the population.

ii) Since the 1970s Richard Lewontin and Stephan Jay Gould have argued against pan-adaptationism<sup>193</sup>: the idea 1. that every element of an organism has been

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<sup>193</sup> This critique should be understood against the background of the view of a) the modern synthesis of the 1940s and b) gene selectionism in the 1970s. a) The modern synthesis emphasised and articulated the concept of adaptation with much more rigor than before, so that it could seem as if its supporters were pan-adaptationist. In Julian Huxley’s *The Modern Synthesis* (1942) we find for instance a chapter 8.1 called “the omnipresence of adaptation”. However, Huxley’s position has to be understood against the background of the discussion with physicists and positivists who denied the existence of any adaptation at all, or at least denied it a place in true scientific discourse. However, Huxley et al. would probably agree with Gould and Lewontin as they stress other factors than just adaptive selection in studying biological problems: “We next come to the origin of adaptations. It has been for some years fashion among certain schools of biological thought to decry the study or to deny even the fact of adaptation. Its alledged teleological flavour is supposed to debar it from orthodox scientific considerations, and its study is assumed to prevent the biologist from paying attention to his proper business of mechanistic analysis. Both these strictures are unjustified. It was one of the great merits of Darwin himself to show that the purposiveness of organic structure and function was apparent only. The teleology of adaptation is pseudo-teleology, capable of being accounted for on good mechanistic principles, without the intervention of purpose, conscious or subconscious, either on part of the organism or of any outside power. And to the second objection, the answer is that since adaptations are facts, it is the study of biologists to study them. If a biologist thinks that he has exhausted the study of a structure or a function merely by showing its adaptive advantage, he is a bad biologist; but so is he who thinks he has done so merely by giving a mechanistic account of its present condition and its embryological development. The truth is of course that every biological problem has its evolutionary as well as its immediate aspect, its functional meaning as well as its mechanistic basis; and both need to be studied” (chapter 8.1, 1963: 412). See also chapter 8.4 entitled “adaptation as a relative concept” and the examples in it. b) In some tenets of gene selectionism à la Dawkins natural selection became almost biological determinism as genes were postulated for almost any phenotypical trait, i.e., a God gene (Hamer 2004), a math gene (Devlin 2001), and a gay gene (Brookey 2001), which were

shaped by selection and, the other way round, that 2. selection is the only force that can account for the richness of biodiversity. The attack on pan-adaptationism resulted in their seminal 1979 paper on *The spandrels of San Marco and the Panglossian paradigm*, which soon became one of the most discussed papers in evolutionary biology. In their paper Gould and Lewontin argue that although the force of selection has shaped organisms in a meaningful way (a point sometimes forgotten when their paper is discussed), biologists should pay attention to alternative hypotheses about the origin of a trait, especially when earlier adaptivist hypotheses turn out to be wrong and biologists (despite playing lip service to those alternatives) fall into the habit of looking immediately for new functional adaptations.<sup>194</sup> The alternative, not primarily adaptivist hypotheses Gould and Lewontin propose comes in many forms<sup>195</sup>, but they can all be said to be about constraints: evolutionary (historical), genetic, developmental, and many others constraints.<sup>196</sup> An example: one could keep searching for a function of male nipples (and thus for adaptivist explanations), but one could also put one step back and entertain the hypothesis that it is a consequence of a common developmental plan behind males *and* females (for whom nipples clearly serve a function). The reactions have been mixed: from a more *general* perspective some have argued in a pseudo-Kantian fashion that although some elements of an organism might not be adaptive, in practice biologists should always look for a possible function behind a trait if they want to further the cause of their discipline, but that does not seem right since non-adaptive hypotheses can be fruitful too; others have argued that one of the advantages of Darwinism above creationism is indeed that it can not only explain design but also the *imperfections* of it (which explanations involving an omnipotent God cannot easily do, see Sober 1994 essay 8); still others have argued that only natural selection can explain adaptation and that although there might be constraints, they are constraints on *adaptations* and thus require Darwin's theory to be properly understood (Dawkins 1982, 1986, 1996) or that at least the case for constraints has been overstated (Rose and Lauder 1996); from a more *practical* perspective biologists have looked for ways to disentangle constraints from adaptations. Pigliucci and Kaplan (2000) give an overview of 20 years of discussion of the Gould/Lewontin paper and argue for a balance between taking constraints seriously and adaptationism. They point out that the fallacy of what they call "spandrelism" (taking constraints too seriously) (68) "is as easy to make as that of adaptationism" and point to a case where Gould saw a spandrel which, however, turned out to be a true adaptation. As a solution for distinguishing spandrels from adaptations they propose large empirical studies in which the fitness (of traits) is

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supposed to be individually selectable and shapeable by natural selection. We discussed gene selectionism in §1.21 and show Gould and Lewontin are partly right here.

<sup>194</sup> Other failed methodological practices of Panglossians are according to Gould and Lewontin that a) consistency between an observed trait and an explanatory story told in terms of natural selection is sufficient for the acceptance of the hypothesis that the trait is adaptive and evolved in just that way and b) that any failure of particular traits to be optimal is accounted for by evoking trade-offs with other traits. These failures together with the immediate search for new adaptivist stories when an older one fails make it hard to falsify the hypothesis that a trait is adaptive at all.

<sup>195</sup> Pigliucci and Kaplan 2000: 67 summarize 6 alternatives: no adaptation and no selection (i.e., drift), indirect selection (co-adaptation/linkage/pleiotropy), selection without adaptation (i.e., selection<sub>1</sub>/fitness<sub>1</sub> increasing without selection<sub>2</sub>/fitness<sub>2</sub> increasing), adaptation without selection (phenotypic plasticity and behavioural flexibility which is selected for, but not as adaptively instantiated), adaptation and selection (two traits equally fit), and finally adaptation and secondary selection (exaptation).

<sup>196</sup> See also Antonovics and Van Tienderen's 1991 paper: *Ontoecogenophyloconstraints? The chaos of constraint terminology*.



measured “under controlled and field conditions” and the mechanisms behind traits are unravelled. We too think this is the only way to disentangle them. Unfortunately, this will often be hard to perform. Biology is partly a historical science and it will be hard to assess (in paleontology for instance) whether something was an adaptation or a constraint-based trait. Multidisciplinary studies that focus on *proximate causes*, by comparing organisms with the same trait, by focusing on evolutionary ancestors and progeny, by building optimality models, etc., etc. are the only way forward here.

iii) catastrophism: as described in §1.1 Darwin formulated his theory on the basis of Leyell’s uniformitarianism. To describe the ‘evolution’ of a certain phenomenon (be it that of rocks or the traits of animals) meant for Darwin that one should only assume forces still in operation today and thus he excluded catastrophies (such as Noah’s flood) etc. that many of his contemporaries used to describe the evolution of life and to interpret the fossil record. However, the modern study of the fossil record recorded several mass extinctions, events in which the number of species decreases rapidly. Although these mass extinctions may be due to a sharp drop in the rate of speciation, it is usually assumed to be due to mass extinction. The most recent mass extinction, the Cretaceous–Tertiary extinction event, occurred 65 million years ago, and has attracted more attention than all others because it killed the dinosaurs. However, in the past 550 million years there have been four other major events when over 50% of animal species died (classical reference Raup and Sepkoski 1982). Speculations about the causes behind these extinctions vary from asteroid impacts, flood basalt events, sea level falls, periods of global warming and cooling, super nova or gamma ray bursts, continental drift events, spreads of radical new diseases, gas emissions, simple out-competition following an especially successful biological innovation, etc. (see Hallam’s 2005 monograph *Catastrophes and Lesser Calamities: The Causes of Mass Extinctions*). This is not the place to discuss these extinctions. It is only important to note that if only a handful of the speculations about the causes of mass extinction are true, the reason why a species of animal no longer exists is often not because natural selection disfavoured it, but because of *bad luck*, i.e., because it happened to be the subject of a disruption of the environment in which it lived.<sup>197</sup> Thus, one could conclude that natural selection is not a major force in shaping biodiversity. However, this is a fallacy. Darwin’s original proposal why species go *extinct* might need revision, but his theory is still the only valid account of why the species that went extinct *arose* in the first place.

The textbooks discussing selection further emphasize frequency-dependent selection, heterozygous advantages, gene linkage effects such as pleiotropy and hitchhiking, etc., just as we did above while discussing fitness. We now conclude our discussion of selection and turn to adaptation.

## Adaptations

We define adaptations ( $\text{adaptations}_{II}$ ) as fit traits that are the result of natural selection. We distinguish these from adaptations<sub>I</sub> which call adaptations any trait that increases

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<sup>197</sup> One can argue contrarily that those species that went extinct were simply not well adapted to the new environment. However, this is to stretch the notion of adaptation far too much, especially when one rules out the concept of a genuine pre-adaptation (as one should). One cannot become adapted to events that happen so rarely or possibly only once, unless one means adaptation in the sense of adaptation<sub>I</sub> as that which has caused (accidentally) survival. True adaptation<sub>2</sub> is an adaptation for something in a certain stable (or periodically varying etc.) environment, and Darwin’s theory explains how such adaptations can arise. One can, however, not become adapted by natural selection to phenomena which rarely or only once happen.

offspring (i.e., fitness<sub>1</sub>), and we refuse to call adaptations<sub>1</sub> true adaptations.<sup>198</sup> We will discuss the ways adaptations<sub>2</sub> are assessed empirically, which will be easier given our discussion of the Gould/Lewontin paper. Biologists distinguish at least three ways in which adaptations can be studied (Ridley 2005: 271): a) by engineering models, b) by (altering) experiments, and c) by comparative models. a) in engineering models one for instance uses hydrodynamics to study fish shape and construction engineering for shell thickness. This type of research can be carried out at all levels, from the simple and qualitative through to sophisticated algebraic modelling. They give exact predictions, but design arguments are as we saw often “myopic”: they forget the effects of co-adaptations, trade-offs between different traits, evolutionary constraints why the optimum has not been reached, and the fact that sometimes more than one design is equally optimal (Niklas 1997). b) by altering experiments: almost any hypothesis about adaptation will predict that some specified form of an organ will enable its bearer to survive better than some other form. By experiments sometimes these other forms can be created, after which the fitness of these altered organisms can be assessed: the fitness of animal coloration is often studied in this way. c) by the comparative method: by comparing different kinds of species one can sometimes predict the adaptations. For instance fruits and fruit eaters co-adapt: when you know something about the fruit, you can extrapolate something about the fruit eater. Ridley 2005: 273 gives the example of huge tropical fruits (far too big for current herbivores to be eaten) which suggests that they were probably eaten by large herbivores that went extinct recently. An example of a very systematic way of using comparative methods to study adaptations is allometry: the study about the relationship between size and shape, first outlined by Julian Huxley in his 1972 book *Problems of Relative Growth*, although some results of this science have been known since the measurements of comparative anatomy of the 19<sup>th</sup> century and the work of the biometricians at the beginning of the 20<sup>th</sup> century. In allometry one makes plots of one size parameter versus another of different samples.

In the study of adaptation the samples one uses for instance individuals of different species or individuals of different stages of the evolution of the species. The curve one finds can be informative about adaptation. For instance, when plotting body mass versus brain mass in various species one may find that some points *do not fit* on an otherwise linear curve one can expect selection pressures on having a larger brain. I.e., thus not whether a trait displays a positive, isometric, or negative allometric relationship to another trait is as such informative of adaptation, but only whether some points on the curve (i.e., some species) can be said to *deviate* from the observed relationship (if economies of scale have been taken into account): when one plots body mass versus brain size one expects to find that a bigger body mass brings along a bigger brain size, but this is only interesting when in some species the brain mass

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<sup>198</sup> When there is no selection in a population the Hardy-Weinberg equation can be used to calculate the offspring genotypes from the given genotypes of the parent generation. Sometimes, however, the equation is used the other way round: deviations of the Hardy-Weinberg law are regarded as evidence that selection is at work at a certain locus and thus whether a certain trait or genotype is adaptive or not (Ridley 2005: 103). This is a case in which adaptation is clearly understood as adaptation<sub>1</sub> with all the resulting problems; since the cause of differential survival is not known, chance and random effects are not taken into account, intergenerational and multiple generational effects are ignored, random mating and Mendelian segregation are assumed, etc. Moreover, the Hardy-Weinberg law only works for creatures that reproduce sexually, which makes the scope of its use for studying selection and adaptation even more limited.

increases more than one would expect on the basis of a cross-species comparison.<sup>199</sup> In practice, however, allometric research is complicated, even if one has perfect samples resembling the average individual of a species, because of the choice of species one has to make in doing cross-species allometric research. If one chooses too closely related species (donkeys, horses, zebra's etc.) one may not find deviations (and thus adaptations) as these cladistically related species share a common ancestor and many of their adaptations are due to this ancestor. One should thus include species in the comparison that do not share the common ancestor of the trait. Ideally, however, these species should not also be too far removed from the clades of the last common ancestor as a comparison of the dimensions of a whale and an insect does not make a lot of sense. Finding the balance between which species to include and which species to exclude is hard, especially when one does not have very clear ideas about when a certain adaptive trait arose.

A small complication for the study of adaptation is that the three methods mentioned above do not usually distinguish between adaptations and exaptations, to use the famous terminology of Gould and Vrba (1982). An adaptation is a character shaped by natural selection for current use, an exaptation is a "character, previously shaped by natural selection for a particular function (an adaptation), which is now co-opted for its current use (54)".<sup>200</sup> Examples of these exaptations are the feathers which were once used for heat control, later to fly and now for sexual display (as in peacocks which can hardly fly) and the bones which might have arisen as storage houses for phosphate (which serves vital functions in metabolic processes) and later became exapted for stability and support. The distinction between adaptations and exaptations is thus a historical one: adaptations were selected for their function and exaptations not: they once had a function of their own (or none) and are now used in a functional way. Gould and Vrba call the "operation of an adaptation its function", whereas the "operation not built by selection for its current role" is baptized "an effect". In practice, the distinction between exaptations and adaptations is weak: RNA-strings were once as a replicator both adapted at storing genetic information and being a catalytic agent: are eyes exaptations of photosynthetic cells, which in turn are exaptations of surface skin cells? Are legs exaptations of the fins of fish which in turn are exaptations of the bacterial flagellum? Or can we truly call all enzymes which still depend on the reproduction of RNA exaptations of the original replicators of the RNA-world? I.e., if we follow Gould and Vrba's definition are not almost all organs exaptations instead of adaptations? But that seems unwelcome because then our distinction between adaptation and exaptations becomes useless. To put the point the other way around: how long ago does the original function of an organ have to be lost to become a true adaptation? But this question seems unanswerable too. In evolution original elements can be completely restructured, so that it becomes almost impossible to identify the old organ from the new organ via all its transformations. We therefore propose is to call an exaptation *an adaptation too*, and to use the term

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<sup>199</sup> Another example: when the body mass increases so does the surface area of an animal (when its shape remains similar), but the variables do not increase proportionally. Take a cube of 1 m<sup>3</sup>. Enlarge all of its sides twice so that the object remains a cube. Its volume will have risen 8 times (from 1 to 8 m<sup>3</sup>), but its surface area 4 times (from 6 m<sup>2</sup> to 24 m<sup>2</sup>). In a log-log plot of volume versus area one will still find a linear curve. It is the deviations from the curve that matter for the study of adaptation.

<sup>200</sup> Actually, Gould and Vrba's terminology is a little bit more complex as they also want to use the term exaptation for a character whose origin cannot be ascribed to the direct action of natural selection, which is co-opted for a current use (exaptation as co-optation). We will tend to use the word exaptation in the sense used in the main body of the text, unless stated otherwise.

exaptation when an older adaptation is transformed into a new one or when a useless trait (a byproduct of developmental effects, for instance) is shaped by natural selection into an adaptation (thus to use the word 'exaptation' only chronologically when two adaptations/traits are compared). The examples that Gould and Vrba give justify this terminology as well. Feathers are not used from one moment to another as wings instead of heat protectors, this requires many generations of natural selection: because of that feathers as wings can be called adaptations of their own. What is important about exaptations in describing evolutionary processes, though, is that when one tries to explain a complex design one should find intermediate stages and exaptations often provide them. The discussion around the concepts of adaptation and exaptation can be clarified further by considering the concept of function.

### **Function**

All three methods (engineering models, altering experiments, comparative models) presuppose that one can identify something as a genuine adaptation. But how to recognize an adaptation in the first place? We already discussed Dennett's proposal of an intentional stance (which leaves unexplained how and why we can take this stance and what in nature itself the adaptations correspond to that we are supposed to project upon them) and Dawkins's proposal of complex orderly things that cannot be the result of chance all at once, which did give us necessary, but not sufficient criteria to recognize adaptations/functions as there are complex, orderly things such as crystals which are not adaptations like the vertebrate eye clearly is (see footnote 187). But what does give us necessary and sufficient conditions or at least most of the sufficient conditions?<sup>201</sup> We think that Dawkins should have added that an adaptation exists for the (reproductive) benefit of its bearer (see also Sober 1996: 96), i.e., that an adaptation has a *function* as we just pointed out in our discussion of adaptations. But observing functions may be just as hard as observing adaptations. Hence, what are functions?

We will not pursue this question from the most general perspective where its multiple uses in math, computer science, physiology, human artifact creation, etc. will probably turn out to be a family resemblance concept like many fundamental concepts that are used in multiple domains. We will only look at the concept of function in *natural science*, or more specifically in *biology*, and our goal is that our definition provides us with a means to identify functions. Even this is already a hard task as a recent anthology edited by Ariew et al. (2002) entitled *Functions – new essays in philosophy of psychology and biology* contains more than 400 pages devoted to this topic. We will concentrate on the consensus view present in this volume and other work on functions, putting details aside. This means that although a lot of the literature on function in philosophy seeks to find a notion of function that includes both the functionality of *psychological* intentions and conscious states (from a

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<sup>201</sup> A lot of the literature on functions seeks necessary and sufficient conditions for calling something a function and its proponents place themselves in the tradition of conceptual analysis. For instance Wright 1976: 97, Boorse 1976: 74, Bigelow and Pargetter 1987: 188 each argue against an account of biological function that presupposes Darwin's theory on the grounds that Harvey did not know about it as he discovered the function of the heart (see Millikan 1989: 290). For reasons outlined in §3.1. We do not believe in the project of conceptual analysis and we do not believe that one is likely to find necessary and sufficient conditions for THE concept of (natural) function. We only want to give a definition of function that Darwin's theory can use and that can be extended to uses of Darwin's theory outside of biology.

naturalized perspective) and natural dispositions, we will only concentrate on the latter.<sup>202</sup>

Definitions of the concept of function from the perspective of natural science ran “at least six lines long and four variable deep (Godfrey-Smith 1993: 197)” in the era of the classical philosophy of science of the fifties and sixties and their view of explanation, which had a hard time fitting the concept of function into the reductionistic framework of the D-N model or similar models that took physics as their paradigm. The debate around the concept of function was strongly restructured in the seventies by the seminal paper of Larry Wright (1973) simply entitled *Functions*, followed two years later by Michael Cummins’s almost equally influential paper *Functional analysis* (1975). These papers should both be seen against the background of a more liberal view on theories according to which the strength of theories in explaining events is judged relative to other theories (see §.2.3) and only requires that an explanation cites (statistical) factors that make alternatives less plausible instead of ruling out *all* alternatives (which is virtually impossible). Both Wright’s and Cummins’s papers are remarkable for the shortness of their definition, but unfortunately they hold very different views on function; in a lot of later work on functions the definition grew longer again as people tried to criticize and revise one of two definitions, thus pulling the balance in one or the other direction (for instance Bigelow and Pagetter 1987, Millikan 1989), or trying to reconcile the two views of function from a higher perspective (Kitcher 1993 with his concept of *design*), or as they argued both definitions are valid but that there “is no strong reason for using the same word (Godfrey-Smith 1993: 207).” We will argue that the two definitions are compatible and that if one takes the framework of the Darwinian theory there will be no confusion.

Wright (1976: 81) defined function as follows (letters changed for consistency): “The function of X is Y *means* (a) X is there because it does Y and (b) X is a consequence (or a result) of Y’s being there,” whereas Cummins defined function as: “The function of X in *s* [a system] is to Y relative to an analytical account A of *s*’s capacity to Z just in case X is cable of Y-ing in *s* and A appropriately and adequately accounts for *s*’s capacity to Z by, in part, appealing to the capacity of *x* to Y in *s* (1975: 762).” We will discuss these two concepts of function first, before we give our own definition based on this discussion.

i) The Wright definition of function is too general. Godfrey-Smith, modifying some examples of Boorse (1976) on anorganic nature, asks us to imagine a small rock holding up a larger rock in a fast-moving stream. This small rock would be washed away if it did not uphold the larger rock. Hence, the small rock is there (at its present location) because it holds up the bigger rock, whereas the bigger rock is there as the result of the small rock’s being there. However, nobody would want to say that the *function* of the small stone is to uphold the larger stone; thus Wright’s definition does not fit the use of the word ‘function’. One can remedy this situation by restricting functional ascription to biological entities, but then the scope of Wright’s definition which was thought to apply to all sorts of functions (the function of the police is to catch criminals etc.) is lost and just restricting the definition to

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<sup>202</sup> For a discussion of intentionality and its role in language use, see §3.13. Intentionality depends on having a mind; minds have evolved for a function during natural selection. Thus the *existence* of intentionality depends on natural functionality and not the other way round. Intentionality does complicate the definition of functionality in general, though, as humans can intentionally use the normal function of something in an a-functional way.

biological entities would not do as being part of a biological system does not make something functional: replace the example of the little rock by a small blood prop which upholds a larger blood prop in the blood stream: we would not call the function of the small blood prop upholding the larger one. The restriction to certain domains only helps if the restriction has something to do with what functions are. This is exactly what Millikan's (1984) elegant restriction of Wrightian-like functions does, as she restricts it to entities that exist in replication lineages and upon which a selection force acts.<sup>203</sup> Roughly speaking, she defines the function of something as whatever past tokens of that reproductively-defined type did (and were explicitly selected for) that explains the existence of past tokens. This definition can be used outside of biology when we find replication lineages and selection forces, so although Millikan's definition restricts functional ascriptions in general (and maybe even so strongly that normal functional ascriptions as in human artifact production when there is no or hardly any lineages of artifacts), it does not restrict it as strongly as the restriction to biological systems *sui generis*. This sort of definition has two problems, though: a) (a general problem) the concepts of selection and adaptation rely on the notion of function, whereas function in its turn depends on selection, b) (a more specific problem) this definition is backwards-looking: functions are functions of what has proved to be functions in the *past* and seems to miss either current or future uses.

Regarding a), the interdependency need not be a problem, just as a dictionary is a book full of interdependent references but is still useful in explaining how the concepts relate and the words do not turn out to be completely synonymous. When "rowing" is defined as "activity that a rower does" and a "rower" as "someone who rows" we still have learned something, namely that rowing is an activity done by humans etc. (i.e., compare "raining" which cannot be defined this way). Of course the definition becomes more useful as "rowing" is described as "using the oars of a boat to move through the water", but for some fundamental concepts such an explicit definition is impossible and then interdependent, yet not completely synonymous, definitions can be helpful.

Regarding b), relating function to past selection can lead to misinterpretation of b1) exaptations (in our sense of the word defined above) and b2) ad-hoc functional uses of organs. We show, however, that this is not the case. b1) exaptations were once selected for a different function than they serve today (e.g., feathers for heating instead of flying); however, to become an exaptation new selection processes were required that shaped the previous adaptation into a new one. Some have proposed to solve the backward-looking character of selection in ascribing functions to entities by a forward-looking account such as the account of Bigelow and Pargetter (1987) in which functions are derived from *present propensities* to succeed under selection. However, by admitting the difference between

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<sup>203</sup> The standard counterexample to this idea is that of the accidental doubles. Take an object that evolved by natural selection and has functional traits with a double of it, molecule for molecule exactly the same, brought into existence "through a cosmic accident resulting in the sudden spontaneous convergence of molecules, which, until a moment ago, had been scattered about in random motion (Millikan 1989: 292)." This double would not have functional traits according to the restrictive definition as they have not evolved through natural selection in reproductive lineages. However, such cosmic accidents are just secular names for Gods and indeed if we allow Gods to exist our ideas about functions would have to be reconsidered. Since all we want to do is to give a definition of function on the basis of science, which just rules out such accidents just as spontaneous DC-8 creations (see footnote 187), we can forego a discussion of this counterexample.

exaptations and adaptations and by restricting the time at which selection acted to *recent* episodes, one need not change the structure of evolutionary theory as radically as Bigelow and Pagetter want with their pre-adaptations.<sup>204</sup> b2) ad hoc functional uses: a dog can use its teeth to pick up something that blocks its path and thus picking things up and removing barriers (which is done regularly) seems to be the function of teeth; however it seems that a distinction between the “accidental” use and the “proper use” (Millikan 1989) of teeth such as grinding can solve this problem if we restrict *true* functional ascriptions to *proper* functions. However, how can we distinguish between accidental and proper uses? By frequency analysis? How often are teeth used to grind food and how often for picking up things? But what if we find that a dog uses its teeth to eat for 10 minutes a day, whereas it plays by picking up things for an hour a day? We think one can distinguish these functions by considering what a function ultimately helps *on average*<sup>205</sup> *in the process or system of replication*. Since the proximate mechanism of a functional trait to replication can in principle be investigated this makes functional ascriptions *objective* ascriptions.<sup>206</sup>

ii) Cummins’s definitions: Cummins’s definition looks complex but is not if we look at the example of a heart which functions by pumping blood through the veins. In this case we define the system ‘s’, the pulmonary circulation system which is itself part of the larger cardiovascular system and whose function Z is to carry oxygen-depleted blood away from the heart, to the lungs, and return oxygenated blood back to the heart. This system consists *inter alia* of the veins, the arteries, the lungs, and the heart. The function Y of the heart X is to pump blood through the veins and this function should be seen as part of the function Z of the system s to enrich the blood with oxygen. So far so good. But just as in the case of Wright’s definition it seems we have to restrict the definition. Think again of the system s of the small rock upholding the large rock, which blocks the stream. We could say on the basis of this definition that the function Z of this combined system s is to block the stream, and that the function Y of the small rock X is to uphold the large rock within this system. We could restrict this definition also to biological systems or to entities that evolved under the pressure of a force of selection on reproductive lineages. However, this feels more natural in the case of Wright’s definition as it

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<sup>204</sup> In our view present propensities are just another word for the problematic concept of pre-adaptations. Adaptations can only be identified as pre-adaptations from a backward looking perspective and thus one does not really need this concept to describe current evolution; exaptations suffice to characterize what has happened to older adaptations reshaped by selection.

<sup>205</sup> On average: in accidental cases picking up things with its teeth can save a dog’s life, for instance, when it is by accident trapped. However, it is clear that on average the function of grinding food is much more important for survival and reproduction than picking up things.

<sup>206</sup> Objective ascriptions of function have been dubious since the critique of mechanistic philosophy on teleological thinking (see also footnote 193). A (classical) teleological analysis of function saw functions as dispositions (or potentialities) for realizing a certain goal, a goal which was considered to exist somehow prior than the potentiality (as something more objective or more real than the potentiality) and as such it was held to be responsible for its own fulfillment. This a- or anti-chronological account of function is not the concept of function which we use when we speak of objective functional ascriptions. The science of cybernetics has shown how a mechanical program can give rise to functional, goal-directed behaviour in the agent who carries out the program, without violating chronology. This behaviour is an objective property of the agents behaviour. If DNA is seen as the program of a phenotype (as biocybernetics does), the functional behaviour and the functional traits of an organism (for which the DNA codes) can be seen as objective functions (in the sense cybernetics uses the word ‘function’). But even apart from the perspective cybernetics offers can it be seen that biological functions in the sense described above are objective properties that do not violate chronology: there are big chronological cycles of replication (as in the sense of **e**) on genes) and in these cycles traits develop that have a objective function in this chronological process of replication.

includes an existential why already that Cummins's ahistorical definition does not. Nevertheless, though we may include a restriction to entities that evolved under the pressure of a force of selection on reproductive lineages, this does not make Cummins's and Wright's definitions equivalent. Cummins's definition can encompass two cases Wright cannot very easily deal with: a) the case of unselected parts in larger functional systems that have been selected for, such as the bones in the skeleton which provide it with stability, and b) the case in which something from the environment is taken by an organism to function in a certain way. a) the skeleton system  $s$  exists to realize a certain morphological form with certain degrees of freedom (i.e., the function  $Z$ ), within this skeleton the bones  $X$  provide the *function* of stability  $Y$ , but bones may never have been selected for this stabilizing function explicitly.<sup>207</sup> b) take the case of a small stone again, this time not in a river but on a table to hold the paper in place, this anorganic stone thus functions as paper holder (in the system of the stone and the paper<sup>208</sup>); however, the stone has not been selected for this task. Again one could think of a playing child who builds a dam into a river with a small and a large rock in case of which the small stone does function as upholder of the larger stone. However, these two cases seem to depend on human or psychological intentionality, which we excluded from our discussion from the beginning. Let's thus look at non-human forms of artifact use or "animal architecture"<sup>209</sup> as the cases of extended phenotypes of organisms that almost have no mind, such as bees. Bees can use parts of the environment to build hives, for instance comb hives, but also natural places that fit the size of a hive, such as holes etc. Then the hole functions *as* a home and its earth *as* building ground for the hive. In these cases we have functions that are not the result of selection; of course the hive-building disposition and the shape of the hive through this disposition have been selected for, but the actual hive itself not (since it varies too much from case to case to speak of true lineages, see i) extended phenotypes). But the functionality can be perfectly understood from the perspective of Cummins's definition: the bees and the beehives form a system  $s$  (with the function  $Z$  of reproducing) in which the part beehive  $X$  has the function  $Y$  of being their home.

Again we must stress that Cummins's definition works only when there is one element involved that has been selected by natural selection. This can either be: i) a higher system  $s'$  (or  $s''$  or  $s'''$  etc.) encompassing  $s$ , ii) the system  $s$  as a whole, or iii) an element of system  $s$ , as will be clear from examples. In reversed order: iii) the bees in the system of the bees with their hives (and not the hives), ii) the skeleton system  $s$  as a whole but not the bones in them, iii) or the skeleton system  $s'$  for the bone system as a whole  $s$  which has not been selected for and whose parts or components (the molecular combination) may also not have been selected. Thus, in Cummian functional ascriptions we either have a system as a whole that has been selected or parts of it. Ultimately, the system of a whole in biology is usually the system of replication (or the more encompassing system of the system of replication *and* its interactions with the environment and things in the environment) that is the

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<sup>207</sup> This is just an example which need not be true. Bones function for many different purposes in our body, some of which are clear exaptations. These functions are: protection (i.e., the skull protecting the brain), shape (morphology), movement (manipulation of the body in three dimensions), blood production, acid base balance and sound transduction (in hearing). It is speculated that all bone functions are exaptations of their original function of mineral storage (which bones also still have), and sound transduction is again an exaptation on bones as protection and shape.

<sup>208</sup> Or better, as we shall say, as the system of "paper/stone + humans" who want to organize their stuff.

<sup>209</sup> For splendid examples, see Gould and Gould 2007. See also Hansell 2007 and Scott Turner 2002.



basis of functional ascriptions. As a positive heuristic we find again that we can often<sup>210</sup> identify the function of parts by looking to the relation they bear to the system of replication.

In sum: in a world without organisms or other entities that arise by natural selection we do not need functional ascriptions, but when these exist we need such ascriptions as selection on fitness means selection on functional traits. The function of parts can often be identified by looking at the role they ultimately or indirectly play in the process of replication. Both Wright's and Cummins's definitions need to be restricted to cases in which an entity that reproduces is involved which exists in replication lineages and upon which a selection force acts. The Wright and the Cummins definition take a different focus: Wright's definition works for the entities that have been the object of selection itself; Cummins's definition also works for entities that have not been as such the object of selection but are either used by entities like this or play a functional role in systems that have been the object of selection. Thus, both definition are needed in Darwinian context, but need to be modified as described above.

We will always need to see whether the traits of replicators outside of biology are *functional* traits in the sense that they ultimately help their own replication.

## §1.3 Criticisms of Darwinian explanations and an assessment of their relative strength

*In this section we will briefly look at three criticisms of Darwinism in general: the critique that Darwinism is tautologous, untestable, and not being able to provide future predictions; this discussion paves the way for the discussion of the strength of Darwinian explanations, both in its general form and especially regarding its specific instances. Important criteria to assess the strength of an explanation are its scope, completeness, precision, and how much better or worse it does than its competitors in the contrast class of alternative theories. We will see that the strength of a Darwinian explanation depends strongly on the identification of proximate mechanisms.*

### Three criticisms

We first look at some general criticisms of Darwinism. We already encountered some forms of criticism in our Whig history of Darwinism in §1.1 when we considered the reasons why Darwinism did not become mainstream until the 1930s, almost 75 years after the publication of the *Origin*. Here we concentrate on three big criticisms: a)

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<sup>210</sup> Often and not always: consider the case of a system comprised of a man and a gallow. If the man uses the gallows for suicide, this function has of course nothing to do with replication. However, this is a case in which intentionality seems to play a role and we excluded that beforehand. It would be different if we found cases of functional uses (without intentionality) that had ultimately nothing to do with replication. Of course one can think of malfunctions which do not help organisms in replicating and in which something is used a-functionally (i.e., functionally but wrongly); but malfunctions can be handled within the framework of Wright and Cummins (see also Godfrey-Smith 1993: 211) as they are malfunctions of functions that are ultimately related to replication. The same holds for maladaptations, these are maladaptations of adaptations and would not exist if it were not for the adaptations from which they stem.

Darwinism is said to be tautologous; b) Darwinism is said to be unable to make future predictions; and c) Darwinism is said to be untestable.

**a) the tautologous nature of Darwinian explanations:** two or three decades ago a vehement debate existed around the question whether Darwinian explanations are tautological and thus devoid of explanatory power. Smart (1963), Manser (1965), Popper (1974), Peters (1976), Tuljapurkar and Orzack (1980), and Jongeling (1985) claimed this. Their claims were again and again immediately countered by biologists and philosophers of biology, such as Ferguson (1976), Caplan (1977), Castrodeza (1977), Stebbins (1977), Ruse (1982), Stearns and Schmid-Hempel (1987), Henle (1991) and Krimbas (2004). To understand this debate we have to distinguish between the different senses in which the word ‘tautology’ is used. After that we will be able to rebut the different objections to Darwinism that go under the heading of “the tautologous nature of Darwinian explanation”. Our discussion is an extension of Caplan (1977), Castrodeza (1977), and Sober (1984a), who also point out that numerous senses of the word ‘tautology’ are confused in the debate.

‘Tautology’ is a linguistic and philosophical term. We shortly discuss some of the literature on tautologies i) to come to our own definition of a tautology, ii) to better understand the senses in which the word ‘tautology’ is used in the debate around the nature of Darwinian explanations, and iii) because this discussion will be of use in chapter 3 on language. Traditionally, tautologies are equated with analytical sentences. The notion of an analytical sentence was formally introduced by Immanuel Kant (1781/7) in his *Critique of Pure Reason* (A6-7/B10-11), where he defined it as a proposition whose predicate concept is contained in its subject concept (A6-7/B10-11). This contrasts with a synthetic sentence: a proposition whose predicate concept is not contained in its subject concept. Examples that Kant gives include “bachelors are unmarried” (analytic and thus tautologous) versus “bachelors are happy” (synthetic).

This simple distinction is complicated in Kant’s own philosophy and that of his successor by the fact that he crosses the analytic/synthetic distinction with another one: the a priori/a posteriori distinction. According to Kant, a priori propositions do not rely on experience, while a posteriori propositions do rely on experience. These cross-cutting distinctions yield four possibilities: 1) analytic a priori propositions, 2) analytic a posteriori propositions, 3) synthetic a priori propositions, and 4) synthetic a posteriori propositions. As is well known, Kant’s philosophy allowed for the possibilities 1) and 4), as well as the controversial possibility 3). Not all philosophers followed him in this; Hume, for instance, only allows for possibility 1) and 4).<sup>211</sup> The details of their philosophies need not worry us here.

What is important is that the simple Kantian analytic/synthetic distinction on the basis of which tautologies were understood, was seriously complicated in the twentieth century, partly on the basis of the other Kantian distinction between a priori and a posteriori propositions. In 1951, Quine wrote his highly influential paper “Two dogmas of empiricism”. Historically, this paper was a critique of Carnap’s *Meaning and Necessity* and other currents in logical positivism, in whose philosophy a Neo-Kantian distinction between analytic and synthetic propositions played a fundamental role.

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<sup>211</sup> This is of course an anachronistic observation as Hume’s work is older than Kant’s work.

The definitions logical positivists provided of an analytic proposition vary. They defined it as a proposition whose truth depends solely on the meaning of its terms, a proposition that is true by definition, or as a proposition that is made true solely by the conventions of language. Synthetic propositions were then defined as: propositions that are not analytic. These definitions applied to all propositions, regardless of whether they were of subject-predicate form. Thus, under these definitions, the proposition 'It is raining or it is not raining,' was classified as analytic since it is true under all valuations (or truth-assignments or interpretations) of the proposition 'it is raining', while under Kant's definitions this proposition was neither analytic nor synthetic. On the basis of truth assignments even *whole logical arguments could be considered analytic* (written down as a material conditional with the conjunction of all the premises as antecedent): " 'if it rains then the streets become wet [is true]' and 'it rains [is true]', then [thus] 'the streets become wet [is true]' " were called analytic (i.e., in fact *all* statements that can be deduced by logical deduction were called tautological and analytic truths by positivists such as by Wittgenstein in his *Tractatus*).

The logical positivists further denied the existence of the category of synthetic a priori propositions, in which Kant had placed (true) mathematical and metaphysical propositions. The logical positivists denied the existence of meaningful metaphysical propositions and they classified mathematical propositions such as '7 + 5 = 12' as analytic (see for instance Ayer 1946, 1930). These mathematical propositions were thus treated on a par with definitional propositions like 'bachelors are unmarried men' and with other linguistic propositions that can be seen as derivative from definitional propositions, such as 'a bachelor is unmarried' (i.e., the exemplary cases of analytical subject-predicate sentences).

Although the logical positivists believed that the only *necessarily* true propositions were analytic (and all others *contingent*), they did not define 'analytic proposition' as 'necessarily true proposition' or 'proposition that is true in all possible worlds'. Sometimes the *linguistic* notion of analyticity was further explained by the *epistemological* notion of a *prioricity*: analytic propositions are propositions that are irrefutably true since they cannot be refuted by a posteriori empirical discoveries (i.e., they are a priori true since one only needs to examine one's own concepts to know that they are true). Because of that, the notions of a priori and analytic were sometimes conflated (which was an easy thing to do, especially given the fact that the positivists denied that synthetic a priori propositions existed and thus the extensions of the concepts 'analytic' and 'a priori' were the same for them.)

Quine denied that the logical positivists could maintain a clear-cut analytic/synthetic distinction, and that this distinction in general and the semantic reductionism it presupposed were untenable. Quine argues that definitions of analytic and synthetic statements are circular in the sense that the concept 'analytic statement' is defined in terms of 'synthetic statement' or the other way round without providing any independent means to assess the meaning of these terms. Consider again the two statements we already used: 'a bachelor is unmarried' versus 'a bachelor is happy'. How are we to distinguish the former statement as analytic? One might opt for synonymy between 'being a bachelor' and 'being unmarried', but according to Quine the notion of synonymy needs just as much elucidation as that of analyticity; moreover, Quine denies that words are exact synonymous because, for instance, the pope is *unmarried* but it sounds strange to call him a bachelor. Another proposal might be a definitional account, such as given in dictionaries where a bachelor is defined as an unmarried man. However, a dictionary or a definition again presupposes that one

already knows what analyticity is. Finally, one might opt for ‘interchangeability’ (*salve veritate*) of the two concepts. A good test is sentences with the modal word ‘necessarily’. It seems we can distinguish the analytic statement ‘a bachelor is unmarried’ from ‘a creature with a heart is a creature with a kidney’ by comparing ‘necessarily, all bachelors are unmarried men’, which is true, from ‘necessarily, a creature with a heart is a creature with a kidney’, which is not true. However, to understand the concept of necessity we need to appeal back to the concept of analyticity and thus according to Quine we still have no independent means to assess the distinction between analytic and synthetic.

After that Quine turns to the second dogma, that of reductionism and the the logical positivists’ verification theory of meaning, according to which a statement is analytic if it can be shown to be synonymous with (with the same method of verification as) a logical truth. The problem that naturally follows is: how are statements to be verified? An empiricist would say that it can only be done using empirical evidence; hence, only synthetic statements can be verified (which meant in positivist terms that they would be reduced to the elementary sense-data language). However, Quine questions this reductionist theory with his own semantic holism, according to which scientific theories form patch-work with more-central and more-peripheral parts in which all parts (even logic, mathematic given quantum-logic, intuitionist mathematics etc.) can in principle be revised. Because all statements of the patch-work are revisable there is no means to single out the synthetic statements and there is still no way to assess the analytic/synthetic distinction according to Quine. The upshot, he claims, is that we should drop the distinction altogether.

Although Quine’s precise arguments and their status are contested,<sup>212</sup> on their basis the rejection of the analytic-synthetic distinction is “still widely accepted among philosophers, even if for poor reasons” (Boghossian 1996). This complicates the traditional definition of a tautology since that was based on analyticity.

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<sup>212</sup> An early critique was by Grice and Strawson 1956. Among other things, they argue that Quine’s skepticism about synonyms leads to a skepticism about meaning. If statements can have meanings, then it would make sense to ask ‘What does it mean?’ If it makes sense to ask ‘What does it mean?’, then synonymy can be defined as follows: Two sentences are synonymous if and only if the true answer of the question ‘What does it mean?’ asked of one of them is the true answer to the same question asked of the other. They also draw the conclusion that discussion about correct or incorrect translations would be impossible given Quine’s argument. Four years after Grice and Strawson published their paper, Quine’s book *Word and Object* was released (Quine 1960). In the book Quine took the bait and presented his theory of indeterminacy of translation. In §4.24 on child language acquisition we come back to the problem of the indeterminacy of translation and show that it arises from a bad theoretic perspective on meaning and meaning attribution, and that there is no such problem in practice.

In another famous paper entitled ‘Two Dogmas revisited’ Hilary Putnam argues that in attacking the two dogmas Quine is attacking two different notions of analyticity. Analytic truth defined as a true statement derivable from a tautology by putting synonyms for synonyms is close to Kant’s account of analytic truth, as a truth whose negation is a contradiction (first dogma). Analytic truth defined as a truth confirmed no matter what, however, is closer to one of the traditional accounts of a priority (second dogma). Putnam does maintain that Quine is right in rejecting the existence of analytic a priori truths. On the basis of the work of Kripke Putnam points out three distinctions: a) the epistemological distinction between apriori and a posteriori, b) the logical distinction between necessary and contingent, and c) the linguistic distinction between analytic and synthetic. According to Kripke’s and Putnam’s controversial possible world semantics there are necessary a posteriori analytic statements such as ‘Water is H<sub>2</sub>O’ (i.e., purified water), which is said to be (logically) true in all possible worlds by virtue of its meaning (analytic), since no empirical discovery can be done that refutes this claim (since we supposedly do not call any stuff water unless it is H<sub>2</sub>O, regardless what other characteristics it has, although nevertheless we once empirically *discovered* that water is H<sub>2</sub>O (a posteriori)). As such the analyticity of a proposition, although revisable, rests on definitional accounts.

We will here presuppose (like common sense) that there exists such a thing as synonymy (as between 'terrible' and 'awful'), although two words are perhaps never used in exactly the same way. We presuppose this because people can easily give synonyms for words and because different people judge relatively the same on questions which words are synonyms. When synonyms exist, it is a small step to acknowledge the existence of pleonasm such as 'white snow' or 'to binge a lot' in which an adjective or adverb is superfluously used. We now define a tautology as repetition of meaning, using different words that effectively say the same thing twice. Usually definitions of tautology include the remark that these repetitions are unnecessary (and usually unintentional), but we look here only formally at what tautologies are, and we are not interested in the possible stylistic or explicative function tautologies can serve. Examples of tautologies as defined here are definitions, sentences in which synonyms are equated, and pleonastic phrases. Whole arguments can also be called tautologous by extension when they are (semantically) *uninformative*. Examples are the *logical* pseudo-statements such as: 'it is the case that it is raining or not', but tautologies can also occur in scientific arguments when for instance the cause is defined in terms of the effect, as in the classic case of the *circulus in explanans* of Molière's doctor in *le malade imaginaire* who attributed a *virtus dormitiva*, a sleep-inducing power, to a certain medicine to explain why it made one sleepy. Many scientific arguments may look tautologous (especially when reformulated within the *logical* Deductive-Nomological formalization scheme), but usually they are not. Consider an example from Newton's theory: why does a stone fall towards the earth? Because of gravity. But what is gravity? That which makes the stone fall towards the earth. The sentence 'the stone falls towards the earth because of gravity' thus seems to be just as tautologous as the example from Molière. However, this is not the case since gravity is further analyzed by Newton as a true quantitative notion on the basis of which all events of something's falling towards the earth can be precisely described, in such a way that we gain information.

When we now look back at the debate over the tautologous nature of Darwinian explanations we can distinguish two main senses: i) tautologous as empirically untestable, uninformative, and void of explanatory power, and ii) tautologous as semantically analytic. The former are not strictly tautologous but since the epistemological notion of a posteriori empirical testability and the semantic notion of non-tautology were long conflated it is reasonable to discuss these claims of untestability of Darwinian explanations here shortly. We will discuss other, partly-related, claims that Darwinian theories are untestable in §1.3c.

i) The claim that (many) Darwinian explanations are untestable (or 'tautological' as the debate wrongfully calls them) comes in at least two forms: 1) Gould and Lewontin claimed in their critique of certain types of adaptive explanations that supporters of the adaptationist paradigm come up with one adaptationist story after another when a particular adaptationist story fails, thus forgetting that a trait *need not* be adaptive. For Gould and Lewontin this proves that the claim that a trait is an adaptation is in practice not testable and tautological, insofar as its truth is assumed *a priori*. Others hold that the explanations are untestable because 2) they do not lead to true (future) predictions, which a real theory should. These claims of tautology can be answered easily: 1) Gould and Lewontin are right that one should look for the possibility that a trait is not an adaptation, but if one does that, adaptive explanations becomes empirically testable and non-tautologous (see **adaptation** in §1.23); 2) Darwinism can in rare occasions make true future predictions (see §1.3b). And even if this is not acknowledged, one cannot deny that

retrodictions (i.e., predictions about what fossils will probably be found) should be counted as predictions too. It can then be claimed that these predictions are void insofar as Darwinism only tells us what the past winners of the struggle for existence were by considering what forms have survived, which does not explain anything but just equates or labels what has survived *post facto* with that which was selected for. However, although Darwinian explanations work indeed by crowning the past survivors, such crowning acts need not be meaningless if the crowning act mentions (as it should) *why* an organism deserves its crown, i.e., if one comes up with fitness<sub>2</sub> calculations that show because of what trait an organism survived better than his competitors.

ii) The criticism of a real (semantic) tautology comes in multiple flavors too: 1) some claim that the fundamental phrase ‘survival of the fittest’ is a tautology in the sense of a pleonasm; 2) others hold that the term ‘fitness’ is a primitive term that, like all fundamental terms (e.g., ‘space’ and ‘time’) can only be defined in less fundamental terms that presuppose this fundamental concept, so that explications or definitions of fitness are necessarily tautologous although Darwinian explanations that use the term ‘fitness’ are not tautologous just as physical explanations using the terms ‘space’ and ‘time’ are non-tautologous; and 3) still others maintain that the functional uses or functional definitions of the concept of fitness are tautologous insofar as the fitness of *traits* can only be considered on the basis of the fitness of *organisms*, so that when one tries to cite the functional fitness value of a trait to explain the fitness of an organism one forgets that to assess the functional fitness value of a trait one already needs to know the fitness value of an organism (i.e., we thus have a classical case of a *virtus dormitiva*).

1) This flavor is the most common. The reasoning is that, if we take the term ‘fit’ to mean ‘endowed with phenotypic characteristics that improve chances of survival and reproduction’, then ‘survival of the fittest’ can simply be rewritten as ‘survival of those who are better at surviving’ or even ‘survival of those who survive’. Thus fitness is taken as entailing survival (i.e., Mills and Beatty 1979 etc.). Another possibility that amounts to the same thing is defining survival ultimately as reproducing, and fitness as a propensity to reproduce so that ‘survival of the fittest’ then means ‘reproduction of those who have the highest propensity to reproduce.’<sup>213</sup>

2) The idea that fitness is a primitive term is most prominent in the work of Williams (1973, 1980, 1981), Rosenberg (1978, 1982, 1985, 1991), and Rosenberg and Williams (1986). They argue that definitions of fitness can be given in everyday language and that the fitness concept is even based upon the everyday metaphor of something fitting in an environment, but that the *scientific* term fitness is nonetheless not further analyzable in more fundamental concepts, so that all scientific definitions of fitness in other less fundamental scientific concepts are tautological because they already use the concept of fitness. They argue strongly against the fitness<sub>1</sub> definition as necessarily tautological (see footnote 188), but interestingly do not hold that fitness is an empirically void term: as a fundamental term it

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<sup>213</sup> See also footnote 145 that explains how Darwin himself used the phrase ‘survival of the fittest’.

functions just like the terms ‘space’ and ‘time’ in physics in quantitative models and explanations.

- 3) Some, for instance Ollason (1991), hold this view (see footnote 185): the fitness of traits is defined in terms of the fitness of the organism, i.e., a trait is only called fitter if it helps in begetting more offspring, thus accordingly one should not opt for the “tautologous” fitness<sub>2</sub> definition but for the fitness<sub>1</sub> definition (which Ollason does not consider tautologous).

All three points can be answered simultaneously by looking more carefully at the double meaning of the word ‘fitness’ again: the fitness of an entity (of a certain kind) can mean i) the (statistical average) number of its offspring (fitness<sub>1</sub>) but also ii) the fitness *to do something* (*to eat, to digest, to attract a mate, etc.*) (fitness<sub>2</sub>). The supposed tautology of ‘survival of the fittest’ depends on the first meaning of ‘fitness’, not on the second. The correct use of the second meaning of ‘fitness’ requires ideally that one determines the *proximate causal mechanisms* for how a trait is ultimately fit to do something and the *proximate causal mechanism* for how this trait arose in the first place. This makes examples of survival of the fittest testable and not semantically tautologous (contra claim 1). Our own fitness<sub>2</sub> definition further shows that, although ‘fitness<sub>2</sub>’ is a *fundamental* term, it is definable and scientifically explicable and thus not *primitive* in Rosenberg’s and William’s sense; in our definition the concept of fitness is related to two other fundamental, or if one likes “primitive” concepts (as the fitness<sub>2</sub>-definition refers to replicators and variation). This is not a problem but an advantage; it just shows the intricate web of interrelations of concepts, just as a dictionary does. We do agree with Rosenberg and Williams, though, that on the basis of the fundamental notion of fitness, we can make empirically testable models. Finally, the fitness<sub>2</sub> definitions are not ‘tautological because they can only be identified on the basis of their effects for replication.’ Although one has indeed to presuppose that fitness<sub>2</sub> traits influence reproductive chances, they can be defined in independent physical measures, and by demonstrating the proximate mechanisms behind replication one can construct a perfectly sound causal explanation of differential reproduction.

Our argument becomes clearer when we compare three different explanations for the whiteness of a polar’s bear fur (after Thompson 2001: 557 with modifications):

- (1) because white bears have been selected for white fur.
- (2) because white bears have been selected for camouflage.
- (3) because disproportionally reproducing bears have been selected for disproportionate reproduction,

(1) and (3) are clearly tautological, but (2) is not because it mentions the cause of the evolution.

In sum: to be non-tautologous, explanations should rest upon fitness<sub>2</sub>-definitions, which concentrate upon proximate *causal* mechanisms. We therefore claim: *a Darwinian explanation can be non-tautologous and it is the more informative the more and better proximate mechanisms are described within the adaptive explanation.* If a Darwinian explanation does not contain these mechanisms it is indeed tautologous.

**b) future predictions:** The main warning Darwinism gives us regarding its use to predict the future reads as the disclaimer of a risky financial product: past results are no guarantee of the future. This is not a problem unless one believes that science is

only science when it makes future predictions. The nature of a Darwinian explanation would then be thoroughly unscientific, because its object is the past. It gives a historical account of why a property evolved the way it did. One cannot expect more from it. Dennett (1995) speaks in this regard of ‘retrospective coronations’: Darwinism cannot tell you what the winners of today are, but it can inform you about the winners of the past, which are awarded their crown by scientists in retrospect. This crown should be awarded when one can give an analysis on the basis of fitness<sub>2</sub> characteristics why a certain type of organism was selected for. On the basis of actual models of past evolution one can also make predictions about, for instance, fossil or DNA-sequences in related species that are likely to be discovered in the future.

A more controversial point is whether one can also make *true future* predictions. We think Darwinism *can*, although not often and we cannot give *general* criteria when it is able to do so and neither will the predictions be 100% secure – far from it. But they do not need to be so secure to be acceptable predictions. Think of the stock exchange: if only you could consistently predict the stocks 1% (or even less) better than anybody else, your theory would be very useful and make you a lot of money.

To illustrate the status of future predictions by Darwinism we choose some examples of predictions Darwinism claims to have made, some general and some concrete ones: (general) a) the identification of genes, b) the possibility of artificial selection, (concrete) c) cases of convergent evolution, d) rules of thumb from ecology, e) parasite-host evolution and other arms races, f) virus evolution, and g) the case of HIV in which an almost complete overview of variation exists.

a) the identification of genes: one could argue that Darwin’s theory made the identification of genes possible and thus predictive tests for the BRCA 1- and BRCA 2-genes, whose positive results are a 60 to 80 percent guarantee of developing breast cancer. But these tests could also have been developed, if only a Mendel (and a Watson and Crick etc.), and no Darwin ever existed. Hence, the development of methods to identify genes (based on the prediction that we could identify genes) could have proceeded without Darwin’s theory present.

b) the possibility of artificial selection: since Darwin’s theory, a lot of research has gone into identifying natural variation; the existence of natural variation makes it possible to predict that artificial selection as in crop cultivation and animal husbandry will almost always be possible and can have drastic results.<sup>214</sup> However, historically, this supposed prediction of Darwinism is wrong: Darwin used the fact of artificial selection, which had long been known to farmers and layman alike, to argue for natural selection. Hence, the theory of natural selection has not predicted that artificial selection is possible. Moreover, Darwinism does not predict natural variation but presupposes it; this variation can be measured independent of Darwinistic theorizing, as the biometricians did at the beginning of the 20<sup>th</sup> century. Thus the

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<sup>214</sup> See Ridley 2005, chapter 9 on quantitative selection, especially §9.7 and 9.8. “A population can only respond to artificial selection for as long as the genetic variation lasts. Consider, for example, the longest running controlled artificial selection experiment. Since 1896, corn has been selected, at the State Agricultural Laboratory in Illinois, for (among other things) either high or low oil content. As figure 9.7 [a graph of oil content versus time] shows, even after 90 generations the response to selection for high oil content has not been exhausted. However, the oil content finally became negligibly low in the line selected for low oil content. The seeds had become difficult to maintain and the “low oil” experiment was discontinued after 87 generations (Ridley 2005: 236).”



possibility of artificial selection (on the basis of variation), although true, is not a consequence or prediction of Darwin's theory.

c) convergent evolution: convergent evolution happens when two independent lineages of the phylogenetic tree of life evolve similar traits as a result of having to adapt to similar environments or ecological niches. An example of convergent evolution is the similar nature of the flight/wings of insects, birds, pterosaurs, and bats. All four serve the same function and are similar in structure, but each evolved independently. The striking similarity between hummingbird moths and hummingbirds is another example of convergent evolution.<sup>215</sup> However, predictions that convergent evolution will happen are hard to make for at least the following reasons: i) common background structure, ii) external similarities, and iii) uncertainty *when* it will happen.

i) common background structure: it may seem that the eye is a perfect example of convergent evolution: in many unrelated branches quite similar eyes have evolved. However, although it is true that the *lens* of eyes have evolved independently in various animals (as well as some other features of the eye), most experts nowadays believe that the eye is monophyletic; that is, all modern eyes, varied as they are, have their origins in a proto-eye believed to have evolved some 540 million years ago (Halder et al. 1995, Tomarev et al. 1997). The analogous structure of wings may also be due to common ancestry, although this is usually disputed. Of course, when we go far enough back all life has a common ancestry and so an opponent of convergent evolution may always argue that the example of convergent evolution is not really convergent because of some far-away common ancestor. In practice, giving examples of convergent evolution is therefore hard because of the decision one should make whether the evolution of a trait in a certain direction is still due to constraints because of common ancestry or because of common selection pressures. This problem is basically a special case of the problem of how much the evolution of an adaptive trait by natural selection is constrained.

ii) the viewpoint of convergent evolution focuses on *similarities*, especially the eye-catching external ones (although examples of convergent evolution of enzymes exist). This may hide the less visible dissimilarities behind a trait. Since a trait can be judged on almost indefinitely many characteristics or properties (just as almost indefinitely many characteristics can be used to determine a type, see §1.22 on the species problem) it may be hard in practice to say whether something is truly an example of convergent evolution or not. Whales, mammals that returned to the sea, may look like ordinary fish, only bigger than others (and this may be due to ancient hox genes reactivated, thus a common background structure), but this case of convergent evolution may overlook the different techniques of actual whale locomotion etc.

iii) uncertainty when it will happen: although cases of convergent evolution can be found in the fossil record, the fact that they exist does not provide us all of itself with a criterion *when* it will happen in the future (although observation of the cases of convergent evolution can be used to find these criteria). Only when one thinks that constraints on evolution are so strong that evolution will always converge to certain life forms, baptized "life's solution" by Conway Morris (2003), can one easily make a case for future convergence. We saw, however, that most scholars do

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<sup>215</sup> Many more examples can be found at [http://cas.bellarmine.edu/tietjen/Evolution/convergent\\_evolution\\_examples.htm](http://cas.bellarmine.edu/tietjen/Evolution/convergent_evolution_examples.htm) and [http://en.wikipedia.org/wiki/Convergent\\_evolution](http://en.wikipedia.org/wiki/Convergent_evolution), both accessed 20 January 2007 .

not think constraints are so strong, so the onset of convergent evolution is hard to predict, except for the three cases and similar ones described from d) to g) below.

d) rules of thumb from ecology: ecology and resource utilization can give us some hints at the direction of evolution. The best example is the rules of  $r/K$ <sup>216</sup> strategy theory (see Lewin and Foley 2004: 155f), which resulted from the work of ecology and population dynamics on island biogeography. Typically,  $r$ -selected species produce many offspring, each of which is unlikely to survive to adulthood, while  $K$ -selected species invest more heavily in fewer offspring, each of which has a better chance of surviving to adulthood. It is thought that  $r$ -selection predominates in uncertain environments, as the ability to reproduce quickly is crucial and there is little advantage in adaptations that permit successful competition with other organisms (since the environment is likely to change again). Traits that are thought to be characteristic of  $r$ -selection include: high fecundity, small size, short generation time, and the ability to disperse offspring widely. Examples of  $r$ -selected species are bacteria, weeds, and mammals such as small rodents.  $K$ -selected species are the opposite. For them the ability to compete successfully for limited resources is crucial, and populations of  $K$ -selected organisms are typically very constant and close to the maximum that the environment can bear. Traits that are thought to be characteristic of  $K$ -selection include: large size, long life span, and the production of fewer offspring that are well cared for.<sup>217</sup> Examples are elephants, whales, and especially humans. Schematically, after Lewin and Foley (2004: 157).

<b>Factor</b>	<b>r-selection</b>	<b>K-selection</b>
<b>Climate</b>	Variable and/or unpredictable; uncertain	Fairly constant and/or predictable; more certain
<b>Mortality</b>	Often catastrophic, nondirected, density dependent	More directed, density dependent
<b>Survivorship</b>	High juvenile mortality	More constant mortality
<b>Population size</b>	Variable in time, nonequilibrium; usually well below carrying capacity of the environment; unsaturated communities or portions thereof; ecological vacuums; recolonization each year	Fairly constant in time, equilibrium; at or near carrying capacity of the environment; saturated communities; no recolonization necessary
<b>Intra- and interspecific competition</b>	Variable, often lax	Usually keen
<b>Selection favours</b>	<ul style="list-style-type: none"> <li>- Rapid development</li> <li>- High maximal rate of increase 'r'</li> <li>- Early reproduction</li> <li>- Small body size</li> <li>- Single reproduction</li> <li>- Many small offspring</li> </ul>	<ul style="list-style-type: none"> <li>- Slower development</li> <li>- Greater competitive ability</li> <li>- Delayed reproduction</li> <li>- Larger body size</li> <li>- Repeated reproduction</li> <li>- Fewer, larger progeny</li> </ul>
<b>Length of life</b>	Short, usually less than 1 year	Longer, usually more than 1 year

Table 1.4:  $K$ - and  $r$ -selection

<sup>216</sup> The letters 'r' and 'K' come from the equations of population dynamics. Think of an empty island that becomes colonized. At first, because there is no serious limitation on survival, the number of individuals of a species will reproduce at a rate  $R$  that can be described by  $R=rN$  with 'R' the rate of population growth, 'r' the maximum fecundity of an individual, and 'N' the number of breeding individuals. At first, species with the highest 'r' will dominate the island (hence we speak of  $r$ -strategists), but as competition increases and resources deplete the island will start to reach its carrying capacity 'K'. The population will now no longer increase. It can be described by  $R = rN (1-N)/K$ . In these circumstances  $r$ -strategists are (partly or wholly) replaced by so-called  $K$ -strategists that have fewer offspring but care better for them.

<sup>217</sup> See MacArthur and Wilson's *The Theory of Island Biogeography* (1967), in which the theory of  $r$ - and  $K$ -selection was first formulated *in extenso*.

No matter how beautiful this scheme may look like, there are unfortunately clear exceptions to the rules of r/K-selection theory such as long living trees with numerous offspring dispersed widely and long living turtles with high fecundity, but high rates of attrition among infants as well as almost no care for the young. However, as we saw, these rules of thumb are not without exception and indeed nothing but helpful shortcut devices that often work.

e) parasite-host evolution and other arms races: when two species co-evolve, especially in an antagonistic way, one can predict that this process will continue in the future and that the typical product of parasite host-evolution – co-phylogenies of hosts and parasites – will tend to give rise to new branches of the tree of life. This prediction is an extrapolation of what has happened in relatively well-defined cycles in the past.

f) virus evolution: a corollary of parasite-host evolution is that bacteria will become resistant to our penicillin and other antibiotics. On the basis of the existence of multi-resistant viruses one can then predict that a new world-wide fever epidemic caused by a virus, called a pandemic, will one day happen. Using statistics and results from the past, biologists might even calculate a reasonable probability that such a pandemic could strike us within the foreseeable future, so that policy makers can take precautionary measures as the case of the infamous bird flu H5N1 and the purchase of inventories of anti-viral medicine like Tamiflu by governments and health organizations demonstrates.

g) The case of HIV: HIV is an RNA-virus which means that it mutates very quickly so that the individual virus particles in affected host are rarely all exactly the same (see Ridley 2005: 45f). HIV suppressors have been found that attach themselves to specific sites of the HIV-virus; these suppressors at first seemed to promise a recovery from the infection. And indeed, after intake the number of particles in the blood decreases rapidly, only to return a few weeks later as the mutant HIV-particles that had a slightly different site to which the virus killer attached itself had a differential reproductive advantage and now became prominent in the blood stream. One can even calculate the average time it takes for a person to have just the same amount of HIV-particles in his or her bloodstream as before the drug intake (or actually, the half-value time at which 50% of the carriers using the drug are back at the level of virus particles in their bloodstream before the drug intake, just like the half-value times of radioactive decay). Only a drug cocktail of three different suppressors can bring the number of particles back to levels so low that they do not pose a risk to a person (simultaneously targeting three attachment loci of the virus), since a mutation that simultaneously outsmarts these three suppressors (and still remain virulent) is extremely small. All these predictions are possible because HIV is so well-studied and the characteristics of the virus-particles are well-known. One even knows which exact mutation happens (normally) when a certain suppressor is used (the evolution of HIV is highly constrained as only a few of the mutations of the 100.000 bases yield viable, fecund viruses and the force of selection is in this case also well-known). It thus seems one can predict evolution when one has a complete overview of the possible states into which an entity can mutate and in what amount of time. However, surprise mutations can always happen (different phenotypic forms of the same base combinations, rare non-point mutations such as whole insertions, extensions to the 100.000 virus bases by viral sex, etc.) and thus the predictions are never 100% accurate.

In sum: sometimes evolutionary processes can be extrapolated to the future. However, never with 100% certainty; Darwinism is primarily a historical science with a backwards-calculating hermeneutic. True exceptionless future predictions seem possible if a complete overview of the possible future evolutionary states can be given, but that is never completely the case. Predictions with a high degree of (statistical) certainty are, however, possible in that case.

**c) the supposed untestability of Darwinism:** in this section we will first show how Darwin himself thought his theory could be tested. As his tests turn out to be weak in practice, we discuss claims that Darwinism is untestable, then continue with a short general discussion of how scientific theories should be tested. We argue that the best way to test a theory is to see whether it does better than its competitors on the following five criteria: scope, accuracy, consistency, simplicity, and fruitfulness. We will show that it is hard to assess Darwinism as a whole on these criteria as it is neither a uniform whole nor a historically constant entity. We end by showing that these criteria can be used to test which of the competing specific Darwinian explanations is the best.

### **Darwin on testing his theory**

Darwin himself realized the difficulties of testing his theory. In the *Origin* he mentions two famous tests of his theory: a) it could be found that the earth just did not exist long enough for evolution to have taken place<sup>218</sup>, and b) there might be things so irreducibly complex that they cannot have been formed by slow gradual steps.<sup>219</sup> We will show that his falsifiers are hard to use in practice, then we will continue with a discussion on how Darwinism (if it is not easily falsified) should be tested according to our own view.

Darwin's *first* criterion is a bit hard to use. How are we to test whether time suffices for the evolution of the enormous biodiversity on earth today? We cannot do experiments on the time scale of billions of years and thus a real experiment is impossible. And even billions of years to experiment would not suffice to demonstrate that life can or cannot arise by natural selection (especially when in the experiment life does not arise), since the outcome of the experiment is (probably) highly dependent on initial conditions and thus we would need *billions* of billions of years instead of simply billions of years to do many experiments with different initial conditions to be reasonably sure that there was enough time for life to emerge on earth.

We could of course discover that fossils are not as old as we used to think and that thus the timescale for evolution to have taken place is too short. In practice, however, people are so convinced of Darwinism that geologists often use fossils to date layers of the earth's crust instead of the other way round; but indeed, it is true that there are independent methods such as measuring the amount of radioactive isotopes or the carving of rivers into the landscape to determine the age of a piece of the earth's crust. It would be interesting to see whether scientists would choose to drop evolution on the one hand or for instance the supposition that the decay rate of isotopes has always been the same and the supposition that rivers carry equal amount

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<sup>218</sup> See Darwin 1859: 102 "Though nature grants vast periods of time for the work of natural selection, she does not grant an indefinite period."

<sup>219</sup> Idem, p. 189 "If it could be demonstrated that any complex organ existed, which could not possibly have been formed by numerous, successive slight modifications, my theory would absolutely break down."

of water and thus carve their way equally for centuries on the other hand, if it were found that the two observations did not agree on the time scales involved.

One could also try to extrapolate from actual observed microevolution to macroevolution to test the hypothesis of the time scale, but the justification for this extrapolation is wanting, especially when not all variation mechanisms are known. People initially believed that mutations were due to point mutations: single replacements of one of the 'letters' of a piece of DNA. And indeed, point mutations can cause evolution. Since these mutations are believed to occur randomly over the genome, mutations in the 'non-coding DNA' which is not transcribed in the cell during protein synthesis (and on which thus no selection pressure exists), can be used to trace accurate temporal evolutionary pathways. However, this 'biological clock' is not constant over time and often species-dependent.<sup>220</sup> Moreover, a lot of variation and evolutionary development is now thought to take place because whole pieces of DNA are inserted elsewhere in the genome during both normal and erroneous recombination. Most apes are bichromate, whereas we humans – apart from colour-blind people – are trichromate. The third colour gene which codes for a protein sensitive to green light is a mutated copy of the original gene for sensitivity to light of red wavelengths, which gradually mutated (Dawkins 2004). There thus was not a whole new process in which a green-light-sensitive protein had to evolve. The process could use the genetic template of an already-existing protein. Actually, the acceptance of the idea of mutations as whole insertions happened rather quickly because of the fact that this process could as a by-product clearly speed up evolution. Thus, new ideas about variation mechanisms can change the time scale on which evolution takes place. This observation makes it hard to assess the time needed for the evolution of our current biodiversity and thus make it even harder for Darwin's first criterion to be applied.

The *second* way to refute Darwin according to himself has been followed by more of Darwin's critics. Many organs have been claimed to be irreducibly complex. Eyes, for instance, and wings. One of the most recent examples is the work of the American biochemist and politically-active supporter of the controversial<sup>221</sup>

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<sup>220</sup> See Ridley 2005, chapter 19 on evolutionary genomics, the molecular clock and variation mechanisms and chapter 21 on the rates of evolution and the many means to measure it.

<sup>221</sup> Most of the controversy about Intelligent Design (ID) arises from the fact that its supporters, almost all Christians, purposefully disguise a religion idea as science, although they know it will never turn into a fruitful scientific research program. We define Intelligent Design as the theory that holds that a) there are irreducibly complex forms in nature that could not have evolved by means of natural selection alone and b) whose existence thus implies the existence of a Designer. The supporters of ID purposefully restrict themselves to these two claims, leaving open the much more controversial questions: 1) how the designer constrained evolution so that the design he intended would arise (is fiddling with the parameters of the natural laws enough for this, and if so how, please give a testable model) and 2) who the designer is. According to the 'letter' of ID, the designer could be an alien civilization as the astronomer Fred Hoyle held (Hoyle 1982, 1983, 1984), the collective of Hume's deities who work just as bad as human engineers and make a couple of miscreations until they come up with a (still imperfect) design that works only partially (Hume 1779) etc., but according to the (hidden) 'spirit' of ID or at least to that of many of its supporters the factual existence of a designer seems to point in the direction of the God of the Bible because the design is thought of as a good (well-taken-care-of) design. We guess not many supporters of ID would be interested in ID if an irreducibly complex organ was found that was nonetheless badly designed (see also Sober 2007).

Note that since claim ii) about the existence of a designer hangs on claim i) which will turn out to be weak, claim ii) is weak itself. Not saying who the designer is and how his design plan worked makes claim ii) even more uninteresting, especially when the designer is thought of as some supernatural agent. However, even if irreducibly complex organs (given the age of the earth) were found and thus one had to

Intelligent Design-movement Michael Behe.<sup>222</sup> Behe believes that such a (seemingly) simple organ as a bacterial flagellum, but also the complex machinery of the cell and the immune system, provides a good example of irreducibly complex design. If we were to try to make the case of Behe as strong as possible by providing an example of ‘irreducible’ complexity, we would probably opt for the transitions from unprotected replicators to genes coding for their protecting shell.

However, the principle of ‘irreducible complexity’ is again hard to use in practice. When is an organ irreducibly complex? At least two things could be meant by ‘irreducibly complex’: i) irreducibly complex *as such* and ii) irreducibly complex *given the age of the universe* (i.e., the universe is not old enough for these complex forms to have arisen yet). ii) is the one we just discussed. It was a much stronger falsifier in Darwin’s own day as theologians still thought the earth was 6.000 years old and geologists were thinking about 100.000’s of years instead of the current estimate of 4 billion years, and as biologists knew little about variation mechanisms that sped up evolution. i) on the other hand is conceptually strange: what does the concatenation ‘irreducible complexity’ mean? It cannot mean ‘exists without parts’ because every complex has parts; it can also not mean ‘(completely) impossible to come into existence by natural means’ because every baby animal develops the supposed irreducibly complex organ in a natural fashion, so it can only mean  $\alpha$ ) logically or  $\beta$ ) physically impossible to come into existence *for the first time*. Often supporters of ID try to explain this further by saying that the whole of the organ is ‘earlier’ than its parts, i.e., that the parts are useless without the whole of which they are parts and that thus the whole would have had to arise at once. Behind this explanation still lingers the old teleological image of an animal as a whole subjugating all of its parts. Since organs are formed by a set of genes that control elements of embryology, the claim of ID in the 21<sup>st</sup> century would mean that a combination of genes cannot come into existence for the first time by natural means. However, Darwinians can almost always come up with a story in which earlier versions of genes coded for some preliminary version of the organ or in which earlier organs out of which the later organ evolved were functional in and of themselves for a different purpose. For the claim  $\alpha$ ) that it is logically impossible the story Darwinians tell need not even be true, since this strong claim is about in principle irreducibly complex organs and to debunk this claim one does not need actual but only potential falsifiers that tell a plausible story about the origin of the organ. Thus only claim  $\beta$ ) about physical impossibility stands. For this claim, the supporter of ID needs to do the very hard calculation that all currently and future proposed mechanisms are impossible given the physics we know today (or even given future physics?). As yet, no supporter of ID has given such calculations and it is unlikely they will do so any time soon.

Finally, Darwin’s theory shows how out of completely blind mechanic processes design can result without the help of a designer (Darwin thus decoupled design from the idea of a designer). Of course one can still hold that the same thing might have evolved because a designer planned it, but that is unnecessary. One can explain heat flow by postulating warm and cold particles (‘caloric’ and ‘frigoric’), but also by postulating only warm particles. According to Occam’s razor the latter practice is best; the same holds for Darwin without a designer and thus without ID,

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admit that some designer existed, for instance an alien, this would still not refute Darwinism, because the alien could have evolved by a Darwinian process on its own planet.

<sup>222</sup> See Michael Behe *Darwin’s Black Box* (1996).

unless it can substantiate claim  $\beta$ ). Until now, ID has not given rise to a fruitful research program that has even remotely demonstrated anything that comes close to claim  $\beta$ ). That is a true and crying shame for any research program that so strongly announces itself as science.

### **Putting Darwinism to the test**

Thus, Darwin's two criteria are too dull to really put Darwinism to the test. Nevertheless, in my view there have been two real 'tests': i) our Whig history of the rise of Darwinism showed how Darwinism, despite fierce competition, became accepted because it could do things its competitors could not (see section §1.1), and ii) on the basis of Darwinism many detailed and informative accounts of how particular properties have evolved historically have been given. The Whig history showed what the benefactors of Darwinism were and how the threats to it were overcome; the many rich, detailed explanations of phenomena put flesh on the bones of Darwinism.

We will first say a few words about verification/falsification, Bayesianism, and revolutions in science. Then we distinguish more precisely than before Darwinism in general from specific Darwinian explanations and argue that Darwinism in general is not just the sum of all specific (current and future) Darwinian explanations. We argue that we need different principles to account for the empirical success of Darwinism in general and the success of specific Darwinian explanations; the success of the former cannot be disentangled from the history of science and the evolution of society in general and the benefactors mentioned in §1.1 as well as the overcoming of the threats mentioned there. For specific Darwinian explanations we can be more specific; some general norms for theory choice will do to characterize the success of a certain explanation as well as compliance with our definitions of replication, variation, and selection.

### **Philosophy of science on theory choice**

Philosophy of science is a complex field with highly sophisticated and often technical discussions on what science is and how it works. One of the important problems of this field is that of theory choice: which of a group of competing theories, if any, should one choose. Here, we can only confine ourselves to the broad outlines of this discussion.

As is well known, early philosophy of science spoke of verificationism: a theory is meaningful only if its predictions can be verified by empirical data (and true if they are in fact verified). A theory was replaced by another if it described/predicted the data more accurately and/or in more domains, so that the progress of science was seen as a process towards broader and broader theoretical subsumptions (and as theories themselves were seen as hypothetical-deductive models, progress was thus a process of entailment by theories with ever broader deductive scopes). Verificationism was criticized by Popper: many models can predict or explain the same empirical data, which makes the demand of verificationism one that could be satisfied too easily. What Popper demanded instead was theories that could be *falsified* (in as many ways as possible), noting an asymmetry between verification and falsification: one falsification can show that a theory is false, whereas one can never verify a theory as even an indefinite set of verifications of empirical predictions of a theory are not enough to support the claim that a theory is true (as the same theory can also make indefinitely many wrong predictions). Unfortunately, it is now commonly believed that Popper's criterion is almost just as sterile as that of the verificationists. This belief is expressed in the Duhem-Quine thesis (although neither

Pierre Duhem nor William van Orman Quine exactly held this thesis), which states that it is impossible to test a scientific hypothesis in *isolation* because an empirical test of the hypothesis requires background assumptions (also called *auxiliary assumptions* or *auxiliary hypotheses*). Because of this, a hypothesis cannot be conclusively falsified through empirical means unless the background assumptions are indubitably true as they (almost) never are. One can try to remedy this situation by a) explicating the background assumptions and b) verifying/falsifying them; however, in practice, this will not work since a) the number of background assumptions is an indefinite list and b) verifying/falsifying background assumptions in isolation is also impossible insofar as an empirical test requires further background assumptions.

Thus it seems that verifiability and falsifiability are hard to achieve in practice. The historian and philosopher of science Thomas Kuhn (1962/1970a) integrated this conclusion in his highly influential book on scientific revolutions. As is well known he postulated the existence of periods of pre-paradigmatic science, long periods of normal science within a paradigm, and short intermediate periods of revolutions and crises when one scientific paradigm was replaced by another. The notion of a paradigm was rather vague in scope (from whole branches of science like mechanics, to subfields like molecular spectroscopy), and its constituents included things such as exemplars, tacit knowledge of the practice of a certain scientific field, etc. Kuhn's most controversial claim, especially in the face of the reigning hypothetical-deductive model of explanations and the idea of progress as theoretical subsumption, was his insistence that the results of science in different paradigms were incommensurable, i.e., not comparable on a common criterion insofar as every paradigm gave its own meaning (and thus reference) to theoretical terms within a paradigm-specific holistic field of meanings, thus making evidence and facts also paradigm specific. This incommensurability seemed to question the possibility of real linear progress in science.

Kuhn has been criticized by many friends and allies; some went even further along his lines by regarding even normal science as a battle of competing paradigms or research programs; others found the concept of paradigm to be far too vague and tried to revitalize elements of the old hypothetical-deductive model by arguing for non-holistic singular reference (possible world semantics) and the sociological claim that often new theories (or models) included many of the results of older theories (or models), since new theories only got accepted if they explained the success of previous theories and left their main results untouched. Be that as it may, the undisputed, lasting result of Kuhn's work is that a) philosophy of science became more and more intertwined with the history of science and that b) when testing a theory one should not only check whether the individual theories (and their background assumptions) explain the facts, but also whether they do this better or worse than other competing theories. These two results are a bit opposed. a) Early philosophers of science held their models of scientific explanations to be models of what scientists were *actually* doing, or, if they were not doing this, at least as models of what scientists *should* ideally be doing if they were rational. Historians of science have shown that actual science progresses in multiple ways, often even when a theory contains clear contradictions (as Bohr's first atomic model etc.), that the justification of theories has been really diverse and absolutely not like the ideal models of philosophers of science, and that the acceptance of a theory by a research community has a strong social dimension (Waller 2004). The work of historians further showed that the Popperian distinction between a context of discovery and the context of justification (or the context of acceptance by the community as the ultimate sign of



justification) was not always clear. Thus the description of the actual processes behind scientific progress, a progress which had been seen as the epitome of rationality by philosophers of science, undermined the *normative* dimension of their models. b) On the other hand, the realization that in science often many theories are competing made the demand for norms to decide which theory to choose even stronger.

Since we will have to judge different Darwinian explanations outside of biology we need such norms too. Norms that are often mentioned include: “accuracy, consistency, scope, simplicity and fruitfulness.”<sup>223</sup> Unfortunately, these criteria cannot be formalized exactly, so that theory choice remains something done by experts on the basis of experience and pragmatics, instead of something that can be computed exactly. Fruitfulness cannot be formalized because the different fruits of different theories usually cannot be measured in a single currency. Simplicity is widely held as a theoretical virtue and is akin to the principle of Occam’s razor that the explanation of any phenomenon should make as few assumptions as possible, eliminating those that make no difference in the observable predictions of the explanatory hypothesis or theory. However, although complexity has been studied extensively since the end of the Second World War (Warren 1948), especially in information science/computer science and general systems theory, and although some speak of the science of complexity as “the science of all sciences” (Johnson 2007), there is as yet no single, general principle to calculate the complexity of any scientific theory. Neither can simplicity be the sole criterion of theory choice as Einstein’s theory is much more complex than Newton’s yet explains the observational data better. Consistency is a formal notion. We can distinguish intra-theoretic consistency from extra-theoretic consistency or consilience (see footnote 29), although the border between them cannot always be drawn clearly because of vagueness of what counts as a theory and what is part of it and what not. Of course, consistent theories are preferred over inconsistent ones. However, in actual science we often find inconsistencies of theories with other theories in different domains (absence of consilience) and sometimes we even find clear intra-theoretic inconsistencies in theories that are nevertheless employed successfully. We for instance find clear contradictions in the suppositions of Bohr’s early model of atoms, and even more we can find that “anomalies” or “local contradictions” are present in a theory, i.e., inconsistencies that are thought to be solvable if the presuppositions are qualified.<sup>224</sup> This is almost always possible as theories contain many propositions that can be qualified. Finally, accuracy and scope: these can be contrary principles as theories that are highly accurate (models of H<sub>2</sub>-atoms using the rule of thumb Rutherford potential) but limited in scope (just such atomic models); it would thus be great if a common measure of a theory’s accuracy and scope could be used to

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<sup>223</sup> See Kuhn 1977: 321. Kuhn’s theory of paradigms was criticized for making theory choice irrational, since the choice of a paradigm seemed to depend on “techniques of persuasion” or “argument and counterargument in a situation in which there can be no proof”. However, for Kuhn, as he notes in his Postscript 1969 (Kuhn 1970a), the decision reached by a community of scientists is the best form of rationality and objectivity we can have. The considerations that scientists use to persuade each other to accept a certain scientific theory, although they cannot be rendered logically explicit and precise, are systematic. They use, amongst other things, the standard criteria mentioned by Kuhn (and many others) above. See also Kuhn 1970bc.

<sup>224</sup> A beautiful example of a science where consistency is most important (mathematics) is given by Lakatos in his *Proof and Refutations* (1976) where it is shown in a pseudo-historical way how many proposed theories again and again were found to be inconsistent, the continuous removal of which by qualification of certain presuppositions (which led to new inconsistencies) allowed the theory to grow and to be successful.

assess its quality; however, accuracy and scope normally cannot be measured in a single currency; moreover, whereas the accuracy of a prediction can be determined relatively easily, the scope of a theory is harder to determine except in the case of entailment of an older theory by a newer theory, but exactly this idea of entailment within a scheme of hypothetico-deductive models has justly been criticized. Outside of these models, to our knowledge no general formal measure of scope (“amount” of “domains” in which the theory is applicable) has been given.

We have thus a situation in which the criteria for theory choice cannot be completely formalized and that their application and interpretation can be different from scientists to scientist. Ultimately, this would mean (as it sometimes seems to mean for Kuhn) that theory choice is ultimately subjective. That might be the case, but that conclusion is not as devastating as it may seem. Competing scientists still owe us an account why they think that their view scores better on the criteria mentioned. Outsiders (i.e., other scientists) can then judge which account they consider best. The scientific practice has shown that despite remaining different views, remarkable consensus can be achieved, and as such, despite the fact that they cannot be formalised, the 5 criteria we mentioned are in our view the best tools we have for choosing between competing theories.<sup>225</sup>

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<sup>225</sup> Another popular method for choosing amongst scientific theories is Bayesianism, but we think unjustly so. According to Bayesians one should opt for the theory or hypothesis  $T_x$  that assigns the highest value to Bayes' formula:  $P(T_x | E \& B)$  with  $P$  the conditional probability of  $T_x$  a theory, given  $E$  the evidence and  $B$  the background information. Bayesianism has clear uses in mathematics and game theory (for instance when certain cards are drawn it can be calculated how more likely it has become to get a full house, or a royal flush, etc.) and also in empirical diagnosis such as medicine when on the basis of symptoms a most likely treatment is to be chosen. That is because in these cases a complete (statistical) overview of the cases can be known: i.e., the 52 cards, or the possible diseases with their frequencies: 50% measles, 30% acne, or 20% other, etc.). However, in the case of theory choice in science such an overview cannot be had. Bayesians suppose “that there is a class of sentences that express all hypotheses and all actual or possible evidence of interest (Glymour 1981: 64).” Of course one can try to formulate a couple of hypotheses together with a “catch-all” hypothesis (Salmon 1990a: 195); however, this set is surely indefinite and unmanageable in size if the concept of it makes sense at all (it is like the unqualified set of all elementary propositions, which is a typical philosopher's dream too).

More practically, as the size of the set is not known, the relative weight that should be given to the hypothesis is unknown too and as we have every reason to believe that our current theories are false (given our historical track record) the probability of the catch-all hypothesis should almost certainly be assigned 100%. Other problems for scientific Bayesianism are that theories, evidence, and background in science are not explicit sets of homogeneous and elementary propositions. Moreover, if Kuhn is right, every theory brings along its own evidence and background information so that in practice different scientific theories are not tested against the same evidence and background information (especially when revolutions are taking place in a field of study). Further, there are multiple ways to calculate Bayes' formula (i.e., more explicit formulas that spell out the conditional probability), and which one should we choose? (Salmon 1990a). In these formulas, the input consists of degrees of belief in evidence, background information, etc. However, although we seem to have ‘grades’ of belief, can we be certain we also have true quantitative degrees of belief? Thought experiments on maximal bets someone wishes to make do convert grades into degrees, but do these experiments offer a realistic model for belief attribution (Glymour 1981)? These degrees of belief are, moreover, subjective and “can be determined by all sorts of idiosyncratic and objectively irrelevant considerations (Salmon 1990a: 185)”; thus a theory then becomes something personal, unless a person already lives up to the standard criteria for theory choice discussed above (there is a whole body of literature on subjective versus objective Bayesianism, which deals with the problem of what degrees of belief are and how they should be determined, but we forego this discussion here). Further, in science you have a theory and either it can explain existent observations or it can predict new (surprising) observations that when true lend support to the theory. Bayes' formula does not contain a temporal dimension and unless one plays a little with the algorithm for calculating the conditional probability (by introducing prior probabilities, the expectedness, likelihoods, etc. [which introduces problems of its own with for instance theoretical entailment of evidence, etc.]), this temporal dimension is completely lost in the Bayesian view of theory choice. Finally, the five natural notions for theory choice

**Application of the five criteria of theory choice to Darwinism as a whole**

If we now turn to Darwinism to assess its strength we should distinguish Darwinism as a whole from specific Darwinian explanations (such as an explanation of a certain form of a bird’s beak or of the length of a giraffe’s neck etc.). Darwinism as a whole cannot be demarcated clearly, both historically (i.e., over periods of time) and at any given moment in history, since it is neither a diachronic nor a synchronic whole. Ernst Mayr (2004, chapter 4) makes that very clear as he speaks of Darwin’s five theories of evolution: evolution as such, the theory of common descent, gradualism, population thinking and speciation (as multiplication of species instead of just one-to-one transformation of one species into another), and natural selection. Ernst Mayr shows that many people in Darwin’s time, even when they called themselves Darwinists, did not accept all five theories. Results are summarised in the table 1.5 below, after Mayr (2004: 98).

	<b>Evolution as such</b>	<b>Common descent</b>	<b>Gradualness</b>	<b>Populational speciation</b>	<b>Natural Selection</b>
<b>Darwin</b>	Yes	Yes	Yes	Yes	Yes
<b>Haeckel</b>	Yes	Yes	Yes	?	In part
<b>Neo-Lamarckians</b>	Yes	Yes	Yes	Yes	No
<b>T.H. Huxley</b>	Yes	Yes	No	No	(No)
<b>H. De Vries</b>	Yes	Yes	No	No	No
<b>T.H. Morgan</b>	Yes	Yes	(No)	No	Unimportant

*Table 1.5: Elements of Darwinism in the work of biologists after Darwin*

The question is: what is Darwinism? Given Mayr’s scheme it seems logical to say: belief in all five theories. However, we think that this cannot be maintained. Mayr’s account is too hagiographic, emphasising what the hero Darwin already foresaw that his contemporaries and latter-followers did not. Mayr only takes those elements of Darwin which we still believe in today. But we could formulate other theories of evolution (in Mayr’s sense) present in Darwin’s work: Darwin’s ‘sixth theory of evolution’, called uniformitarianism, which many of his contemporaries did not believe and which is again problematic today given well-supported ideas of punctuated equilibrium, evolutionary constraints etc.; or Darwin’s ‘seventh theory of evolution’: the pangenesis theory of heredity using gemmules as the basis of the heritance behind differential reproduction; or Darwin’s ‘eight theory of evolution’: natural selection as a force operative next to Lamarckian forces, which modern Darwinians would deny (except when considering pseudo-Lamarckian epigenetic rules, but then the Lamarckian process differs from Lamarckianism); or his ‘ninth theory of evolution’: natural selection next to the different process of sexual selection (whereas nowadays sexual selection is often reduced to a special case of natural

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(accuracy, scope, etc.) cannot or at least not easily and without discussion be mapped onto the Bayesian scheme (Glymour 1981: 88f). All in all, Bayesianism seems to bring along more problems than it solves; before it gets off ground, it can easily be shot down. That is why we ignore it and probably also why there do not exist true historical studies (thus bracketing the highly ahistorical constructed cases) that show that previous scientists used Bayesian thought, nor any current debates in science in which proponents of one theory use Bayesian norms to convince the proponents of the competing theory to change camps.

selection, see §1.5); or finally Darwin's 'tenth theory of evolution': selection works upon organisms and in a naïve fashion also upon groups of organisms (in contrast with sub-organismal selection of selfish genes, etc., and multi-level selection which replaced the naïve group selection of modern Darwinians). Considering the current state of Darwinian theory, Darwin would not have been called a Darwinist *if* these five other theories were included in a definition of what Darwinism is. Hence, what Darwinism as a cluster of theories and propositions is, is historically variable; we cannot speak of one true form of Darwinism. That makes testing Darwinism hard, insofar as there is no unique definition. But even if we had such a definition, it would be hard to make out *when* Darwinism was tested.

Let's for the moment assume that Darwinism or the idea of natural selection can be equated with the following statements (i.e., our initial summary above §1.2):

Animals replicate and they replicate more than is strictly needed to keep the population number constant; animals of the same species vary a little from each other, and this causes a struggle for means of existence like food and of reproduction like sex partners; in this process only the fittest, or best (i.e., the animals that are best *adapted* to their world) will survive and reproduce, and their offspring will inherit their well-adapted constitution.

We can ask: when was this idea tested? At the moment Darwin came up with his theoretical model, which opened up the hitherto-unthinkable possibility of the existence of design without an intentional designer? At the moment Darwin interpreted, in strong contrast with Cuvier, the fossil record as evidence of common descent? At the moment Darwin calculated that even of top-predators many animals died without reproducing (something Darwin's contemporaries had held to be unimaginable)?<sup>226</sup> At the moment Darwin investigated his finches at the Galapagos Islands and made the observation that finch species had multiplied? At the moment Mendel discovered the laws of genetics? As De Vries and other saltationists rediscovered these laws and combined them in Darwinian models in which the force of selection was but a mild force eliminating the very weak? As Dobzhansky found that the natural variation measured in the lab by biometricians was also present in nature? Since Fisher published the first *quantitative* models of evolution in populations? As the modern synthesis was formulated (but whose formulation)? As the discovery of DNA buttressed ideas about Mendelian heredity and postulated point mutations? As further molecular biology replaced naïve ideas about "one gene-one trait" or discovered more variation mechanisms? As the idea of selection was supplemented by consideration of constraints? As multi-level selection was introduced? Or because of the empirical tests? As Kettlewell and Ford investigated the melanic form of the moth *Biston betularia*? Or since these came under attack, since

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<sup>226</sup> Darwin considered one of the slowest breeding animals, the elephant, in more detail. He probably chose elephants because even baby elephants do not have apparent predators, apart from an occasional lion and of course man. By assuming that the earth was only 6000 years old and that God created just a single couple of elephants back then as the theologians of his day thought – he calculated, estimating a probable minimum rate of increase, that there would be "at the end of the fifth century (...) fifteen million elephants, descended from the first pair (Darwin 1859 cited by Dennett 1995: 81)." This is clearly nonsense; thus, not all animals replicate themselves; hence, selection exists and nature produces many more animals than can possibly all survive and beget offspring of their own. Nature does superfluous things, to the astonishment of Darwin's contemporaries.

their research has been redone in the period 2000–2007 by Majerus? Or is not more than one famous case needed? Thus, since more studies in the wild on selection forces are done with all the careful precautions (see Endler (1986) Darwinism in the wild) such as the careful 25+ year studies of the Grants on Darwin's finches (2007)? Or since DNA-analysis of various organisms vindicated the idea of common origin and descent? Or since truly new species were found such as the famous nylon eating bacteria, a species (discovered in 1972 by Japanese scientists) that is able to break down the byproducts of nylon such as the linear dimer of 6-aminohexanoate, which are not known to have existed prior to the invention of nylon in 1935 and as such seemed to be an ideal candidate to refute creationist claims that random mutation and natural selection can never add new information to a genome, and the statement that the odds against a useful new protein such as an enzyme arising through a process of random mutation would be prohibitively high? Or is Darwinism still not tested?

We think that the question what (biological) Darwinism in general is and how it can be tested cannot be defined univocally, and this makes it hard to say when it is tested. That is why most introductions in Darwinism or defenses of it just tell a general story about Darwin's revolutionary idea and then sum up some evidence like fossils, DNA's being common in all organisms (except for some Uracil-Cytosine differences), common organismic development with ontogeny recapitulating some of phylogeny, etc. (for instance Ruse 1998, Dawkins 1995, 1986). Darwinism in general is just too vaguely defined to be tested better.

### **Choosing between Darwinian explanations of a phenomenon**

We can have a short look at the criteria for theory choice such as accuracy, consistency, scope, simplicity, and fruitfulness to evaluate whether Darwinism, as vague an historical entity it is, does better than its competitors. An evaluation of Darwinism on these criteria is not very simple, especially given the multiple meanings of the concepts discussed above.

*Consistency:* Darwinism is *consistent*, although one should realize that its use of words like 'design' does not imply the existence of a designer (thus the metaphorical 'deduction' from design to designer is blocked); however, the theories with which Darwinism competes were also consistent (even though Dawkins 1982 tries to argue that Lamarckianism and orthogenesis are a priori unintelligible and thus inconsistent).

*Simplicity:* Darwinism as defined above is relatively simple, although not so simple that it was so trivial that it could easily be discovered by anyone: it required a whole new perspective on nature. It is hard to determine whether it is more or less simple than its competitors such as orthogenesis or Lamarckianism; when in the definition of Darwinism more complex proximate mechanism are included it becomes less simple.

*Accuracy (precision):* Darwinism in the above description is not very precise, as it is a highly general theory and only says that natural selection has shaped the design of organisms; its predictions about the past and its observations are then just qualitative. However, when Darwinism is understood as this general scheme *plus* the proximate mechanisms it invokes in its explanations (such as Mendelian genetics, DNA-analysis) it suddenly becomes highly accurate. As a general theory it is not much more accurate than its competitors, but insofar as it can invoke proximate mechanisms it is.

*Scope:* the scope of Darwinism is hard to determine; one could say that its explanatory scope is all design in nature (and so was that of its competitors).

However, one could argue that the scope of Darwinism was not fixed beforehand. As is well known, the co-discoverer of natural selection<sup>227</sup>, Alfred Russell Wallace restricted the scope of natural selection, especially in his later spiritualistic period, to biological nature excluding humans. Another reason why the scope of Darwinism was not fixed beforehand is that it could be found that other *natural* mechanism than natural selection could account for design such as Darwin himself believed (as he proposed Lamarckian forces and sexual selection next to natural selection to explain design); in the twentieth century the neutral theory of evolution by Kimura questioned whether natural selection could be used to explain molecular evolution. Thus, questions about the scope of Darwinism are empirical which makes it impossible to compare Darwinism's scope a priori with that of its competitors.

*Simplicity*: Darwin's theory looks very simple, even though the idea of natural selection was felt as counter-intuitive by many of Darwin's contemporaries. However, the idea of natural theology was even simpler and in practice Darwinian explanations can become really difficult (see below as we assess competing specific Darwinian explanations on the criteria for theory choice).

Finally *fruitfulness*: history has left Darwinism now, at the beginning of the 21<sup>st</sup> century, virtually unchallenged: as such, it has been really fruitful. This fruitfulness stems from the many empirical explanations on the basis of the Darwinian scheme, i.e., the way the general method inaugurated a perspective from which more specific Darwinian explanations could be proposed that would employ accurate, empirically testable, proximate mechanisms. The proximate mechanisms alone would not satisfy for a specific Darwinian explanation: it needs the general outlook of Darwinism for a determination of the direction in which it should invoke proximate mechanisms. For instance one could look at fossils and determine their atomic composition, their size, their morphological characteristics, etc., and indefinitely many properties of them could be investigated; however, Darwinism makes it interesting to test for specific elements that fulfill a key role in life, to order fossils in chronological order, to measure variances in morphological characteristics on which selection is thought to have operated, etc. Thus the five criteria do not help us a lot, or at least not as much as we might hope, in evaluating Darwinism *in general*.

Let's now look at the specific Darwinian explanations in more detail; here the situation is the opposite. Despite the possible power of Darwinian explanations, it remains unfortunately true that many proposed Darwinian explanations have a strong tautologous, untestable character. Stephan Jay Gould baptised explanations like these 'just-so stories'.<sup>228</sup> An example of such a story – let's take the case of man's walking upright – would probably run along the following lines: since walking upright allows people to have a better view of their territory, walking upright has a lot of advantages; hence there is a selection pressure on walking upright and thus nowadays almost every child learns to walk. Yet this explanation (and similar ones), leaves a lot

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<sup>227</sup> One could argue whether Wallace really held the same or highly similar views as Darwin. True or not, Darwin himself always generously considered Wallace to be the co-discoverer of the principle of natural selection (although he tended to be equally generous with who held even remotely similar views as his to be his co-discoverers or predecessors, in an effort to find support for his theory. See for instance the historical essay at the beginning of the sixth edition of the *Origin*. For a good and balanced discussion of the relationship of Darwin and Wallace and their ideas see Fichman 2004, Shermer 2002 and Raby 2002.

<sup>228</sup> Stephan Jay Gould coined this term after the title of a book by Kipling, to describe the nature of the wild growth of so-called Darwinian explanations. They sound plausible at first, but after a little reflection the problems with these explanations become all too problematic.

unexplained. Just by pointing out a possible function of walking upright – a better overview of the territory – a Darwinian explanation is not yet tested.

Craig Stanford is highly skeptical of ad hoc functional ascriptions in Darwinian explanations. In his book *Upright* he reviews thirteen different Darwinian theories of the origin of man's being a biped animal (Stanford 2003). Many of these point to different functions: crossing flatter territories quicker using less energy, not drowning when crossing a deep river, exposing less surface skin directly to the hot savanna sun (only the head gets heated up) and thus being better at heat control, keeping a better overview of the territory, the ability to use the front 'legs' (i.e., the hands) for tool manipulation, the freeing of the position of the mouth for speaking (in which case walking upright is a secondary consequence of functional selection on speaking), and six other theories. To find out which (or which combination) of these theories is right, one needs to do more than just armchair thought experiments. One needs to look at the fossils and the area in which they were found for clues (is walking on two feet, at certain speeds and during certain times, energetically less costly than walking on four feet? Was the environment in which walking evolved aqueous or not? Did walking evolve at the same time as speech organs? Etc.), one needs to make a clear balance of costs and benefits for the proposed function, making sure it is positive and checking whether the function is not a secondary function on the basis of selection pressure on other things. Finally, one needs to find the intermediary stages of the evolution of the ancestor without the property to the current organism with this property, checking along the way whether the evolutionary balance for *each step* remains positive too (for instance if walking evolved for reasons of speech, an initial freeing of the hands for tool use could be exapted in even more freedom of the hands for manual signing which started to interfere so heavily with tool use that speaking evolved, etc.). This is really a lot of work and it will often be impossible, due to gaps in the fossil record and other reasons, to find a completely satisfying explanation that is beyond any doubt. The only thing this discussion thus wants to emphasize is that true Darwinian explanations are not of a freely floating tautologous nature; they are difficult and require a lot of research. The more proximate mechanisms confirmed and the more alternative propositions falsified (including non-functional or secondary functional ones), the stronger a specific Darwinian explanation becomes.

If we look again at the five criteria – accuracy, consistency, scope, simplicity, and fruitfulness – we now see their use. A specific Darwinian explanation should be consistent: it should for instance not presuppose the pre-evolution of a trait that has no function or it should not continue to hold that something is an advantage (e.g., walking upright for walking faster using less energy) when it is shown that it is disadvantageous; accuracy: the explanation that can better account for the order of the fossils is the better one, etc.; simplicity: a simple explanation is better (at first at least) than a complex, many-step one such as that walking evolved as a secondary consequence of speech; scope: when an explanation explains a functional trait *and* its maladaptive properties as well as secondary developments which could as yet not be explained functionally, its scope is better than that of a competitor that only gives an explanation for the functional dimension of a trait, etc. And finally, fruitfulness: an explanation is often fruitful when it does better than its competitors at the four above criteria. When it fits in a consilient view of other functional traits and gives rise to new hypotheses that also fit into its consilient scheme, it is even fitter. Thus, in the case of specific Darwinian explanations, the five criteria described above can be used to compare competitors.

## §1.4 The ideal Darwinian explanation

Given this long discussion we can now postulate what the ideal Darwinian explanation in biology looks like:

A specific Darwinian explanation in biology is ideally:

- a non-tautologous (and the more informative, the more proximate mechanisms it describes, the better)
- historical narrative (that can sometimes be extended to the future)
- that uses the three key concepts of replication (with inheritance), variation, and selection in the following way:

- replication: *Darwinian replication in biology is the temporal process in which one or more initial relatively complex but stable tokens of a certain kind (called replicators) become two or more either similar or highly similar tokens of the same kind (and their intermediate forms) that at least in principle can themselves be the subject of replication too in such a way that i) they form univocal (usually vertical but possibly sometimes also partly horizontal) lineages and sometimes branches of generations of replicators and that ii) the replicators causally determine, either directly or indirectly, their own replication process in a systematic way*

- variation: *variation of a trait in biological evolution consists in 'continuous' or 'discrete yet small' differences of a certain biological property between two or more individuals of the same kind, that are mechanically caused by differences in the replicators or the environment. While using this definition, two things have to be taken into account: 1.) usually only the differences in the replicators are heritable (and are thus the true object of adaptive selection), 2) the 'kind' that the definitions speaks of is determined by a pragmatic combination of cladism and some typological parameters.*

- selection: *selection consists in repeated cycles of replication, variation and environmental interaction so structured that environmental interaction causes the fitness of traits to be increased (and thus replication to be differential) or acquired fitness is maintained*

- fitness: *a trait A is fitter than trait B if the trait A confers upon its bearer(s) at level X a net heritable advantage expressible in trait-dependent currencies usually given by the proximate mechanisms involved, an advantage which is ultimately thought to be related to the expected number of offspring or a representable measure thereof over a number of generations or within a fixed amount of time and in a 'fixed' abiotic and biotic environment, whereby the representable measure of offspring, the number of generations or the time period and the environment are chosen in a trait-dependent relevant way.*

- to explain (the origins of) natural design and its less-than-optimal maladaptive traits (which includes a cost-benefit analysis of the trait)

- in a way that is on the whole better than its competitors (i.e., other specific Darwinian explanations) on the factors of accuracy, consistency, scope, simplicity, and fruitfulness,

- whereby its success and acceptance is further dependent on sociological factors and discoveries and developments in other branches of science, especially regarding consilience.

This ideal can almost never be attained, and we will often find that some elements are lacking in concrete explanations. Of course in general, the more elements missing, the worse the explanation. However, lacking some elements is worse than others.



Being tautological is a mortal sin for a Darwinian explanation, and an explanation that lacks one of the three key principles is also not a true explanation. However, we cannot simply arrange the specific Darwinian explanations from best to worst, since there are too many characteristics the ideal has and it is hard to determine in general whether an explanation that lacks a certain element 'a' is worse than an explanation that lacks element 'b': is an explanation that does not specify the exact details of the reproductive mechanism better or worse than an explanation that only gives a qualitative instead of a quantitative measure of fitness, and again is this explanation better or worse than one that fails to give an explanation for the suboptimal or maladaptive elements of a trait? Etc. Thus the ideal portrayed here is like a mountain peak that can be reached from various directions when new elements are added to existing explanations (although hardly ever will an explanation reach the ideal). The five factors – accuracy, consistency, scope, simplicity, and fruitfulness – give independent criteria to judge how far away different Darwinian explanations are from the ideal relative to each other.

## §1.5 Sexual selection

*In this section we will describe the theory of sexual selection. We will use it as test case of our ideal as set forth in the previous paragraph, but we will also describe it because it is an important evolutionary theory in its own right that is often invoked outside of biology as our ability for language is often explained as product of sexual selection: e.g., men who could sing and talk attract more females or men who could talk better could use more concepts and devise better strategies and thus their intelligence let them outsmart their competitors etc. We will discuss sexual selection extensively as the concept of sexual selection is far from univocal, although the plurality of its meanings is not always recognized (we know of no account that comes even close to distinguishing the cases we do). This is important because when a linguist sees the origins of language in sexual selection it makes a huge difference how this sexual selection is understood: as ornamental selection or as for instance intra-sexual male-male selection. Some researchers as well as Darwin himself saw sexual selection as different from natural selection. However, we will show that all cases of sexual selection are just simple cases of natural selection, except ornamental selection. We will finally demonstrate that ornamental selection is principally less well corroborated than simple instances of natural selection.*

The theory of sexual selection was Darwin's second major achievement in biology. It states that the frequency of traits can increase or decrease depending on the attractiveness of the bearer. The bigger, latter half, of his *On the origin of man and Selection in relation to sex* (1871), a title often abbreviated as *On the origin of man*, deals with sexual selection. Without overstatement it can be said that the late Darwin spent far more time on this theory of sexual selection than on his early theory of natural selection. Some speculate that this is the case because Darwin and Wallace co-discovered the theory of natural selection (though their theories have subtle differences), whereas the theory of sexual selection was Darwin's own brainchild. Helena Cronin in her excellent *The Ant and the Peacock: altruism and sexual selection from Darwin to Today* (1993) sketches the discussion between Darwin and Wallace on sexual selection, a theory the latter never acknowledged.

Cronin also shows that the interest in sexual selection has always been overshadowed by the research on natural selection; only recently the theory of sexual selection made a strong comeback, also in the theories about Darwinian selection outside of biology that we are interested in in this book. Part of the earlier disinterest in sexual selection was due to social factors: Darwin's theory seemed to imply that females had an active role in mate choice, a thought Victorian England was not ready to accept (had they but read Jane Austen...).<sup>229</sup> But lack of interest was also due to scientific reasons: a) the theory had a limited realm of application; b) the theory seemed to undermine Darwin's own theory of natural selection; and c) the theory seemed to presuppose principles that were problematic from a scientific or naturalistic point of view. We will first delineate more precisely what sexual selection is about and then return to these problems.

The term 'sexual selection' is ambiguous. It normally refers to mate choice (usually by females) on the basis of attractiveness, but it can also refer to some related phenomena. We will look at four of these in more detail.

i) First of all and in the most general sense, 'sexual selection' can mean selection of organisms on reproducing sexually or not (i.e., cloning). This phenomenon is still poorly understood, although a lot of ethological and morphological information on the huge variation in sexual behaviour and gender now exists. The origin of sex in single cell organisms (i.e., fission in which parts of DNA are exchanged) is unclear, but the existence of sex in multicellular organisms in which the production of gametes is separated from the evolution of other cell lines by the famous Weismannian barrier is a far bigger puzzle. In fission an exchange of DNA takes place in which the net amount of DNA remains constant; from the gene's perspective the exchange is in first approximation a neutral process: a piece of DNA is indifferent to the fact whether it is replicated by the machinery of one cell or another.<sup>230</sup> From the gene's perspective, however, the existence of sex in multicellular organisms is much more puzzling because in the process of meiosis all gametes are left with only 50% of the DNA of their progenitor; from the gene's point of view the cost of sex is thus 50%, which poses two questions: 1) how could the complex machinery of sex (meiosis) have arisen in the first place? 2) Why does sex not go extinct in favour of asexual reproduction, i.e., what 200% benefit does sex bring to an organism today to outweigh the cost of sex?<sup>231</sup> The two questions are related but different: the benefits sex confers to organisms today can shed light on the question why sex arose in the first place, although this need not be the case: sex today can have a very different function or be the result of luck if, like the little fins of whales, it once had a function but now remains because of developmental constraints.

The latter case is implausible because we find at quite a few unrelated final branches of the phylogenetic tree of higher organisms that sex disappeared in favour

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<sup>229</sup> Historians have also noted that Wallace had previously had his own problem with 'female choice': he had been left at the altar by a woman of a higher social class.

<sup>230</sup> In second approximation the pieces of DNA shared by fission are of course not indifferent to fission: the machinery of one cell can be a much faster replication system than that of another, so that a shared piece can be copied much faster than before. The pieces of DNA shared in fission are of course also transcribed in the cell and, when they code for an enzyme that upgrades the cell machinery of their new host, fission can result in more efficient replication of this piece of DNA. Recent speculations on the origin of fission take the DNA point of view as they suggest that fission is the result of transposons (pieces of selfish DNA that copy themselves from one part of the genome to other places) that manipulated their original host to fuse with a new cell in order to spread themselves virus-like within the DNA of a new cell.

<sup>231</sup> In fact the costs are even higher than 200%: an organism that clones itself need not spend time and energy looking for a mate.

of parthenogenetic reproduction, as we find in lizards (Ridley 2005: 312). Hence, sex can disappear and initially asexual reproduction can spread quickly because of the 200% advantage; yet in the long run asexuality seems to be an unfavourable mutation because, although the mutation can easily occur, it is not widespread in the phylogenetic tree of higher organisms and where it occurs it is but a recent mutation (hence one that has not stood the test of time). This finding easily leads to a group selectionist's account of the benefits of sex to the group: for the individual, asexual reproduction may be advantageous, but for the group (or species) not. The advantages to the group would be genetic mixing so that populations can evolve faster (for instance to fight off parasites) or to remove deleterious mutations from the population.<sup>232</sup> Nevertheless, since group selection is controversial (see §1.23) researchers have kept searching for advantages of sex to the individual or the gene. The reasons sex is said to confer benefit to the individual partly overlap with the advantages of sex for a group (getting rid of deleterious mutations etc.); other reasons mentioned are the possibility to repair DNA damage (called double strand repair), and the lottery argument: to win the jackpot you have better odds if you buy 50 different tickets rather than 100 of the same ticket. The question what benefits sex confers *today* is still a very fruitful research program; theories about the evolution of sex as such, from the stage of single cellular fission to multicellular meiosis, are still highly speculative.<sup>233</sup> Note that without the existence of sex as such, sexual selection in the more narrow sense we are interested in here can of course not occur.

ii) 'Sexual selection' can also mean the process in which *gender* arose and in which soon intrasexual competition arose. Originally, sex took place between hermaphrodites inseminating each other. The gametes were isogametes: both partners contribute the same amount of food and energy to the gametes for the development of their offspring and these gametes thus had the same size. (Think of organisms in which there is no parental care, which would only complicate the picture.) Later on, gametes became specialized and anisogamous, some big and immobile (female eggs) others small and mobile (male sperm). This distinction between males and females by the size of their gametes is better than all other general criteria to distinguish males and females: aggression might be a characteristic of males, but female swans are much more aggressive than their male counterparts; average bigger size might be a characteristic of males and caring might be seen as a characteristic of females, but the male sea horse is the exclusive parent that cares for the babies. The type of gametes is, however, so universal, that it could be taken as a measure to determine which of the two genders is male and which female.

How anisogamy arose is uncertain<sup>234</sup> and why there are only two genders too<sup>235</sup>, yet given anisogamy, secondary characters may evolve and these can be the

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<sup>232</sup> Sex can of course also result in more deleterious mutations: if you mix Albert Einstein with Marilyn Monrow you can, as it were, get an ugly and stupid as well as a beautiful and intelligent human being. By mixing, both good and bad mutations can accumulate; however, the force of selection on bad mutations can be stronger (because not all organisms have this mutation in a population as in local asexual populations) and thus they can be removed more easily from the gene pool.

<sup>233</sup> For a speculative yet informed reconstruction, see the work of Maynard Smith and Szathmáry (1999: 87ff and 1995).

<sup>234</sup> There are mathematical models that suggest that adult size is the key parameter making the labour distribution involved in anisogamy worthwhile.

<sup>235</sup> There is a lot of variation in nature; why are there only two types of gender, i.e., two basic types of gametes that can fuse with each other? Why not three types or four or hundreds of basic types, where not necessarily every gamete needs to be able to fuse with all others? The answer to this question is still rather speculative; one answer given relates the two genders to the inheritance of other cell bodies containing

object of sexual selection in a more narrow sense. The central logic is that eggs are costly to produce and sperm cheap. Males will thus develop characters to spread as much sperm as possible, females characters to detect which male can offer the best investment in their future offspring. Due to these different gender interests *intersexual* competition and conflict may arise. Take for instance the case of fruitflies: “Male fruitflies seem to transfer chemicals with their sperm that act as hormones in the female and accelerate egg production. The accelerated egg production may not be in the interest of the female. Her optimal rate of egg production will be some trade-off between her survival and reproduction. (...) The male gains extra eggs in the short term, at the cost to the female of reduced lifetime fitness (Ridley 2005: 336).” In a well-known series of experiments Holland and Rise (1999) tested this reasoning experimentally. They bred for monogamous fruitflies and as predicted the male fruit flies evolved to be less harmful to their mates and females evolved to be less resistant to males. Also the total reproduction output per female increased; Holland and Rise calculated that the conflict between males and females reduced the fitness of the average member by as much as 20%. Intersexual competition is thus bad for the species as such and this again speaks against group selectionists’ accounts of sexual selection.

iii) Sexual selection can mean intrasexual selection when partners of the *same sex compete* for access to mates. The traits selected for in this process are called weapons or ornaments. Intrasexual selection on ornaments (by female choice) is the topic we are interested in below; intrasexual selection on weapons is also called ‘male to male combat’ selection. Darwin already distinguished these two forms of sexual selection. In the latter, *males* are the ones developing weapons.<sup>236</sup> This is in conformity with Bateman’s principle, which states that the sex that invests most in the offspring is the limiting resource and that the gender that invests less will be the one developing weapons. Since the limiting resource is most often females, males develop weapons (and ornaments).<sup>237</sup>

Interestingly enough, however, the weapons are not used very often: fights are relatively rare and the fights that are fought are hardly ever lethal (a fact widely known ever since Konrad Lorenz’s famous 1966 pioneering study *On Aggression*). This is because fights are costly; in a fight both fighters can get hurt, so that a third may take the prize. Taking this fact into account it becomes likely that the weapons are not only meant for actual fights, but also for preventing fights, i.e., weapons are used as a signaling device amongst males to create a dominance hierarchy, also

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DNA, such as the mitochondrion and chloroplasts. Such cell bodies can only be inherited from one of the parents; otherwise this would give rise to selfish cell bodies, a process that might kill a cell. “Given the need for uniparental inheritance of organelles, it is hard to evolve more than two mating types, one that provides the organelle and the other that does not (Maynard Smith and Szathmáry 1999: 92).”

<sup>236</sup> See Darwin, 1871 “The sexual struggle is of two kinds: in the one it is between the individuals of the same sex, generally the males, in order to drive away or kill their rivals, the females remaining passive; while in the other, the struggle is likewise between the individuals of the same sex, in order to excite or charm those of the opposite sex, generally the females, which no longer remain passive, but select the more agreeable partners.”

<sup>237</sup> Actually Bateman’s original principle explicitly says that men are the gender developing weapons and ornaments, because females are the limiting resource. The most well known exceptions to Bateman’s principle are the existence of sex-role-reversed species such as pipefish (seahorses), phalaropes, and jacanas, in which the males perform the majority of the parental care while the females are highly ornamented and territorially aggressive (Knowlton 1982, Berglund et al. 2005). One could argue that since in these species males are the limiting resource these counterexamples do not so much rebut Bateman’s principle as affirm the general logic behind it. A better counterexample are female spiders with their big weaponlike teeth used to behead and eat males for their proteins after copulation.

known as a pecking order, without unneeded detriment and fatality. That is why weapons *used in intrasexual selection* are not hidden from sight, but clearly displayed to the world. Their evolution can simply be described as natural instead of sexual selection. However, since it is likely that if females choose men, they will choose them on the basis of the dominance hierarchy and thus indirectly on the basis of their weapons, weapons can become the subject of sexual selection in the narrower sense as well. Recent research on dinosaurs has shown that many organs previously seen as weapons such as the horns on the noses and their panzer-like skins are actually too fragile to serve as weapon and can better be described as ornaments. These secondary characters are then the subject of sexual selection in the narrower sense we are concerned with here. Thus *sexual dimorphism*<sup>238</sup> – males and females looking differently – is a necessary, but not a sufficient condition for sexual selection in the narrow sense. Note that intra-sexual selection is not restricted to males.

iv) Sexual selection can also mean gamete selection. Gamete selection can be the result of the male- or female-related actions and traits. Males: when multiple males copulate with the same female, the amount, quality, and packaging material of male sperm will be selected for (a process called sperm competition). Related to this phenomenon is the selection pressure on the size and shape of the testes: promiscuous chimpanzees have larger testes relative to body mass compared to polygynous gorillas (Harcourt et al. (1981)); some male insects have some sort of spoon attached under their reproductive organ allowing them to shovel the sperm of a competitor from the interior reproductive organ of a female before inseminating her themselves.

But sperm selection is not just a process between males; females also have a part in sperm selection. Recently, much attention has been given to *cryptic female choice*, a phenomenon in internally fertilizing animals such as mammals and birds, where a female may simply dispose of a male's sperm without his knowledge. Whereas male sperm competition could simply be described by natural selection, cryptic female choice seems to be an example of sexual selection in the narrower sense and it seems to raise complex questions such as: on the basis of what do females choose? This brings us to ornamental selection, which will be demonstrated to be less easily reducible to a case of natural selection.

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<sup>238</sup> Note that not only animals, but also plants can be sexually dimorphic, especially their flower size. Male flowers are generally bigger than female flowers. Abraham 2005, following Hermann Müller, explains this by natural selection: bigger flowers attract more insects. Thus insects will then – in general – first enter male flowers (pollinators) before they visit female flowers (pollen acceptors). Abraham's experimental data confirmed this theory. For the rest of this chapter we will exclude sexual selection in plants. These are secondary effects based on the evolution of perception devices in animals (flowering plants are a relatively recent innovation in the realm of evolution).

We can summarize this paragraph thusfar in a table:

Type of sexual selection	Subtypes	Reducible to a <i>simple case of natural selection</i>
Evolution of sex as such	bacterial sex	yes, although still incompletely understood
	meiosis	
Evolution of gender		yes, although speculative scenario's
Intra-sexual selection	Male weapon selection	yes
	Female choice and weaponry	yes
Gamete selection (intersexual selection)	Sperm competition	yes
	Cryptic female choice	yes
Ornamental selection	Fisher (run-away selection)	yes
	Zahavi (handicap selection):	no, with qualifying more "problematic" than revealing handicaps, revealing handicaps more than conditional handicaps
	a) qualifying handicaps	
	b) revealing handicaps	
c) conditional handicaps		

Table 1.6: Varieties of sexual selection

### Sexual selection as ornament selection

We now concentrate exclusively on ornamental selection such as the peacocks tale on the basis of which females choose which males to mate with. Except for being chosen by females, these traits confer no benefits to the males at the level of the proximate mechanisms involved in survival and reproduction. This type of sexual selection presupposes that there is i) sex, and ii) sexual dimorphism, and that the attractive traits cannot be explained as iii) weapons alone, and finally iv) that the traits are externally perceptible. Given the fact that sex is scarce in the complete phylogenetic tree, and that of all sexually reproducing creatures only a minor part shows sexual dimorphism, and that only few traits that are sexually dimorphic are externally visible ornaments (instead of other gender specific-traits and weapons), it can safely be concluded that sexual selection in the narrow sense is but a marginal phenomenon within the domain of life governed by the law of natural selection: the origins of most organisms and their wonderfully adaptive traits can be explained by natural selection alone; only a scarce group of traits requires the principles of sexual selection.

Nevertheless, in these cases sexual selection co-exists with natural selection, and although sexually selected traits are scarce, in *our isolated branch* of the phylogenetic tree (multicellular sexually reproducing organisms) these traits are relatively common and we need not explain how they evolved.

Natural selection explains the existence of adaptive complexity; we saw that natural selection acts not like a Panglossian architect and that the process of natural selection can coexist with neutral non-adaptive evolution. We also saw that due to path dependencies suboptimal adaptations (seen from an all encompassing God's-eye point of view) could be the *outcome* of the process of natural selection, although the individual steps leading to this outcome might have been adaptively optimal at the

time of selection. However, sexual selection seems to imply that the direction of selection is *negatively* adaptive! The peacock's tail is a nuisance to its bearer, which can hardly fly anymore. This seems to endanger it to potential predators. Hence, why did natural selection not prevent the evolution of the peacock's tail?

The first reactions to Darwin's proposal of sexual selection were indeed to deny that it existed. A lot of Darwin's examples of sexual selection concerned the bright coloured wings of male birds (in contrast to female birds).<sup>239</sup> His colleagues simply denied that these colours were the result of sexual selection and explained them as the result of natural selection. The colours of birds then either function as camouflage or as identification markers of the group or species of the individual bird. And indeed, some of the beautiful colours of bird wings can be understood as camouflage: the silver-white snow owl lives in northern icy territories, the (rose-ringed) green parakeet who found a new habitat in the Amsterdam Vondel park lives normally in the ever-green tropical rain forest, and more yellowish birds thrive in deserts. Some animal colours can also be understood as group markers; a good example are the cichlid fish that are famous for the huge number of species with beautiful colour patterns that have evolved in East African lakes (of whom many have died due to the introduction of the even more famous Nile perch into these lakes). Cichlids that are coloured differently are prezygotically isolated, but not postzygotically: cichlids of different colours do not copulate with each other but when researchers artificially inseminate female cichlids with sperm from a differently-coloured male, the offspring is perfectly healthy and fertile.<sup>240</sup> The colours thus function to identify group members. Another function of seemingly sexually-selected ornaments might have been as a disguise mechanism: the conspicuous eyespots on many species of butterflies and fishes confuse predators and we already saw that ornaments can be weapons too.

Nevertheless, some cases of colouration and the behavioural reactions depending on it can simply not be understood as camouflage or identification etc. Take again the case of the peacock. Peacocks have a highly peculiar mating ceremony called the leg. In a leg all males peacefully line up next to each other, displaying their beautiful feathers to the females; these females then come together and collectively choose one partner with whom all of them mate: a winner-takes-all situation. The evolutionary reason why all females choose the same partner (i.e., the reason why their collective behaviour is unconsciously evolved like that) is that in the next generation the only males that will mate are the ones that are chosen by all females collectively; if one female decides to mate with a male the other females do not choose her male offspring probably does not have the properties that make a male favourable to females and thus her offspring will die childless. This strange ceremony seems to serve no other function than choosing a mate and it can thus not be explained by natural *adaptive* selection.

This clear case of sexual selection begs for an explanation. How could costly 'a-functional' ornaments ever evolve? Darwin just supposed that female choice existed and used this assumption to explain a lot of data. But the real question is of

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<sup>239</sup> Darwin also observed that even if males and females looked almost alike, the colours of the males were often still a little bit brighter. Any visual ornithology guide can support this observation.

<sup>240</sup> It is sometimes said that these cichlids are on the brink of speciation, that they are already reproductively isolated and that it is only a matter of time before offspring of differently-coloured cichlids is either impossible, born dead, or infertile. Note that due to pollution the sun cannot penetrate as deeply as it used to into the East African lakes; due to this the colours of the cichlids are less obvious and they may start to mix more.

course why female choice should evolve in the first place. Two main answers have been given to this answer; one by Fisher (1930) and another by Zahavi (1975, 1997).

Fisher supposed that female choice, like any other character, can be selected for. Choice is here nothing but an automatic propensity to mate with partners having a certain characteristic. If females with one type of preference produce more offspring than females with another, selection will favour the more productive preference. The hard part is, however, to understand how female preference for a non-adaptive, costly ornament could evolve and stay fixed. Fisher was the first to realize that once such a preference had evolved, it would remain fixed. We already gave the argument Fisher formulated above: when the preference is present no female should mate with an unfavoured male, lest her male offspring die childless.<sup>241</sup> Fisher's problem remained thus only to elucidate how the preference evolved in the first place. Here Fisher presupposed the theory of natural selection; he believed that all ornaments began as natural adaptations that were preferred by females: longer tails might have had an initial natural advantage and thus females with a preference for this characteristic had an advantage too. The tails of males thus grew longer and longer until growing longer tails was no longer beneficial. At that point, however, female preference had long started to lead its own life, forcing male tails to get even longer and longer although this decreases their fitness. This process will not end until there is a balance between the decrease in fitness and the increase in replication chances of males; most ornaments of species today are at this equilibrium point.<sup>242</sup>

Fisher called the process just described run-away-selection, which Van Valen nick-named the 'red queen effect' after the red queen in Alice in Wonderland. Just as Alice cannot outrun the red queen since every time she reaches the former location of the red queen the queen is already one step further, so female preference is never satisfied and forces males to grow longer and longer tails. Note that this presupposes that female preference is not absolute (this or that absolute tail size), but relative (the male having a longer tail than the others); and this in turn presupposes enough male variation over relatively long periods of time.

Fisher's theory of sexual selection is in fact – although this is seldom stressed – just a version of *natural selection* with historical lock-in. The other theory that explains sexual selection comes from Zahavi. Contrary to Fisher, Zahavi introduces a new principle next to natural selection: the 'handicap principle'. Zahavi starts his theory with the observation that in sexually reproducing creatures, especially large mammals, all males are usually fairly well adapted to their natural surrounding and that many will survive into adulthood. The biggest competitors of these males are thus often not natural elements or other species, but males of their own species. This competition with their congenitors is about access to mates. From the female perspective choosing a better mate than average becomes hard when many males survive into adulthood. They might for instance opt for strong muscles, a genuinely good quality for a female to select. "But then what is to stop males from growing dummy muscles with no more real substance than human padded shoulders. If it costs a male less to grow false muscles than real ones, sexual selection should favour genes

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<sup>241</sup> Choosing for long-tailed males, when all other females do this, despite the fact that long-tailedness reduces the natural fitness of a peacock is an example of an Evolutionary Stable Strategy.

<sup>242</sup> See R.A.Fisher 1930: 16 "It is important to notice that the conditions of relative stability brought about by these or other means, will be far a longer duration than the process in which the ornaments are evolved. In most existing species the runaway process must have been already checked, and we should expect that the more extraordinary developments of sexual plumage are not due like most characters to a long and even course of evolutionary progress, but to sudden spurts of change."



for producing false muscles (Dawkins 2006: 159).” This in turn would lead to counter-selection of females who can see through the deception, so that a man must not just “*seem* a good quality male: he must really *be* a good quality male, otherwise he will not be accepted as such by skeptical females (idem).” So far so good. This part of Zahavi’s theory can easily be integrated into Fisher’s theory.

Zahavi’s next step in which he proposes his own ‘handicap’ principle is, however, much more problematic; Dawkins once called it a “maddeningly contrary” principle.<sup>243</sup> Zahavi wants us to believe that in sexual selection costly ornaments (i.e., handicaps) evolve *because* they are handicaps. A peacock with a long beautiful tail displays to females that he can survive, *in spite* of his tail. Females thus choose him. This handicap can be called a *qualifying* handicap: only if a peacock is really successful, can he survive with this handicap that increases his visibility to predators and prey, lessens his chances to get away from the former, and decreases his chances to catch the latter. His tail thus signals his over-all quality and that is why females choose him.

Zahavi’s original theory has been updated by some to include *conditional* or *revealing* handicaps. Conditional handicaps are handicaps that only develop in high quality males (the presence of the handicap thus reveals information about the genetic quality of the male); one can think of highly symmetric faces and shining feathers that males only develop if they are able to fight off parasites and if their genome is free from deleterious mutations (remember that these were reasons why sex was said to have evolved in the first place).<sup>244</sup> Revealing handicaps are handicaps in which a male shows he is able to perform onerous tasks that reveal his high quality. One can think of the time- and energy-consuming courtship rituals some species entertain.<sup>245</sup>

Qualifying handicaps are more problematic than revealing handicaps, revealing handicaps more than conditional handicaps, although it is sometimes hard to distinguish them.<sup>246</sup> In theory, qualifying handicaps suggest that females do their best to choose males with one leg and one eye, because if they can survive, they must be really good! Nevertheless, even the evolution of revealing and conditional handicaps is far from easy. Why would a male not reveal his superb quality by approaching a predator playing “see-if-you-can-catch-me” to show off to a female – a thing which is extremely unwise to do!<sup>247</sup> Of course, the structure of a Darwinian

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<sup>243</sup> See also Kirkpatrick 1986. Note that Dawkins’ views have changed substantially in favour of Zahavi. See Dawkins 2006: 309ff and below. His change of minds rests on the work of his Oxford colleague Grafen (1990ab). However, the work of Grafen is itself a matter of controversy. See footnote 253.

<sup>244</sup> See also Møller 1991.

<sup>245</sup> Such as the ‘Bright male hypothesis’ of William Hamilton and Marlene Zuk, suggesting that male elaborations might serve as a marker of health, by exaggerating the effects of disease and deficiency.

<sup>246</sup> Qualifying handicaps are equally costly to all males; only for strong males is it less problematic to bear the costs; conditional handicaps are less costly to high-quality males (their genes are such that they fight off parasites without a problem), and in the case of revealing handicaps the best male is able to make the highest costs.

<sup>247</sup> This handicap is less harmful if only males develop it. (Many organs evolve immediately in both genders, even gender-related ones such as nipples.) Sexual selection by female choice forces males to evolve risky strategies. This is by the way not because males are more interchangeable than females: one male can inseminate many women, whereas women can give birth to only a limited number of offspring, hence a few males more or less would not matter. On the contrary, one of the best-understood elements of sexual selection is the fixed 50/50 gender ratio we almost always encounter. The reason for this is that, whereas from the species perspective fewer males would be better, the individual (or gene) selection favours the 50/50 ratio. If one imagines that in a population females beget only females as a strategy to increase their number of children, a mutant female who will beget more males has in such a scenario sons that can impregnate many females. These in turn have many ‘mutant’ sons and soon the population will be balanced again. (The argument works the same for a population of females who beget more sons.) R.A. Fisher already gave this argument in his famous 1922 paper.

explanation is in general *backward looking* (see §1.33), and thus a Darwinist need not explain why this dangerous behaviour did *not* arise; he only needs to explain why the existent handicaps did evolve as they did. Nevertheless, future scenario thinking is often possible in the case of natural selection, because of the strong constraint of functional adaptation.<sup>248</sup> This constraint is not so strong as to uniquely determine *future* evolution and with this constraint alone many past scenarios can be thought of. Sometimes many functional scenarios exist to explain the evolution of a certain organ, as we saw in the case of walking upright, for which alone thirteen genuinely different explanations were given. These explanations were in *principle quantifiable and testable*. The case of sexual selection is, however, different; there are many more handicaps than adaptations (or put otherwise: many more deleterious mutations than profitable ones) and these are but weakly constrained since handicaps are said to evolve *because* they are handicaps; only when a handicap is too severe to make life possible can it be excluded from theorizing. Predictions about future selection are hard to make in the case of natural selection, but virtually impossible in the case of sexual selection.<sup>249</sup> It is in this vein that Dawkins remarks: “I find the prospect [of Zahavi’s theory being true] rather worrying, because it means that theories of almost limitless craziness can no longer be ruled out on commonsense grounds (Dawkins 2006: 311).” Examples of such crazy theories abound. Take for instance Joe Abraham’s (1998) *female sabotage hypothesis* which says that females choose to mate with males who are unlikely to survive because of their burdens. By doing this females ensure that as the males die, more food and other resources will remain for them and their young, whereas at the same time – as a secondary effect – their risk of being hunted decreases if males’ burdens are more likely to draw the interest of local predators...<sup>250</sup> Or the theory of Ryan and Rand (1995) that male frogs exploit the senses of females by producing irresistible sensory sensations so that they cannot refuse to mate.

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This balancing mechanism is so strong that even small deviations from the 50/50 ratio are informative of some special processes going on. At birth, human males outnumber females, even in countries where infanticide in favour of boys can readily be ignored. This is because human sperm containing a shorter Y- instead of an X-chromosome is a few percent less heavy and thus more mobile; the length of a Y-chromosome, which its many repetitive gene sequences (also needed because no X-copy can be used for comparison) can be seen as an adaptation: males die more often before entering reproductive age. Sometimes the sex ratio is strongly askew. For instance when older children help their mothers in raising younger offspring (so-called ‘helpers at the nest’), the sex ratio is adaptively biased in favour of the gender that invests most in offspring (usually females). See also Matt Ridley 1993 and Mark Ridley 2005: chapter 12.5.2.

Note that in less technical literature one can often find arguments for risky human male behaviour that are based upon some version of the redundant male hypothesis. Since one male can impregnate many women they could evolve more risky behaviour so that a few of them can die without any consequences for evolution. See for instance one of the best known authors on popular science and biology, Desmond Morris – writer of *The naked ape* – in his recent *The naked woman* (2005), chapter 1. This group selectionist account seems to be wrong, since the unit of selection is not the species but the (gender specific) individual or his genes unless one can give very strong reasons why in this case group selection is stronger than intrasexual selection (a thing that is hardly ever done).

<sup>248</sup> We understand here natural selection as functional (i.e., adaptive) selection. For a critique of the critique of the adaptationist program by Gould *et al.* See §1.23.

<sup>249</sup> Scenarios about sexual evolution in the past become arbitrary in the strong sense as well. Of course the actual past evolution cannot be different from what it was, but the reason why once this sexual ornament evolved instead of that one becomes rather arbitrary.

<sup>250</sup> Note that the female sabotage hypothesis of sexual selection actually turns sexual selection on ornaments into natural selection between the sexes (intersexual selection).

*Individual* theories informed by the handicap principle can sometimes be tested; the theory that says that males display their shiny feathers to demonstrate that they are parasite free and thus good quality partners, can be falsified if it turns out that these males have just as much or even more parasites. It can also be falsified if it turns out that the females do not choose males on the basis of the shininess of the feathers. Every individual example of the handicap theory thus needs to show at least two things: a) that the handicap is indeed a handicap (or costly ornament), and more in particular whether it is a conditional, qualifying, or revealing handicap<sup>251</sup>, and b) that the females do indeed choose males on the basis of this handicap. These are factual matters that can be tested in a controlled laboratory environment, but it is hard to combine them in a total fitness measurement (the fitness decrease due to the handicap plus the fitness increase due to female choice). However, similar things often hold for the evolution of normal organs: they bring both costs and benefits.

The reasons *why* handicaps evolve, however, are even harder to test. Experiments on the type of handicap might tell one something about this, although not much. A function (as in sexual selection) is clearly a function *for* something, but a handicap is not so clearly a handicap *for* something. In first approximation a handicap is *just* a handicap<sup>252</sup>; only in second approximation is a handicap something that allows you to do or show something. In second approximation something a-functional becomes again functional and one could say subject to natural selection. The exact function is hard to discern, whether it is female choice, female sabotage, irresistible male signaling, male showing-off, etc.

We do not know any empirical study that tries to single out the exact (secondary) function; the little empirical work that exists on ornamental selection suggests that female choice is influenced by genetic quality. Welch et al. (1998) performed a nice experiment with frogs; they fertilized half of female's eggs with sperm of preferred males, and half with sperm of unpreferred males: the offspring of preferred males had higher fitness. More empirical work is needed. There is probably not a one-size-fits-all explanation for all instances covered under the name of ornamental selection. The fact that one can postulate a disadvantage with a positive side, means that crazy – hardly constrained – scenarios remain possible as there are virtually no constraints on what type of handicaps evolve and not on how these handicaps turn out to be an advantage after all.<sup>253</sup> In the end, only fitness counts and

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<sup>251</sup> A possible test is to investigate whether all or most males display the trait almost equally (a thing conditional and revealing handicaps forbid).

<sup>252</sup> Due to a single handicap, there are many things one can no longer do: having only one eye not only decreases one's field of sight, but makes 3D-estimations more difficult, seeing a task that tires one sooner, etc. Nevertheless, often a handicap is a handicap of a certain organ (that had a certain function); in such a case a handicap can be said to be primarily a handicap to the function of that organ; sometimes a handicap is itself a whole new organ, such as the horns or the noses of brontosaurus that are too weak to fight with; in such a case the horn is a handicap to the functioning of more or less the whole organism.

<sup>253</sup> The only theory we know of that gives strong criteria that constrain sexual selection is Alan Grafen's ESS version of Zahavi's work (Grafen 1990ab) in his *Sexual selection unhandicapped by the Fisher process*. In this theory males have 'knowledge' of their own fitness and choose to display the truth about their fitness or not; females in turn can opt to believe, partially believe, or disbelieve males' signaling. Grafen found out that if males signal truly (hence if they are truly fit they grow the best ornaments) and if females would blindly believe males this could be an evolutionarily stable strategy (ESS). ESS models are normally used in ethics to explain cooperation and the persistence of cheating; in fact, Grafen's ESS model transforms sexual selection into a complex theory about cooperation (truth-telling) and cheating.

The strong constraint of Grafen is thus that handicaps are truly handicaps. However, Grafen's model suffered from some mistakes (Siller 1999, Grafen 1999). Siller showed that there are multiple stable

a Darwinian a priori concludes that somehow a handicap must have an advantage (or have had an advantage once). Nevertheless, it remains to demonstrate how exactly a handicap ultimately contributes to fitness. The same holds for natural selection: adaptation can be equated with increased fitness, but that does not free one from showing what good a particular adaptation is for (what function it serves).

Insofar as the process of sexual selection is much faster than that of (normal) natural selection (as it *should* be because of the *positive* feedback-loop between male characteristics and female preferences) the unconstrainedness is actually not a defect of the theory of sexual selection, but a necessary presupposition for it. We saw that both Fisher's run-away selection and Zahavi's handicap principle can account for the positive feedback loops. But there are differences. On Fisher's account the male character was an advantage at first, whereas Zahavi needs the character to be costly right from the start. That is why Fisher – in concrete examples of sexual selection – always has to show what initial advantage a trait served, a task Zahavi can bypass. Finding an initial advantage for such extreme ornaments as golden feathers seems hard. Zahavi on the other hand has the problem of a two-fold analysis of handicap and benefit which does not have many constraints.

There is little empirical research on whether Fisher or Zahavi is right. Mark Ridley states that Fisher presupposes that (a property of) an ornament such as “male tail length is heritable (2005: 334),” whereas “in Zahavi's theory, the advantage of female choice does not depend on the inheritance of male character [so that] (...) choice could be maintained even if all members of the population had the same genes for tail length.” However, according to Ridley, there is in Zahavi's theory an ‘analogous condition’: “some males must have good genes, others bad. This is a condition called heritability of fitness (*idem*).”

Ridley's book is normally crystal clear, but we find these two conditions rather vague. Does not Zahavi's theory presuppose too that tail length is heritable (how else could every male develop a tail)? One could argue that the property of ‘having a tail’ is heritable but the length of the tail not.<sup>254</sup> This is, however, hard to demonstrate: there is usually not one gene (one single piece of DNA) that exactly determines a phenotypical property. On the other hand, the analogous condition of Zahavi's theory is one that Fisher has to presuppose as well. For Fisher too some males have good and other males bad genes, and of course he presupposes the heritability of fitness, as does any theory of natural selection. If no further general criterion is given, what counts as good? Good genes are just the same as fit genes and thus the condition becomes tautologous in the Darwinian framework. Of course, Ridley may reply that the fact that a male has a beautiful tale might be due to the accidental fact that in the year of his birth there was simply more food available; his competitor might have better genes – if they were raised in the same year – yet his tail is less beautiful due to developmental problems in times of food shortage. In that case the tail is no indicator of good genes and mating with a beautiful male does not give a female fitter offspring. But this holds for natural selection as well: due to accidental (environmental) causes good genes may not be duplicated whereas some

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strategies of trusting and lying that are in unstable competition with each other. Thus Grafen's model does not really constrain sexual selection; as such it is not superior to other theories of ‘limitless craziness.’

<sup>254</sup> The length could then be under control of the general genes for length: big animals have big tails, small animals small tails.

bad genes are. Only on average will good genes be selected for by natural selection; the same holds for sexual selection.

Finally, the two conditions face a common difficulty: while selection operates on a character, it reduces its differential heritability: since natural selection causes beneficial (or 'fit') features to replace their disadvantageous counterparts, the beneficial features become increasingly more common with each generation and thus the *differential* heritability decreases. In sexual selection, due to female choice in which many females can choose the same partner, the decrease in differential heritability is even stronger. This phenomenon is also known as koinophilia: it can be assumed that disadvantageous features – due to natural selection – are rare in a population; a sexual creature, therefore, wishing to mate with a fit partner, would be expected to avoid individuals sporting unusual features, while being especially attracted to those individuals with a predominance of common or average features: koinophilia.<sup>255</sup> Koinophilia makes experimentally testing the two conditions involving differential heritability even harder.

We thus think both Fisher and Zahavi need to presuppose that some heritable male characteristic increases his fitness (in the first sense of fitness as the number of offspring). Ridley mentions some general empirical research that tests sexual selection. For instance sexual selection requires open-endedness and this can be tested. Swallows whose tails had been artificially lengthened by a researcher were chosen by females instead of normal swallows (Møller 1994). There is some research too on the costs of an ornament as well (Ridley 2005: 333). No research, however, effectively distinguishes between Fisher's and Zahavi's theory.

We concluded in the paragraph on the testability of adaptive explanations that depending on what counts as tested, Darwin's theory has been confirmed ever since Darwin, since Mendel, since the synthesis of Darwin and Mendel, since the modern synthesis, since the discovery of DNA, since quantitative Darwinian models, since studies in the wild etc., or that it is still not confirmed. The same can be asked about sexual selection. When is sexual selection confirmed? When we exactly understand how sex arose in the first place or why there are (only two) genders? When we can clearly (quantitatively) distinguish between intrasexual and intersexual selection? When we can exactly distinguish how much of an organ can be explained by natural and how much by sexual selection, as in the case of weapons? When we know whether Fisher or Zahavi's theory is right? When we can find some general criteria to prefer one of the many versions of Zahavi's theory? When the differences between initial destabilizing ornament selection and stabilizing koinophilia are taken into account? When the theory is not just a model but also tested in the wild? Etc.

Our conclusion in the case of natural selection was that all answers could be defended. We stressed, however, that we believe that the current wild growth of

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<sup>255</sup> Koinophilia has as an important side effect that mates displaying mutant features (the result of genetic mutations) are also avoided. This, in itself, is also advantageous, since the vast majority of mutations are disadvantageous. Since it is impossible to judge whether a new mutation is beneficial or not, koinophilic creatures will avoid them all with equal determination, even if this means avoiding the very occasional beneficial mutation. Thus, koinophilia, while not perfect or infallible in its ability to distinguish fit from unfit mates, remains far and away the best bet strategy when choosing a mate: it will be right far more often than it will be wrong. And, even when it is wrong, a koinophilic choice always ensures that the offspring will inherit a suite of tried and tested features.

There is some tension between the two concepts of sexual selection: ornament selection and koinophilia. Whereas the first can be used to understand the origin of secondary sexual organs, the second can be used to understand the permanence of these organs. As such they are two aspects of the same process of sexual selection: the first destabilizing, the second stabilizing.

competing evolutionary explanations for all sort of natural phenomena (for instance for walking upright) have shown that giving a Darwinian explanation is hard and requires a lot of empirical research, preferably quantitative to show that something is truly an adaptation for something. Looking back at sexual selection, we think we should conclude that little empirical research on sexual selection can as yet live up to this standard. Given the special nature of sexual selection in which a seeming disadvantage turns out to be an advantage, it is probable that it will take quite some time before the research on sexual selection becomes as good and trustworthy as that on natural selection.



## Chapter 2: Darwinian explanations of culture

*In this study we concentrate on Darwinian explanations of language. What Darwinists or other scientists mean by a “Darwinian explanation of language” is often not specified. However, as we will see in chapter 4, they consciously or unconsciously apply a certain “Darwinian model of culture”: a model of how the use of Darwin’s theory can be extended from the realm of nature to the realm of culture. The analysis and evaluation of Darwinian explanations of language is greatly furthered if the model to be used is clear from the start. We start with a discussion of the concept of culture, which we define as all non-genetically determined behaviors, ways of life and beliefs as well as the artifacts and institutions of a population that are passed down from generation to generation. We discuss the elements of this definition taking the results of the study of animal culture into account (§2.1). After that we turn to ideas in the existing field of evolutionary studies of culture, into which Darwinian approaches have been expanding (§2.2). Finally (§2.3), we discuss 8 evolutionary models of culture, 2 ontogenetic models and 6 phylogenetic ones. Some of these models have been compared to each other in the literature (most often socio-biology and evolutionary psychology), but as far as we know never have so many models been compared and evaluated simultaneously. On the basis of the foundations laid in chapter 1, we identify the dual-inheritance theory as the most promising Darwinian model and update the definitions in §1.4 to include the evolution of culture (§2.4). The dual-inheritance model will play a pivotal role in our discussion of Darwinian explanations of language in chapter 4.*

### §2.1 The concept of culture

*We start with a discussion of the concept of culture i) to see the complexities of labelling a phenomenon or trait as cultural and ii) to see the important difference between human culture as cumulative and animal culture as mostly non-cumulative.*

The word ‘culture’ is a family resemblance concept and thus impossible to define explicitly;<sup>256</sup> what makes it even harder to define this word is the normative connotations it has as ‘the opposite of a primitive state’, so that definitions of culture that exclude certain phenomena can be criticised as discriminatory, racist, or speciest (unjustly favouring the human species above other living beings). Originally the word ‘culture’ had to do with the cultivation of plants, i.e. agri-culture, but it came to encompass all things that human sedentary cultures typically engage in (e.g., building houses, marriage customs etc.). Its use was later extended into the past to include the earlier mode of existence of pre-sedentary pastoral and hunter-/gatherer-cultures, but also into the future to include the culture of industrial and post-industrial society, as well as its subcultures (artistic cultures, corporate cultures, etc.). The word ‘culture’ was also used to characterize communal life of a group with a common descent, so that a group of a certain type of cells (resulted from cell division

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<sup>256</sup> An early critical, book-length review of the many different concepts of culture can be found in Kroeber and Kluckhohn 1952. The authors already distinguish more than 50 definitions of culture.



in the past) could be spoken of as a cell culture; this use is exemplified in biologist Colin Tudge's *The cultural life of trees*, a book that discusses how trees grow in groups.

Although the word 'culture' is thus impossible to define unequivocally, we need some demarcation of it to understand what the Darwinian models of culture are about. As our perspective on culture is that of Darwinism and in Darwinism the continuity of the descent from animals into humans is central, we define culture in such a way that it can encompass both human and animal culture (traditions carried on from generation to generation in animals, such as bird song dialects and the potato washing of apes). Our tentative characterization of culture is as follows: *all non-genetically determined behaviors, ways of life and beliefs as well as the artifacts and institutions of a population that are passed down from generation to generation.*<sup>257</sup> As such, it includes codes of manners, dress, language, religion, rituals, norms of behavior such as law and morality, gastronomy, arts, sports and the sciences.

We focus on two elements of this definition: a) being *non-genetically determined*, and b) *passing down* or the mode of transmission. The complexities of the modes of transmission become apparent when we discuss the controversial field of animal culture in further depth. We show that each generation of animals may learn the same things, but that we still refuse to speak of culture unless the transmission of the learned trait proceeds through imitation or emulation. We finish with c) a note on the dependency of cultural transmission on the environment.

**a) Non-genetically determinedness:** one of the key-elements of our definition is the "non-genetically determinedness" of culture. In chapter 1 under **p)** behavioral patterns we already distinguished three types of behavior: i) genetically programmed behavior, ii) learned behavior and iii) a combination of both. Both ii) and iii) are types of behavior that (partly) fall in the realm of culture. In practice, however, the three types can be hard to distinguish. It is likely that the behavior of organisms without a brain, organisms that live solitary lives and species without parental care is largely determined by genetic factors (in relation to the environment). In humans, though, types of behavior are often determined by both genetic and learned factors. The influence of each factor is an empirical question. Clearly, there are purely genetically controlled types of behavior such as grinding food before swallowing, at least at first sight, purely learned types of behavior such as dressing in black when you are a Goth or putting gel in your hair to dress up like a Punk (although all these purely learned types of behavior presuppose a lot of genetic predispositions: the genes for hair, the genes for warm-bloodedness that force us to wear clothes in Northern regions, the genes behind our need for being part of a group, the genes behind the mechanisms used in imitation, etc.). Putting the clear cases behind, we have to admit that many types of behavior are a combination of nurture (learned) and nature (genes). Examples are learning how to walk (more genetic than learned) and learning how to sing (probably more learned than genetic). In both cases we have a genetic predisposition (a whole body adapted more or less well to bipedalism and many organs ready for singing) that is fine-tuned by practice: mothers help their children learn to walk, but they only learn it by falling often and practicing a lot on their own. Mothers or others teach children how to sing, although children start to babble rhythmically with little prompting or training.

It is thus hard to disentangle genetic and learned factors, to disentangle nurture and nature. This is even harder as there are also (non-cultural) environmental

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<sup>257</sup> This definition is our own, although inspired by Richerson and Boyd 2005.

factors involved in the development of certain traits. A child may fail to learn because food shortage a few years earlier led to an underdeveloped brain, whereas his younger brother with whom he shares most of his genetic material and with whom he lives in the same house, has no learning problems at all (see c) below on the environment of cultures). It is, however, necessary to indicate some means of disentangling nurture and nature (and the non-cultural environment) if our definition of culture is to be applied. The best way to disentangle the two are twin and adoption studies, as well as genetic comparisons between people with an identified trait (e.g., schizophrenia) and those without it.<sup>258</sup> Such studies are, however, often very coarse as they cannot be done in the lab under controlled conditions (ethical considerations alone would forbid that). Further, they usually measure complex traits such as IQ, social success or language use. The complexity of these traits has serious consequences for testing whether a trait is more nurture or nature for two obvious reasons:

i) a complex trait involves many elements, each of which can be biologically determined or learned or both: take language use, which involves speaking, hearing, interpreting, reasoning, intentional goal-setting, and thus many biological organs such as ears, brains, vocal organs, as well as many linguistic elements (phonemes, phonological rules regarding word structure, individual words and grammatical construction etc., see chapter 3 for more detail). It is often hard to determine which element of behavior is learned and which is genetically determined. Take shared attention, which is so essential to language acquisition (see §3.24). Is this learned or genetically determined? How are we to dissect this complex phenomenon<sup>259</sup> into discrete elements and devise empirical tests to see which parts are genetically determined and which learned?

ii) complex traits often involve complex learning mechanisms. We already discussed the controversial Baldwin mechanism (p) iii) which relied on general learning mechanisms that over time became specialized as quasi-instinctual mechanisms. We were highly critical of the proposed mechanism behind this evolution, but the division between general and specialised learning mechanisms is important. Sometimes learning proceeds so automatically that it looks like an instinct. This can lead to strong claims about genetic innateness of *specific* abilities. But as we discuss evolutionary psychology we will see that too strong an adherence to modularism (specific genetically determined modules in the brain for highly specific functions) can be dangerous too.

Despite these difficulties, some progress can be made in disentangling them: by bigger and bigger twin and adoption studies and the statistical averages over large populations they provide, by learning more about the enzymes (and the genes behind them) involved in the development and use of traits, by learning more about brain mechanisms, and so on. The controllable lab studies on animal culture to which we turn now are also promising. Given these avenues of progress, we may hold out hope that eventually it will be possible to disentangle nature and nurture, thereby validating the “non-genetically determined” aspect of our definition.

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<sup>258</sup> Several thousands of peer-reviewed papers (accessible through for instance [www.pubmed.gov](http://www.pubmed.gov)) have identified mild to strong genetic predispositions for traits as diverse as dental caries, BMI, aging, substance abuse, sexuality, cognitive abilities, personality, values, and a wide range of psychiatric disorders. See also Carey 2003.

<sup>259</sup> It is for instance hard to determine, at least without language use as in the case of language acquisition of pre-verbal children still learning their mother tongue, whether mother and child are attending to the same thing, in more or less the same regard.

**b) The mode of transmission, the question of animal culture and the fragility of cultural transmission:** as we discussed in chapter 1, section p), cultural transmission presupposes contexts in which i) parents or other care-takers correct their children or children know how to imitate their parents while correcting themselves, and/or ii) new-born animals have ‘spare-time’ to do trial-and-error-learning. *In the former case replication of the pattern comes to a halt when children grow up without parents, in the latter this need not be the case as any animal can reinvent the trait by fine-tuning (unless developing the trait is a prerequisite for reproduction).* In the latter case, when every animal learning the trait learned it through self-directed trial-and-error rather than imitation, we can hardly speak of replicating patterns; every time it is learned, the trait is *reinvented*. One aspect of the discussion of whether animals have culture focuses on the question whether traits are truly transmitted by intentional teaching and corrections and/or imitation, or whether every animal learns the trait again by trial-and-error learning due to accidental environmental circumstances (the learning is clearly not genetically hard-coded, though a disposition to learn may be hard-coded).

Take for instance the famous potato washing of the Japanese macaques on Koshima Island. Some claim these monkeys didn’t learn this behavior by imitation, let alone by explicit intentional teaching, but by accidental trial-and-error learning: monkeys have friends or allies in whose proximity they spend a lot of time. When one monkey who accidentally learns to wash a potato and becomes inclined to take potatoes to the river, his ally may also get potatoes near the river and accidentally learn to wash them too, simply because he is in the same circumstances (or by picking washed potatoes out of the water-covered hands of his ally etc.), instead of by imitation. The latter interpretation pleads that many monkeys never learned the behavior and that the monkeys that did learn it, did not learn it overnight but after quite some time. The important question is: even though the potato washing is not genetically programmed behavior and even though it can be present in a local population of monkeys for many generations, should we call it a cultural phenomenon? Since our definition involves “passing down” from generation to generation, we would say no, as “passing down” presupposes a causal mechanisms that *exists for* or *explicitly tries to achieve* the transmission of the cultural traits, be it just imitation or parental and peer correction and teaching.

A more detailed discussion of the research on animal culture can help us to clarify the notion of ‘existing for’ or ‘explicitly trying to achieve’. One of the central themes in this field has been the study of foraging behavior, but other types of behavior such as tool use and social behavior have also been studied extensively.<sup>260</sup> Studies can be divided into two groups: a) studies in the wild and b) controlled lab studies.

a) Studies in the wild have concentrated on chimpanzees and have shown that chimpanzee groups exhibit cultural variation in subsistence techniques, tool use and social behavior (Whiten et al. 1999, Wrangham 1994, McGrew 1992). For instance at M. Assirik in Senegal, chimpanzees strip the bark from twigs before using them to fish for termites, while chimpanzees at Gombe Stream Reserve use the same plant for termite-extracting tools but discard the twig and use the bark. Chimpanzees in the Mahale Mountains of Tanzania clasp hands and then groom each other’s

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<sup>260</sup> In their review of social transmission of foraging behavior, Lefebvre and Palameta 1988 list 97 examples of socially learned variation in animals as diverse as chimpanzees, baboons, macaques, sparrows, lizards, whales, dolphins and smaller fish. See also Richerson and Boyd 2005: 104-120.

exposed armpits (a behavior that is often performed and by all members of the group), whereas the Gombe chimpanzees, less than one hundred kilometres away, groom but never perform the hand clasping. These cultural traits were already exhibited in the population when the chimpanzees were first observed, but in a few cases the spread of a novel form of behavior has been observed. The famous case of the potato washing macaques has already been mentioned. Despite this enormous body of field work, many ethologists remain sceptical about the cases of the transmission of socially acquired traits and prefer controlled lab studies. It is for instance hard to exclude the possibility that some obscure differences between the environments gives rise to the observed differences in tool use and greeting behavior of neighbouring chimpanzee groups.<sup>261</sup>

b) The best lab studies have been done with songbirds. The most famous case is the transmission of song dialects in the white-crowned sparrow (Baptista and Trail 1992). Juvenile sparrows imitate the song patterns of adults, so because the songs of this species varies from place to place, different young sparrows learn local variants, also known as *dialects*. Experiments show that when young birds do not hear the local variant of their conspecific song in early life, they will only sing a highly simplified song which is typical of their species and independent of their habitat location. However, if young birds are exposed to adult singing, they acquire the local dialect in all its complexities.

Lab studies are critical of the abilities of our nearest ancestors, the chimpanzees, with regard to learning. Lab studies distinguish between imitation and other, simpler forms of social transmission. These simpler forms are for instance *local enhancement* and *stimulus enhancement*. Local enhancement occurs when, for instance, the activity of older animals in a particular location increases the chances that younger animals will visit that spot and then learn the behavior on their own. For instance, a young chimpanzee that accompanies its mother to a termite mound is more likely to acquire termiting skills than a chimpanzee that does not accompany its mother or whose mother does not termite. Stimulus enhancement occurs when a social cue makes a given stimulus salient to an animal. For example, smelling food odours on nest mates makes rats more likely to sample these foods when foraging. Imitation, on the other hand, occurs when an animal learns a certain type of behavior by *precise* observational learning, i.e. copying, be it motor or other patterns such as speech.<sup>262</sup>

Precise observational learning is, however, rare. Michael Tomasello (1996) did experiments in which chimpanzees and young human children were shown how to use a tool to obtain out-of-reach food. Chimpanzees that had watched the experimenter demonstrate how to reach the food performed much better at this task than the control group of chimpanzees that had not seen the demonstration. Remarkably, however, they did not imitate the *precise* method of the experimenter. Children on the other hand used the same method as the experimenter. Tomasello baptized the behavior of the chimpanzees *emulation* rather than *imitation*: apes had learned that the tool could be used for some desirable effect without paying close attention to the details of the tool use. By contrast, children imitated so faithfully that they persisted in using inefficient techniques in favour of better alternatives, whereas

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<sup>261</sup> Field-workers feel equally strong that lab experimentalists get carried away by their supposedly secure experiments. They claim that laboratory environments do not provide much opportunity for animals to show off their best tricks, and that the more complex the behavior of an animal the less it is fit to study in a lab. See for a good entry into the controversy the commentaries on Rendell and Whitehead 2001.

<sup>262</sup> An overview of the evolution of imitation in animal culture can be found in Moore 1996.

chimpanzees did some trial and error learning and abandoned the inefficient techniques. Thus, ironically, human children seem to ape more than primates do! Further research is needed to guarantee that the difference was not due to physiological differences between human and chimpanzees and that the apes were really not paying attention to the details, but Tomasello's distinction between emulation and imitation seems very useful to characterize the mode of transmission (and thus the definition of culture).

The overall conclusion by lab experimentalists is that when laboratory experiments distinguish true imitation from emulation and even simpler forms of social transmission such as local enhancement and stimulus enhancement, true imitation is absent except for the *specialised* (possibly modular) song-learning system of some birds (Tomasello, Kruger and Ratner 1993, Whiten and Ham 1992). Our definition of culture contained the notion of "passing down" which we explicate as involving a causal mechanism that 'exists for' or 'explicitly tries to achieve' the transmission. This is only the case for true imitation and debatably emulation. Hence, according to our definition only sparrows and animals that emulate can be said to exhibit culture.

While researchers fiercely debate the existence of culture in animals, one thing is fairly clear: "only humans show much evidence of *cumulative* cultural evolution (Richerson and Boyd 2005: 107) [their emphasis]." Cumulative evolution happens when cultural variants, artifacts or behavioral types, are transmitted and modified over many generations, leading to more complex artifacts and behaviors. Humans can add one innovation to another until perfect artifacts such as DVD-players and Stone-Age tools like spears that are composed of "a carefully worked, aerodynamic wooden shaft, a knapped stone point, and a hafting system to fasten the point to the shaft (id.)" are invented. Animals only seldomly add one durable innovation to another, the many elements of sparrow song dialect and the hammer-plus-anvil-nut-cracking technique of chimpanzees and a few other examples being the exceptions to the rule. This is the case because cumulative evolution seems to presuppose *precise* imitation rather than mere emulation. Thus it can be said that the capacity for imitation has made man a 'uniquely unique' species amongst all the other unique species, to quote Richard Alexander.

A good question is *how* humans evolved this capacity, and *why* humans only and other creatures not. Because of culture, there are now 6 billion humans, compared to some thousands of our nearest ancestors: the chimpanzees and bonobos. Cumulative culture seems to be clearly advantageous. Why, then, is it so rare in the animal kingdom? Why did culture not arise multiple times by convergent evolution as other really advantageous traits have? We postpone this question to § 3.1, where we briefly return to it.

### **c) The (non-cultural) environment of cultures:**

Thus far we have only spoken about the *genetic* and *learned* components of cultural traits. But of course we have already seen that genes are always genes in an environment, just as learning is always learning within a certain context. The transmission or learning of cultural traits can fail because of a bad teacher (just as gene transcription can fail), but it can also be that a disruptive environment inhibits learning, as when a school is located near a noisy airport. These environmental considerations influencing learning processes can be extended ad infinitum: diseases, family situations, quality of teachers, etc. They further complicate the question whether cultural transmission is determined by nurture or by nature.

Interestingly, some researchers such as the proponents of cultural ecology have claimed an easy solution to this complex aspect of the nature-nurture problem. They simply assume a high plasticity of both genes and learned behavior *and* a very strong influence of the environment so that the environment is the key factor shaping cultural evolution. Due to this there will be one-to-one correlations between the environment and the way culture evolves. The idea is simple: Eskimos wear coats made from the hides of seals because this material is available in their environment to protect them against the cold. By contrast, Bushmen hardly wear any clothes because it is so hot near the equator and because their desert environment provides few sources of clothes. Any questions on how far the behavior of Bushmen or that of the Eskimos is determined by nurture or nature are bracketed as interesting, but secondary, questions. Both genes and learned behavior are thought to be plastic enough to be shaped by environmental factors towards the same evolutionary results.

The intuitive correlations of the ‘choice’ of clothes of different human tribes and their natural environment are hard to reject. However, some of the proponents of cultural ecology also want to relate other cultural phenomena such as marriage customs, religious views, political organizations, etc. to environmental factors. For instance, theorists since Montesquieu claim that women in hotter climates beget children earlier because in these climates life expectancy is decreased and menstruation begins earlier. From this it “naturally” follows, they claim, that in hotter climates women have less education and are more dependent on their husbands. Worse education means that a woman without a man can survive less easily; hence, polygamy is accepted in these cultures. During times of war many men die, leaving many widows that need a man because they are dependent on the protection of a man to survive in their society. Or, since in hotter climates meat becomes tainted easily, people salt the meat and forbid the drinking of blood. Eventually, blood becomes tabooed and menstruating women are perceived as untouchable.

As logical as these explanations may seem with the key role for the environment in each of them, one can easily point to a lot of cultural traditions that either seem to have nothing to do with the environment or have originally arisen in one environment but continue in other environments. Think of the Jews: although many of them assimilated to the customs of the countries they migrated too, many have retained to their religious practices in all sorts of other cultural environments, even if harsh and unjust punishments were put on their customs. Thus environmental conditions cannot so easily be matched one-to-one with cultural customs, as some cultural ecologists may have thought or hoped.

A good test to see whether you believe whether cultural phenomenon are strongly influenced by the environment or not, is the so-called common-garden experiment:

“So which is more important in determining people’s behavior: genes, environment or culture? You can calibrate your own position on this question by considering the following thought experiment. Choose two groups of people who live in different environments and behave differently – say Eskimos and Trobriand Islanders. Next, suppose a population of Eskimos moves to an empty island in Melanesia and a population from the Trobriands moves to the high Arctic. Then, allow enough time for the *individuals* in each group to learn as much as they can about how to best behave in their new environment. Now here’s the test: do you think that the political system, religious practice, or kinship system of the Trobriands living in the

Arctic will resemble their Eskimo neighbours more than their Trobriand ancestors? If so, then you are one of those who minimize the importance of culture. Or, will the political system, religious practice or kinship system of the Trobriands living in the Arctic resemble that of their Trobriand ancestors more closely than their neighbours, the Eskimo? If that is your position, you think the natural environment was not the source of the original variation in these characters – there must be something else that is transmitted through time. It could be culture, but it could also be the genes. (Richerson and Boyd 2005: 20).”

Although we cannot perform the common-garden experiment in detail, we believe that there are strong reasons that the two cultures will remain clearly different as the example of the Jews showed and as many other similar examples could demonstrate too.<sup>263</sup> Further, we believe this difference is due to culture and not to genes since although *individual* variation in genes can be relatively high, on the *population* level the variation between communities is relatively small.<sup>264</sup> To understand human behavior and that of some other animals one thus needs to take cultural evolution into account, if some doubted that at all.

In sum: our definition of culture presupposes that we can distinguish genetically determined behavior from learned behavior and environmental influence, for which we have provided some tests such as the common garden experiment and further research on genetic and learning mechanisms. In labelling a trait as culturally determined it is very important to know the mode of transmission, whether it is true imitation, emulation or social learning by local enhancement. Only in the case of imitation and possibly emulation can we speak of a true cultural trait.

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<sup>263</sup> Rigorous empirical studies of what happens when culturally similar people occupy quite different environments are rare. A landmark study is Edgerton 1971, who focused on four East African tribes. Some communities of each of the tribes lived in moist highlands where they relied mainly on farming, while other communities of the tribes live in dry lowlands where herding is more important. In all cases the highland and lowland groups had been in place for some generations although there had been some contact between the groups. Edgerton measured the attitudes of each group using psychological tests and interviews. If culture played little role in shaping human behavior, the attitudes Edgerton measured should be associated with their form of living and not with the culture of the tribe. In that case the attitudes of the farmers and herders of one tribe should be more similar to the attitudes of the farmers and herders of the three other tribes, than the attitudes of farmers and herders within one tribe. If culture is important, though, then tribe may be more important than subsistence. Edgerton's results show the importance of culture. He summarizes: “We (...) conclude that there can be no doubt that if we wished to know how someone in these four tribes would respond to the interview administered in this research; we would best predict that person's responses by knowing the tribe to which he belonged” (p. 271).

<sup>264</sup> The question of genetic differences between the races of the world is a touchy subject for obvious reasons, and some even wish to get rid of the notion of race entirely as a biological non-concept. We use the word ‘race’ only in the sense of ‘identifiable sub-group’. It is uncontroversial that racial bodily differences such as the colour of the skin and the hair, the form of the eyes, the height of the body, and other often sexually selected traits are genetically determined. The same holds true for genes related to diseases such as sickle cell anaemia, schizophrenia, Down syndrome and lactose tolerance. Further, genetic markers can be used to trace ancestry. (See the pioneering work of Cavalli-Sforza et. al. 1994). More controversial is the question how much behavior between races varies. This will not likely become an object of study as in general biologists suppose that the genetic differences between human races are small, at least much smaller than between races of other species. See Ridley 2005: 234 who indicates a low inter-species genetic variation of humans of 8%. The fact that all human races can easily have offspring with each other buttresses this fact: the formation of races is usually a first step towards speciation, but there is clearly no sign of speciation amongst humans.

## §2.2 Evolutionary versus non-evolutionary theories of culture

*The idea that cultures change over time is natural, but it is a big step from this idea to Darwinian models of cultural evolution. What is needed for this step are at least a) the idea of cumulative evolution, but b) at the same time the abandonment of progress or direction<sup>265</sup> in evolution, c) the idea of multiple cultural phenomena in competition with each other for survival and d) the idea of culture as being shaped by selection. These four elements are logically independent and are all needed for Darwinian evolution: despite similarities between Darwinian stories about evolution and older ones, when these elements are not present we have no Darwinian explanation.*

Reflections on the evolution of culture are almost as old as philosophy. In Hesiod's *Work and Days* we find a story of five stages of cultural evolution, or more specifically 'degradation', from a mystical Golden Age via a Bronze age to the current Iron age in which humans live an existence of toil and misery, and not much later we find less mythical reflections on one's own culture and city compared to the life of barbarians or the life in other cities, in the remnants of the work of Xenophanes, Heraclitus, the Sophists, and Plato amongst many others.

Conjectural histories of the evolution of mankind in which three or four stages are acknowledged are also very old and their stages are often remarkably similar to modern Darwinian stories about the evolution of humans from a stage of hunting and gathering, to pastoralism and nomadism, to agriculture, and finally a stage of commerce and city life. Stories like these can be found in the works of the Stoics, Seneca, Montaigne, Hobbes, Rousseau, and authors of the Scottish Enlightenment such as Adam Ferguson, John Millar and Adam Smith etc.

What sets these ideas apart from a true Darwinian theory is that they often see a clear direction in evolution and think in terms of the evolution of mankind as a whole. This is especially clear in the so-called *uni-linear* sociocultural evolution theories of the 19<sup>th</sup> century by for instance Edward Burnett Tylor, Herbert Spencer, Benjamin Kidd, Lewis Henry Morgan and William Graham Sumner in the Anglo-Saxon world and Helvétius, the Marquis de Condorcet, Saint-Simon, Hegel, Comte and Marx on the continent, who all claimed that societies start out in a *primitive* state and gradually (but "necessarily") become more *civilized* over time. In these models mankind was seen as a single entity and different societies were held to be at different stages of social development towards a fixed goal (specified by a measure of social complexity and/or freedom, and intellectual, technological, theological or aesthetic sophistication) in accordance with a determinate logic that science should uncover.

Most 20<sup>th</sup>-century approaches, however, focused on changes specific to *multiple*, individual societies and their *multilineal* evolution.<sup>266</sup> Moreover, they reject

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<sup>265</sup> *Cyclical* theories of evolution such as reincarnations or reinventions of culture after a world fire or a great flood which occurs every x number of millennia are also directional theories as they predict a clear, and necessary course for a culture.

<sup>266</sup> A clear exception is Leslie White who wrote *The evolution of culture: the development of civilisation to the fall of Rome* (1959) in which he attempted to create a unilineal evolutionary theory explaining the entire history of humanity. White rejected the normative terminology of primitive and modern societies but did argue that cultures could be distinguished on the basis of the amount of energy harnessed with more energy allowing for greater social differentiation (White's law) on the basis of technology that this energy



directional change in the strong sense of the word (i.e. orthogenetic, teleological or progressive change towards a goal), just as Darwin did for biological evolution. Cultural anthropologists at the beginning of the 20<sup>th</sup> century such as Franz Boas and his students like Ruth Benedict and Margaret Mead explicitly rejected the distinction between primitive and civilized societies altogether, by pointing to the complexities of supposed primitive societies; they also tried to demonstrate the ethno-, Euro-centric presuppositions behind this distinction, thus further undermining the idea of progress. In addition, they rejected conjectural histories as unscientific speculations and denied that cultures are those isolated, bounded and distinct entities earlier models had thought they were, by demonstrating that cultural traits and forms often cross social boundaries and diffuse through many different societies. They relied heavily on empirical data (and later anthropologists even more so as they criticized their pioneers for not being empirical enough) and tried to refrain from value judgments. In these early 20<sup>th</sup> century theories we do find b) the abandonment of progress or direction in evolution and some idea that c) there are multiple cultural phenomena, although this co-existence was usually not portrayed as competition. In fact, because these anthropologists rejected conjectural histories and focused on empirical material at hand, they often did not speak about the evolution of culture at all. The strong structuralist presupposition of a fixed division of elements of a given (primitive) culture into binary oppositions in the field of anthropology at the time only strengthened their distaste for evolution. Further, the idea of d) culture as being shaped by selection is even less present in 20<sup>th</sup> century anthropology. The closest approximation is found in certain functionalist speculations in sociology, such as in the work of Durkheim, who thinks of cultures as being adapted towards man's well-being in a group, with better- and tightly-organized groups being better at confronting other groups and the natural environment.<sup>267</sup> Most anthropologists regarded the many totems and taboos in primitive cultures (the few taboos on incest and certain easily spoiled types of food put aside) as generally non-functional for a group's well-being, and many modern Darwinists agree: cultural elements need not be adapted to humans, but can be adapted – so to speak – for the cultural elements' own sake: just as parasites are not adapted towards human well-being but to exploit

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supply made possible, arguing that social systems are determined by technological systems. In *Power and Prestige* (1966) and *Human societies, an introduction to macrosociology* (1974) Gerhard Lenski expanded on the work of White. He shared the former's conviction that progress was due to technology but he did not understand the development of technology as the ability to harness energy, but as the ability to use and store information.

More recent work in the unilineal tradition is often done by non-specialists such as the speculative work by the political philosopher Francis Fukuyama: *The end of History* (1992), written after the fall of the Berlin Wall as world-wide capitalism and liberal democracy seemed to be the logical outcome of world history (Fukuyama has since revised his ideas several times), as well as some popular, half empirically based depictions of modernization or globalisation as a world-encompassing network that leaves no room for traditional cultures (Friedmann 2005, Castells 2001). Others, with the same grand gestures, speak on the contrary of a tendency of regionalization within the globalised world (Naisbitt 1982) or imagine a clash between civilisations (Huntington 1995). More empirically orientated sociologists and anthropologists are usually highly critical of these big stories of unilineal or pseudo-multilineal evolution of cultures, just as their predecessors were once critical of the grand speculations of the 19<sup>th</sup> century. Some more empirically oriented anthropologists that nonetheless still work in the unilineal tradition are Robert Caneiro 2003 and Allen Johnson & Timothy Earle 2000. Their work tends to be about the general trend towards greater complexity of the human society that has become (on average) increasingly bigger over the past thousands of years.

<sup>267</sup> The Darwinist and firm supporter of group selection David Sloan Wilson has recently picked up Durkheim's ideas in trying to explain religion and religious customs or rules as adaptations for group life.

humans as much as possible, so cultural elements may use people for their own replication.

After the Second World War, the idea of evolution and cultural adaptationism made its come-back in anthropology and sociology under the banner of neo-evolutionism, which indicates a whole patch-work of more empirically orientated modern theories. Early examples are the work of Leslie White and Julian Steward from the 40's onward. White still believed in uni-linear evolution although his work was much more empirically orientated, but Steward, rejected the notion of uni-linear, goal-directed progress and called attention to Darwinian evolution with its crucial concept of 'adaptation' in his book *Theory of culture change: the methodology of multi-linear evolution* (1955). Steward wanted to understand cultures as more or less perfect adaptations to their ecological environment, just as Darwin had understood the design of organisms as more or less perfect adaptations to their ecological niche. Just as there is no real progress in evolution in the many lineages of the tree of life (although 'more complex' organisms like zebras can only evolve because less complex organisms like amoebas once did), so the multiple lineages of cultural evolution show no real progress.<sup>268</sup> Steward's work gave rise to the branch of cultural ecology which we discussed briefly above and which we will discuss in more detail below.

Most anthropologists and sociologists today, however, are either i) focused primarily on empirical studies without much theorizing or ii) work in the tradition of general *systems theory* that examines cultures as emergent systems that should not only be accounted for by their relation with their environment, but also from the internal dynamics of the social environment including political and economic relations (cf. Luhmann et al.). A typical example of evolutionary work in the tradition of systems theory is Talcott Parsons: *Evolutionary and Comparative perspectives* (1966) and *The system of modern societies* (1971). In Parsons's model evolution is divided into four subprocesses (note from the onset the top-down perspective of systems theory compared to the more bottom-up approach of Darwinism): 1) division of the system into functional subsystems, 2) adaptation of these subsystems to higher degrees of efficiency, 3) integration of elements hitherto unrelated and 4) generalization of values to legitimize the ever more complex system. Systems theory with its complex terminology has been criticized as pseudo-science for its lack of explanatory power and predictions, as well as for its general tendency, like structuralism, to downplay human intentions in favour of systems that constrain human actions. Systems theory may sound Darwinian as all 4 elements seem to be present (cumulative evolution, multi-linear evolution etc.). However, the top-down approach to a system as a given that becomes more and more complex by further integration over time is not Darwinian at all; a Darwinian explanation would want to explain how a system (a

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<sup>268</sup> Marshall Sahlins 1960 synthesized the uni-linear work of White and the multi-linear work of Steward. Sahlins divided the evolution of cultures into 'general' and 'specific' evolution. General evolution is the tendency of systems to increase in complexity, specific evolution is local adaptiveness.

A modern variant of Sahlins' work is the pseudo-Darwinian 'punctuated equilibrium' model of Bloomfield (1995, 1993), developed on the basis of the work of Gould and Elredge (1972). According to Bloomfield punctuated equilibria in cultural evolution mean a sequence of a stable society followed by a wild transition phase which results in a new stable society with greater complexity. The transitions he imagines are from a stable animal society, to a stable tribal society, to a stable peasant society; currently we are in a transitional industrial society which will, if changes can be extended, lead to a future stable automated society.

Note that models that combine multi-linear evolution with a uni-linear evolution towards increasing complexity reduce easily into complete uni-linear theories with hardly any empirical support or explanatory power.

complex design) arose in the first place and would deny that all evolution is necessarily aimed at further integration.<sup>269</sup> The adaptation system theorists talk about is also not measured empirically using models with cultural variants having different rates of replication. We will see in the next paragraph what such empirical models look like.

Of the scientists who focus strongly on empirical studies without much theorizing, however, there is a subgroup of anthropologists who reject *all* evolutionary models for two reasons: 1) they claim that all evolution or development is due to historical contingencies, accidental cultural contact, etc. or 2) they claim that cultural models are not *explaining* anything, but just *redescribing* easily understandable processes such as the spread of a new technological innovation like a new plough by unnecessarily complex models and terminology.

These two objections are distinct claims, although they have a common basis: many evolutionary models (especially the uni-linear ones) are such vast and naive over-simplifications that they cannot be called scientific at all. When one wants to understand the rise of modern industrial society, one cannot omit the intricate web of details from the history of science, technology, economy, medicine, politics, colonialism, religion, etc. But the fact that theorists to date have been naïve does not mean that their theories contain no grain of truth. Before we turn to a description of Darwinian models of cultural evolution in the next section we will discuss whether the objections of these empirically minded scientists also hold for Darwinian models.

1) Historical contingencies: field biologists spend many years of their lives observing animals in the wild and they could tell a great many details about the lives of the individual animals they have studied. If they did only this, biology would be a historical science just like ordinary history, but fortunately they do not. Field biologists make models in which they try to abstract from the contingencies of the lives of individual organisms towards evolutionary models of changes in (fitness) traits in *populations* of organisms. A priori, one cannot argue that this is impossible in the case of cultural evolution too.

2) Cultural models as complex re-descriptions without explanatory power: cultural models seem trivial at first. When a new or better plough is invented which does not cost a lot more effort or resources to produce, it is easy to see why others in the population will imitate it, so that the invention will spread first slowly as only a few people know the innovator, but a little later faster as more people get to know it, until the *tempo* of spread will decrease as almost anyone in the population has the new plough except for some conservatives and the few environments in which the old plough might still be better (because of different conditions of the soil etc.).<sup>270</sup> In this vein, Eliot Sober remarks about the uselessness of population-based models of cultural change for social scientists: “My main reason for skepticism is that these models concern themselves with the *consequences* of transmission systems and fitness differences, not with their *sources*” [his emphasis] (1991: 18). To understand why some innovations spread but others do not, you need to know people’s learning rules, their transmission biases and the like. Why did someone invent a given cultural variant in the first place? Why is it attractive to others? You have to know which ideas will be imitated and which will be ignored. This knowledge does not come from within the Darwinian framework, Sober argues, but has to be supplemented by,

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<sup>269</sup> We ignore the dimension of legitimization of Parson’s theory here.

<sup>270</sup> I.e. the plot of the adoption of a new technology in a population versus time will look like the famous S-curve familiar from business cycles etc.

as we would put it, *proximate mechanisms* and *historical contingencies*. These are what really interest social scientists and historians respectively. “In other words, Sober thinks that population-based theories take all the important stuff as given, and concentrate on the stuff that nobody cares about. The hard parts of social science do not involve its population-level properties, and the population level, unlike the biological case, is trivial” (Richerson and Boyd 2005: 96).

However, it is not easy at all to predict the evolutionary outcome of population dynamics, and we humans are not very good intuitive population thinkers, people that have a good intuitive grasp of how population statistics and dynamics work. In recent years there have been models which have shown how individual learning processes can have surprising effects at the population level; consider for instance the result of Rogers (1988) that in a population of ‘self-learners’ and ‘copiers’ (people that imitate the self-learners, but at a fraction of the costs), in an environment that changes once in a while, the individuals in the mixed population have the same average fitness over the long run as individuals in a population of self-learners only. More surprising results appeared as other scientists tried to revise Rogers’s highly simplified models to demonstrate how culture (learning by imitation) could be adaptive if people *both* self-learn and imitate.

We will see more examples of surprising general results of cultural evolutionary theory below. Even for historically contingent episodes Darwinian models of culture may have surprising explanations. Drawing on the work of Joseph Henrich (2004a) and Jared Diamond (1997) on the disappearance of important aboriginal technologies, Richerson and Boyd (2005: 138) suggest that the maintenance of technologies and the associated behavior required to produce and operate them require a *population that is large enough* for the rate of innovation to offset the degradation that results from the standard error-prone imitation of individuals. Other examples of useful cultural models are when two opposing cultural processes occur at the same time, for instance when higher educated parents will have lower rates of infant mortality, but also fewer children that live longer, are healthier and are also higher educated. One would clearly need *quantitative* models to predict whether higher educated people will replace lower educated people with higher birth rates but also more infant mortality.

## §2.3 Eight Darwinian models of multi-lineal cultural evolution

*In this paragraph we distinguish 8 models of cultural evolution from a Darwinian perspective – 2 ontogenetic models and 6 phylogenetic ones. We will first briefly describe examples of Darwinian explanations outside of biology showing the need for general Darwinian models of cultural evolution to describe Darwinian theories of language, and then describe the models in detail expanding the work of Flinn (1997) and Struwel (2002). The 2 ontogenetic models are I) operant conditioning and II) neural Darwinism. The 6 phylogenetic models are III) Clifford Geertz’s ideational culturalism, IV) Marvin Harris’ cultural materialism and other models of cultural ecology, V) memology, in particular Dennett’s universal Darwinism, VI) (straw-man models of) sociobiology, VII) evolutionary psychology and VIII) dual-inheritance theory.*

Darwin's theory is a theory about the evolution of biological organisms; almost immediately after the *Origin of Species* appeared in 1859, people realized that his theory might be used to explain the origin of other complex forms of design such as that of cultural elements. As early as June 1863 the novelist Samuel Butler, best known from his satire *Erewhon* and his posthumously published *The Way of all Flesh*, sent a letter to the newspaper, entitled 'Darwin among the machines' in which he – half seriously, half satirically – spoke of the “mechanical life” of artifacts, comparing their evolution with that of animals and plants, imagining that one day man would become enslaved to highly evolved machines.<sup>271</sup>

Other areas (the list is far from exhaustive) where we find applications of Darwin's ideas outside of biology (or what usually counts as biology) are applications involving *our sense of numbers* (Dehaene 1997) or *our general ability to do math* (Devlin 2001, Lakoff and Luñez 2000), the development or *evolution of mathematical theories* (De Cruz 2007, 2006ab, Van Kerkhove 2006, Lakatos 1976), *cosmological speculations of universes* that replicate by creating new universes (baby-verses) through black-holes (Smolin 1999) or *tornadogenesis* where one tornado can give rise to multiple ones (Markowski, Straka and Rasmussen 2002),<sup>272</sup> the self-evolving computer code of Genetic Programming, a machine learning technique to optimize a population of computer *programs* according to a fitness landscape determined by a program's ability to perform a given computational task (Koza et al. 2003, 1999, 1994, 1992, Fogel 2000, 1998, Banzaf et al. 1998), evolutionary epistemology both in cognitive science (Thewissen and Nummela 2008, Geary 2004, Sterelny 2003, Goodson 2002, Donald 2001, 1991, Shettleworth 1998) and philosophy (Sober 1995, Bradie 1994, Dretske 1971), the evolution of scientific theories (Hull 1988, Toulmin 1967, 1972), the biological ability or 'bio-program' to produce artifacts (Gould and Gould 2007, Hansell 2007, Scott Turner 2002) or that of artifacts themselves (Dyson 1996, Petroski 1992, Basalla 1988) or even self-replicating artifacts (Freitas and Merkle 2004, Freitas and Valdes 1980, Von Neumann 1966), such as religious experiences (Hamer 2004, Newberg et al. 2001) , religious customs (Wilson 2002) and religious ideas (Dennett 2006, Dawkins 2004, Boyer 2001), economic firms (Pitelis 2002,

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<sup>271</sup> See Butler 1914:180. “We have used the words mechanical life, the mechanical kingdom, the mechanical world and so forth, and we have done so advisedly, for as the vegetable kingdom was slowly developed from the mineral, and as in like manner the animal supervened upon the vegetable, so now in these last few ages an entirely new kingdom has sprung up, of which we as yet have only seen what will one day be considered the antediluvian prototypes of the race. We regret deeply that our knowledge both of natural history and of machinery is too small to enable us to undertake the gigantic task of classifying machines into the genera and sub-genera, species, varieties and sub-varieties, and so forth, of tracing the connecting links between machines of widely different characters, of pointing out how subservience to the use of man has played that part among machines which natural selection has performed in the animal and vegetable kingdoms, of pointing out rudimentary organs which exist in some few machines, feebly developed and perfectly useless, yet serving to mark descent from some ancestral type which has either perished or been modified into some new phase of mechanical existence.”

<sup>272</sup> People that study tornado's speak of the *life cycle* of a tornado. Details of this 'life cycle' are unimportant to us. The *life cycle* would be just a metaphor for being weak ('small'), getting stronger ('bigger') and finally extinguishing, thus comparing the life cycle of a tornado with that of man: baby (weak), adult (strong), geriatric (weak), if not for the ability of tornado to could 'give birth' to new tornados. A tornado arises when a strong thunderstorm develops a rotating mesocyclone a few miles up in the atmosphere. This rotating mesocyclone is dragged towards the ground due to an area of quickly descending air which itself is formed by an increased rainfall of relatively high temperature (the power source of the tornado). Then an area of cold surface air wraps itself around the tornado, which cuts it of from its warm water supply and the mesocyclone dissipates, which causes the storms inflow to be concentrated into a new smaller area closer to the center of the storm. Then a new tornado cycle can begin (or even more than one).

Penrose 1959), complex financial products (Beinhocker 2006) and the free market as agent of selection (Nelson and Winter 1982).

Each of these examples would require multiple pages to portray fairly and to analyze to what extent they can truly be called Darwinian. Take for instance Butler's example: the Darwinian aspects of his 'theory' are the general idea of evolution by selection, the acknowledgment of variants among machines and the idea that just as organisms can have rudimentary organs, so "machines can have useless parts (...), feebly developed and perfectly useless, yet serving to mark descent from some ancestral type which has either perished or been modified into some new phase of mechanical existence" (Butler 1914: 180). There are non-Darwinian aspects as well, however, like Butler's insistence that the evolution of machines necessarily leads to machines that can replicate themselves without the need of man (a fixed course of evolution) and that the evolution of machines is not blind and random, but guided by human intentions etc.<sup>273</sup>

For all the other examples from the literature on Darwinian explanations outside of biology, we could find other successful analogies and other areas in which the analogy breaks down, either because the assumption that mathematical objects or theorems (which are often thought to exist in a realm of eternal, a-priori truths grasped by some form of intuition) evolve similarly to biological organisms is hard to justify, or because the highly speculative and untestable theory of baby-universes does not really include the principle of selection as all universes generate offspring in the multi-verse, or because the evolution of computer code in genetic programming where the replication is strictly controlled so that each generation has the same number of individuals and the end-point of the evolution (the maximum number of iterations) is well-defined.

What we are primarily interested in in this study, however, are applications of Darwin's theory to the evolution of language (with what we mean by 'evolution' and 'language' defined in chapter 4). These applications exhibit a rather similar picture of more or less successful analogies (see §4.2). A discussion of the different Darwinian theories in linguistics would be helped greatly if we had some overview of the models Darwinists use consciously or unconsciously when they apply Darwinian principles to a cultural phenomenon such as language use. We will discuss 8 of these models, which would also be of use for an analysis of some of the other examples enumerated above. Two things can be concluded up-front.

1) Not all Darwinian explanations outside of biology can be categorized under Darwinian models of culture: (pseudo-)Darwinian theories of replicating universes, of self-modifying computer code, and, as many (Platonistic) philosophers of mathematics would argue, of the evolution of mathematical theorems do not fall in this category, whereas the examples of evolutionary economy, scientific theories, religion, etc. do. The obvious criterion for inclusion or exclusion is: is the object of explanation usually categorized under culture or not (where there are of course border cases)? What this means for the application of Darwinian explanations of language is that not all aspects of language or its study will be covered by these

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<sup>273</sup> Later evolutionary thinkers such as Kenneth Boulding have tried to downplay human intentions in the evolution of artefacts: "A car is just an organism with an exceedingly complex sex life." Like a technological virus, it takes over a complex social structure and redirects the resources of a large fraction of it to reproduce more of its own kind. (as quoted by Wimsatt 1999: 298). This example looks remarkably similar to that of 'nests-using birds to reproduce' and we will see as we discuss memology below that examples from cultural evolution like these suffer from similar flaws.

explanations (such as the mathematical properties of languages, logic, computer languages etc.).

2) Darwinian models of culture often have two sides on which they can focus: a) *the human ability to do understand or master something in the realm of culture*: i.e. a talent for science, a knowledge acquisition mechanism, the ability to manipulate artifacts, a sense for religion, economy etc. and b) the cultural elements which have some form of existence independent of (the body of) men: scientific theories, artifacts, religious buildings or books, economic products and firms etc. For language the two sides would be: the ability to master language use (at all levels: vocal, cognitive etc.) and the evolution of linguistic elements such as meaningful sounds, words, syntax etc. Sometimes, however, the two sides are hard to disentangle. Take the songs of birds: a bird's actual use of its ability to sing largely coincides with what the songs are, so that the evolution of the ability to sing coincides with the evolution of the songs. However, some elements of the song of for instance white-crowned sparrows are not genetically encoded but learned. In that case the evolution of the ability to sing does not coincide completely with the evolution of songs and only then do we have true cases of cultural evolution.

An important question in the next chapter will be, how much of language use is under genetic control and how much can evolve independently through learning by parts of the brain not under direct genetic control. All 8 models we describe make different presuppositions about that.

As described in §1.1, Darwinian theories explain the ontogenetic development and adaptations of individual organisms by referring to the selection on the phylogenetic ancestors. As such Darwin's theory seems to be primarily about phylogenesis. We will describe 6 phylogenetic models in a moment. Interestingly enough, however, some people have applied Darwinian principles to the growth of knowledge and abilities of an *individual*. We baptize these models 'ontogenetic models' because all the Darwinian selection processes of these models encompass only the life span of a single person. Its proponents, however, would probably not like this name as they will stress the multiple iteration cycles (phylogenetic generations) the selection processes in the brain of the individual goes through during the development of the abilities.

As we discuss the 8 models we criticize them along the way, using our definitions from chapter 1. We clearly favour the dual inheritance model. This model will lie at the basis of the definitions we provide in §2.4. The other models will be used to categorize Darwinian explanations of language in chapter 4. Our criticism of these models in this paragraph will be applied to concrete examples of these models in chapter 4.

## **Two ontogenetic models**

We distinguish two ontogenetic models of cultural evolution: I) Darwinian operant conditioning as based on the work of Campbell and Skinner and II) neural Darwinism as based on the work of Edelman. We discuss the operant conditioning in quite some detail since we will get back to operant conditioning in the chapter on language, since some have claimed that word learning is also like *operant conditioning*. As the theory of operant conditioning has a Darwinian flavor, a theory of language acquisition based upon operant conditioning almost automatically has a Darwinian flavor too. Work in this tradition is still the most comprehensive account of an ontogenetic Darwinian theory of language acquisition. After a discussion of operant

conditioning we briefly comment upon neural Darwinism, which is more metaphor than theory.

### **I) Darwinian operant conditioning**

Donald T. Campbell was one of the pioneers of the study of the development of adaptive behavior from random responses (1956ab), which resulted in his famous BVS (blind variation and selective retention)-model (Campbell 1960, 1974). This work is closely related to B.F. Skinner's theory of operant conditioning, which deals with the ontogenesis of individual behavior. Skinner himself claims that his work is based on a Darwinian *selectionist* model (Skinner 1981). Most recently Darwinian accounts of operant conditioning have been defended by Sigrid S. Glenn (Hull et al. 2001, Glenn and Field 1994, Glenn et al. 1992, and Glenn 1991).

Operant conditioning is the use of consequences to modify the occurrence and form of behavior. *Operant conditioning* is distinguished from classical conditioning (also called respondent conditioning, or Pavlovian conditioning) in that operant conditioning deals with the modification of 'voluntary' or 'operant behavior' which 'operates' on the environment and is maintained by its consequences, while classical conditioning deals with the conditioning of respondent behaviors which are elicited by antecedent conditions (Domjan 2002, Skinner 1938). Thus, operant conditioning in contrast with classical conditioning does not require an antecedent external stimulus (Skinner 1953: 64) as it proceeds from "impulses" internal to the organism (although in more complex examples external stimuli may play a role).<sup>274</sup> Two examples may make the difference between classical and operant conditioning clearer. In the classical *conditioning* experiment by Pavlov, a bell was sounded *before* dogs were given food (the antecedent condition); after multiple training sessions dogs started to associate the bell with food and upon hearing the sound they started to produce saliva although the food itself was not present yet. Examples of experiments on *operant* conditioning usually involve lever presses and key pecks by rats or chickens. In a typical experiment chickens are conditioned to a certain pecking behavior by changing the conditions under which key-pecking gives them food (for instance Blough 1963): e.g., by pecking only with a certain force, by pecking with a certain latency (inter-response time), by pecking with one's head in certain direction, they get food. What typically happens in such experiments is that a chicken 'voluntarily' adjusts its random pecking so that its food intake is maximized.

Usually this maximization, along with operant conditioning in general, is explained by four *procedures or mechanisms* called *positive* and *negative reinforcement* and *positive* and *negative punishment*. Reinforcement here means a consequence that causes a behavior to occur with greater frequency, punishment on the other hand is a consequence that causes a behavior to occur with a lesser frequency. Positive reinforcement occurs when a behavior (response) is followed by a favorable stimulus (commonly seen as pleasant) so that the frequency of the associated behavior increases. Negative reinforcement occurs when a behavior (response) is followed by the removal of an aversive stimulus (commonly seen as unpleasant) thereby increasing that behavior's frequency (think of an experiment in which loud noise continuously

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<sup>274</sup> The reason why the study of classical conditioning was gradually supplemented or even replaced by the study of operant conditioning was that the researchers felt that the nature of what an organism was conditioned to was hard to determine in practice (is it something in the environment, a bodily disposition of the researcher that the animal can sense, the time interval between the stimuli, the properties of the stimulus, in case of a bell sound: the length, pitch, volume of the sound or a combination of them etc.)?



sounds inside a rat's cage until the rat engages in the target behavior); positive punishment (a bit of a misnomer) occurs when a behavior (response) is followed by an aversive stimulus, such as introducing a shock or loud noise, resulting in a decrease in that behavior; negative punishment occurs when a behavior (response) is followed by the removal of a favorable stimulus, such as taking away a child's toy following an undesired behavior, which ultimately results in a decrease in that behavior. Other related procedures are *avoidance learning* (not touching fire) and *extinction*: the lack of any consequences following a response, which given the existence of reinforcement usually leads to a decrease in the frequency of the action whose performance had no consequence.<sup>275</sup>

Some researchers try to construe these operant conditioning *procedures* or *mechanisms* as Darwinian selection processes, a "second kind of selection" (Skinner 1981: 501), a process that is itself ultimately the result of the "first kind of selection." I.e. all behavior is (ultimately) caused by natural selection (the first kind of selection), but *some*<sup>276</sup> of the ontogenetic evolution of behavior during a single organism's lifetime is supposedly caused by the second type of selection of operant conditioning. Thus in this model 'voluntary adjustments' of the organism's responses are called 'selection'. The properties this selection works upon are the properties of operant responses such as "duration, latency (interresponse time), force, form, direction and relation to antecedent events" (Hull, Glenn et al. 2001: 523).

To make the parallel between biological evolution and operant conditioning learning sound better than just a metaphor, however, we need to find all the three principles of Darwinism, as outlined in §2.4, back. Campbell took a first step in this direction by postulating his BVS<sub>R</sub>-model to characterize operant conditioning as a combination of blind *variation* and *selective* retention. We will, however, use more modern models to see the extent to which the three Darwinian principles are present in operant conditioning.

Darwinian models of operant learning start by defining a quantity called 'operant repertoire', the whole of behavior that originates in operant conditioning whose "interrelated parts are made up of behavioral lineages, each having its origin at a different time in the history of the organism" (Hull, Glenn et al. 2001: 522). These lineages are *temporal* sequences of operant responses; changes in one lineage can lead to changes in other lineages in the organism's repertoire. Selection on these lineages "adapts organismic activity over time to fit the environment in which it occurs (id.)" and just as "in the evolution of species, operant behavior fits the present environment because of past selection and not because of any future state of affairs (id.)."<sup>277</sup> Some

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<sup>275</sup> In Skinner's classical box experiment, extinction occurred as the rats did not receive a food reward anymore from pushing the lever and eventually ceased pushing it.

<sup>276</sup> As said operant selection is usually studied using lever presses and key pecks. These are of course very simple types of behavior. However, they are used as exemplars of a process that is held to operate with respect to more complex behavior. How complex that behavior can be is an empirical question. There are of course many learned types of complex behavior that cannot be described by operant conditioning, especially forms of learning that don't require training, as for instance when one learns the use of a word by its definition so that one can immediately use the new word (first language acquisition by children with its over- and under-extensions of initial word use, however, comes closer to operant learning). Finally, apart from behavior that is more complex than operant behavior there also exists simpler behavior, such as directly genetically programmed, instinctive behavior which Konrad Lorenz called 'fixed action patterns'.

<sup>277</sup> One can say that operant responses are forward-looking insofar they are under 'voluntary' control and the animal seems to decide what it will do in the future. However, operant conditioning usually deals with unconscious processes in any strong sense of the word conscious such as planned, intentional etc. (although it rests on consciousness in the sense of perception based). Because of the absence of any actual planning, the differences between the selection of operant responses and of natural selection can be downplayed.

of these lineages may come in clusters such as the cluster of finding a way out of a maze, the cluster of complex tricks a dolphin does in shows in dolphinariums etc. Let's look at the three principles using the criteria of §1.4 in more detail.

**Variation:** we find *blind variation* in operant conditioning since “variants that fail to meet the selection contingency continue to appear in the lineages, at varying frequencies at different times during the history [of the conditioning process] (id., p. 524),” when for instance a chicken that gets food after 30 pecks starts to peck more strongly, slower or only after certain intervals; just as in biological evolution (often deleterious) variants *keep* popping up. Moreover, the variation between two responses in a lineage, which are of course never alike in all respects, can be described as “most likely randomly generated (id., p. 525),”<sup>278</sup> just as in biological evolution. Further, the variation between different steps in operant conditioning is usually small,<sup>279</sup> and determinable by cladism (‘descent’ from *response lineage* X) and typological parameters (*type* of response Y) so that there is no extra ‘species problem’ regarding operant conditioning. It can also be imagined that sometimes (clusters of) “behavioral lineages are merged (id., p. 525)”<sup>280</sup> just as in biological symbiogenesis sometimes big steps are taken in natural evolution. Finally, the only relevant variation for operant conditioning is variation that can be selected and that thus is heritable. We discuss *heritability* under replication.

**Selection** (fitness): we will first look at selection in operant conditioning and then at the fitness of operant lineages; we already saw that some researchers called operant conditioning a second form of *selection* and we mentioned some of the selected traits of the behavioral lineages.<sup>281</sup> Some of the traits in *clusters* of lineages may even be selected together, so that the locus of selection and the unit of replication (retention) are not the same.<sup>282</sup> Insofar, selection in operant conditioning is parallel to natural selection. If we look more carefully at our definition of natural selection, however, we see differences. We defined selection as *consisting in repeated cycles of replication, variation and environmental interaction so structured that environmental interaction causes the fitness of traits to be increased (and thus replication to be differential) or acquired fitness to be maintained*. The concept of selection hangs thus on the concept of replication and this concept will turn out to be problematic: “If the environment must have multiple and differing copies of a replicator concurrently available for

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Note that although natural selection is not forward-looking, its *results* are such that organisms have good chances to survive in the future when it changes not too fast or to unpredictably (i.e. non-periodically or cyclically over long periods) so that it seems that natural selection is nevertheless just as forward-looking as operant conditioning.

<sup>278</sup> Glenn gives the example of Page and Neuringer 1985: 447, who showed that in their responses “pigs behaved as quasi-random generators.”

<sup>279</sup> But not always, and that may be a difference between the variation in operant conditioning and the variation in biological evolution.

<sup>280</sup> Glenn and Madden 1995 explicitly compare the merging of some behavioral lineages in an organism's behavioral repertoire to the appearance of eukaryotes in biological evolution.

<sup>281</sup> One can wonder what the behavioral lineages were before their properties were selected by conditioning, i.e. what the *origin* of the organized activity in behavioral lineages is. Traditionally, their origin has been thought of as an inherited supply of “uncommitted behavior” (Skinner 1984: 219), i.e. in genetically programmed routines that are still flexible or plastic so they can be shaped during ontogenetic development. Glenn likens them to the behavior “prominent in the repertoires of human infants” or in that of “retarded adults” or “adults submerged in water or isolated for a long time in an empty room.” The emergence of organized activity from undifferentiated movements can be seen to occur in real life all the time, but exactly locating it is hard (compare what we said about emergent properties in §1.22).

<sup>282</sup> This is especially true for so-called scheduled reinforcement tasks when multiple tasks have to be performed after each other before a certain goal is achieved. The *primary* locus of selection is here the multiple tasks taken collectively, whereas the ‘replicators’ resemble the individual lineages.

selection to occur, operant behavior seems definitionally excluded. [... However], all that may be required [for selection to occur] is a process that retains features of [... clusters of lineages] across generations in a lineage (id, p. 525).” That is why Campbell in his pseudo-Darwinian BVRS-model spoke of blind variation and selective *retention* instead of selective (differential) *reproduction*.

Finally, there is an interesting parallel between the different definitions of selection ( $\text{fitness}_I$  and  $\text{fitness}_{II}$ ) in biology and those in the field of operant conditioning. Skinner’s work can be seen as a generalization and a more rigorous treatment of the work of Thorndike on the way cats learned their way out of mazes. When first constrained in the boxes, Thorndike’s cats took a long time to escape. With experience, however, ineffective responses occurred less frequently and successful responses occurred more frequently, enabling the cats to escape in less time over successive trials. In his ‘law of effect’ Thorndike (1898) sought to explain this learning effect by stating that responses to stimuli that produce a satisfying or pleasant state of affairs in a particular situation are more likely to occur again in the situation. Around 1930, Thorndike shortened the law of effect to simply state that responses to stimuli that produce a satisfying or pleasant state of affairs in a particular situation are more likely to occur again in the situation. This law was criticized as circular insofar as it is not obvious how to define a ‘satisfying’ or ‘annoying’ state of affairs independently of the ability to induce instrumental learning. B.F. Skinner therefore proposed to define learning in operant conditioning on the basis of ‘reinforcers’: stimuli which, when presented after a response, leads to an increase in the future *rate or frequency* of that response. Skinner’s model is more akin to biological models built on  $\text{fitness}_I$  definitions (that relate fitness only to relative increases in replicators), Thorndike’s original model to  $\text{fitness}_{II}$  definitions (that relate fitness to functions that explain *why* a replicator increases in frequency). Just as we prefer  $\text{fitness}_{II}$  definitions to  $\text{fitness}_I$  definitions, we prefer definitions of operant conditioning that explain *why* a certain operant response increases to ones that do not. Although increases in a certain behavior need not always be advantageous to an organism (from the perspective of its biological fitness), as drug abuse is a typical case of operant learning, we think that in general the explanation *why* an operant response increases is further determinable by searching for the proximate mechanisms involved that have evolved by natural selection (and not by operant selection itself). If so, Skinner’s ‘second kind of selection’ is much more dependent on the ‘first kind of selection’ than his model seems to acknowledge: the fitness of operant lineages ( $\text{fitness}_{II}$ ) is not the fitness of the lineage (as its properties can only be seen as indirectly responsible for its retainment), but the fitness of the organism of whose behavior repertoire they are a part.

**Replication/reproduction with inheritance:** replication with inheritance is the most problematic concept in Darwinian operant conditioning. To make this clearer we discuss replication apart from heritability.

Replication: if we want to speak of replicators in operant conditioning it seems we have to identify them with the responses whose frequency is changing (in the behavior repertoire of an individual and that of a population). Unfortunately, being able to identify a replicator is not sufficient to speak of Darwinian replication. As already noted, there seem to be no *concurrent* replicators (similar to concurrent

responses) active in the behavior of an individual, whereas the concurrent existence of replicators seems unconsciously to be assumed in normal Darwinism.<sup>283</sup>

“However,” Glenn correctly notes, “there appears to be no reason to assume that all replication processes involve concurrently existing events or objects (id., p. 525).” That is true, even in biological evolution this is not needed as some animals lay eggs and then die before the eggs are hatched so that replicators need not exist concurrently (although at a population level they exist concurrently). However, what *is* required in Darwinian replication is that the replicators *multiply*. As only one response can be present in the behavior of an organism at one time, this multiplication seems impossible. But, the supporter of Darwinian operant conditioning can respond that the frequency of a response over certain intervals of time is increased, and that this comes close to multiplication in biology (the increase in the frequency of genes in a gene pool etc.).

This possible objection forces us to examine more carefully in what sense this process is not replication. We focus on the two other elements of our definition of replication as outlined in §1.4: a) be *in principle* the subject of replication too, b) causally determine, either directly or indirectly, their own replication process in a systematic way.

a) As the lineages of operant conditioning *principally* end when the single individual animal dies, the first condition is not fulfilled, just as in the case of the immune cells. The only way to overcome this replicative ‘dead end’ seems to be (just as in the case of immune cells) that the replicators or responses can be transported or copied to another individual so that the lineages continue. One can think of general teaching or learning mechanisms that could accomplish a thing like this. A father learns his way through the jungle by operant conditioning (trial and error way-seeking) and then teaches this to his son who learns his way around even better, again by trial and error conditioning. However, the teaching mechanism of this example cannot be based upon operant conditioning, so that the explanation of the origin of behavior can in a case like this not solely be explained by the supposed Darwinian processes.

One might object that the origin of the traits of life is not solely explained by ultimately Darwinian processes, as it involves a great number of proximate mechanisms. However, general learning is in this case not a proximate *mechanical* mechanism as in natural selection, but a conscious, intentional mechanism that comes much closer to an ultimate mechanism in the sense that it is a *functional* mechanisms that can be held responsible for *why* certain things are the way they are (§1.1). Thus it seems that there are no endless lineages without general learning mechanisms, but that with general learning mechanisms there is no complete *ultimate* explanation in terms of Darwinian principles.

b) Operant modifications of the lineage occur during the life-time of organisms, but they are not encoded in the germline. As such the replicators in operant conditioning do not *causally* determine their own replication. Of course, certain (traits of) responses *causally* increase the fitness of the organisms that inhibit them; however, they do not *causally* increase the fitness of the responses themselves (and not only because every new generation has to relearn the responses from scratch). In a thoughtful critique of Darwinian models of operant conditioning

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<sup>283</sup> Of course animals can be multi-taskers doing multiple things at the same time such as breathing, digesting, being on the look-out etc. However, it is logically impossible that two variants of the same type of response are simultaneously present.

William Baum (2001: 528) noted that “in no useful sense are repeated occurrences of a behavioral pattern copies. [...] If I brush my teeth every night before I go to bed, in no sense is my brushing one night a replica of my brushing the night before. Even though we are ignorant of the way the workings of the brain affect behavior, nothing we know suggests there might be a replica or representation of tooth brushing the night before. Even though we are ignorant of the way the workings of the brain affect behavior, nothing we know suggests there might be a replica or representation of tooth-brushing in the brain either. Rather, as with other natural events, such as sunrise, hurricanes [...] the event occurs when the conditions are right.” Baum suggests that if one wishes to extend the ideal Darwinian model to include operant learning one needs to include “recurrences”, “meaning just ‘occurring again’ or ‘turning up repeatedly’ (id.)” Biological replication would then be just one type of recurrence, namely recurrence by copying.

However, one may wonder whether generalizing the Darwinian model by including recurrences is not like throwing the baby away with the bath water; a Darwinian explanation is about design, if one allows for recurrences many non-designed things enter the realm of Darwinism such as hurricanes,<sup>284</sup> sunrises, chemical elements in cyclical or auto-catalytic reactions,<sup>285</sup> cells in the Game of Life etc.<sup>286</sup> that

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<sup>284</sup> See footnote 272.

<sup>285</sup> See appendix 1.

<sup>286</sup> The Game of Life is a cellular automaton devised by the British mathematician John Horton Conway in 1970 (see Gardner 1970). The universe of the Game of Life is an infinite two-dimensional orthogonal grid of square *cells*, each of which is in one of two possible states, *live* or *dead*. Every cell interacts with its eight *neighbours*, which are the cells that are directly horizontally, vertically, or diagonally adjacent. At each step in time, the following transitions occur:

- Any live cell with fewer than two live neighbours dies, as if by loneliness.
- Any live cell with more than three live neighbours dies, as if by overcrowding.
- Any live cell with two or three live neighbours lives, unchanged, to the next generation.
- Any dead cell with exactly three live neighbours comes to life.

The initial pattern constitutes the input or ‘seed’ of the system. The first generation is created by applying the above rules simultaneously to every cell in the grid, so that births and deaths happen simultaneously and each generation depends only on the generation before. The rules continue to be applied in new iteration rounds to create further generations.

Although the rules of the system are simple, some remarkable structures are able to evolve on the grid on which the game of life is played. These have been given names as: die-hard, glider, space-ship, and pulsar corresponding to their shapes and their actions in subsequent iteration rounds. Beautiful examples can best be appreciated by running the Game of Life oneself on one’s computer as many free versions are available on the internet (a free website dedicated to the Game of Life: <http://www.ibiblio.org/lifepatterns/>).

The remarkable emergent structures have been used by philosophers to convey the somewhat counterintuitive notion that *design* can spontaneously emerge in the absence of a designer. Daniel Dennett for instance used it to illustrate the possible evolution of complex phenomena such as consciousness (1992), life (1995) and free will (2003) from a relatively simple set of deterministic physical laws governing our own universe. However, as an illustration of *emergence in general* the Game of Life is fine, but it cannot be used well to elucidate the Darwinian emergence of life or of organisms as its workings are even less analogous to natural selection than that of operant conditioning: there is no true replication with inheritance (only *singular* cells ‘replicate’ (in the sense of returning in the next generation), not *complexes* of cells, even though some stable cell patterns are ‘repeated’ over a number of generations such as the pattern named ‘die hard’), no true variation exists as the variation is non-random and hard to recognize as such, since it is not really *variation of a replicator*, but only variation of cells in the neighborhood of cells, and no true adaptive selection as ‘extinction’ of collection of cells is not really due to their functional constitution (although it is based on their constitution). Moreover, the patterns in the Game of Life are not causally (mechanically) involved in their own evolution as Darwinian replicators are. Further, the game of life has problems on the edges of the web of the grid as the rules have to be modified for these cells.

Finally some criticise the parallel of the Game of Life with natural selection also on the grounds that the Game of Life is deterministic and that its outcome can be computed whereas the outcome of

we had good reasons to exclude as true explanatory object of Darwinian explanations. What may be different though is whether the recurrence was based on retention or heritability.

**Heritability:** we already said that properties are *retained* instead of *replicated*. Retention is a more general concept than heritability as heritability can be defined as a special form of retention, e.g. as *retention across generations*. In the case of operant conditioning generations would be ‘generations of responses’, and insofar operant conditioning is still equivalent to natural selection. The carrier of hereditary information in biology has been identified since the discovery of DNA; however, the carrier of the retentiveness of the operant lineages is unknown. As such operant conditioning is in a state of development as natural selection was in Darwin’s own days, before the discoveries of Mendel and Watson and Crick (§1.1, §1.33). That makes the theory not untrue, but not as well tested as natural selection is and thus in need of further study.

Research on the biochemical mechanisms underlying memory and learning seeks to identify the long-lasting changes in the nervous system by which learning progress and operant behavior are maintained (Donahoe and Palmer 1994). However, “the range of possibilities for memory maintenance is large. None of the proposed models have been firmly excluded, and there seems to be no clear candidate [that stands out above the competition] (Lisman and Fallon 1999: 340),” be it dopamine administrations on spontaneous bursting frequencies of neuron firings or supposed analogies of connectionist models<sup>287</sup> with neural networks. That was as true in 1999 as it is today.

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natural selection seems to be non-determinate. However, physical nature is also usually conceived as deterministic and in so far the outcome of natural selection is also determined beforehand, although we compute it. Moreover, the Game of Life can start from a *random* beginning with vastly different end-results and the random beginning is not contained in the nature of the game (see also §1.22).

<sup>287</sup> Connectionism is an approach in the fields of artificial intelligence, cognitive science and neuroscience. In philosophy of mind its main proponent is Paul Churchland (1989, 1995). Connectionism models mental or behavioral phenomena as the emergent processes of interconnected networks of simple units. The form of the connections and the units can vary from model to model. For example, units in the network could represent neurons and the connections could represent synapses. Another model might make each unit in the network a word, and each connection an indication of semantic similarity. Current models have grown much more sophisticated by including a hidden layer of units between the input and output units of the connectionist networks.

The most profound feature of these networks is their ability to learn, which involves modifying the connection weights between the units. A famous example is mine-detection: a network of units is trained to discern radar beams that landed on mines and beams that landed on the bottom of the sea; after some time the network was able to recognize new mines on the basis of new radar beams it had to analyze, and interestingly, it was able to do this *without having an explicit representation of the mine*.

This was swearing in the church of computationalism, the then dominant view in cognitive science which argues that mental activity is computational, *i.e.*, that the mind operates by performing purely formal operations on symbols (representations), in a way similar to a *serial computer* or possibly even as a Turing machine. According to the supporters of computationalism such as Fodor (1998, 1994, 1987, 1975) and Pinker (2007, 2002, 1997, 1988) connectionism was a reversion towards associationism and the abandonment of the idea of a language of thought, something they felt was mistaken. But for many this was just the attraction of connectionism as it meant getting rid of the mystical language of thought and the idea that the mind stores by explicit symbolic representations and innate rules of manipulation present in all cognitive tasks (not only in doing mathematics or linguistics) as well as getting rid of a host of specific modules computationalism usually presupposed in favour of general mechanisms of learning from external stimuli (see for modularity versus generality below on *evolutionary psychology*).

However, the biggest advantage of computationalist models is often thought to be that the models have *biological plausibility*, especially computationalist models using *recurrent* neural networks (Mandic and Chambers 2001). First of all because *mathematical* computational models seems instantiated by *biological* neurons and the synaptic connections between them, some of which form a directed cycle, but

Of course, Skinner's hypothesis that what constitutes a response on the behavioral level may not be what is strengthened in operant selection, may be true. Skinner imagined clusters of atoms to be the ultimate 'units of behavior'<sup>288</sup> susceptible to operant selection (1953: 94). However, even if we could find some brain elements using fMRI or other brain-probing devices that showed that some neural regions were activated during operant conditioning this region, this still would not give us the ultimate 'units of behavior' as this supposed neural region is likely only a very small part of "complex neuronal circuitry associated with reinforced response (Stein et. al. 1994: 156)." Real progress towards identification of the units of heritability is not likely to come soon. This means that Darwinian operant conditioning will probably remain in a pre-Watson and Crick stage for quite some time.

However, even if real progress were made and the proximate mechanisms underlying heritability were identified, Darwinian operant conditioning would still not mirror natural selection sufficiently to be called truly Darwinian. As we saw: operant lineages do not really replicate and they are all principally dead-end lineages.

## II) Neural Darwinism

The highly inchoate nature of our knowledge of neuronal processes does not prevent the supporters of neural Darwinism from making Darwinian-flavored speculations on the mechanisms behind (operant) learning and other cognitive processes. The founder of neural Darwinism, Gerald Edelman, received the Nobel Prize for his work on immunology that showed how certain white blood cells, capable of binding to a foreign antigen, increased by differential clonal multiplication following the discovery of an antigen (see chapter 1 **i**) cells immune system). This proved that the human body is capable of creating complex adaptive systems as a result of local events and feedback mechanisms encompassing the whole body. Edelman's model for neuronal Darwinism is parallel to his model of the immune system with its *directed replication* or *amplification* of certain white blood cells.<sup>289</sup> Our critique on why the

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also because the Parallel Distributed Processing (PDP) of relatively autonomous subparts of neural networks is much more plausible from the perspective of evolution than the serial computations of computationalism (Slurink 2002: 48): 1) the first multicellular organisms that evolved sensors or even specialised neural networks were not necessarily equipped with the means to integrate all the information offered, in various parts of their bodies different processes could go on, as long as they remained to some extent adaptive to the whole, 2) in the struggle for life speed is essential, insight is at best a side effect; even in complex organisms with a lot of central information processing many autonomous sub-circuits and automatic reflexes can be found which ensure that life goes on whatever is ordered from 'above', 3) neural networks are much more resistant to damage than devices which depend on serial computing, because the former are characterized by a multitude of input-output connections while the latter are characterized by only one long string which can break at any point.

However, even if a connectionist network can learn certain cognitive functions of humans and the human neural machinery can be constructed analogous to a connectionist network, and even if the evolution of the neural machinery adds plausibility regarding speed, non-centrality and damage correction, this still doesn't prove that in humans the specific cognitive functions are learned and implemented in the same way as in these networks.

We come back on the debate of computationalism versus connectionism as we discuss the work of Chomsky who was a supporter of (a mild form of) computationalism.

<sup>288</sup> Unit of behavior is a concept of Glenn (Hull, Glenn et. al 2001). It is constructed as a parallel to the unit of replication (i.e. DNA) in biological evolution. One can, however, wonder whether this terminology is not wrong in the sense that behavior is not so easily 'atomizable' or divisible into elementary parts. But, Glenn may reply, hereditary information was once conceived to be indivisible as well, but it nevertheless turned out to be available in discrete chunks in the genome.

<sup>289</sup> Other, but similar, accounts of neural Darwinism are defended by Changeaux 1985 and Calvin 1996ab. Edelman's theory ultimately sought to explain how we experience the world in spatiotemporal continuity

workings of immune system cells were not well described by a *direct* application of Darwinian principles also holds regarding neural Darwinism.

Edelman's model contains two parts: i) Anatomical connectivity in the brain occurs via selective mechano-chemical events that take place epigenetically during development. This creates a diverse *primary* repertoire by 'differential reproduction'. ii) Once structural diversity is established anatomically, a second selective process occurs during postnatal behavioral experience through epigenetic modifications in the strength of synaptic connections between neuronal groups. This creates a diverse *secondary* repertoire by differential amplification. Both processes are hard to understand, in part because so little is understood about the development of neural tissue.

i) The first process leads to a more or less fixed anatomical structure of the brain. This ('phylogenetic') structure can be said to have evolved by natural selection just as any other organ has, and genes (although mostly unknown) can be considered responsible for this development. Edelman, however, regards the 'ontogenetic' development of the fixed anatomical brain itself as a selection process in which neural cells *dynamically control* their binding to other neural cells using cell adhesion molecules so that group aggregates of neural cells are formed. Be that as it may, even if the details of the process Edelman describes are correct, we fail to see what this has to do with Darwinism. The process has more in common with chemical self-organization (see appendix 1) or even with the Game of Life (see footnote 286) than with a true instance of a Darwinian process in which we can distinguish replication, blind variation and environmental selection giving rise to functional complexity.

ii) Once the basic variegated anatomical structure of the brain is laid down during early development, it is more or less fixed. But given the numerous and diverse collection of neural cells available for circuiting and re-circuiting, Edelman postulates a second *selection* process amongst functionally equivalent (albeit anatomically non-isomorphic) neuronal groups capable of responding to certain types of sensory input. According to Edelman, this creates a competitive environment where circuit groups, proficient in their responses to certain inputs, are 'selected' through the enhancement of the synaptic efficacies of the selected network. This leads to an increased probability that the same network will respond to similar or identical signals at a future time as the neuron-to-neuron synapses are strengthened again and again (i.e. *directed* replication as in immunology, but this time directed *pseudo*-replication just as in the case of operant behavior). This process too has more in common with chemical self-organization or with the Game of Life than with a true instance of a Darwinian process in which we can distinguish replication, blind variation and environmental selection giving rise to functional complexity.

Given the little that we understand about neural mechanism it is small wonder that in its current state no neuro-scientist of note supports neural Darwinism. Although it is uncontroversial that man's flexible brain with its plastic neural wiring exists so that he can *adapt* his behavior (e.g., by trial and error learning) to new and ever changing environmental conditions, and although it is uncontroversial that neural states are adjusted (or 'selected' if you like) by new input from the external senses (and of course due to gene transcription in different life phases on a more

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by discrete sampling of stimuli from our many senses (a longstanding problem in both cybernetics and the brain sciences), but that need not interest us as we are concerned with his pseudo-Darwinian model of neural development as such.



fundamental level), neural Darwinism is more metaphor<sup>290</sup> than theory as it does not use the three fundamental concepts in a clear way.

### Phylogenetic models

In §2.1 we showed that cultural evolution depends on genes, other cultural elements present, our mind and the environment in which the evolution occurs. A table in which these factors are summarized (after Struwel 2002: 175, with slight modifications) can help us to gain a quick overview of the phylogenetic models, before we describe them in more detail. Note that in reality the supporters of the different theories do not always see them as completely distinct. Supporters of dual inheritance theory (Richerson and Boyd 2005) consider their theory to be a special version of evolutionary psychology. Some speak of cultural products as memes (McElreath and Henrich 2006). Supporters of cultural ecology can claim that although their methodological starting point is different, their theories are perfectly compatible with dual inheritance models and evolutionary psychology (Smith 2000). In 1996, the successful journal *Ethology and sociobiology* was renamed *Evolution and human behavior* because its contributors felt that their research scope had gradually widened from what we call sociobiology to evolutionary psychology. We present them here, nonetheless, as distinct theories. This will help us to get a better grip on the field of cultural evolution.

In the table, only the *major explanatory* factors of the theories are mentioned. For instance, all models acknowledge that the environment influences the evolution of culture in major ways; yet the factor from the environment to culture is nonetheless only specified in more detail for the model of cultural ecology, because this model seeks to *explain* cultural differences *primarily* as a straight consequence of environmental factors.

In the remainder of this § we briefly describe these theories and their problems and then summarize them in another table. The first evolutionary model of culture is or used to be a dominant model in contemporary (anthropological) literature that contrasts nicely with the Darwinian models.

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<sup>290</sup> The metaphorical nature of neural Darwinism becomes even more apparent in the work of William Calvin. Calvin wrote a favourable review of Edelman's book (Calvin 1988), and has been a supporter of neural Darwinism ever since. Like Edelman, Calvin is ultimately concerned with the continuity of our conscious experience of the world, which he also relates to competing substructures at the neuronal level. He elaborates his ideas in his popular publication *How Brains Think: Evolving Intelligence, Then and Now* (1996) and in *The Cerebral Code*, his research monograph from the same year. In both books, Calvin starts with the seemingly harmless division of brain processes into two types, those that depend on 'cerebral ruts' (hardware) and those that dance more freely through the brain and that depend on 'firing patterns' (software). Calvin's more audacious step comes when he suggests that the pattern of action potentials in any particular neocortical minicolumn can be replicated and spread through the cortex like a piece of software code and can be 'played' on the millions of other minicolumns in the same way you can play a million copies of a CD on a million CD players. The key difference is that while all CD players are designed to do basically the same task, the various cortical minicolumns can all have their own unique 'ruts' and the copies of the firing patterns are not exact duplicates. This allows for a 'cerebral symphony' rather than a million-fold amplification of the same tune and a 'survival of the fittest' process whereby those firing patterns that resonate best with the existing pool of 'ruts' will dominate our consciousness and generate intelligent behavior. Even if the speculative neurological details were correct, it would still be clear that this neural symphony is an elaborate metaphor and not a true application of Darwin's theory.

<b>Theory</b>	<b>III. Culturalism</b>	<b>IV. Cultural ecology</b>	<b>V. Memology</b>	<b>VI. Sociobiology</b>	<b>VII. Evolutionary psychology</b>	<b>VIII. Dual inheritance</b>
<b>Environment and its role in evolution</b>	Little to no causal power	Main force in evolution	Mainly environment of other memes	Social environment, main force selection	Human environment, main force selection	Natural & cultural environment, main force selection
<b>Culture (consisting of)</b>	Programs / ideas controlling the mind	Material artifacts in certain environments	Replicating units jumping from brain to brain	Social behavioural patterns	Mainly (maladaptive) human group behaviour	Adaptive & maladaptive behaviour patterns & material artifacts
<b>Nature of mind / brain</b>	Blank slate, ready to receive ideas	Blank slate, subject environmental forces	Blank slate ready to copy memes	Infinitely malleable by genes	Main explanatory force	Malleable, yet constrained by history
<b>Nature of genes</b>	Not discussed	Not discussed, treated as infinitely malleable	Genes and memes treated completely parallel	Main explanatory entity in theory	Malleable, yet constrained by history	Malleable, yet constrained by history

Table 2.1 Six phylogenetic models of cultural evolution

### III) Culturalism (ideational selection)

Culturalism also known as ideational selection is the view many anthropologists share. It is the view that the human mind is almost infinitely malleable, and that human life is determined to a large extent by cultural rather than biological factors. It is of course acknowledged that humans need to eat, drink and have sex. But even these biological needs are said to be inscribed within cultural systems of food customs and taboos, gastronomical traditions, moral and religious views of marriage etc. It became popular after the Second World War as pseudo-scientific *biological* theories of race were discarded for obvious reasons. It fitted well with an optimistic reform agenda of social engineering, as malleable human minds could now be molded into citizens of a more righteous society. But, paradoxically perhaps,<sup>291</sup> culturalism has also

<sup>291</sup> This paradox pervades Western social science and philosophy in the second half of the 20<sup>th</sup> century. On the one hand, people wanted to emancipate themselves and others from all sorts of oppression to create an ideal society in which everyone is free and equal: men and women, straight and gay, black and white etc. On the other hand, we found under the same banner of emancipation and anti-colonialism the readiness to accept every culture as it is. Any prescription of what other cultures should look like, any interference (even intellectual interference), would be a form of suppressive neo-colonialism. This is essentially the paradox of liberalism or democracy. Do you tolerate people who are not tolerant towards your views? Or in politics: should one allow democratic parties that want to abolish democratic institutions? This paradox

been linked to cultural (value-)relativism: as there is no human nature shared by all humans and no shared values (the tradition of natural law was but a relic of a dark past), all cultures were regarded from a detached, scientific point of view as equally worthy and beautiful. Culturalism finally meant that social scientists generally *ignored* the facts of natural science about humans, as they regarded culture as a ‘superorganic’ phenomenon that could only be explained from within.<sup>292</sup> Some even tried to *downplay* the importance of the findings of natural science (truth-relativism); these were criticized as the contingent findings of a male-dominated, heterosexual, white and Eurocentric power apparatus that created only truths that served to justify and continue its reign (which culminated in the famous Science Wars of the last two decades between natural and social scientists).

Culturalism has been enormously valuable for our understanding of the many subtle differences between cultures. When a culturalist tries to explain or describe a cultural practice, for instance Ancient Greek science, this is always done with many reservations: Greek ‘science’ is not a homogeneous whole, because different Greek ‘scientists’ practiced very differently. Perhaps it is like our modern science, as it is more a form of *contemplation* than an attempt to quantify and control nature; but wait, do not identify Greek culture too easily with ours. Contemplation is not like our meditation or yoga, an innocent and relaxing pastime after a stressful work-day. For the Greek scientists, contemplation was the highest form of *life* or *self-realization*. But wait: *life* for the Greek scientists is not just biological birth and death, but also participation in the *cyclical cycle* of the cosmos. And *self-realization* is not a realization of subjective preferences, as in our time, because the Greek self-experience was not that of a modern subject but the realization of what a man ‘objectively’ should be. But wait: views of *time* in Ancient Greece were not homogeneous, unarticulated ideas; neither were the views of *manhood* etc. In such a way, the explanation of a single cultural element can lead to an explanation of a whole cultural system or program. This cultural system or worldview is said to provide the people that live in it with *meaning*, something biologists are sometimes accused of ignoring or misunderstanding.

The problems of culturalism are many. To see this, we discuss one of the best-known discussions of culturalism by one of its main proponents, Clifford Geertz, in his *The Interpretation of cultures* (1973). Geertz starts his book by criticizing the Enlightenment view that there exists a universal human nature underlying all cultural differences – as large as those between Tibetan Buddhism, the culture of Ancient Greece and modern industrial society. According to him, the notion of an underlying human nature is an illusion; men unmodified by specific cultural practices have never existed, and are in fact impossible to imagine. The activities of men, without what Geertz calls “cultural programs,”<sup>293</sup> would be “virtually ungovernable, a mere chaos

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is a priori unsolvable. It requires pragmatic decisions from wise politicians in situations in which this paradox appears in social reality; when religious groups demand certain rights in a democratic society, for instance, sometimes these can be granted and sometimes not.

<sup>292</sup> For instance, Kroeber 1948: 7: “Particular manifestations of culture find their primary significance in other cultural manifestations, and can be most fully understood in terms of these manifestations; whereas they cannot be specifically explained from the generic organic endowment of the human personality, even though cultural phenomena must always conform to the frame of this endowment.”

<sup>293</sup> Clifford Geertz was highly influenced by systems theory and contemporary developments in the related fields of cybernetics and computer science. He sees the human mind as a cybernetic ‘control mechanism’. Along these same lines, he claims that “culture is best seen not as complexes of concrete behavior patterns – customs, usages, traditions, habit clusters – as has, by and large, been the case up to now, but as a set of control mechanisms – plans, recipes, rules, instructions (what computer engineers call ‘programs’) – for the

of pointless acts and exploding emotions, his experience virtually shapeless (1973: 44).” Geertz explicitly denies the view that culture is but an add-on to an otherwise ordinary animal. He also criticizes the ‘stratigraphic level approach’ of the relations between biological, psychological, social and cultural factors in human life as “a halfway house between the eighteenth and twentieth century”, claiming that any “parallelism between biological and cultural requirements” is “a mere correlation, and that intuitive, of separate findings (42).”

To uphold this idea, Geertz presupposes a *tabula rasa* view of human nature, which he compares to the hardware of an unspecialized multi-purpose machine. Culture is then compared to software, which can change this multi-purpose machine into a very specific machine. We can put this naive *picture* of the mind as a serial computer aside, but it is clear that for Geertz the human mind consists of only a few general talents, such as the ability to speak; all else is the product of specific cultural practices and ideas. In this scheme, ideas have an enormous “causal power” to transform human nature. This is why it has been dubbed the “ideational” concept of culture (Durham 1991).

One can wonder *where all these ideas come from* and how they can evolve. Geertz, unfortunately, doesn’t tell us this; his idea that culture is a self-contained system at the supra-organismal level rules out a biological explanation. However, “Would it be possible to create a culture in which food, sex, safety and status are completely irrelevant and in which other factors determine our behavior? Would it be possible to create a culture in which, for example ten sexes are acknowledged and in which children are deemed wiser than elderly people? (Slurink 2002: 180).”

Of course not. Cultural variants may have some or many maladaptive sides from the perspective of natural selection (vegetarianism, celibacy), but the *continued* transmission of cultural ideas cannot be seen as independent of our biological nature.<sup>294</sup> Although almost nobody will deny that human behavior is transformed by ideas, this transmission is *initially* dependent on the biological nature of the human mind (what can we imitate?) and *ultimately* (for their *continued* transmission) on how they affect human reproduction and survival. True Darwinian models focus on exactly these questions. The other five models have a more realistic view of human nature.

#### **IV) Cultural ecology**

Cultural ecology, also known as behavioral ecology, is the attempt to explain cultural phenomena in terms of environmental variables (Krebs and Davies 1997). It originates in the work of Julian Steward and was popularized by Marvin Harris (1974, 1977, 1979, 1980). Good modern overviews can be found in Cronk (1991), Smith and Winterhalder (1992), Borgerhoff Mulder (1991), Borgerhoff Mulder et. al. (1997; 1994), Winterhalder and Smith (2000). We already discussed the view of cultural ecology as we discussed the ‘common garden experiment’. Although the environment does influence cultural phenomena, we were critical of the notion that the environment is the primary causal factor determining culture. It seems much

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governing of behavior (...) man is precisely the animal most desperately dependent upon such extragenetic, outside-the-skin control mechanisms, such cultural programs for ordering its behavior (44).”

<sup>294</sup> Another reason culturalists think that cultural evolution is independent of biology is that they think evolution is something that happened in the past and that humans transcend nature and no longer evolve. However, that argument is wrong for two reasons: 1) natural evolution never stops, and if it seems to have stopped, that is due to stabilizing natural selection (see §1.23); 2) even if natural selection has stopped, that does not mean that cultural selection is independent of brain mechanisms that evolved by natural selection.

more likely that humans ‘invent’ cultural phenomena by taking the environment into account, and not the other way round. Nevertheless, the attraction of the explanations of cultural ecologists is their wonderful economic efficiency: they “explain much by little (1980: 63),” as Harris puts it. Take for instance Jared Diamond’s bestseller *Guns, germs and steel: the fates of human societies*, in which he tries to explain why the Europeans were strikingly successful at conquering and dominating the Americas, Australia, New Zealand and many other smaller islands, after the voyages of discovery. In contrast, though Europeans dominated and colonized Asia (China, India, Muslim Central Asia), the degree of domination was much less complete and much less enduring. Why is that? Diamond argues that due to the greater size of the Eurasian continent with its east–west orientation, it had more innovations per unit of time than smaller land masses. These innovations could easily spread throughout long east–west bands of ecologically similar territory. The Americas are not only smaller, but are also oriented north–south, making it difficult to diffuse useful cultural elements, like maize from (say) temperate North America to temperate South America, or domesticated animals like llamas in the opposite direction (Diamond 1997).

Behavioral ecology has four key assumptions (Smith 2000: 29): i) ecological selectionist logic, ii) the piecemeal approach, iii) a reliance on conditional strategies, and iv) the phenotypic gambit.

**i) Ecological selectionist logic:** the word ‘ecological’ implies looking for environmental features (e.g., resource density, competitor frequency) and examining the co-variation of them in these features and the behavior of interest (e.g., territorial defence). The word ‘selectionist’ implies predictions about what natural selection will favor (see points iii and iv).

**ii) Piecemeal approach:** the piecemeal approach means standard reductionism instead of the cultural holism of culturalism. It is thus an attempt to model a complex phenomenon (such as marriage patterns) into its elements (such as female preferences for mate characteristics such as wealth and rank in the group, male preferences, population models and the ecological and historical determinants).

**iii) A reliance on conditional strategies (‘decision rules’):** the hypotheses of behavioral ecology are often formulated in alternative rules: ‘in environment ‘X’, do ‘a’, in environment ‘Y’, do ‘b’.’ In these rules, the fundamental idea of behavioral ecology is most prominent: variation in environment is responsible for variation in behavior.

**iv) The phenotypic gambit:** the decision rules of iii) assume that this adaptive variation is governed by evolved mechanisms that instantiate the decision rule. This assumption takes a ‘black-box’ approach to the actual mechanisms involved. Grafen (1984) calls it the *phenotypic gambit*. You take the calculated risk that the phenotype is adopted so that the decision rule is indeed instantiated by the phenotype, even though you know nothing about the details of inheritance (genetic or cultural), the cognitive mechanisms involved and the phylogenetic history. In other words: one assumes total phenotypic plasticity (bodily and mainly brain plasticity<sup>295</sup>) and current (optimal) adaptation.

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<sup>295</sup> Brain plasticity means that the view behavioral ecology has of the brain is not so different from the view of culturalism. As Harris puts it: “the brain has only few *bio-psychological drives* without which even environmental explanations would not do such as the need to eat, to rest, to have intercourse and to need love and affection, but if you wish to postulate that human beings also naturally seek to create music and art, to dichotomize, to rationalize, to believe in God, to be aggressive, to laugh, to play, to be bored, to be

The last claim of the phenotypic gambit fits well with other ideas current in Oxford at that time Grafen postulated it. For instance, Richard Dawkins i) used the word gene in the sense of that which codes for a complex phenotypic property ( $gene_2$ ) without knowing the DNA responsible for it ( $gene_1$ ), and ii) usually ignored constraints and the possibility of less-than-optimal design. We criticized i) and ii) in §1.21 on genes and §1.23 on adaptation. Ad) ii) you cannot assume that evolution leads to perfect adaptation as the idea of mal-adaptation arising due to constraints is a fundamental part of Darwinian explanations (although the brain sciences are still in development: *total* brain plasticity is likely far from the truth) and ad) i) you cannot assume that every aspect of the phenotype is contained in the genome.

Despite these problems, behavioral ecology can be useful. It has been used to predict forms of subsistence, resource exchange, parental investment and reproductive strategies (polygyny versus monogamy versus polyandry). When you have almost no information about an organism, the rules of behavioral ecology can be a first educated guess. Thus work in behavioral ecology can supplement other analyses of cultural evolution.

## V) Memology and Dennett's universal Darwinism

We will first discuss Vi) memology and its problems, then Vii) Dennett's account of universal Darwinism (1995), which is a special version of memology.

**Vi) memology and its problems:** memes were introduced by Richard Dawkins (1976, 1982, 2006) into biology, as entities that (as the name attests) should be understood as actors on equal par with genes. Memes are said to comprise a unit of cultural evolution that propagates from one mind to another, analogously to the way a gene propagates from one organism to another as a unit of genetic information in biological evolution. As such actors, they can influence human brains and external reality. Dawkins himself did not provide a sufficient explanation of how the replication of units of information in the brain controls human behavior and ultimately culture, since the principal topic of the book *The Selfish Gene* was genetics. Perhaps the rise of memology is partly due to the fact that Dawkins did not give a complete treatment of the topic, but only raised it in a speculative spirit. Dawkins contented himself with a rather heterogeneous collection as examples of memes, comprising amongst other "tunes, catch-phrases, beliefs, clothing fashions, ways of making pots, and the technology of building arches." The examples of tunes and fashion are especially well-chosen, because these truly do seem to stick in our minds.

Meme theory became highly popular during the internet revolution, as all sorts of data (news, music etc.) could almost effortlessly flow from one computer to another. Its young wave of supporters published one of the first journals to be published solely online: *the journal of memology*. Recently the hype has quieted down, as more and more people have come to acknowledge the many problems with memology (or memetics, as it is also called). Important proponents are Daniel Dennett (1995) and Susan Blackmore (1999). Unfortunately, as far as we know, there doesn't exist an academic textbook on memes that defines and develops memology with true rigor. Most of the literature introduces a lot of new terminology for memes that mirrors some elements of biological (gene-based) Darwinism; there is 'memplex' (collections of memes), 'memotype' (the memes 'phenotype'), as well as

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free, and so forth (...) and succumb to the temptation to open this list to all nominations, you will rapidly succeed in reducing every recurrent cultural trait to the status of a biological given (1980: 63)."

many different versions of memetic selection. The literature also excels at giving examples of memes such as religious ideas, scientific theories, ideas on chastity or means of anti-conception. Finally, more recent literature defends the concept of memes against all the current attacks on the concept. All meme theorists, however, contend that memes evolve through an analogous process of replication (with inheritance), variation and selection. The metaphorical claim that memes jump from brain to brain just as viruses jump from body to body is also prominent in the literature (Brodie 1996). The most important contention of meme theorists follows from this metaphor. Memes most beneficial to their hosts will not necessarily survive; rather, those memes that replicate most effectively spread best, which allows for the possibility that successful memes may prove detrimental to their hosts. Examples of such detrimental ‘parasite memes’ are some religious ideas, as Dawkins maintains in *Viruses of the mind* (1993) and Dennett confirms in *Breaking the spell: religion as a natural phenomenon* (2006).

The problems with memology are manifold and have to do with:<sup>296</sup> a) heterogeneity of memes, b) the discrete nature of memes, c) the parallel with genes and their phenotypes, d) the dependency of the evolution of memes on the nature of the brain, e) the autonomous *evolution* of memes, intentionality and natural selection, f) the error-prone transmission, g) identification through general transformation or transformation in other media, h) identification through replication from multiple sources, i) fruitfulness of the research program. We will discuss these objections and (if possible) note what defenders of memology can answer to them, so that the picture of memology as here presented is as strong as possible.

**a) Heterogeneity of memes:** Dawkins’s original list of examples of memes already contained very disparate entities: tunes, fashion, architectural ideas etc. that come from many different domains. Other meme theoreticians give even more heterogeneous examples – such as scientific theories, religious customs, and political systems.<sup>297</sup> Meme theory, then, does not take the special nature of certain domains and the typical problems that arise in these domains into account (problems of mathematical truth in the evolution of mathematical memes etc., see §2.2). That is why meme theory may be too general to tackle the problems that Darwinian accounts of culture run into when applied in certain scientific disciplines. Meme theoreticians may, however, counter that the highly general principles of Darwinism can be used in many different realms of life (its generality is its strength), and that in different domains different proximate brain mechanisms are involved which still need to be discovered. That is true, but the case for memology would be helped if a successful (empirical) application of the theory in at least one domain could be demonstrated. Such an account has not yet been written.

**b) The discrete nature of memes:** related to the problem above, this is the lack of a consistent, rigorous and precise understanding of what typically makes up one *unit* of cultural transmission (Sperber 1995). The units need to be discrete in the sense of *discernable*, albeit not ‘quantised or atomised,’ so that ‘memplexes’ (combinations of memes) need not consist of a combination of a few basic memes as their building blocks. If we follow its proponents, a meme could consist of a single word, but also of a sentence, or a whole text (and in this example the memes remain

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<sup>296</sup> One of the best critiques on memology is the first part of Wimsatt 1999. Four of our nine objections can also be found in his paper.

<sup>297</sup> Blackmore 1999 re-stated the meme definition as “whatever is copied from one person to another person, whether habits, skills, songs, stories, or any other kind of information.”

in the same domain of written text, but see h). If its proponents really believe this, it would lead to enormous problems concerning the identity of memes: when do we have the same meme?<sup>298</sup> When we see a written word in another text or only when we see the whole text replicated? Some meme theoreticians draw a parallel between genes and memes. Genes are, roughly speaking, any piece of DNA of any length that is consistently replicated (see §1.21), and thus genes can be a few groups of bases but also whole chromosomes. Analogously memes are individual words, whole texts, etc. However, a DNA-string is much more identifiable as the same string within multiple generations; its neighbouring DNA-strings remain the same, so that clear replication lineages can be formed. Words and sentences, however, continuously reappear in new contexts. It is then hard to decide whether they are part of the same (generational) lineage or not.<sup>299</sup>

**c) The parallel with genes and their phenotypes:** the meme was constructed as a close parallel to the gene, but genes code for a certain phenotype, whereas memes seem to have no pheno- or memotype. Therefore they resemble more **a)** the chemical replicators of chapter 1 than DNA, and that is why meme theoreticians prefer to speak of memes as (brain) viruses. Viruses also have their own minimal phenotype, their capsid, but nonetheless know how to manipulate the cell machinery for their own sake. However, in the case of memes there doesn't even seem to be a capsid. Talk of memotypes thus seems nonsensical. Some have portrayed the meme (a tune, a dish) as the memotype and the real meme as the musical notes or the recipe that is in some way responsible for them. However, these are arbitrary parallels, because such a distinction cannot be made for other memes or memotypes. Some have proposed that the brain states ultimately responsible for the tune or the dish are the memes behind the memotypes (see Richard Brodie and Aaron Lynch).<sup>300</sup> However, as long as we do not know how brain states relate to behavior, let alone a tune or a dish, this highly speculative suggestion is even worse than the highly speculative neural Darwinism we discussed earlier.<sup>301</sup> But when no memotype as

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<sup>298</sup> See also Wimsatt 1999: 284 "All these point to a broader set of issues which suggest a deeper unravelling of the relevant fabric, for cultural inheritance has lots of problems with units and boundaries. How would we know when a meme corresponded to a gene? a chromosome? a gamete? a genotype? or the gene pool or pools of a family? Or even of a whole ecosystem? To give up these distinctions is to forego most of the structure one can find in an evolutionary genetic formulation of a problem."

<sup>299</sup> Another way the discreteness of memes is shown to be a problem is by pointing out (as in culturalism) that culture is a holistic phenomenon that does not come in discrete units. However, the meme theoreticians can answer that the same was said about organic beings as holistic entities that could not be analyzed as sums of parts, whereas gene theory has shown that organisms can nevertheless be analysed from a reductionist perspective. In gene theory also lies the answer meme theoreticians give to the charge that culture is a holistic entity. First of all, they claim that some elements of a culture are clearly discernable as distinct (sports, arts, gastronomic customs) just as animals have organs, and that just as some genes evolve in clusters so do memes.

<sup>300</sup> Within memetics, there is a heated discussion on externalism (the identification of memes with observable artifacts or patterns of behavior) versus internalism (which identifies memes with brain states). The main rationale for externalism is that internal brain entities are not observable, and memetics cannot advance as a science, especially a quantitative science, unless it moves its emphasis onto the directly quantifiable aspects of culture. Internalists counter with various arguments: that brain states will eventually be directly observable with advanced technology, that most cultural anthropologists agree that culture is about beliefs and not artifacts, or that artifacts cannot be replicators in the same sense that mental entities (or DNA) are replicators. A good overview of the debate can be found in Auger's 2000 congress bundle. Robert Auger is himself an internalist and has most recently synthesized his position in his book *The Electric Meme* (Auger 2002).

<sup>301</sup> See Charles L. Lumdsen and E.O. Wilson's *Genes, minds and culture: the co-evolutionary process* (1981), in which they argue that the fundamental biological units of culture must correspond to *neuronal networks* that function as nodes of semantic memory. (In his later work *Consilience: the unity of knowledge* (1998), E.O.



analogue to a phenotype can be found in meme theory, then almost all the tools of evolutionary dynamics (usually presupposing a distinction between a gene and a phenotype) become useless (Wimsatt 1999: 284). It then becomes highly unclear what value of meme theory has to science.

**d) The dependency of the evolution of memes on the nature of the brain:** meme theoreticians picture the evolution of memes (from the perspective of memes) as a battle for brain space. They even imagine that memes gradually change so that they slowly become better at hijacking our brain. In this scheme, our brain is a rather passive vessel, ready to be occupied by any sort of meme. As such, meme theoreticians' model of the brain isn't much more sophisticated than that of the supporters of culturalism or cultural ecology: it is more or less the same, scientifically naive blank slate. Further, in the picture memetic evolution offers, the brain is more or less passive while the memes are actively involved in self-replication; we normally imagine the brain to be active and memes or ideas to be passive. However, meme theoreticians can note that talk of active and passive is relatively arbitrary in evolutionary theory, as these qualifications of proximate mechanisms can easily be inverted (§1.21). Every passive disposition can be reinterpreted as an active action. Passive stasis can be reinterpreted as actively waiting for something. What counts in evolutionary theory are the ultimate changes in frequency of replicators. Whether a biological virus is more active than the cell machinery it sets in operation is *ultimately* (for the continued survival of the virus) uninteresting.

Nevertheless, there is some sense in which we can speak of real replicators being active and others not. We qualified replicators as *causally determining, either directly or indirectly, their own replication process in a systematic way*. We used this qualification to discard extended phenotypes such as beaver dams as non-replicators from genes as true replicators. Let's see whether such a parallel is possible in the case of memes and the brain. Do memes or the brain causally determine their replication in a systematic way? To answer this question, we have to decide on which factor the replication of memes is most strongly dependent. Two important factors are: a) brain dispositions such as cognitive preferences (as coded by genes) and b) other memes (i.e. you can learn Einstein's theory only if you know Newton's theory, calculus etc.). It seems that brain dispositions are much more stable than memes. That is why the arrow of causality is probably more directed from the side of the brain towards the memes, than the other way round. See e).

**e) The autonomous evolution of memes, intentionality and natural selection:** meme theoreticians imagine meme replication to be something that just happens by (blind) imitation, or because the memes are so persuasive that our brain can't resist their hijacking. However, in reality (human) selection of memes is *not blind*, but often intentional – and so is their variation (transformation). Think of scientific debates or discussion on law. This implies a serious violation of the concept of variation as outlined in §1.4. Some meme theoreticians are, however, not impressed by examples like these. They allow that variation of memes in lineages is not always *completely* blind or random, but maintain that one can *picture the evolution of memes as a blind evolution in which once in a while an intentional intervention changes the course of evolution*. As long as these interventions do not happen very often, the

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Wilson called these neuronal networks memes.) This neural network is to the meme, as the gene is to the phenotype. However, as we saw under epistemology, all the work in neural Darwinism remains highly speculative, and the problem of identification is not gone as long as we cannot identify individual neuronal networks and find a way to compare them across individuals.

evolution of memes can still be described rather well as parallel to natural evolution (Dennett 1995), *and the better the less often interventions happen*.

It is an empirical question how often these interventions in fact occur, but the acceptance of these interventions by meme theoreticians means that the evolution of memes cannot *solely* be described by memology, whereas biological evolution can be described by blind Darwinian processes. However, meme theoreticians could counter that when memes arrive on the scene (as for instance the meme of birth control, Blackmore 1999), biological evolution cannot solely be described by blind Darwinian processes and needs to include memes and their evolution; thus biological Darwinism would be confronted with the same problem of intentional interventions as memology. Be that as it may, it is usually the meme theoreticians who forget that the evolution of memes depends on biological processes and not the other way round (see d). And meme theoreticians should make *models* in which the evolution of genes and memes are combined, instead of insisting on the autonomous nature of the replication of the memes. *Genes can copy without memes, but memes cannot copy without genes that code for brains*.

**f) The error prone transmission:** we humans copy very imperfectly. Just take the example of a circle of children who whisper a message or a story they have heard from neighbor to neighbour. When the message is back to the original sender, the message is unrecognisably changed and valuable information is lost. Now children are notoriously bad copiers, but even adults are bad at these kinds of games. When the transmission introduces *too much* variation and when the variation is *too discrete*, the replicators are not *stable* enough (as the definitions of §1.4 require) *to identify* lineages of replicators. In other words, it then becomes nearly impossible to identify ‘species’ or types of memes. However, the meme theoretician can counter that we can identify songs or habits despite variations in them, so that the problem of error prone transmission or the impossibility of identification is not a real problem. Moreover, some memes are remarkably stable over multiple generations. Words from over 2000 years ago are still recognisable as the Indo-European ancestors of our current words today (Oudemans 2008). However, meme theoreticians would have to acknowledge that in some cases error prone transmission is a serious problem.

**g) Identification through general transformation or transformation of media:** In our definition of replication in §1.4 we included ‘and their intermediate forms’ to allow for the rare contingency that organisms could reproduce before they had gone through all their different life stages, if they were for instance sexually mature before the final stage of metamorphosis. In the case of metamorphosis, however, an organism goes through a *fixed* sequence of a limited number of stages. In the evolution of memes, on the other hand, metamorphosis occurs all the time, and – even more problematically – not through *fixed sequences* or even through sequences in which (a copy of) the original replicator comes back at the end of an arbitrary cycle. I learn a recipe from a cookbook, I tell it to my mother, she changes the recipe a little by adding some ingredients, her friend learns it from the dish she tastes from which she can deduce what the ingredients were, and she in turn can write the recipe on the internet – changing the recipe again in such a way that it is equal or (probably) not equal to my version in my cookbook. Can we still say that it is the same recipe? And if so, when is it the same? Is it the same when my mother transforms it? If so, then it is like f) *error prone transmission*, (but this time not due to accidental but to intended changes). And is it the same when somebody learns it from a cookbook or through the appreciation of the dish (i.e. does a meme remain the same when it is learned through another medium)?

**h) Identification through replication from multiple sources:** we humans often do not learn a complex theory from a single person, but from multiple, sometimes slightly contradictory accounts (Boyd and Richerson 2000). Sometimes we also forget elements and have to relearn them, or we require different explanations of the same theory by different people in order to gain true understanding. This makes a meme account based on replication very hard, since from whom did we replicate the theory? From nobody, it seems. This even further complicates the drawing of *lineages* of meme-like replicators. In biology, we identify species by a combination of cladistic and pragmatic criteria. Because of the multiple sources, cladistic methods become hard to use.

**i) Fruitfulness of the research program:** meme theoreticians seem to be able to explain why a meme or custom such as ‘wash your hands after going to the toilet’ can spread, because this custom prevents one from catching an illness. However, if one cannot empirically quantify the benefits and costs of these memes (how much does it cost to have fresh water around, what damage does washing ones hands with non Ph-neutral soap do to the natural protection of one’s skin?) it remains a just-so story. Beyond highly general explanations of highly complex phenomena (especially religion), meme theory has yet to produce a solid case study of a concrete phenomenon that has gained acceptance among either natural scientists or social scientists. All that memetic studies has done so far is translate conventional social thinking into ‘meme language’ without adding new explanatory value. That is a deadly sin in a research-based field.

In sum: memology is worse off than Darwin’s own theory in his own time. i) Darwin didn’t know about Mendelian genetics and the physical basis of discrete, quantised strings of DNA behind it, but his units of selection (the individual organisms or the units of inheritance behind them) were at least discrete and delimited. This is not the case for memes. ii) Darwin didn’t know as much about replication as we do today, but it was at least uncontroversial to say that animals replicate due to natural processes. It is unclear, however, by which *proximate mechanisms* memes ‘replicate’: by imitation, by instruction, by fear, by mechanical action (when books are printed), by word-of-mouth marketing or by all of them?<sup>302</sup> In our ideal of §1.4. we stressed that Darwinian explanations become better as we identify more proximate mechanisms. In this case, however, we have too many proximate mechanisms through which replication proceeds, from which we know far too little.

Finally, one can even wonder whether “replication” is still the right word in the case of the transmission of memes: “Most cultural items are ‘re-produced’ in the sense that they are produced again and again – with, of course, a causal link between all these productions – but they are not reproduced in the sense of being copied from one another. (...) Hence they are not memes, even when they are close ‘copies’ of one another (in a loose sense of ‘copy’, of course) (Sperber 2000: 164–5)” If we cannot use the word “replication” regarding the evolution of cultural elements, Darwin’s theory needs serious revision to account for cultural evolution. Such a revision is part of the content of the next evolutionary theory we discuss: the dual-inheritance model.

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<sup>302</sup> Just as a biologist who studies the evolution of a wing must know about natural selection and the physics of aerodynamics, so a meme theoretician has the even harder task of knowing about meme selection and psychological (imitation, instruction, mass fear) and economic mechanisms (market mechanisms, product cycles).

**Vii) meme theory and Dennett's Universal Darwinism:** given the above description of the principles of evolution, we are now in a position to assess Dennett's influential description of universal Darwinism, which is akin to memology. Dennett gives an elucidation of the process of natural selection by understanding it as an *algorithmic* process.<sup>303</sup> Dennett characterises these algorithmic processes by their a) substrate neutrality, b) underlying mindlessness and c) guaranteed results.

"[Ad a:] The procedure for long division works equally well with pencil or pen, paper or parchment, neon lights or skywriting, using any symbol system you like. The power of the procedure is due to its logical structure, not the causal powers of the materials used in the instantiation, just so long as those causal powers permit the prescribed steps to be followed exactly. [Ad b:] Although the overall design of the procedure may be brilliant, each constituent step, as well as the transition between steps, is utterly simple. How simple? Simple enough for a dutiful idiot to perform – or for a straightforward mechanical device to perform. (...) [Ad c:] Whatever it is that an algorithm does, it always does it, if it is executed without misstep. An algorithm is a foolproof recipe."<sup>304</sup>

It may seem strange to understand the natural process, a process in which variation and coincidences play such a major role, as an algorithmic process, i.e. a process with a guaranteed result. Did we not say in §1.22 that, if the process of evolution started again with slightly different initial conditions, the outcome would have been radically different? And indeed, the characterisation of Darwinian evolution as an algorithmic process remains vague, if we do not say what *kind of algorithm* evolution is.

To explain the kind of algorithm he thinks of, Dennett asks us to imagine a huge library he calls the library of Mendel. This library is like the library of Babel from a story of Jorge Luis Borges, which contains all possible books (of a certain length), because every possible combination of letters is available as book in this library, for instance a book with 500000 A's, another with 499.999 A's and a B at the end, another with 499.999 A's and one C, another with 499.998 A's a B and a C at the end etc. Hence this library also possesses a copy of *Moby Dick*, many copies of *Moby Dick* with one fault, even more with two faults etc. In contrast to the library of Babel, however, in the library of Mendel the 26 letters (and 10 or so punctuation marks) are replaced by the 4 bases of DNA: A, T, G and C (Adenine, Thymine, Guanine, Cytosine).

This library of Mendel is a "logical [design] space (idem, p. 110)" that contains "all possible genomes (p. 112)." Evolution is now the process by which out of all the different 'books', due to simple mechanical operations, only the *Hamlets* and the *Moby Dicks* remain. How so? Viable 'books', viable genomes *replicate* themselves so they remain into existence and due to simple mutations *variations* occur (shifts in the library). Most of them contain errors, and selection kills them, although sometimes these variations are viable, or even better than the original and thus replace the original volumes. In Dennett's own words:

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<sup>303</sup> Dennett 1995: 51 "We can now reformulate his [=Darwin's] fundamental idea as follows: 'Life on earth has been generated over billions of years in a single branching tree – the Tree of Life – by one algorithmic process or another.'"

<sup>304</sup> Ibid.

“The library of Mendel (or its twin, the library of Babel – they are contained in each other, after all) is as good an approximate model of Universal Design Space as we could ever need to think about. For the last four billion years or so, the Tree of Life has been zigzagging through this vast multidimensional space, branching and blooming with virtually unimaginable fecundity, but nevertheless managing to fill only a vanishingly small portion of the space of the Possible with actual designs. According to Darwin’s dangerous idea, all *possible* explorations of Design Space are connected. Not only all your children and your children’s children, but all your brainchildren’s brainchildren must grow from the common stock of Design elements, genes and memes, that have so far been accumulated and conserved by the inexorable lifting *algorithms* [our italics], the ramps and cranes and crane-atop-cranes of natural selection and its products (p. 143f).”

There are many problems with this view. First of all, not all possible combinations are included in this library because the size of a book or a string of DNA can be longer than the 500.000 letters, base pairs (or every other set of elements). More importantly: all possible ‘elements’ are not included. For instance, RNA has Uracil (U) instead of Thymine as its fourth element, hence all RNA strings (the RNA viruses, and the genome of some ancient bacteria running on sulphur) are excluded. Dennett seems to realize this, as he admits that the library of Babel doesn’t contain books written in different characters such as Russian and Chinese (p. 112). Since the number of characters and elements is not determinable, the ‘a priori’ idea of *a library of all combinations* seems to be a hopeless monstrosity already. But an even bigger problem is the inclusion of the library of Babel in the library of Mendel, the library of design space. You need extra elements for this as well, because both are written in *different* units (DNA bases and letters), leading to an even bigger problem: how to organise this library? What principle can we use to write down its catalogue?

Even if we equate for convenience the library of Babel with all the possible books written or transcribed into English, the question comes back: where can we find all the gestures, the artifacts such as tables, spoons, computers, flats etc., that are replicators too according to Universal Darwinism? Dennett should have read another story of Borges about ‘a certain Chinese encyclopaedia’ with an ‘impossible’ ordering: ‘the animals [in of the lemma of this encyclopaedia] are ordered as a) animals that belong to the emperor, b) mummified, c) domesticated, d) sucking pigs, e) sirens, f) fable creatures, g) dogs that walk about freely etc.’<sup>305</sup> Dennett’s library of all replicators can only have the same impossible ordering.

Put differently, even if genes and memes are, according to Dennett, subject to the same algorithmic *logic*, by what *analogy* can we a priori determine a principle that will provide a categorisation for all possible replicators, genes, memes etc.? How can we a priori identify a space of basic elements out of which all possible replicators are built? Universal Darwinism shows that new basic elements of replication can always arise (hence there are no ultimate elements).

At the basis of all these problems lies the idea of Design Space as a *logical* space.<sup>306</sup> Replicators are equated with their *logical* combination of basic units. Some

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<sup>305</sup> Quoted in the preface of Foucault’s *Words and things*.

<sup>306</sup> The emphasis on *logic* also invades Dennett’s description of what constitutes an algorithm. The third characteristic of an algorithm, its infallibility, is the infallibility of *logic*. However, real implemented algorithms – instead of idealised algorithms – can always go wrong, just as meiosis can always produce (unpredictable) mistakes in genetic sequences.

of these combinations are meaningful; others are not. This seems to work nicely for the library of Babel: the fortuitous combination of letters that makes up *Hamlet* is highly meaningful, the book that contains only A's is not. But the library of Babel forgets the background against which *Hamlet* is meaningful: people speaking English, people living under a certain monarchy, people experiencing complex emotions etc. Only because of this background can *Hamlet* be meaningful, and thus a successful replicator (it wouldn't have been in the times of the Neanderthals). Just the same holds true in the case of DNA. My DNA may be a combination of a billion bases. This combination is not *in itself* meaningful. It is only a fruitful replicator because other replicators (other base combinations) exist at the same time: the base combinations of the bacteria in my intestines, the base combinations of my food etc. The human base combination would be meaningless in the time of the protozoan bacteria two billion years ago, when plants didn't exist yet. A DNA string is only meaningful because it *codes* for a certain phenotype that in a certain *time and space* is thus well designed – that, while fighting against the natural elements, its natural enemies and its congeners, manages to replicate itself (and thus these genes). This *time and space*, this 'world,' is, however, completely absent from Dennett's a priori, logical library of Mendel (which is like a sterile Platonic heaven).

The algorithms in Dennett's library have more in common with the Game of Life from *evolutionary computing* (which he indeed also uses to illustrate natural selection) than with true natural or cultural selection. And our critique of the Game of Life as a true application of Darwinism (see footnote 286), also stands against Dennett's view of universal Darwinism.

As Dennett's theory is a general framework for both evolutionary genetics and memology, all criticisms of memology we just discussed also apply to Universal Darwinism: Dennett's view of evolution suffers from a) ignoring the problems in the specific fields of science in which his theory is applied, b) the non-discrete nature of the cultural elements he wishes to put into his Library of Babel, and c) the impossibility of the application of the important tools of evolutionary dynamics that depend on a distinction between phenotype and genotype within his library. Dennett's popular view of universal Darwinism thus seems to beget more problems than it solves.

## VI) Gene determinism (socio-biology)

Extensions of Darwin's theory to theories about society were already being made during Darwin's lifetime by Spencer and other so-called social Darwinists. They have given Darwinism a bad name (Ruse 2006) by transforming the *description* of life as a struggle for existence into a *prescriptive* norm for fierce competition or active selection to prevent the weak from replicating, surviving or being born (eugenetics). Thirty years after the Second World War, Darwinian ideas were re-invoked into sociology, this time usually not as *prescriptive* but as *descriptive* discipline that attempted to explain social behavior in all species by considering the evolutionary advantages the behaviors had.<sup>307</sup>

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<sup>307</sup> Nevertheless, we still find people who, on the basis of their description of evolution, come up with normative statements. Ridley 1993, for instance, actively argues for cooperation and pacifism on the basis of tit for tat; Dawkins 1976 asks us to "rebel against the tyranny of selfish genes" on the basis of his selfish gene theory. The critics of sociobiology argue that these seemingly accidental slips of the tongue from 'is' to 'ought' are inherent to the theory (see Lewontin, Kamin and Rose 1984's *Biology, Ideology and Human Nature: Not In Our Genes*). These criticisms have been somewhat silenced (Alcock 2001, Segerstråle 2000): sociobiologists are more careful not to fall into the traps of the naturalistic fallacy.

This discipline came to be known as *sociobiology*, after the title of Wilson's influential book (Wilson 1975). We already discussed this work and its use of Hamilton's rule under **m) beehives** in chapter 1. In **q) populations** we referred to other sociobiological work on multi-level selectionism (gene, kin, group etc.) and reciprocal altruism. More recent sociobiological work tends to make even more complex models on the basis of general reciprocity with inner and outer group competition etc. Some of it can hardly be distinguished from work in evolutionary psychology (see below e). We summarize the sociobiological models in the table below, as all except generalized reciprocity<sup>308</sup> have already been discussed in chapter 1. This table comes in handy when aspects of the evolution of language (language use for cooperation in groups, for cheater detection etc.) fall in the realm of socio-biology.

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There is currently still a vehement debate between Darwinism and ethics on the origin of our moral behavior, moral feelings and sentiments and moral judgments. This debate is highly sophisticated and complex and will not be taken up in this study, although it is closely linked to issues surrounding the field of sociobiology. It is important to distinguish evolutionary selfishness and altruism, which are highly technical concepts, from the everyday notions of psychological selfishness and altruism (see Wilson 1992). We speak of a psychological selfish action when someone's motives are selfish (egoistic), and of a Darwinian selfish action when it aids the process of survival and reproduction of some entity that is (ultimately) responsible for the action. Not all psychological selfish actions are Darwinian selfish or the other way round: i.e. when you force your neighbor to allow you to babysit because you love playing with children, this is psychological selfish, but not Darwinian selfish as this action has no direct relation to your evolutionary fitness or that of your genes.

<sup>308</sup> The theory of reciprocal altruism can be used not only to explain cooperation between members of the same species, but also to explain symbiosis. An example of this is the case of symbiosis between a hippo and the bird that cleans its teeth. The hippo would have a good meal if after cleaning its teeth it were to eat the bird, yet it doesn't; that is because the *iterative* profit it has of getting its teeth cleaned is greater than the small meal the bird offers (the few hippos that default to eating all birds in their environment soon see their replication changes decrease because of their bad teeth).

Level of selection	Key publi-cation	Basic Idea	Examples of adaptations
<b>Indivi-dual/or-ganism</b>	Darwin (1859)	<b>Natural selection:</b> organisms are made up of evolved adaptations designed to help them survive	Eyes, teeth, legs etc.
<b>Indivi-dual male / female</b>	Darwin (1872)	<b>Sexual selection:</b> organisms evolve physical and mental traits designed specifically to attract mates or to compete with members of one's own sex for access to the opposite sex	Peacock's tail, antlers, courtship behavior etc.
<b>Family and kin</b>	Williams (1966), Hamilton (1964), Maynard Smith (1964)	<b>Inclusive fitness (the gene's view of selection):</b> selection occurs most robustly at the level of the gene, not the individual, group, or species. Reproductive success can thus be indirect, via shared genes in kin.	Altruism toward kin, parental investment, the behavior of the social insects with sterile workers (e.g., ants).
<b>Kin</b>	Trivers (1972)	<b>Parental Investment Theory (PIT):</b> parents allocate more resources to offspring with higher reproductive value; siblings try to get as much as possible from their parents' common share.	Parental favoring, sibling rivalry, runt cannibalism.
<b>Parent-offspring interaction</b>	Trivers (1974)	<b>Parent - Offspring Conflict:</b> parents and offspring may have conflicting interests in general (e.g. when and how much to wean).	Parental exploitation
<b>Parental dyad</b>	Trivers & Willard (1973)	<b>Reproductive Value:</b> the two sexes often have conflicting strategies regarding how much to invest in a) which offspring, and b) how many offspring to have at one time (males choosing to maximize the number while risking the lives of females etc.).	a) Eschewed sex ratios during different phases of life-time favoring for instance (sterile) female workers at the nest in early life, b) Sexually dimorphic adaptations that result in a 'battle of the sexes', timing of reproduction.
<b>Non-kin small group</b>	Axelrod (1981), Trivers (1971)	<b>Reciprocal altruism (tit for tat):</b> one can interact peaceably with non-kin if a mutually beneficially reciprocal relationship is maintained across multiple social interactions, and cheating is punished.	Cheater detection, emotions of revenge and guilt, etc.
<b>Non-kin, small group</b>	Maynard Smith (1982)	<b>Game theory, evolutionary strategies:</b> organisms adapt to competitors depending on the strategies used by them. Strategies are evaluated by the probable payoffs of alternatives. In a population, this typically results in an 'evolutionary stable strategy' (ESS) a strategy that, on average, cannot be bettered by alternative strategies.	Frequency dependent selection, hawk-dove model, prisoner dilemma's.
<b>Traits at multiple levels</b>	Sober and Wilson (1994)	<b>Multi-level selection theory:</b> altruistic adaptations at the group level can be successful when there is strong competition <i>between</i> groups and <i>inner-group</i> competition is kept low.	Altruism at group level, inner-group cheat detection
<b>Non-kin, tribes, large groups governed by rules</b>	Gintis (2000) and others	<b>Generalized reciprocity:</b> peaceful interaction with non-kin strangers even in single interactions with group members can be successful if social rules against cheating are maintained by neutral third parties	To in-group members: generalized altruism, acting like a 'good Samaritan' To out-group members: xenophobia, racism, warfare, genocide.

Table 2.2 Results of socio-biology



Sociobiology is currently a well-developed branch of theoretical biology, and its results are generally accepted. The same standard as outlined in §1.4 should hold for these results.

In the past, sociobiology has often been accused of gene determinism, which holds that all behavior and thus also *cultural behavior* is determined by our genes. It is a straw-man position used by opponents of sociobiology such as Stephan Jay Gould and Marvin Harris, although some sociobiologists have given them every excuse for this portrayal. As one of the founders of sociobiology puts it: “cultural novelties do not replicate or spread themselves, even directly. They are replicated as a consequence of the behavior of vehicles of gene replication (Wilson 1978: 30).” In the same vein, psychologist David Buss (1995: 407) remarks: “culture is not an autonomous causal process in competition with biology for explanatory power.” And in a similar vein, Laura Betzig writes in an editorial to a reader on human nature: “I, personally, find culture unnecessary (1997: 17).”

To substantiate this extreme position, sociobiologists could point to correlations between certain biological needs and dispositions on the one hand and cultural phenomena on the other, and hold genes responsible for the correlation. See table 2.2 below, after Struwel 2002: 232.

<b>Biological/ethological phenomena</b>	<b>Human socio-cultural correlates</b>
Need for food (carbohydrates, proteins, vitamins etc.)	Hunting, gathering, horticulture, agriculture, industry, administration
Biological rhythms/clocks	Schedules
Sex, sexual selection, mate-guarding	Prostitution, marriage forms
Physical protection, sexual ornaments	Clothing
Safety, physical protection, home base	Huts, igloos, tents, houses
Health, hygiene	Medicinal plants, pharmaceutical products
Safety in numbers, sociability	Clans, groups, clubs, societies, school, sects, churches
Strive for dominance	Social stratification
Physical and behavioral sex differences	Gender roles
Individual differences	Different tasks, roles, functions
Dominant individuals, caring for harmony within group	Shamans, priests, prophets, ministers, psychologists

*Table 2.3: Correlates between biological and socio-cultural phenomena*

However, even if these correlations are true, they cannot be wholly explained by gene determinism. If we take genetic determinism as the hypothesis that our psychological mechanisms are perfect instruments for inclusive fitness maximization, and that culture is simply the deposit of all the adaptive decisions of all individuals, it becomes unclear why there is such a long learning period (involving so much trial-and-error) for cultural behavior. Clearly, pure genetic determinism implies a misunderstanding of the brain as an autonomous organ allowing organisms to dynamically interact with the environment. E.g., pure gene determinism (in the case

of animals with a brain) would require an indefinite reservoir of detailed motor instructions coupled to a typology of all possible situations that an animal could meet. This in turn would require an indefinitely long string of DNA and an enormous brain. This is clearly not the case in nature and thus gene determinism is wrong.

However, in reality, even behavioral geneticists are not proponents of pure genetic determinism: they of course know that the brain is an intermediate agent and know the crucial role of environmental factors in the decisions organisms make.<sup>309</sup> The successful rules sociobiology discovered are to be conceived as instantiated in the brain.<sup>310</sup> This intermediate agency of the brain is more prominent in evolutionary psychology.

## VII) Evolutionary psychology

The best-known Darwinian current in psychology is the paradigm of evolutionary psychology (Tooby and Cosmides 1992, Buss 1995). Evolutionary psychology attempts to explain mental and psychological traits – such as memory, perception, emotions or language – as *adaptations*, i.e. as the functional products of natural selection.<sup>311</sup> We will first discuss evolutionary psychology in more detail, and then compare it to the other evolutionary models we just described.

Tooby and Cosmides explain evolutionary psychology with five principles.<sup>312</sup>

- 1) The brain is a physical system. It functions like a computer, with circuits that have evolved to generate behavior that is appropriate to environmental circumstances.
- 2) Neural circuits were designed by natural selection to solve problems that human ancestors faced while evolving into *Homo sapiens*.
- 3) Consciousness is a small portion of the contents and processes of the mind; conscious experience can mislead individuals into believing that their thoughts are simpler than they actually are. Most problems experienced as easy to solve are actually very difficult, and are driven and supported by complicated neural circuitry.
- 4) Different neural circuits are specialized for solving different adaptive problems (a view also known as massive modularity or the Swiss-Army-knife model of the mind).
- 5) Modern skulls house Stone Age minds (a view also known as mismatch theory or the Stone-Age view of the mind).

The first three principles are self-explanatory, but the last two may need further elucidation. The Swiss-Army-knife model of the mind is lip-service to evolutionary psychology and to the computationalist view of the mind (which was dominant at the time evolutionary psychologists formulated their theory), which holds that the mind is not a general learning device, but consists of many modules

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<sup>309</sup> Pure genetic determinism does not even apply to dandelions, because these grow in an interaction with their environment and in reaction to the available nutrients and the available light. Perhaps one can say that genetic determinism only holds for simple chemical replicators, since at the moment phenotypes arose, the pure determinism of the gene was already broken by environmental interactions of the phenotype.

<sup>310</sup> Only in creatures *without a brain* in whose behavioral pattern we nevertheless discover sociobiological rules (think of the cooperation between bacteria) are the sociological rules a more or less direct result of the genes (and the phenotype they code for).

<sup>311</sup> In the words of their key proponents Tooby and Cosmides (2005: 5): “Evolutionary psychology is the long-forestalled scientific attempt to assemble out of the disjointed, fragmentary, and mutually contradictory human disciplines a single, logically integrated research framework for the psychological, social, and behavioral sciences – a framework that not only incorporates the evolutionary sciences on a full and equal basis, but that systematically works out all of the revisions in existing belief and research practice that such a synthesis requires.”

<sup>312</sup> See the primer of evolutionary psychology on <http://www.psych.ucsb.edu/research/cep/primer.html> from 1997, retrieved at November 22<sup>nd</sup> 2008.

that are specialized in specific tasks. It fits well with evolutionary theory, as highly specific evolutionary pressures can be imagined to have shaped individual modules. The Stone-Age view of the mind means that selection pressures that once adapted our mind to the way of living in the Stone Age need no longer be present; therefore an adaptation can turn out to have maladaptive features. An example: evolutionary psychologists explain the rise of diabetes in people worldwide from our need for fat and sugar in the Stone Age; currently, we still have a Stone-Age mind with its craving for fat and sugar. But as fat and sugar are currently cheaply and widely available, and we cannot resist ingesting them, we suffer from a maladaptive obesity pandemic.

The causal chain of evolution according to evolutionary psychologists is as follows:

Genes → mental predisposition → choice of available cultural variant → choice of behavior → fitness effect on behavior and replication → genes of offspring → mental predisposition of offspring.

In this scheme, the evolution of cultural variants is thus ultimately more or less a byproduct or consequence of gene/phenotypical fitness effects, and their evolution is not made an object of explicit study. The best results of evolutionary psychology relate to mate preferences (Buss 1995). In the case of mate preferences, there is clearly no cultural variant with an independent existence of man (such as an artifact) whose evolution has to be treated independently.

Evolutionary psychology has been heavily criticized (see Rose and Rose 2000). Some of the criticism simply echoes criticism of Darwinism in general: there are claims that evolutionary psychology is reductionist, that its explanations are ad hoc or tautologous, and that it suffers from pan-adaptationism. Possibly these criticisms are so often raised in evolutionary psychology because of the immaturity of the field, or because of the nature of the problems the field deals with. These problems are i) complex phenomena that are difficult to classify as biological adaptations or as culturally transmitted phenomena and ii) controversial subjects (adultery, homosexuality, partner choice etc.). We answered these charges in §1.3, in regard to the criticism of Darwinian explanations in general. Other forms of criticism are also raised against sociobiology, such as the accusation of strong genetic determinism and the reproach of falling prey to the naturalistic fallacy. These charges were answered above as we discussed sociobiology. The field-specific criticisms are i) the idea that adaptationist stories are dispensable for social scientists, and critiques of the ii) 'Stone Age' and the iii) 'Swiss army-knife' models of the brain evolutionary psychologists support, and finally iv) the reification fallacy.

i) Indispensability: It is argued that social scientists do not need adaptationist logic. And indeed, it is true that social scientists investigate many proximate mechanisms of social processes and that this research is independent of the *reasons* that these social processes are the way they are, just as physiologists can investigate many 'proximate' mechanisms of the body without considering the 'ultimate' reason that they evolved (see §1.1). However, physiologists often arrive at interesting research problems with surprising results when they take evolutionary processes into account. In similar vein, modern social scientists could discover an interesting research agenda about proximate mechanisms by taking the results of evolutionary psychology on ultimate mechanisms into account.

ii) Stone–Age mind: Some people criticize the idea of mismatch by claiming that it wrongly suggests that evolution has more or less stopped.<sup>313</sup> However, no evolutionary psychologist (as far as we know) has claimed that. They only hold that cultural developments have gone so fast that in this case genetic adaptation has lagged behind, due to which mismatch arose.

iii) Modularity of the mind. The strongest criticism is that the mind is no Swiss–Army knife; it does not consist of multiple problem–specific modules, but only of general–purpose problem solvers. The debate between the camps (modularity versus generality) is complex and based upon the little we know of the brain. That much seems clear; however, categorically opting for modularity or general purpose devices is wrong. Some brain functions are probably implemented by a series of specialized modules, such as perception or hearing (i.e. low–level sensory processing), but other functions such as mathematical problem solving may not have a module at all and instead rely on general cognitive and perceptual learning devices. The more specific a task (i.e. solving a mathematical integral versus thinking logically, or constructing the plural of nouns versus deconstructing the speech stream in words) and the less related to survival (recognizing clouds versus recognizing faces), the less plausible is the claim of an innate, specific module.

Despite this initial implausibility, supporters of modularism use one general argument in many different versions to plead their case of massive modularity, an argument called the frame problem (e.g., Tooby and Cosmides 1992).<sup>314</sup> The frame problem was initially formulated by John McCarthy and Patrick J. Hayes in their article *Some Philosophical Problems from the Standpoint of Artificial Intelligence* (1969). This article discusses the possibility of expressing a *dynamical domain of actions* in logic without explicitly specifying which conditions *are not* affected by an action. Later, the term acquired a broader meaning in philosophy, where it is formulated as the problem of limiting the beliefs that have to be updated in response to actions. The *logical* problem as formulated by McCarthy and Hayes was solved in the 1980s,<sup>315</sup> but

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<sup>313</sup> See Rose and Rose (2000): 1f. “Thus, for evolutionary psychology, what its protagonists describe as the ‘architecture of the human mind’ which evolved during the Pleistocene is fixed, and insufficient time has elapsed for any significant subsequent change. In this architecture there have been no major repairs, no extensions, no refurbishments, indeed nothing to suggest that micro or macro contextual changes since prehistory have been accompanied by evolutionary adaptation. The extreme nature of this claim, granting the huge changes produced by artificial selection by humans amongst domesticated animals (...) in only a few generations is worth pondering. Indeed, unaided natural selection amongst the finches in Darwin’s own islands, the Galapagos, studied over several decades by the Grants is enough to produce significant changes in the birds’ beaks and feeding habits in response to climate change. If for birds and beasts, why not humans?”

<sup>314</sup> In linguistics, for instance, this question comes back as the poverty of the stimulus argument in the argument for the innateness of grammar. See chapter 3.13.

<sup>315</sup> The frame problem in logic becomes apparent when one sees that specifying *only* which conditions are changed by the actions do not allow, in logic, a conclusion that all other conditions are not changed, so that one cannot logically conclude descriptive propositions about the world involving actions. This frame problem can be solved in first instance by adding the so–called ‘frame axioms’, which explicitly specify that all conditions not affected by actions are not changed while executing that action. However, one such frame axiom is necessary for every pair of i) action and ii) condition ‘such that the action does not affect the condition’. Thus, in formulating the action frames, one already needs the action frames. In other words, the real frame problem is that of formalizing a dynamical domain without explicitly specifying the frame axioms.

The problem seems easily solvable by a frame–independent extra condition added to the temporal logic – such as Descartes’ first general principle or law of inertia from the *Principia* (1644/1647), which states that “*everything* is presumed to remain in the state in which it is”. However, this solution is unsatisfactory in general, as Steve Hanks and Drew McDermott (1987) argued on the basis of their Yale shooting example. In this scenario, a turkey is initially alive and a gun is initially unloaded. Loading the

the general problem of representing and reasoning within dynamical domains was not.

Tooby and Cosmides hold that no learning is possible without constraining frames. Only *innate* modularism can provide these general frames (Cosmides and Tooby 1997), and that is why modularism is supposedly true, since we are all able to learn. However, Tooby and Cosmides have not fully understood the paradoxes of the frame problem concerning the *real* world. The problem is not that you need frames, but that you need *indefinitely* many frames. Modularism doesn't solve the problem of *indefinitely* many frames either, and thus it does not have a better theory of the prerequisites for learning than its competitors. We will show this using a well-known example from Wittgenstein's *Philosophical Investigations (PI)*. Wittgenstein (PI 185) asks us to imagine a student learning the row 2, 4, 6, 8, 10, 12 etc. After he seems to have understood this row, the student is asked to apply the rule he has just learned and which is called the 'add-two rule' to the number 1000. However, instead of writing down 1002, 1004, 1006 etc., the student writes down 1004, 1008, 1012 – as if he had changed the rule 'add two' into 'add four' when the numbers contain four or more digits.<sup>316</sup> "We say to him, "Look what you have done!" But he does not understand us. "You had to add two [the 'essence' of the row]; look, how you have started the row!" – He answers: "Yes! Is something wrong? I thought that's the way I should do it." – Or imagine him pointing at the row saying: "I did continue the row in the same way" – It wouldn't help us at all to say "But do not you see....?" – and to repeat the old explanations and examples (PI 185)."<sup>317</sup>

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gun, waiting for a moment, and then shooting the gun at the turkey is expected to kill the turkey. However, if inertia is formalized in logic by minimizing the changes in this situation, then it cannot be *uniquely* proved that the turkey is dead. This is the case because the simple action of shooting is a logically complex action consisting of loading, waiting, and shooting. If one formalizes this in sloppy logic ('there is a man with a gun and a living turkey, if the loaded gun goes off -> the turkey dies, the man loads the gun, the man waits, the man shoots'), then one cannot *logically* conclude 'that the turkey is dead', although there is no reason to believe that the turkey died before the gun has been shot. The problem is that the formalization above only includes the effects of actions, but does not specify that every temporal state not changed by the actions remain the same. In other words, a formula must be added to formalize the implicit assumption that 'loading the gun' *only* changes the condition of the gun and not the condition of the turkey (alive or dead).

Real solutions to the *logical* frame problem are given by predicate completion (which, roughly speaking, changes every 'if...then'- statements into 'if and only...then'-statements); other solutions are Reiter's version of the situation calculus, the fluent calculus, and action description languages.

<sup>316</sup> Or to 'if the number is above 100', or 'if I have done the experiment already three times before', or 'if the second digit is a zero' etc. It is impossible to be absolutely certain what rule somebody is following, because every instance of its use, tests the (supposed) rule again. And we are more tolerant in assuming somebody makes a mistake with children than with grown-ups, as Wittgenstein would add.

<sup>317</sup> Wittgenstein used this example to demonstrate that there is no essence of the number row that we could mentally grasp and that would account for the fact that we continue the row in the right way. However, since Kripke's interpretation of it (generally considered historically inaccurate) in his book *Wittgenstein on rules and private language* (1982), this example has been used to argue for meaning scepticism (in this case of number rows), as we never know with absolute certainty how we should continue the row or how we should apply the rule of +2 in new contexts; Wittgenstein would certainly not be a meaning sceptic and hold that the sequence of the row was fixed by a communal tradition of punishing and teaching until the current generation of students did continue the number row as everybody else. Because of the historically inaccurate portrayal of Wittgenstein (but see Kusch 2006 who argues contrarily), many philosophers refer to the subject of Kripke's book as 'Kripkenstein'. Historians and interpreters of Wittgenstein such as Baker and Hacker argue that Kripkenstein's point is a pseudo-problem that stems from a confused, selective reading of Wittgenstein and that misunderstands the verb "to know" as applicable to private knowledge instead of communal practices – something Wittgenstein would certainly deny (Baker and Hacker 1984). Others find Kripkenstein interesting in its own right.

This case shows that there is a theoretical problem with finding a rule behind some input given (be it mathematical sequences, grammatical sentences etc.), because multiple (or even indefinitely many) rules fit the data. Modularism seems to offer an explanation of how we can nevertheless learn the rule correctly: the rule is already innate (or a more general version of the rule is present that is calibrated by the input). However, that doesn't solve the problem, since the *application* of the rule in new cases runs into the same problem as finding the rule in the first place. The application of a rule depends on the input, and can take many forms as well. Thus modularism doesn't solve the problem it was supposed to solve, and thus one can instead opt for the ontologically cheaper option of as few modules as possible. And as for the real reason that we do learn to use the rule in the same way as other people: a learning mechanism that looks for the *simplest* rule<sup>318</sup> (until input data shows that the rule has to be more complex) can account for that. That mechanism can just as well be a general learning mechanism as a more specialised domain specific module.

Thus modularism and general learning theories are philosophically on the same footing, and the mind's modularity thus remains an empirical question.<sup>319</sup> Current consensus (if one can speak of consensus in the inchoate field of neurology) is that the (human) brain is clearly lateralized and that there is some form of localization in the plastid brain. Stronger forms of modularism are, however, not as popular as they once were, and there is "little sign of genetic determination of the fine structure needed for more specific modules."<sup>320</sup> Since but few evolutionary psychologists actually test or use modularism (Tooby and Cosmides are a clear exception), they can easily drop this element from their theory, which was once perhaps added opportunistically to be in agreement with contemporary neuroscience.

A final warning is that modularism as a strong form of innateness often leads to claims about the importance of mentalism, the inner perspective of the mind, in studying the brain. Strong innateness modularism postulates and mentalism are different hypotheses that can be defended or criticized independently, but in the literature the two are often found together. This is especially true in linguistics, where the mentalistic (neo-Cartesian) position is present in the work of people who speak of *mentalese* (the language of thought) whose structure is syntactically innate and whose content (words, symbols) refers via an internal, mentalistic sense to objects in the world. For instance, in the latest work of the cognitive scientist and linguist Pinker with the evocative title *The stuff of thought: language as a window into human nature* (2007), we find massive modularity, combined with a computational view of the mind.<sup>321</sup> This view supposedly applies rules to explicit symbolic representations, known to the mind, which ultimately refer via their mental sense to objects in the world. However, since Wittgenstein's private language argument, philosophers have been rather skeptical about theories of reference via mental senses (see §3.13). It would be interesting to see if the supporters of linguistic modularism dropped the

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<sup>318</sup> Simplicity is a system-relative property and has no precise definition, except in highly specific context such as that of Shannon's information theory. Nevertheless, simplicity as a more or less intuitive notion suffices for our account.

<sup>319</sup> Atkinson and Wheeler 2004 also argue that the right question may not be *whether* our brain is modular, but rather *how* much modularity there is – but they warn us that even this question is tricky: "there is no way of establishing a principled and robust distinction between domain-specific and domain-general features (2004: 147)."

<sup>320</sup> Conclusion of the extensive overview of research on brain plasticity in evolutionary development by Johansson 2005: 112. The only exception for modules Johansson makes is for 'low-level sensory processing.'

<sup>321</sup> See footnote 287 for more background on computationalism.

idea of mentalist reference, whether they would reconsider ideas about *mentalese* too, which in turn could convince them to drop computationalism and finally also the brain structure of modularism based on this theory.

iv) The reification fallacy: the reification fallacy is committed when abstract behaviors are treated as real ‘objects’ within the mind, when there is no sufficient evidence to suppose that such behaviors represent true discrete ‘traits’. The classic example is IQ. An IQ score is a statistical principal component (dubbed *g*) taken from the scores of *several* artificial mental tests. Despite this fact, many researchers early in the 20<sup>th</sup> century came to treat this *g* as a genuine *thing* within the brain.<sup>322</sup> Evolutionary psychologists respond that hypothesized psychological traits such as IQ, that cannot be measured directly, may be described as psychological ‘constructs’: theoretical hypotheses about how people differ, or how components of the mind work. The degree to which a construct is accepted in the scientific community depends on empirical research that demonstrates ‘construct validity’ (especially, predictive validity). Take the concept of schizophrenia, which is constructed as a list of seven or eight relatively unique factors. When someone who displays three or four of these factors is diagnosed as schizophrenic, one can predict finding the other ones with some certainty. The fruitfulness of the predictions determines whether a construct is valid. Researchers further assume that when people differ on a psychological construct, there are underlying neurological differences between them. However, until neurology gets off the ground and we have true causal (physically and physiologically buttressed) explanations, ‘construct validity’ is the best we have.

In sum: the criticisms of evolutionary psychology can be answered, although the insistence of the mind as a Swiss Army Knife may remain problematic. After this introduction, we can now easily compare evolutionary psychology with the other models. Evolutionary psychology is closely linked to sociobiology, but its supporters, especially in the early days, have stressed their relative differences. Whereas sociobiology focuses on sociological phenomena such as communality, altruism, trust, cheater detection etc. and does so for *all sorts of organisms*, evolutionary psychology only deals with organisms with a *nervous system* (often even restricting itself to humans and their mate preferences), although its scope is not restricted to sociological phenomena. Evolutionary psychologists once criticized the supposed gene determinism of sociobiologists, since in their work there is great emphasis on the intermediate agency of the mind between genes and the environment: although genes do program the mind (which is not a blank slate ready to be imprinted by any environmental stimuli as early empiricist thought), the mind makes its own decisions. However, when it comes to the adaptive pre-programming of the mind by genes, evolutionary psychologists often also invoke the gene-level perspective. Other key differences between the two fields include the emphasis on *domain-specific* rather than *domain-general* mechanisms; we saw that evolutionary psychology views the mind as a sort of Swiss-Army knife,<sup>323</sup> whereas sociobiologists focus on behavior and learning in general without theorizing about the psychological mechanisms behind them. Finally, whereas supporters of evolutionary psychology stress mismatch (possibly

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<sup>322</sup> See Gould’s 1981 *The mismeasure of man* and the debate surrounding Herrnstein and Murray’s controversial *The Bell curve* on the relations among IQ, social class and race, as for instance collected by Russell Jacoby and Naomi Glauberman 1995.

<sup>323</sup> Evolutionary psychologists do not often use general concepts such as ‘culture’, ‘learning’, ‘rational choice’ and ‘fitness maximization,’ as they consider these insufficiently modular.

because they focus so much on humans), sociobiologists usually work from the assumption of fitness maximization.

Supporters of evolutionary psychology are often highly critical of memology, and rightly so. As they do not believe that cultural elements replicate, they tend to pay no attention to the evolution of cultural elements, picturing cultural evolution as a process in which genes determine mental predispositions that in their turn partly determine, in confrontation with the socio-cultural environment, which of the available cultural variants a person chooses. In this scheme, the evolution of cultural variants is thus ultimately more or less a byproduct or consequence of gene/phenotypical fitness effects, and the evolution of these variants is not made an object of explicit study. Dual inheritance theoreticians, who sometimes consider their field a sub-branch of evolutionary psychology, do make cultural variants and their evolution an object of research. These theoreticians also show how culture (although always analyzed in relation to genes and biological fitness) cannot be merely a byproduct or another proximate mechanism, but is a real ultimate mechanism in evolution. Dual inheritance does not oppose evolutionary psychology, but supplements it in a meaningful and necessary way.<sup>324</sup>

### VIII) Dual-inheritance theory

Dual inheritance theory can be seen as a combination of evolutionary psychology and the few good elements of memology. It does not restrict itself to evolutionary explanations of our cultural abilities as normal evolutionary psychology does, but also considers the evolution of cultural elements just as memology. This dual perspective is the source of its name. According to dual inheritance theory, the evolution of human life is shaped by the *two-sided interaction* of inherited cultural abilities evolved by natural selection and inherited cultural elements evolved by cultural evolution. Differing from memology, dual inheritance theory will never consider the *autonomous* evolution of cultural elements, but will always calculate how the evolution of cultural elements will influence the natural fitness of human beings (e.g., their successful survival and reproduction). Moreover, it doesn't speak of the *replication* of cultural elements as memology does, but only of their *transmission*. Differing from evolutionary psychology, it is more quantitative and generally focused on cultural adaptation instead of maladaptation (though it does not deny that cultural phenomena can be maladaptive from the point of view of natural selection). Further, evolutionary psychology tries to understand the existence of cultural variants from innate psychological *dispositions*: our preference, especially as a child, for sweet things etc. That makes it hard for evolutionary psychologists to understand (from their theoretical perspective), why we should like to take bitter medicine that makes us healthy. Dual inheritance tries to explain how we sometimes learn to *override* our innate dispositions in favor of adaptive cultural variants.

We already saw some examples of the strengths of dual-inheritance theory, as we discussed that cultural models have real explanatory value (contra the claim of them being trivial redescriptions), and as we pointed out that humans are often bad naive population thinkers (see §2.2). The ideas of dual inheritance theory are relatively new. The general idea of co-evolution itself is only as old as Ehrlich and Raven's paper from 1964 on the co-evolution of insect and plant species. Co-

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<sup>324</sup> It also places its emphases differently, as it doesn't take a stance in the modularism debate and it highlights the adaptive sides of culture more than the mismatched or otherwise maladaptive sides of culture.



evolution between a *biological species and a cultural custom* was first noted and experimentally tested by Simoons (1970, 1969), who suggested that the ability to digest lactose evolved in response to a history of exposure of dairy products, thus explaining why lactose tolerance existed only in areas in the world where dairy culture had been common (see footnote 83). Simoon's hypothesis was controversial at the time, but subsequent genetic data confirm that adult lactose digestion is controlled by a single dominant gene, and careful statistical work indicates that a history of exposure to dairy culture is the best predictor of a high frequency of this gene. Moreover, careful quantitative models that make some simple assumptions regarding genetic evolution (natural evolution), the spread of dairying (cultural evolution) and the natural fitness benefits of dairying (thus illustrating both dimensions of dual-inheritance) have demonstrated that there has been plenty of time for this gene to spread since the origin of dairying (Cavalli-Sforza, Menozzi and Piazza 1994, Holden and Mace 1997). The example of lactose digestion as example of co-evolution between a biological species and a cultural custom remained an isolated one, until it was placed within the general frame-work of an evolutionary theory of co-evolution between the human body and cultural customs in the beginning of the 80's.

The main, often co-publishing proponents of dual inheritance theory are Peter Richerson, Robert Boyd (Boyd and Richerson 1985, Richerson and Boyd 1992), Joseph Henrich (2004ab, 2001), Richard McElreath (Henrich and McElreath 2003, McElreath, Boyd and Richerson 2003) and Gil-White (Gil-White 2001, Henrich and Gil-White 2001). Their work is highly akin to the once more-prominent theory of 'gene-culture' coevolution (Sperber 1996, Durham 1991, Lumdsen and Wilson 1981, Cavalli-Sforza and Feldman 1981, Pulliam and Dunford 1980). The best current book-length account of the theory of dual-inheritance theory is Richerson and Boyd 2005, and a good summary provide Henrich and McElreath 2006. The typical problems evolutionary psychologists consider are cultural diffusion of innovations, inner-group altruism (reciprocal and group-level), the evolution of ethnic marks and outer-group discrimination, punishment of free-riders and defectors etc.

A defense of dual-inheritance best starts with anecdotal evidence on the significance of (primitive) culture: modern westerners with plentiful modern equipment have died while crossing rough environments such as the Kalahari Desert, Death Valley or the Arctic regions – regions that supposedly primitive people like Bushmen, Indians or Eskimos not only cross, but also inhabit. This is due to the enormous amount of cultural knowledge that is *adaptive* for successful survival and reproduction<sup>325</sup> in the specific regions that Bushmen, Indians and Eskimos have: knowledge of edible and poisonous plants, knowledge of predators and prey, routes and safe resting areas, medicinal herbs, transport techniques and buildings adapted to the climate (kayaks, igloos), reproductive strategies that take the climate or subsistence means into account etc. Having cultural knowledge is thus a selective advantage (or even requirement) to survive in these regions.

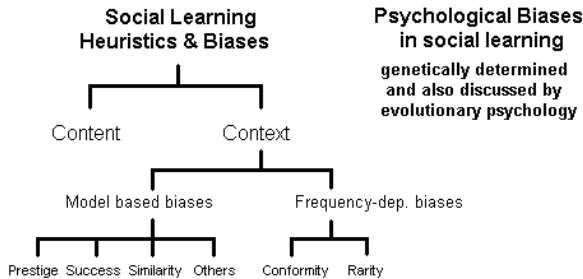
The cultural knowledge could in principle be won by self-learning (trial-and-error learning) or by learning from others (by imitation, instruction etc.). Supporters of dual-inheritance theory point out that the amount of knowledge is too

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<sup>325</sup> As the knowledge is adaptive for our survival, it can be called an ultimate instead of a proximate cause (Boyd and Richerson 2005: 13), in line with our terminology of ultimate and proximate mechanisms of §1.1.

extensive and the risk to experiment with these things too great to believe that this knowledge was won by individual trial-and-error learning. There must be social learning by parents and peers, whereby dual inheritance theorists typically downplay the role of parents (vertical transmission) in favor of learning from others (horizontal peer and vertical generational transmission, as with teachers etc.)<sup>326</sup>, even coming up with a conjectural adaptive reason why this is the case. “Social learning is about collecting adaptive information from the surrounding social environment. Increasing the size of the sample increases your chance of acquiring useful information, because a larger sample makes all kinds of biased transmission more effective. These forces, like selection, depend on variation. The more models surveyed, the more variation the bias has to work with. This is easiest to see for what we call content bias, an ability to judge the utility of a cultural variant directly on its merits. Mom may be an inefficient or poorly informed gatherer, and an aunt, grandmother, in-law, or friend may be much better. But if you can only learn from Mom, you are stuck with her way of doing things. By searching more widely you increase the chance that you will observe something worth learning (Richerson and Boyd 2005: 157f).”

Most models of cultural dual inheritance, however, are not focused on *content biased* transmission of cultural variants, but on “adaptive information often embodied in socially learned rules, techniques, and heuristics that are applied with little or no understanding of how or why they work (Henrich and McElreath 2006: 2).” The social mechanisms behind this form of learning without understanding are *context based* mechanisms, such as imitation of the person with the highest prestige, or imitation of the cultural variation that is most common, or the other way round. These biases can be small, just as evolutionary advantages can be small, yet be successful in evolution (Richerson and Boyd 2005: 84). Henrich and McElreath (2006: 8) give a useful overview of the learning mechanism that we here reproduce with some changes:



<sup>326</sup> Boyd and Richerson 2005: 156: “Most Americans (at least most American parents) mistakenly think that parents are the main source of their children’s beliefs and values. True, children normally form close bonds to parents, and in some cultures, parents make strenuous efforts to shape their children’s beliefs. True also that beliefs and attitudes of children and parents are often quite similar. However, much evidence indicates that parents play at best a minor role in many domains in determining the final cultural variants their children adopt [Harris 1998]. Behavior genetic studies indicate that most of the similarity between the personality traits of parents and children is due to genetic inheritance, not vertical cultural transmission. At the same time, these studies also detect a large amount of ‘environmental’ variation that is *not* shared within families. Children learn a lot from one another, and from adults other than their parents. Immigrant children in the United States usually learn English from their peers, and come to prefer it over their native tongue. When people move from one region to another, their children usually use the local dialect rather than their parents’ (Labov 2001, ch. 13). In other domains, transmission from nonparental adult to children is also influential, particularly when formal education is important.”

Content biases and heuristics arise from the interaction of human psychology and the characteristics of ‘cues’ associated with the cultural element being transmitted. These biases affect the likelihood that a particular cultural variant will be transmitted based on what it is, its ‘essence’ or some of its ‘accidental properties’. Context biases and heuristics arise from the learning environment. Model-based biases result from cues or characteristics of the potential model (‘an individual who is good to imitate’), and make the cultural variant more likely to be transmitted than those held by other individuals. Other model-based biases may include age, sex, ethnicity, healthful appearance etc. Frequency biases use the commonality or rarity of a behavior as a cue.

The reason dual-inheritance theorists usually exclude content biases is that they are a) almost infinitely variable, b) partly already discussed by normal evolutionary psychology, and c) often trivial.

a) Almost infinite variability of cultural variants: there are a great number of cultural variants. Successful ones are adopted for many different reasons: being more efficient, being cheaper, being easier to use, being more beautiful etc. Note that these content biases need not be conscious, although they usually are (people may buy new clothes to follow the current fashion without being able to make explicit why they have made these choices. Unconsciously, they may have picked up what is ‘hip’). Dual inheritance theorists generally ignore the infinitely variability of content biases, because they are interested in *general* models of the diffusion of cultural elements – although they can sometimes give explanations for historically contingent processes, such as the disappearance of certain technologies in Tasmania due to a strong population decline.

b) Overlap with evolutionary psychology: some of the content bias is in reality (partly) a bias of the psychological disposition of the person who adopts it. As evolutionary psychology already studies why certain cultural variants are adopted on the basis of our (genetically) coded psychological dispositions, dual-inheritance theorists do not focus on them, although they do acknowledge the existence of the psychological disposition.

c) Triviality: the abacus was replaced by the pocket calculator for reasons too obvious to require theoretical explanations.

The supporters of dual-inheritance seek empirical material to support their claim that many of cultural variants are not adopted because of their content, but because of context bias, i.e. because a prominent or successful person in a community has adopted it, or because people display herd behavior instead of thinking independently when it comes to adopting a certain cultural variant (on the basis of its content).<sup>327</sup> An example is the research on farming techniques including the rules for inheriting property versus selling etc. adopted by generations of farmer families that emigrated from different parts of Europe to the US (see §2.2 and footnote 263).

However, supporters of dual inheritance theory do not stop with understanding the mechanisms why cultural variants are adopted. They also look at the cultural variants themselves and their adaptive evolution. As the supporters of

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<sup>327</sup> This research is ideally calibrated by the consideration of the ‘common garden experiment’. See §2.2. Contra cultural ecology, supporters of dual-inheritance usually do not believe that the *natural* environment constrains cultural variants strongly (see for instance Richerson and Boyd’s critique of Marvin Harris, Richerson and Boyd 2005: 28f.). The environment of other *cultural elements* may be of key importance though.

dual-inheritance are generally aware of the problems of memology, they do not speak of replication of memes, but of *transmission* of (clusters of) cultural variants. Their models acknowledge that transmission is usually error prone. However, it is also the case that some cultural variants remain the same for a long time or remain at least identifiable for many generations.<sup>328</sup> The error prone transmission so typical of cultural variants can even be overcome by learning from more than one person, a process that can lead to *stabilization* and *refinement* of cultural variants.<sup>329</sup> These cultural variants can be maladaptive as evolutionary psychologists and especially memologists stress, pointing to supposed maladaptive cultural phenomena such as superstition, irrational taboos, but also adaptive such as the knowledge of the Bushmen, Indians and Eskimos. Supporters of dual-inheritance theory go to great lengths to argue that most cultural knowledge is adaptive and that cultural maladaptations only occur because they exploit the same knowledge and learning mechanisms as adaptive cultural variants (Richerson and Boyd 2005: chapter 4).<sup>330</sup>

Finally, long periods of stabilization and refinement of cultural variants can affect the genes of the adopters of these variants (think of the cultural custom of dairy farming which affected the lactose tolerance genes of the farmers, see Richerson and Boyd 2005: chapter 6). Thus it is clear that the supporters of dual-inheritance never

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<sup>328</sup> Dairy farming (as pastoralism) has probably remained more or less the same for 10,000 years since its adoption until modern times. With 30 years per generation, this means stability for about 330 generations, or 660 generations of transmission. See also Boyd and Richerson 2005: 87 “Historical linguistics suggest that [cultural learning] mechanisms can maintain detectable similarities in language over hundreds of generations. Sir William Jones, the Chief Justice of India, launched the discipline of historical linguistics at the end of the eighteenth century by demonstrating that Sanskrit has certain remarkable resemblances to European languages such as Greek and Latin, resemblances too numerous to be explained by chance. Instead, these languages and a variety of others belonging to the Indo-European language family are all descendants of a single language known as Proto-Indo-European [spoken many thousand years ago]. (...) To be conservative, let’s suppose that Proto-Indo-European was spoken six thousands years ago, or roughly 240 generations long. Each generation, children learned the sound-meaning associations from adults, and then served as models for the next generation. Thus the similarities that historical linguists use to link these languages have survived 480 generations of cultural transmission, indicating that cultural transmission can be quite accurate indeed.”

<sup>329</sup> See Henrich and Boyd 2002. This study also addresses the charge that cultural elements are not discrete. Discrete can mean two things: i) non-holistic and ii) non-continuous. Henrich and Boyd reject the first version of the charge, as in memology, by pointing out that some cultural elements can clearly be isolated from others; they reject the second on the basis of their analysis of learning. They show how, due to the biases described above, the transmission of cultural variants (which are in principle often continuous: what percent of your income do you give to the church, for example?) reduces to a special case of the discrete-trait replicator model commonly used in both culture and gene evolution, if the biases are sufficiently strong relative to the causes of variation in cultural traits.

Note finally that whereas the fact of learning from multiple sources was a serious challenge to memology, this fact is of vital importance to dual-inheritance theory to demonstrate how error-proneness can be overcome.

<sup>330</sup> Supporters of dual-inheritance theory usually explicitly suppose that cultural and social knowledge transmission mechanisms evolved *directly for the evolution of adaptive cultural variants, i.e. for transmitting useful knowledge and customs*. The trigger event for the evolution of these mechanisms would have been food starvation in certain habitats, which required homonoids to share knowledge on food patches and food types to survive. We discuss this idea in more detail and why we think it is wrong in §3.1 on homonoid brain growth; there we will argue that the adaptive reason for brain growth is probably the evolution of competitive social intelligence and not the transmission of knowledge on resources. If true, knowledge transmission of resources could be seen as exaptation of competitive social intelligence and knowledge; cultural variants could then still be adaptive, neutral, or maladaptive to human survival. However, we think that there are good reasons to believe like the supporters of dual-inheritance, that in general they are most likely more often adaptive than maladaptive as the costs of social learning mechanisms are high and humans would otherwise have evolved more mechanisms to defend themselves against the negative aspects of cultural evolution if cultural variants were usually maladaptive.

regard the evolution of cultural variants as an autonomous process, but as an co-evolutionary process with the evolution of the genetically coded disposition of the brain and the genes in general. Their work is also not just a redescription, as the research has resulted in surprising facts for us ‘naive population thinkers’.

Let us now look at the three concepts: replication, variation and selection.<sup>331</sup> Both i) for the genes and ii) for the cultural variants in the dual-inheritance models.

i) For the genes, we can simply claim that the ideal of §1.4 holds and there is no reason to suppose it would not. The cultural variants are simply part of the environment that causes selection pressure on the genes. Therefore the dual-inheritance model, from the perspective of the genes, is just a special version of evolutionary psychology (with its particular oddities, such as modularism, removed). However, it may sometimes be more illuminating to incorporate the evolution of cultural variants into the model of §1.4 *as another proximate mechanism*, so that the selection pressure on the genes may be described better.

ii) For the cultural variants, the picture is different. We already saw that cultural variants do not replicate. Instead, they are transmitted by a variety of error-prone mechanisms from multiple sources. We will modify the definitions of §1.4 to take this fact into account. We propose the following definitions for the principles of the evolution of cultural variants:

*Transmission of the cultural variants in the dual-inheritance model of evolution* is the temporal process in which a stable cultural variant (or several variants collectively) within the cultural repertoire of an individual are (partly) adopted, strengthened or readopted within the cultural repertoire of another individual due to *relatively unconscious or unintentional* content or context biases, in such a way i) that the cultural variants form univocal (though sometimes hard to unravel) vertical intra-generational and horizontal inter-generational *lineages* and (sometimes) *branches*,<sup>332</sup> and ii) that the cultural variant can systematically influence the fitness of the individual in whose repertoire it is present, which indirectly influences the frequency and adoptive nature of the cultural variants themselves.

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<sup>331</sup> Dual inheritance theorists usually do not define their key concepts, but base them on a loose analogy with the definitions for natural selection mixing both the natural evolution of biological mechanisms involved in cultural transmission and the cultural evolution of cultural variants: “The logic of natural selection applies to culturally transmitted variation every bit as much as it applies to genetic variation. For natural selection on culture to occur, 1) people must vary because they have acquired different beliefs or values through social learning, 2) this variation must affect people’s behaviour in ways that affect the probability that they transmit their beliefs to others, and 3) the total number of cultural variants that can exist in the population must be limited in some way. Or, in other words, cultural variants must compete. You can substitute the appropriate genetic terms in this list to recover the standard textbook accounts of how genes evolve by natural selection (Richerson and Boyd 2005: 76).”

Our own definition tries to sharpen the above definitions by using the insights from our discussion in chapter 1 and disentangling the two aspects of dual-inheritance theory: evolution of natural abilities and evolution of cultural variants. What is fortunate about these definitions is that the concept of variation is explicitly linked to variation in the probability of transmission of the cultural variant. However, too much emphasis on this element makes the dual-inheritance theory akin to memology with memes influencing their own replication/transmission chances. We have included the variation affecting the chances of transmission in the term ‘content bias’ in our definition of ‘transmission of cultural variants’.

Note the cognitive elements of Richerson and Boyds definitions: behaviour and transmission is depended upon beliefs and values. Darwinian ideas about behaviour are usually less cognitive. However, without much of a problem, the cognitive elements of the definitions can be downplayed as in our own definitions.

<sup>332</sup> Branches occur when two or more children learn the same cultural variant from one teacher at the same time, but each adopts it differently.

*Variation in the dual-inheritance model of evolution* consists in continuous or discrete yet small differences of a certain ‘cultural variant type,’ caused by changes to it during the lifetime of the individual of whose repertoire it is a part, and ultimately by the transmission process in which it is due to accidental ‘mechanical’ errors introduced by the transmission mechanism (excluding intentional transformations). The type of a ‘cultural variant token’ is to be determined by a pragmatic combination of cladism and typological parameters.

*Selection in cultural evolution* consists of repeated cycles of transmission, variation and environmental interaction (and ultimately biological replication of the individual), so structured that environmental interaction of the cultural variant causes i) the fitness of the individual (or the group of whose cultural repertoire it is a part) to be increased or ii) acquired fitness to be maintained; in this process the cultural variant itself becomes indirectly more frequent (cultural fitness<sub>i</sub>) and suited for transmission (cultural fitness<sub>ii</sub>).

*Fitness of cultural variants:* a cultural variant is fitter than its competitors if the variant confers upon its bearer at level X (the individual or group) a net heritable advantage expressible in trait-dependent currencies usually given by the proximate mechanisms involved, an advantage that is ultimately related to the expected number of offspring or a representable measure thereof, over a number of generations or within a fixed amount of time and in a ‘fixed’ abiotic and biotic environment *for the bearer*. The representable measure of offspring, the number of generations or the time period and the environment are chosen in a way relevant to the cultural variant under discussion. When the cultural variant increases the fitness of its bearer, then the cultural variant itself also becomes more frequent within the population in the long (cultural fitness<sub>i</sub>); if it becomes more frequent in this fashion, the cultural variant is *so shaped* or *so adapted* that it gives a higher fitness to the bearer at level X (cultural fitness<sub>ii</sub>).

Most elements of these definitions are self-explanatory given our extensive explanations of the elements of our definitions in §1.4.

It is important to stress the *relative unconscious or unintentional* content of context biases. These biases need to be unconscious and almost mechanical; otherwise the dynamics of cultural evolution cannot be modelled, as the transmission of a cultural variant to each individual could be highly different. It is an empirical question how much of the adoption of cultural variants proceeds semi-automatically while one grows up, and how much intentionally, especially in earlier ages with less formal education.

It is also important to see that the fitness of cultural variants is explained by the fitness they confer upon their *bearer* (who does not need to be conscious of the advantages or fitness of the cultural variant). On the basis of the fitness differences they give to their bearers (at the individual or group level), can these cultural variants themselves be called fitter? Evolutionary psychologists or memologists may disagree and stress the maladaptive side of many cultural variants that nonetheless spread quickly through the population. However, *ultimately* negative effects of cultural variants on their bearers will result in fewer bearers, and thus in a decrease in the frequency of the cultural variants in a population. Moreover, the bearer in the definition can be an individual or a whole group. Ascetic priesthood may be bad at the level of an individual, but beneficial at the level of the group (although in

practice this will be hard to measure). Of course, some cultural variants are clearly maladaptive yet rise in frequency in a population, but so do some biological variants that replicate faster, but find themselves outcompeted in the long run (e.g., think of r and K-selection as discussed in §1.3).

**Conclusion of all six phylogenetic models of the evolution of culture**

Culturalism, cultural ecology and memology are not serious candidates for an evolutionary model of culture. Sociobiology has been surpassed by evolutionary psychology, and evolutionary psychology itself needs to be supplemented by elements of dual-inheritance theory for a complete theory, whereas dual inheritance naturally includes the elements of evolutionary psychology. Therefore we choose dual inheritance as the best model for describing the evolution of culture by Darwinian means. In table 2.4 below, the results of this paragraph are summarized.

<b>Theory</b>	<b>Human mind / ability to learn</b>	<b>Origin cultural ‘content’</b>	<b>Function culture</b>	<b>Examples of domain of application</b>
<b>III. Culturalism</b>	No human nature or blank slate; mind ready to receive any program	Unclear; culture programs the mind, but what determines culture is unclear	Providing ‘meaning’ (Geertz 1973)	<i>Description</i> of all sorts of cultural elements in their relation to other cultural elements
<b>IV. Cultural ecology</b>	Phenotypic gambit/some predispositions & ability to acquire elaborate socially learned responses	Environment in relation to a small number of basic human needs (eating, convenience, sex, love etc.)	Balance between production (survival) and reproduction, adaptation to local environment	Adaptation to current environment regarding subsistence, resource exchange, parental investment, and marriage systems
<b>V. Memology</b>	Unclear: blank slate or evolved abilities	Differential reproductive success of memes	Unclear, parasitic	All (discrete) cultural elements
<b>VI. Sociobiology</b>	Unclear, trait specific	Genes programming human nature	Inclusive fitness maximization	Altruism, kin selection etc., see <i>sociobiology</i> above
<b>VII. Evol. psychology</b>	Multiple learning modules	Adaptation of cultural elements that fit biological predispositions	Inclusive fitness maximization	Mate preference, cheater detection, language use, mismatch maladaptations
<b>VIII. Dual Inheritance</b>	Simple learning biases: imitation, conformity	Learning biases and transmission processes	Increasing fitness of individuals, groups	Cultural diffusion of innovations

*Table 2.4: Six phylogenetic models of cultural evolution, after Slurink 2002 and Flinn 1997*

## §2.4 The ideal of §1.4 reconsidered for cultural evolution

*We will here restate the ideal of §1.4, as modified for cultural evolution. This ideal relies heavily on our discussion of the dual-inheritance model. We believe that we need a separate ideal for the evolution of a) the (genetically coded) ability behind the production of cultural variants and b) the (non-genetically coded) cultural variants themselves. Our definitions are based on the ones we gave as we discussed dual-inheritance theory.*

a) For the genetically coded abilities, we can simply restate the ideal of §1.4. The cultural variants are part of the environment that causes selection pressure on the genes. Thus the dual-inheritance model, from the perspective of the genes, is just a special version of evolutionary psychology (removed from its particular oddities, such as modularism etc.) However, it may sometimes be more illuminating to incorporate the evolution of cultural variants into the model of §1.4 *as another proximate mechanism*, so that the selection pressure on the genes may be described better. The final line of the ideal as stated in §1.4: *whereby its success and acceptance is further dependent on sociological factors and discoveries and developments in other branches of science, especially regarding consilience*, may also be more important than in the cases of the evolution of other biological traits. This is because the successful application of Darwin to our ability to do (for instance) math, may be obstructed by metaphysical or ontological barriers that prevent them from being easily incorporated into the naturalistic perspective of Darwinism (i.e. mathematical proof, intuition etc.)

b) For the evolution of the cultural variants we need more serious modifications. On the basis of §2.23 we can say:

A specific Darwinian explanation in cultural evolution is ideally:

- a non-tautologous (and the more informative, the more proximate mechanisms it describes);
- historical narrative (that can sometimes be extended to the future);
- that uses the four key concepts – transmission, variation, selection and fitness – in the following way, as defined in the section on dual-inheritance theory just above
  - to explain (the origins of) cultural design *and* its less than optimal maladaptive sides (which includes a cost-benefit analysis of the cultural variant);
  - in a way that is on the whole better than its competitors (i.e. other specific cultural evolutionary explanations) according to the following factors: accuracy, consistency, scope, simplicity and fruitfulness;
  - whereby its success and acceptance is further dependent on sociological factors and discoveries and developments in other branches of science, especially regarding consilience.





## **Part II: Language and its origin**



## Chapter 3: Language in the context of evolution

*The second part of this study is devoted to the Darwinian explanation of the origin of language (use). The simple phrase 'origin of language (use)' has multiple meanings: the origin of speech organs, the etymological origin of names, the origin of meaningful speech as such (from gestures and or cries to meaningful utterances), the origin of logic or reasoning, and the origin of the plurality of languages from one proto-language, to name but a few. In this chapter we will define what we consider language to be and what proximate mechanisms have been discovered by science that could buttress an ultimate Darwinian explanation of (the origin of) language (use) (chapter 3). The relative novelty of our approach is that we include both research on the origin of linguistic abilities and on the origin and evolution of linguistic forms, which we see as the two dimensions of the problem of the origin of language (use). We believe, on the basis of the dual inheritance theory, that results in either area constrain theorizing in the other area. Such mutual constraints are what we need to put limits to the many speculations about language origins that currently exist. We will also ask which aspects of the question of the origin of language can be illuminated by Darwinian explanations (chapter 4).*

*In this chapter (chapter 3), we will first give a pragmatic definition of human language using the work of Hockett (§3.11). After that, we briefly discuss the philosophical problems of meaning opting for pragmatism (§3.12). Then we critically discuss the paradigm of generative grammar that Chomsky inaugurated, as it is one of the main reasons that discussions about the origin of language have generally been ignored in the second half of the 20<sup>th</sup> century (§3.13). When Chomsky's paradigm is no longer obstructing the discussion, research on the origin of language will become much more fruitful. Finally, we discuss six branches of science and what they can teach us about language and language origins (§3.2). Our discussion of the six research fields will be preceded by a general discussion of the advantages and disadvantages of the individual branches for the study of language origins. The current research will show that the obstacles Chomsky's theory posed are almost entirely overcome.*

### §3.1 What is language?

In this chapter we will give a definition of human language, so that we know exactly what is being explained by our Darwinian model.

It is hard to give a definition of language that is not heavily theory-loaden, and we risk defining language so that it fits well into an evolutionary account. For instance: language is a slowly evolved system that confers the benefit of rapid communication to its bearers. We need a more neutral definition if there is anything substantial to be explained by a Darwinian account of language. To find our neutral definition, we use the highly descriptive work of C.F. Hockett on the design features of language. We distinguish between a) the mode of production, b) the nature of the linguistic forms and c) the uses of language (§3.11). After that, we briefly turn to the philosophical problem of meaning and some traditional prejudices, opting for a Wittgensteinian pluralism of language uses (§3.12). Finally, we discuss the general views of linguistics on language, concentrating in particular on the dominant paradigm in 20<sup>th</sup> century linguistics (or at least the dominant paradigm for people

working on syntax) inaugurated by Chomsky and others, and why this paradigm has obstructed discussions on the origin of language (§3.13).

### §3.11 Language, a definition

We speak of sign language, oral language, written language, animal language, computer language and artificial language, a living language and a dead language, of the inner language of thought and the outer (public) language, the language of orders (the language of the army) or the language of requests. We also speak of the ‘universal language of emotions’, the ‘language of dreams’, ‘the language of the stars’, ‘the language of art’, ‘the languages of symptoms’ or ‘the language of atoms’. Further, we speak of the language or symbolic structure of artifacts, social, political or religious systems (take any postmodern, semiotic or structuralist account of these domains to find such phrases – or think of Viktor von Klemperer’s *Language of the Third Reich*, which was not *just* about words used in Nazi Germany), or we speak even more broadly: the language of things etc.<sup>333</sup> Of course, not all these uses defy scientific scrutiny, but they give a good impression of the many uses of the word ‘language’.

Given these multiple uses, the question “what *is* language” becomes hard to answer when this question is understood as the traditional question: what is the *essence* of language? Or: what is the *fundamental* or *unique* feature of language common to everything we call language and without which something would not be language? Such an essence of (all uses of the word) ‘language’ is hard to find. The limited list of examples of the use of the word ‘language’ above already suffices to show that, since what is common to all of them? Is it being produced by humans? No, because we speak also of animal language. Is it being produced by an organism? No, because inner language (for people who believe in it, be it the Fodorian ‘mentalese’ or the inner voice of consciousness) is usually not seen as the product of an (individual) organism, but either as an innate capacity which has arisen through natural evolution, education (the internalized voice of the father) or has been implanted by God. Is it being composed of *artificial* signs? No, since for the language of the stars, dreams (again for people who believe in it), or the ‘universal’ language of emotions, this is certainly not true in any usual sense of the word ‘arbitrary sign’. Is it being communicative? No, because many uses of the word ‘language’ do not fit the usual sender-receiver communication scheme. The language of mathematics (though used to communicate results to other mathematicians) is not really a form of communication, but a logical structure that should guarantee that the proven results are truly proven. Also the language of the stars or the language of symptoms is not thought of as a form of communication, but as a (secret) code that can be cracked.

One can make other proposals about the essence of language, and eventually one may even find one, although it will then likely be so general that it will not apply uniquely to language but also to other phenomena. We will here assume that the heterogeneous list of examples convinces the reader that there is no simple essence of (all uses of the word) language. In that case there are three things one can do. One can simply deny that such an essence exists at all, or one can try to purify one’s definition and call uses that do not fit it false uses or analogical extensions of the

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<sup>333</sup> Google reveals thousands of hits for all these phrases; we are not speaking of outlandish uses.

word ‘language’, or one can admit that all the uses of the word language are valid, but choose a list of criteria for what counts as a language for the pragmatic purpose at hand (an approach that is compatible with the first option). The first option is the one Wittgenstein opts for.<sup>334</sup> The second is the traditional one of the metaphysicians and the third is our approach in this chapter, as we will look for a list of criteria for what counts as (human) language.

However, finding such a list of criteria is still not easy, because we use the phrase ‘human language’ to refer to many heterogeneous things: living and extinct languages, natural human languages or artificial human created languages, gesture, oral and written language, inner language of thought and the outer (public) language etc. And even if we focus only on (for instance) ‘living, natural, oral, outer (public) languages’ we still have to clarify *whose language*: that of a German or a Frenchman, that of an adult or a child, that of a foreigner or a native speaker, that of a ‘primitive’ or ‘civilized’ person, that of a ‘normal’ person or that of someone with a mental or physical speech defect, that of a drunk person or that of a sober person etc. We also have to distinguish between *what the language is used for*: ordering, begging, singing, joking, reasoning, gossiping, counting, dictating, complaining, crying, instructing, informing, telling a story, convincing someone, screaming etc., and *what part of sounds and phonetic patterns* we include in oral language: only words and intonation patterns at word and sentence levels, or also ah’s and oh’s of amazement, the interjections used to gain time while thinking, the meaningful speed-ups, nervous pauses and other prosodic elements of the speech-stream, the handmade sounds that accompany speech such as ironically clapping your hands while exclaiming ‘bravo!’ etc. The task of providing a neutral, or relatively non theory laden definition of human language seems almost impossible.

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<sup>334</sup> If one drops the notion of a traditional essence behind the use of words altogether, one still needs to explain that some uses of the same word do seem to have something in common. Wittgenstein famously explained this with his concept of a family resemblance: all members of a family have features in common with other members (and the more closely related, the more features usually in common, compare our solution to the species problem in §1.32), yet there is nevertheless no essence of this family behind all the members of it, nor any simple list of traits that all members have. Instead, we find resemblances and dissimilarities. The other metaphors Wittgenstein uses to elucidate the concept of a family resemblance are the metaphor of the thick thread and the metaphor of network or web. The strength of the thick thread is due to several thinner, intertwined threads with no single thin thread – just as the ‘strength’ of a word is due to its many dissimilar, but related shades of meaning (use), with no single shade of meaning covering all instances of the word used. In a network or web, there exists no central node, although some elements of the web are more relevant to its interior and others to its exterior, just as a word can have some core and some more peripheral uses.

Interestingly, Wittgenstein not only proposes the concept of a family resemblance, but also implicitly suggests that the concept of *language* could be a family resemblance concept. In the first paragraphs of the *Philosophical Investigations*, Wittgenstein discusses many examples of what he calls ‘language games’. Then in §65, his opponent, the metaphysician who looks for an essence of language, asks what all language games considered so far, or all language games whatsoever, have in common (§65). Wittgenstein answers this question by a detour, by illustrating the family resemblance concept on the basis of the concept of ‘game’ by asking what all uses of the word ‘game’ have in common. According to Wittgenstein, the answer is “nothing” (§67). We believe Wittgenstein suggests that all language games have nothing in common and that the concept of language is a family resemblance concept too.

A metaphysician would of course not yet be convinced and offer a definition of both ‘language’ and ‘game’ (i.e., a game is sequence of events such that a certain goal is intentionally aimed at and in attaining that goal actors accept certain handicaps); cases that do not fit this definition of game or playing: an individual child randomly running behind a ball, the sun playing on the water, would then be treated as analogical extension. However, Wittgenstein’s virtue is not that he ultimately shows the metaphysician is wrong (as he or she will not likely ever be convinced), but that one need not think as the metaphysician does.

Because of these difficulties, it can be tempting to define human language in a way that fits our evolutionary perspective, to wit: human language is that system of symbols humans use to survive, attract a mate and out-compete their competitors. However, that would be cheating, since we need a neutral definition of language to see whether we can give a Darwinian explanation of it. And if we presuppose that language has arisen by natural selection beforehand, we might lose sight of the maladaptive sides of language or the other means by which language might have originated (if any). Moreover, the proposed definition is no good either, since it is almost *non-informational*: any adoptive trait of an animal ultimately serves to increase its survival and replication chances. As far as it is *informative*, it is at most a half-truth because it claims that human language is a *system*, thus possibly overlooking the asystematic elements of human language, and because it claims that human language consists of symbols, ignoring any non-symbolic (pure iconic, indexical or other) elements. Finally, identifying a trait with that which evolution has created, without further qualification, makes it hard to identify whether we have an instance of this trait or not. For example, *when* do we have a true instance of human language? At the very moment at which humans and chimps started to depart from their common ancestor and his use of symbols? But what if the cladistic difference between chimp and man were due to reproductive isolation resulting from differences in tool use? Then human sign use may have started to diverge much later, and then again we must ask: *when* do we have a true instance of human language?

An evolutionary account of the origin of human language can thus not suffice with defining language as a ‘system of symbols humans use to survive, attract a mate and out-compete their competitors’: we need a pragmatic definition to determine whether its explanation is truly an explanation of *human language* and *not some other trait* (a precursor to language or a less complex ability).

### **A definition of human language or speech**

We will try to give such a pragmatic definition by using the work of C.F. Hockett, one of the main proponents of post-Bloomfieldian structuralism in America. Interestingly, Hockett was also one of the first vehement critics of Chomsky (Hockett 1968). We do not claim that our definition can uniquely distinguish human language from non-human language and other phenomena, or that everything we consider human language falls under this definition. However, we hope that the definition fits most uses and, more importantly, can be used within an evolutionary account of human language. Before we discuss Hockett’s work, we will, however, argue why we focus in our discussion on *oral* language use.

### **Language as oral language**

Humans can produce language by many means: by oral sounds (with different languages using different phones and phonemes<sup>335</sup>), by written symbols (using many

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<sup>335</sup> Phonemes are the meaningful sound like elements of which our dual patterned speech is composed. There are two traditions in defining what a phoneme is: the Anglo-Saxon and the Continental tradition, which like Anglo-Saxon and Continental philosophy have respectively more empirical or more idealistic tenets. The key problem in defining phonemes is that they are language-specific. Sound differences in one language may be phonemes in one language, but not in another (the difference in sounds may even be imperceptible to speakers of another language). The Anglo-Saxon tradition defines phonemes as the statistical average of a collection of sounds that people use within a community as meaningful different sounds. The Continental tradition sees phonemes as ideal entities in our mental speech intuition, that can for better or worse be instantiated by our sounds. For our purpose we need not opt for one of the

different symbolic systems), by gestures (with many gesture dialects), by pictures (think of pictograms), by Morse code or other ticking sounds, by Braille script, by singing in all its many forms (even using the ‘false’<sup>336</sup> instead of the ‘true vocal cords’ as in grunts or Tuvan throat singing), by an artificial voice box if the vocal cords are removed, by computer-run speech programs or – speaking of computers – in fact by any (digital) system that can be used to convey a message. In our definition, however, we will use the mode of production that is used most often, or at least that is still used by most humans to learn a language nowadays: *oral language (as accompanied by some face, hand and body gestures)*. Of course, for many people gesture language is their first natural language (think of the famous community of deaf children in Nicaragua who developed their own complex sign language within a generation<sup>337</sup>). Some argue that in evolution sign language preceded oral language, although we will argue that this is not likely to be the case (see §4.3α c). For now, however, we forego a discussion about the chronological primary mode of production and simply assume an ‘end-product’. We assume that the current dominant or primary mode of language use, or at least of language learning, is *oral language (as accompanied by some face, hand and body gestures)*<sup>338</sup>, and that this should be the explanatory object of evolutionary explanations of (the origin of) human language production. In §3.21 we discuss the physiological, neurological and genetic presuppositions of this particular mode of production in more detail.

### Hockett’s approach to language

C.F. Hockett developed what he called the design-feature approach as an attempt to distinguish the similarities and differences among animal communication systems and human language. Although Hockett’s views of the abilities of animals are outdated, his approach has stood the test of time<sup>339</sup> and is still used extensively today in the

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traditions, but the first, more naturalistic definition seems to fit better with a Darwinian approach to language.

<sup>336</sup> The false vocal cords are a pair of thick folds of mucous membrane that sit just above, and protect, the more delicate true folds. They have a minimal role in normal phonation.

<sup>337</sup> For instance in the popular depiction of Oliver Sacks in *Seeing voices* (1989) or the more scientific Polich 2005 and Senghas et al. 2004. The latter stress the similarities between verbal and gesture language and use the case of the Nicaraguan children to demonstrate that humans have an innate universal grammar a la Chomsky. However, there are many other ways to account for similarities between the structure of verbal and gesture language (as big or as small as they really are). As we are highly critical of the Chomskian project in the remainder of this chapter, the alternatives are for us a priori more plausible.

<sup>338</sup> One of the traditional distinctions in linguistics is the Saussurian (1916) division between *parole* (speech) and *langue* (language as a system). This distinction more or less returns in the Chomskian distinction between performance (actual speech)/E-language and the competence/I-language. This distinction itself partly (but only partly) overlaps with the ancient distinction between language as a means for external communication (Jackendoff 2002) or the manipulation of behavior (Catania 2001) on the one hand, and for inner conceptual representation (Newmeyer 2003, Bickerton 1995) or mental cognition in general (Györi 2001) on the other hand. By choosing to restrict ourselves to verbal speech, our definition of language – intended to incorporate a neutral stance towards scientific research – seems to focus on one of the two sides of the distinction. However, that is not the case. It may well be that to explain verbal language one needs to invoke an innate language/grammar, or cognitive processes that are structured like a language; and on the other hand, even researchers who primarily focus on the second side of the distinction will agree that language is (also) verbal speech, so that our definition of language as speech is still neutral towards different research agendas. Nonetheless, we will show that research focusing on the second side of the distinction is often problematic: see §3.12 on the problem with an inner language of thought used for cognition, and §3.13 on the problem with the Chomskian distinction.

<sup>339</sup> A possible reason for this is that Hockett is one of the last proponents of structuralism in linguistics whose research focused on defining the elements of language; Hockett could draw upon this 50-year old tradition of formal language classification that soon afterwards went more or less into demise because of the



study of animal language (for instance Drobovolsky 1997). Hockett initially developed seven features, which were published in his 1959 paper *Animal Languages and Human Language*. However, after some revisions, he expanded the list to 13 design-features (Hockett 1960), and in a subsequent article (Hockett and Altmann, 1968) he added three more features, for a total of 16. According to Hockett, some of the design features of human language could be found in animal language, especially in primate language, but not all. For instance, the first nine features were characteristics of the communication of primates, while the last seven features are what distinguish human language from all other animal language.<sup>340</sup> We will use the same criteria, but have a different assessment than Hockett on which features distinguish human and animal language.

The sixteen design features Hockett distinguishes are:<sup>341</sup>

**1) vocal auditory channels:** spoken language, the language we take as the primary object of explanation, is produced in the vocal tract and transmitted/heard as sound (whereas sign language is for instance produced with the hands and transmitted by light). Hockett viewed this as an advantage for humans because it allowed for the ability to participate in other activities while simultaneously communicating through spoken language.

**2) broadcast transmission and directional reception:** the sound of language is audible in all directions, but listeners will interpret it as coming from one specific direction.

**3) rapid fading:** the sound made by speech diminishes quickly after being released (quickly compared to low-frequency whale sounds that are produced for minutes or quickly compared to the monotonous croaking of frogs); this allows for rapid communication of long, complex messages.

**4) interchangeability:** the listener has the ability to *receive* and to *send* messages, just as the speaker has.

**5) total feedback:** individuals are able to hear the message they have sent (feedback from sending to hearing). The criterion of *total* feedback is probably too good to be true, as we of course never hear *all* aspects of the things we say; however, feedback is present in (for instance) learning to pronounce a new word by saying it multiple times (i.e. speech monitoring of sound *forms*). For Hockett, this design feature also meant that people generally *memorize* the *content* of what they have said. According to him, the design-feature incorporates the idea that humans have insight into their actions.

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rise of generative grammar and more specialist linguistic fields, so that his work was not much improved upon. Hockett's critique on Chomsky, which put him out of the mainstream linguistics of his own time, also makes him stand out today as rather modern.

<sup>340</sup> The advantage of Hockett's list is that it can readily be used in *evolutionary accounts* as it points out what the distinctions are between human language and the language of other animals, and thus emphasizes what elements had to evolve to speak of human language. It is fortunate that he calls them *design* features (where design means having a certain functional constitution for a certain type of language use) because evolutionary accounts also explain design, although perhaps the downside is that all aspects (if they are designed) should confer a functional advantage – and that need not be the case or can be hard to determine for all features. Another downside of Hockett's approach is that by defining human language on the basis of *distinctions*, one might lose sight of the common core of languages and the function of language use in general (if there is one). Hockett wrote this before the extensive studies on chimpanzee and bonobo sign use. For an updated view on animal language in light of Hockett's criteria, see §3.23.

<sup>341</sup> The explanation of the features is ours, unless otherwise stated.

**6) semanticty:** speech sounds can be linked<sup>342</sup> to *specific* meanings. The sounds are not matched to one function only, as is the case in birdsong that is only used to attract a mate; the details of such a song only serve to demonstrate the ‘ingenuity’ or ‘fitness’ of the bird. Note that specificity is a gradual concept: every sign can be called specific insofar as it has a specific function, but only in comparison with other, more specific signs (where all small elements of an utterance have their own meaning) can a sign be called non-specific. Hockett notes that gibbons also show semanticity in their signals; however, their calls are far more vague than human language according to him.

**7) arbitrariness:** there is no *direct* connection between the signal and its meaning; this criterion is used to distinguish natural signs (such as becoming red due to inflammation), from non-natural or arbitrary signs such as human words or the iconograms chimps learn to manipulate. Many aspects of human sign use have been called arbitrary<sup>343</sup>; here the arbitrariness is used to characterize the relation between

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<sup>342</sup> The nature of the ‘linking’ (‘the problem of meaning in philosophy of language’) is discussed in §3.13.

<sup>343</sup> Many elements of human sign use have in the past been supposed to be non-arbitrary (with different meanings of ‘non-arbitrary’). Examples are a) gestures, b) cries, c) sound patterns, d) onomatopoeia and e) Anlaut formation, f) the first words uttered by children.

a) Gestures: it is often thought that gestures are less arbitrary than verbal words, although linguists have often ridiculed this popular misconception, indicating that gestures are just as arbitrary and learned as verbal sounds: for instance, in Bulgaria people nod their head when they say ‘no’ and move their head from left to right when they mean ‘yes’, which is completely contrary to the customs in other countries (McWhorter 2001). However, the opinion of the linguists needs correction in that some gestures are less arbitrary than verbal utterances. When people don’t speak each other’s language, they often try to communicate with hands and feet and insofar these *signs* are at the same time *real actions*, they can be much easier to interpret, as the action of chewing is much easier to interpret than the arbitrary verbal utterance ‘food!’, or ‘Essen!’ etc. Moreover, face gestures expressing emotion are partly genetically encoded and universal throughout all cultures, and are thus easy to interpret. These facial gestures are arbitrary from the perspective of other species (a cat does not understand pointing and will look at the finger instead of the thing pointed at), but are not arbitrary from the perspective of humans, for whom genetically encoded facial gestures are natural to interpret. The other examples concern verbal utterances.

b) Cries: cries such as cries for pain seem universal, natural and uttered involuntarily (you cannot not scream when having pain); however, linguists deny that human language is of course not (solely) composed of cries and that pain behavior, including the accompanying verbal utterances, is just as much learned as other verbal utterances (compare English: Ouch! and Damn! with French: Aie! and Mon Dieu!).

c) Sound patterns: phonology teaches that the phonetic patterns of words follow certain patterns. These patterns help to decipher the speech stream: think of the NeReiS-rule of Greek which tells us that every word in ancient Greek ends with a vowel or with one of the consonants ‘v’, ‘q’ or ‘ç’. It is sometimes argued that the simplest combination of sounds in words are consonant/vowel combinations (CVCVCV or VCVCVC V etc.), a pattern we find in many languages. Japanese words almost exclusively follow this pattern, English words less so. Since a vowel is characterized by an open configuration of the vocal tract, whereas the consonants are characterized by a constriction or closure at one or more points along the vocal tract, the CVCVCV pattern corresponds to a close-open-close-open-close-open pattern, a pattern of maximum *distinctionality* that seems to fit our physiological speech system *well*. However, many languages (such as some language in the Caucasus) seem to use almost no vowels *without any problem*; thus one cannot easily identify CVCV-structure as a universal *design*-feature of human language. Moreover, the phonological rules constrain the form of the word but weakly, leaving a lot of space for arbitrariness.

d) Onomatopoeia: onomatopoeic words imitate the sound of the thing they are describing. Some examples are bam, bang, clang, plop etc., or animal sounds such as ‘oink’. Sometimes the onomatopoeia also forms the name of the thing or process that makes this sound, such as the birds cuckoo, chiffchaff and the peewit, the name ‘vroom’ for an engine, or ‘ping pong’ as the name of a game. People even speak of non-auditory onomatopoeia (almost a contradiction in terms): words that are imitative but not of sounds. A few such words have gained parlance recently, including ‘bling bling’, the ‘sound’ of light reflecting off diamonds. However, onomatopoeia have an arbitrary side too. A Dutch dog for instance says ‘waf’, a Dutch donkey ‘i-ah, i-ah’ and a Dutch pig ‘knor’, whereas an English dog, donkey and pig say respectively ‘bow-wow’, ‘hee-haw’ and ‘oink’.

the form of a signal and its meaning. There is always some relation between the form of a signal and its meaning via its use: the warning cries of some birds are low in frequency so that they are hard to locate, the words humans use most are the shortest and actual used words cannot become indefinitely long. When the relation between form and meaning is nonetheless called arbitrary, we mean that there is no direct causal link between the form of a signal and its meaning.

**8) discreteness:** the units of language can in principle be separated and identified individually, be they phonemes, words, fixed formulas, syntactic patterns etc. Different units (alone or in combination of other units kept constant) usually have very different meanings. In contrast, the waggle dance of honeybees is continuous; small differences in the dance do not cause the bees to interpret the dance radically differently.

**9) specialization:** speech is produced for ‘communication’<sup>344</sup>, not chiefly for some other function, such as echolocation. In §3.21 we discuss the physiological details of the speech organs as designed for this primary function.

e) ‘Anlauts’ are the first sounds of words in (Indogermanic) languages. It is sometimes said that some of these *Anlauts* can tell something about the meaning of a word – not just with wh\* sounds which are typical of the syntactic category of interrogatives (where, who, when, what, whose etc.), but also with sounds such as kn\* or sp\* that are said to be associated with a certain semantic meaning. Kn\* sounds are above average in words that have something to do with bowing, breaking and the sound of it (knack, knapsack (‘eating bag’), ‘knead’, ‘knee’, ‘kneel’, ‘knell’, ‘knight’(‘he who bows?’), ‘knick-knack’, ‘knock-out’ etc.); Sp\* sounds in words that have something to do with a forced outward motion (‘space [v.]’ (i.e. ‘trip’), ‘spade’, ‘spank’, ‘spark’, ‘sparkle’, ‘spout’, ‘spat’, ‘spew’, ‘spastic’, ‘spawn’, ‘spay’, ‘speak’, ‘spear’, ‘speed’, ‘spell’, ‘spend’, ‘sperm’, ‘spike’, ‘spill’, ‘spire’, ‘spirit’, ‘spit’, ‘splash’, ‘splat’, ‘splay’, ‘spleen’, ‘splendid’, ‘splint’, ‘split’ etc.). This is sometimes explained by observing that when you make a kn\*-sound you put your back teeth upon each other, in the same way as when you chew on your food; when you make a pure sp\*-sound, you almost spit, you make a forced outward motion yourself. This may be the part of the reason for the rare experience that upon hearing a new word for the first time one sometimes immediately thinks one understands what is meant, as in ‘splash’ (although this understanding can be due to contextual information, similar word patterns, unconscious previous encounters etc.). However, there are also many words that have nothing to do with bowing/ breaking or a forced outward motion and yet start with kn\* or sp\*: , or there are words that do have something to do with bowing etc. but don’t start with kn\* or sp\*: ‘breaking’, ‘bending’/ ‘whistle’, ‘attack’ etc. Hence there is no unique correspondence. Furthermore, the descriptions of the meanings of the kn\* and sp\* sounds are so general, that a creative person can always find a reason to argue why the word is a case of the meaning associated with its *Anlaut*-sound. Finally, the link between the kn\* and sp\* sounds and their meaning seems to be rather speculative and weak from two sides: why is not the ‘be(h)’-sound the sound in words that have something to do with a forced outward motion? Or why are sp\* sounds not thought to deal with wetness as this fits most of the examples too?

f) The first words of children: the word for mother in many, even completely unrelated languages seem similar to mama (in Asian languages for instance ‘amma’ or ‘oma’ or ‘ammi’ (compare also ‘mummy’) as Roman Jakobson was the first to note. This universal ‘m’ is sometimes explained by pointing toward the sucking movement of the lips of babies; another explanation is that the ‘m’ consonant and the open ‘a’ vowels (or completely closed ‘i’ vowel) are relatively easy to make (compared to for instance a ‘l’-sound or a mid vowel as the ‘schwa’). The most important word(s) for a child need to be one(s) that can be produced easily. The form of these words is non-arbitrary in as far as they depend on the natural evolution of the human speech system. However, the more language children learn, the more arbitrary the combination of sounds of new words they learn, and the more refined the earlier production of sounds; finally, the vocative word for mother does not contain the ‘m’ and ‘a’ sound in all languages.

In sum: some elements of human verbal utterances are non-arbitrary in the sense of having a possible function (shortness of words, phonological rules etc.), but are for the most part indeed arbitrary in the sense that you cannot infer the meaning of an *isolated* word from its form. (Of course when one knows the meaning of one or a few words, one can often infer the meaning of *other* words from contextual information, compositionality of words, fixed word patterns, assumptions of non-massive homonymy, synonymy etc.)

<sup>344</sup> We use the word ‘communication’ in its most general sense that comprises also the function of language we distinguished above such as begging, gossiping, complaining etc.

**10) displacement:** the ability to talk about or refer to things that are not *physically* (directly) present here and now or things in the realm of abstract things.

**11) productivity:** the ability to create new messages by combining already-existing signs (not necessarily words, but also fixed formulas in new sentences etc.) to say things that have never been spoken or heard. In contrast, Hockett notes, apes such as gibbons have a closed communication system because all their vocal sounds are part of a finite repertoire of familiar calls. The feature is also sometimes known as compositionality.

**12) traditional transmission:** the learning of language occurs in social groups and *due to this*, traditions (or dialects variants) of sign use arise; this means that at least the details (if not many other elements) of language are not innate, as are the finite repertoires of signs of many animals.

**13) duality of patterning:**<sup>345</sup> meaningful signs (words) are made of – and distinguished from one another by – meaningless parts (sounds, letters).

The three criteria Hockett added later:<sup>346</sup>

**14) prevarication:** the ability to make false statements (to lie and cheat).

**15) reflexiveness:** language can be used to refer to (i.e., describe) itself, which does not imply a theory of language or linguistics. In everyday speech reflexiveness is common as in 'John said that he would vote for me, which I am happy about'.

**16) learnability:** speakers of one language can learn to speak another (thus second-language acquisition is seen as an inherent and not an extraneous feature of human language).

On the basis of these criteria we define language (forms) as follows: *a primarily orally produced and constrained collection of arbitrary, dual-patterned, discrete linguistic forms spoken in dialect communities, whose meanings are determined by all the many communicative purposes that speakers use the forms for.* In practice, we will refer back to the list of features instead of the definition. The object of giving evolutionary accounts of the origin of language is now also clear: how have humans evolved the ability to produce these language forms, and how have these forms themselves evolved from a simple beginning into their current richness?

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<sup>345</sup> This characteristic is independent of the characteristic of arbitrariness as the latter only means that in principle other signs could be used for the same meaning (by, for instance, a different speech community), whereas the duality of patterning characterizes the inner structure of the arbitrary sign.

<sup>346</sup> We find these later added features less compelling, especially the first. Why not add joking, gossiping, storytelling etc. as typical human usages of language? Of course, within ethology lying is especially important (especially given the current emphasis in ethology on research about cooperation, altruism and selfishness), but from a linguistic point of view this criterion might seem less important. Maybe one can replace it with voluntary control, though, as that is what Hockett probably meant: lying presupposes voluntary control.

### §3.12 Theories in linguistics and the Chomskian paradigm

*In this section we give a very brief description of why theories of the origin of language went out of fashion in the 20<sup>th</sup> century, due to the nature of the fundamental paradigms. We will see that Chomsky's theory, more than any other, has blocked evolutionary accounts of the origin of language. We critically assess the elements of his theory to pave the way for evolutionary accounts.*

Reflections on language are at least as old as classical antiquity, where we can find proto-grammatical categories. The study of language with modern scientific tools (or *linguistics*), however, arose during the Enlightenment. Today, linguistics can be divided into a large number of sub-fields such as phonetics, phonology, morphology, syntax, lexicology, semantics, pragmatics, and biolinguistics (comparing human and animal language), clinical linguistics, first and second language acquisition, psycho- or cognitive linguistics, sociolinguistics, linguistic anthropology, neurolinguistics, computational linguistics, descriptive linguistics, historical linguistics, stylistics, and corpus linguistics amongst others. Looking back at the history of linguistics, we can say that at different periods, different subfields have dominated the research agenda (especially as some of the fields were not yet formed).

During the heyday of philology in the 19<sup>th</sup> century, as scientists started to decipher unknown ancient languages, to read and translate texts from other non-European cultures (India and China, e.g.) and to critically assess and edit texts, comparative and historical linguistics were the central fields of linguistics. One of the most dominant schools was that of the German Neogrammarians, who discovered famous sound laws such as Grimm's law and Verner's law (see §3.25 on diachronic linguistics). The Neogrammarians, as good empiricists, held that the true object of linguistic investigation is what is directly observable in a language: the speech of an individual (or what we nowadays call the idiolect). As the sound level is the most observable aspect of language, they further regarded the sound level as the most important level of description.

At the end of the 19<sup>th</sup> and especially at the beginning of the 20<sup>th</sup> century with the work of De Saussure (1916), and as foreshadowed by the work of Hermann Paul (1886), focus on the idiolect and phonology was replaced by a focus on language as a system<sup>347</sup> made up of different layers, of which syntax and the formal dimensions of phonology were often seen as the most important ones. This meant that the study of language was de-contextualised and de-individualised in favour of the study of the *structural* properties of language (which seem hard to study if you have to take all sorts of individual and sociolinguistic variation into account). The research focus on structural properties of language (hence the name structuralism) meant that historical and diachronic linguistics, so central to the Neogrammarians, left the main stage. When structuralists spoke of a system, they almost invariantly meant a synchronic (atemporal or frozen in time) system, since diachronic elements would only destroy their crystal-clear structures.

Linguistics owes much to the ground-breaking work of the structuralists, particularly the division of work in theoretical linguistics with its division in phonetics, phonology, morphology etc. Structuralism, with its emphasis on

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<sup>347</sup> Or on *langue* (language as a system) instead of *parole* (individual speech) as De Saussure would put it.

synchronic structures and its formal, essentialist approach towards language, made discussions of the origin of language unpopular.

Just before the Second World War, two other paradigms became prominent in linguistics: linguistic relativism and behaviorism. Linguistic relativism is important because it entangles the evolution of linguistic forms with that of all other cultural elements. If linguistic relativism were true, it would seriously complicate an evolutionary account of the origins of language (although we will see that language and other cultural elements evolve rather independently in §3.25). Behaviorism is interesting because its founder Skinner believed that his approach was Darwinian in nature (see §2.3 on Darwinian operant conditioning). If so, his work would be the first Darwinian theory of the origin of language (use) since Darwin's own account in his *Descent of Man* (1871) and *The expression of emotions in man and animal* (1872).

### **Linguistic relativism**

*Linguistic relativism* is the idea that differences in cognition or worldview are *caused* by differences in languages of which its speakers are thought to be unaware. It originates in the work of Herder and Humboldt and becomes prominent, through Boas, in the work of Edward Sapir and especially that of his student Benjamin Whorf: hence the alternative name of *Sapir-Whorf hypothesis* for linguistic relativism. A discussion of linguistic relativism will be of great use for our discussion of the (limited) influence of the evolution of language on the evolution of other cultural elements and vice versa (see also §3.25 on diachronic linguistics).

The Sapir-Whorf hypothesis can easily be misunderstood, as it leaves “language” undefined. It is not sociolects (languages as spoken by social groups such as laymen and experts, men and women, children and grown-ups etc.) that the supporters of relativistic linguistics consider – probably because they would think that the differences in sociolects are only marginal differences (which need not be true) and the result of choice. They also do not hold that there exist mere *correlations* between cultural and linguistic variations: take the system of kin, which is clearly related to culture and is reflected in kin terminology. Differences in cultural kin systems will thus correlate with differences in kin terminology, but this is not what supporters of linguistic relativism mean. Their thesis holds that cultural differences are *caused* by linguistic changes, and this need not be the case with the kinship systems.

What linguistic relativists usually think is that differences in fundamental *grammatical* categories and structures determine our worldview, our most fundamental cognitive and social categories. In one of the most famous studies of linguistic relativism, Whorf compares the sentence structure of Hopi (as spoken by Hopi Indians) with English. In this study, Whorf points to the very different grammatical relations to express an action as ‘cleaning a gun’ in Hopi compared to English. In English, this action describes a subject who pulls the gun cleaner through the gun. In Indian languages, it describes the spatial process in which a gun cleaner is entered into the tube of the gun.<sup>348</sup> For Whorf this implies that Indians and Europeans have a very different experience of the ‘same’ action, in this case a different experience of the spatial, temporal and causal character of the action, and he concludes that Hopi do not have concepts of ‘time’, ‘space’, and ‘cause’ similar to ours. Thus, linguistic relativism does not address superficial cultural differences, but rather deep cultural preconceptions about reality, some of which hardly seem culturally determined (such as the cognition of space and time, part of which is often considered to be innate).

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<sup>348</sup> See the third essay in his collected works (Whorf 1956).

However, one of the central objections to linguistic relativism is how we can *test empirically*, without philosophical preconceptions, that differences in languages cause differences in worldviews. Experiences are hard to test. We can observe that Indians and Europeans handle guns similarly, so where is the difference between worldviews? Further, we can only test the relation between linguistic variations and cultural variations if the cultural variations can be identified independently from verbal means (such as questioning or observation of verbal behavior). Of the few tests that try to distinguish linguistic variations and cultural variations, that of Brown and Lenneberg (1954) on the relation between color vocabulary and color perception (which is measurable without verbal means) has become a landmark study. Brown and Lenneberg showed test subjects a monochrome piece of paper and, after removing it, asked the participants to pick out the color they had just seen from a chart with different colors. Brown and Lenneberg noted a clear correlation between the culture of the test subject and his or her ability to identify colors, correlations that coincided with the available lexical color terms in the languages of these cultures. They concluded that the semantic *structure* of the color terminology influenced visual perception: whether or not people can indicate a certain color in a language with exactly one word *determines* whether or not people can easily identify that color.

However, this is not the only possible interpretation of the (undisputed) correlations Brown and Lenneberg found. One could also interpret these correlations as due to a more fundamental factor, such as a shared environment in which people from the same culture live, which could influence both color vocabulary and the ability to perceive and recognize colors. This hypothesis is strengthened by the results of a famous study by Berlin and Kay (1969), who demonstrated that languages *universally* (and thus not cultural dependently) choose from a set of only 11 basic color words, although humans can physiologically distinguish about two million colors. Berlin and Kay further found that these 11 basic color words were ordered in groups:

black/white < red/yellow/green/blue < brown < purple/pink/orange/grey.

with the following two rules: 1) if a language has words for one of the colors in a group, it also has words for the other colors in that group, 2) if a language has words for colors in a certain group, it also has words for colors in the groups to the left in the list.<sup>349</sup> Later research vindicated much of the research by Berlin and Key; their 11 basic colors are now called ‘focal colors,’ and it is suggested that there are pure physiological reasons why they are so important: children in a preverbal state can already distinguish them in recognition tasks (see papers in Hardin and Maffi 1997).

Although some researchers try to update the work of Brown and Lenneberg to prove that linguistic relativism is true,<sup>350</sup> it is now generally rejected because the previous tests have turned out to be negative and because extensive research by comparative linguists has shown that linguistic variations and cultural variations are almost independent (see §3.25). Further, although a materialistic version of linguistic relativism is imaginable (the use of words influencing the physical or neurological

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<sup>349</sup> There is a language in which “black” and “white” are the only color words: the Indonesian language Dani.

<sup>350</sup> See for instance Arthur Lucy 1996, 1992 and John Gumperz and Stephen Levinson 1996. Those still supporting updated versions of *modest* linguistic relativism are defended by Pütz and Verspoor 2000 and Bowerman and Levinson 2001.

mechanisms of color perception), linguistic relativism originates in more idealistic views of culture. See for instance Humboldt, who was strongly influenced by German Idealism, and Franz Boas with his background in Neokantianism, both of whom take mentalistic intuitions seriously in the explaining (*Verstehen*) of cultures. In its idealistic tenets, linguistic relativism is just as incompatible with Darwinian accounts of linguistics as (for instance) Hegel's idealistic philosophy of biology is incompatible with Darwin's theory of natural selection (Heijdra 2003).

## Behaviorism

Behaviorism denies the existence of (or downplays the importance) of the mental, especially as a *cause* of behavior. Thus it is much more in line with Darwinism than linguistic relativism. Behaviorism itself comes in many flavors: philosophical behaviorism, scientific behaviorism and methodological behaviorism.

a) Philosophical behaviorism is a position in the body-mind debate that holds, roughly, that the mental does not exist and that our *everyday utterances* about mental content, dispositions and processes (also sometimes called 'folk psychology') are in fact nothing but descriptions of forms of behavior (Ryle 1949, arguably also Wittgenstein 1945 (1953)). Often this current of behaviorism is coupled to some form of reductionist materialism, although not necessarily so.

b) Scientific behaviorism is most prominent in psychology. It holds that adequate, explanatory theories of the human 'psyche' should not contain references to mental content, dispositions ('inner states') and processes (especially not as *causes* of behavior), but only to *observable* behavior. This means that behaviorists reject the introspective method as a means to gain information about the human brain (or inner mind, in which they do not believe). Since little was known about the brain, the *classical* or *radical* behaviorism of Skinner (1938) treated it as a black-box and only sought to find correlations between observable sequences of 'voluntary' actions (called responses) and their consequences in operant conditioning experiments. Other behaviorists sought to find correlations between sequences of input stimuli and output responses – as in more classical conditioning experiments which, however, treated the mind/brain as a black-box as well (see §2.2 for a more extensive discussion of conditioning experiments). Nowadays, with the advent of brain-probing techniques such as fMRI, the brain is no longer a complete *terra incognita*. People who reject the study of the human 'psyche' by introspective methods and deny the existence of the mental, but nevertheless accept the role of 'inner skin' observable brain states in the explanation of behavior, are sometimes called post-Skinnerian, theoretical behaviorists. The name is, however, not very current, as the name "behaviorist" sticks so strongly to the black-box approach.

c) Methodological behaviorism: methodological behaviorism is the view that the mental is principally inaccessible for scientific research because our way of accessing it is principally subjective. It does deny the existence of the mental, but restricts the scientific, objective study of behavior to third-person behavior. It has been absorbed into general experimental and cognitive psychology.

Actual scientists or philosophers usually do not distinguish among these uses, but for systematic purposes it is useful to make them. In linguistics we find all three forms of behaviorism<sup>351</sup>; it can be found in, amongst others, the areas of language

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<sup>351</sup> Philosophical behaviorism in linguistics would mean that everyday utterances about language use, the meaning of words etc. are just like other everyday utterances about psychological processes, actually



acquisition, actual language use and the theory of meaning. All approaches draw a close connection between language use and behavior. However, not all approaches that hold that language and other forms of behavior are closely related are behaviorist in nature; for instance, speech-act theory is clearly not behavioristic.

The many versions of behaviorism in linguistics can best be described from the perspective of the behaviorist theory of meaning. The behaviorist theories of meaning, in line with the distinction between classical conditioning (with its focus on the stimuli) and operant conditioning (with its focus on responses), can be divided into two versions: stimulus theory of linguistic meaning and response theories of linguistic meaning. Response theories (given the prominence of Skinner's operant conditioning) were defended more often than stimulus theories.

**Stimulus theories:** stimulus theories explain the meaning of a linguistic utterance in terms of those aspects of the initial situation that gave rise to the utterance. For instance, when you hear the thunder (stimulus), you say 'thunder!' or when you see a friend you say: 'hello old buddy!'. Its two central problems are a) how to identify relevant aspects of a situation or the environment (the stimuli) and b) whether linguistic utterances can truly be said to originate in the external environment. Ad a) A situation or the environment has indefinite many aspects. How can we identify which gave rise to a certain utterance? And can we really divide up a situation or the environment without appealing to intentions and beliefs? Ad b) The linguistic utterances that stimulus theories seem capable of describing do not really rise above the level of a child's games of pointing and naming: 'cat!', 'dog!', or the level of highly stereotypical exchange situations, such as greetings etc. Our common intuition is, however, that a substantial number of linguistic utterances do not spring forth from stimuli in the environment or the situation, but from our inner constitution, our beliefs, values and intentions. Of course in certain *situations* typical *forms* of communication are predictable (when we are with colleagues we gossip or complain about our boss, when we are with parents we discuss family matters etc.), and although our beliefs, values and intentions may be the result of extensive socialization or conditioning, it seems unlikely that linguistic utterances can be accurately predicted and thus be correlated to stimuli.<sup>352</sup>

The best example of a philosophical stimulus theory of meaning is Quine's theory that an interpreter can attribute meaning to the utterances of speakers of a language completely unknown to him or her (in cases of so-called radical translation), although Quine's theory is more than just a naive stimulus theory of meaning. We come back to Quine's theory as we discuss semantic holism in §3.13.

**Response theories:** response theories of meaning were prominent in American structuralism (Sapir, Bloomfield, Harris, Morris). Response theories are stronger than straight stimulus theories because they, like all theories of operant conditioning, start with spontaneous verbal utterances (somewhat confusingly called *responses*) and the effect these have on the hearers (called the *consequences*), which lead to new, updated verbal utterances that in some regard *function* better (i.e. have more 'preferable' consequences). The origin of the spontaneous utterances (which are not caused by the environment as in the naive stimulus theory) is, however, vague; the

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utterances about behavior. For instance expressions such as: '[No, stupid,] I *meant* an A and not a B' (for instance when you correct someone who has not brought the object you asked him to bring), are not to be interpreted as a *description* of an inner (sense) meaning of the object A, that – so to speak – is present to the mind until the required object is brought, but as an *order* to get an A instead of a B.

<sup>352</sup> Stimulus theories of meaning in behaviorism seems to suffer from the same problems as behavioral ecology (§2.3) in trying to explain the inner constitution *directly* from external factors.

mental sphere of beliefs and intentions is excluded from the response theories, just as in all behaviorist theories. The response theories are further more realistic than the stimulus theories because they usually include the social setting of communication and the effects utterances have on others. Most stimulus theories do include the social sphere in the environment or situation, but not necessarily so, since they are only concerned with relations between stimuli that directly cause utterances (without social feedback). Note finally, that as the hearers can also be speakers, the account is symmetrical for both participants in a conversation.

The most sophisticated account of a response theory can be found in Skinner's book *Verbal Behavior* (1957). It contains little empirical work, but it does offer a detailed theoretical framework. Skinner argues that verbal behavior is a function of the speaker's current environment and his past behavioral (and genetic) history. Central to Skinner's framework is the 'three term contingency model' of 'antecedent-behavior (response)-consequence'.

Skinner is well aware of the problem of how to measure verbal utterances. His general position would favor measuring verbal behavior by *rate of response* (Skinner 1938), i.e. the number of times a linguistic form is used within a certain span of time. However, in the case of *Verbal Behavior* this *seems* problematic, since all verbal behavior does not have the same unitary quality of lever presses or key pecks (Skinner 1957: 14). Nevertheless, just as the behavior of chickens is much more complex than key pecks but key pecks were instrumental in modeling chicken behavior, so human verbal behavior is of course more complex (and caused by many factors; id., p. 187), but nonetheless is thought to be modelable by simple response measures. Instead of the rate of a response, Skinner opts in the case of verbal behavior for the *strength* of a response. In ascertaining the strength of a response, Skinner suggests some criteria: actual emission, energy-level (force of the utterance), speed (of the utterance itself or the latency from the time in which it could have occurred to the time in which it occurs) and repetition, but notes that these are all very limited means for inferring the strength of a response as they do not always vary together. They may come under the control of extraneous variables, such as noise, the type of listeners, or the distance between speaker and listener. Actual emission is a yes/no measure, but the other three factors (energy-level, speed, repetition) comprise only *possible indications of relative strength* (id., p. 23f).

Specific examples Skinner gives of functional verbal behavior under the control of operant conditioning are what he calls the *mand*: a form of verbal behavior that is controlled by deprivation, satiation, or what is now called *motivating operations* (MO) as well as a controlling history (id., chapter 3<sup>353</sup>). An example of this would be asking for water when one is water deprived: "thirsty".<sup>354</sup> Others examples of operant-conditioned verbal behavior are 'taking dictation' (chapter 4), and imitation, which Skinner calls 'the echoic' (id.): the attempt to achieve point-to-point correspondence between an auditory stimulus and a vocal response, as when one learns the pronunciation of a word.<sup>355</sup> The most important example of such behavior is, however, what Skinner calls "the tact". A tact is said to 'make contact with' the world, and refers to behavior that is under the control of generalized *reinforcement* by an audience. The controlling stimuli is nonverbal, "the whole of the physical environment." It can undergo many extensions: generic, metaphoric, metonymical,

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<sup>353</sup> Chapter references in this section to Skinner's book.

<sup>354</sup> More examples of mands in Lamarre and Holland 1985.

<sup>355</sup> For experimental work on the echoic in the behaviorist tradition, see Boe and Winokur 1978.

solecistic, nominal etc. and can also be involved in abstraction (chapter 5). An example of generic extension: we might call something a *car*, then seeing something like the old object called a *car*, we call this new stimulus a *car* too; and similarly for the other forms of extension.

On the basis of these many tacts, Skinner then postulates “the autoclytic”: a form of verbal behavior that modifies the functions of other forms of verbal behavior. For example, ‘I think it is raining’ possesses the autoclytic ‘I think,’ which moderates the strength of the statement ‘it is raining’. Syntax as such, just like any form of “compositionality,” is also described as an autoclytic, because syntax is seen as nothing but “the order or grouping of responses,” which may be a function of relevant strength, temporal ordering, or other factors (Skinner 1957, chapter 6, 7, and 8).

Skinner has much more to say about other elements of verbal behavior, such as Freudian slips of the tongue, speaking when tired, poetic use of words, the exact nature of the feedback from the audience and its defects, self-editing one’s verbal utterances etc. But with the characterization of referential words, compositionality and syntax we have a basic picture of Skinner’s scheme, which already has some serious problems. Although some elements, such as taking dictation and the echoic, may indeed be (partly) under the control of operant conditioning, his explanation of other examples of language use (which are far more important) appears wanting for the same reasons the naive stimulus theories of meaning were found wanting: by excluding intentions, values and beliefs from a theory of meaning, one throws out the baby with the bathwater. As a theory of production, response theories are also worse than stimulus theories, because we will see that any Darwinian theory of meaning starts with the presupposition that comprehension reaches further than production and is chronologically prior.

Although behaviorism is generally well compatible with Darwinism due to its denial of mental (non-physical) causation, and although we saw some approaches in which behaviorists applied their ideas to cultural evolution as in the ontogenetic evolution of knowledge (see §2.3), (classical) behaviorists *nevertheless paid little attention to the problem of the origin of language*. This might be due to the black-box approach of the ‘psyche,’ which meant in practice a ‘blank-slate’ model with a ‘psyche’ strongly malleable to all types of conditioning; language was seen as only one of the many forms of conditioned behavior, no more interesting than any other. However, even if behaviorists paid attention to the origin of language, one can doubt whether they would overcome the problems we just noted in discussing linguistic behaviorism, so that even if new behaviorist theories of language were integrated into the Darwinian paradigm, one would seriously doubt whether one had explained the origin of *true language use*.

Classical behaviorism in linguistics more or less ended with Chomsky’s highly critical review (1959) of Skinner’s book *Verbal Behavior*. Chomsky claimed that one cannot learn a language by conditioning, especially not its syntax or grammar.<sup>356</sup> He maintained that language use is creative and based upon intentional action, contrary to non-creative, mechanical conditioning, and that the mind is not a

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<sup>356</sup> Syntax refers to the actual structure of the compositionality of semantic elements; grammar is the theory of (correct) syntactical constructions. In Chomsky’s work the words ‘syntax’ and ‘grammar’ are sometimes used more or less interchangeably as Chomsky believes that syntax is determined by innate grammatical knowledge (which is different from the far less sophisticated school book grammars).

blank slate but pre-programmed. Chomsky's theory is also strongly anti-behaviorist insofar as it relies on *introspective reports* of the grammaticality of sentences by test subjects. Skinner never responded to Chomsky extensively, only claiming (1972: 63) that he had been misunderstood and that his theory was not about true language acquisition (as we will see, neither is Chomsky's) but about the relation of language and overt behavior. He claimed that humans could construct linguistic stimuli that could acquire control over behavior in the same way that external stimuli could, though admitting that contingencies of reinforcement would not always produce the same effects on human behavior as they reliably do in other animals (see his 1969: 283 *Contingencies of reinforcement*).<sup>357</sup> We now discuss Chomsky's theory in more detail.

### **Chomsky and generative grammar**

With Chomsky's highly influential critique of Skinner, the new research program *transformational* or as it was later called *generative grammar* was put on the agenda. As the name indicates, this subfield of linguistics gave *syntax* pride of place (Jackendoff 2002 calls it 'syntactocentric'), although its results were thought to be important for almost all other branches of linguistics. The lasting result of the field of generative grammar is the demonstration that in traditional grammars many rules are not present -- either because they are so obvious that nobody had noticed them, or because they are too complex. The field has produced more detailed analyses of grammar than ever before, including complex transformational rules (for the formation of, for instance, relative clauses or questions) that could be written down with seemingly mathematical precision (thus adding credibility to the scientific character of the discipline of linguistics).

In the last few decades, however, the emancipation of other sub-disciplines of linguistics has led to results that cast serious doubt upon the results and ideas of generative grammar. For some, these doubts are so strong that they want to reject the paradigm of generative grammar altogether; others, however, try to reorganize generative grammar from within. It is tempting to describe this debate as a Kuhnian style revolution (see §1.33) in which an old paradigm (supported by older linguists) is gradually replaced by a new one (supported by a younger generation). However, a simplistic uniform picture of a paradigm replacing another is misleading. The research program of generative grammar research is itself far from uniform. There have always been sub-currents that did not work within the Chomskian paradigm. This paradigm is not replaced by one new overarching paradigm, but instead by several sub-disciplines without 'big theories of everything' in linguistics, and by many small ideas that are tested with descriptive *statistical* models which rely more on observation than theorizing. If these disciplines have anything in common (like a 'paradigm'), it is the methodology of careful statistical, empirical research. The attractiveness of generative grammar was once its mathematical character. Some may regret the demise of the determinate models of generative grammar. However, these statistical models seem to capture much more information about linguistic reality than the idealisations that the models of generative grammar.

In this section, we will critically assess Chomsky's theory for two complementary reasons. First, Chomsky's theory still figures prominently in

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<sup>357</sup> The relation between Skinner's book and Chomsky's review has been the subject of further research by MacCorquodale 1970, who claims that Chomsky didn't understand behaviorism at all, and Stemmer 1990, who is especially critical of Chomsky's mentalistic turn.

linguistics, and discussions on the origin of language often focus on elements that are at the core of Chomsky's theory (language universals, types of language complexity, the existence of an innate grammar or I-language etc.). To be able to take a position in the debate, we need to assess these elements of Chomsky's theory. Second, and more importantly, Chomsky's position is, to put it bluntly, anti-evolutionary and anti-Darwinian (though some try to 'reconcile Chomsky and Darwin' [Bickerton and Calvin 2000] and in Chomsky's most recent work, after having been a lifelong sceptic of Darwinian explanations of the origin of complex traits such as wings or language, Chomsky calls himself a "firm supporter of the adaptationist program"). We will ask which elements of Chomsky's theory are not easily integrated in a Darwinian framework.

A Darwinian approach (to the evolution of linguistic forms) focuses on how publicly-observable primitive sound forms gradually evolved into complex strings of meaningful symbols, stressing the use and semantics of the forms at all intermediary stages; Chomsky, on the other hand, only accepts a highly complex grammatical language (innate rather than publicly observable) as the only true language, stressing the mathematical structure of the language instead of its use. Because of the clear misfit between Chomsky's theory and Darwinism, and because of the dominance of Chomsky's theory in linguistics in the second half of the 20<sup>th</sup> century, research on Darwinian origins of language (especially that of linguistic forms) may have been blocked as a valid research program even before it got off the ground (see §4.1). A critical assessment of Chomsky's theory can thus pave the way for Darwinian explanations of the origin of language.

We are well aware that Chomsky's theory has changed significantly over time and that others have also given alternative accounts of generative grammar.<sup>358</sup> Moreover, there are other research programs in linguistics in the 20<sup>th</sup> century that

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<sup>358</sup> The different Chomskian versions of generative grammar, i.e. transformational grammar, government and binding theory, the minimalist program etc., are discussed in the next section. Non-Chomskian versions of generative grammar are:

i) Categorical Grammar: the formalism of natural language syntax, motivated by the principle of compositionality, and organized according to the view that syntactic constituents should generally combine as functions or according to a function-argument relationship (Steedman 2000). The *Montague grammar* is the best-known example (it is based on the idea that there is no essential difference between the semantics of natural languages like English [which Montague himself investigated] and formal languages like predicate logic, Partee & Hendriks 1997).

ii) Lexical Functional Grammar: a version of generative grammar developed in the 1970s by Robert Kaplan and Joan Bresnan, which considered the multiple levels of the Chomskian hierarchy (syntax, morphology, semantics, phonology etc.) not as separate levels of the constituent (syntactic) structure representation being mapped onto each other sequentially, via transformations, but as levels that mutually constrain each other. It further held that grammatical-function changing operations like passivization are lexical, which means that active and passive verbs are both listed in the lexicon (Bresnan 2001).

iii) Generalised Phase Structure Grammar (GPSG) and Head Driven Phase Structure Grammar (HDPSG): GPSG was an attempt to create a context-free grammar, with some suitable conventions such as meta-rules (to ease the formulation of a grammar) and semantic annotations to syntactic structures (to make the computation of a compositional syntactic phrase easier) (Gazdar et al. 1985); it has now been abandoned in favour of HDPSG (Pollard and Sag 1994), an alternative to Chomsky's government and binding theory, which offers a highly lexicalized, non-derivational generative grammar underscoring the rich (grammatical) structure of the lexicon (which was long considered in the generative tradition as just a list of entries).

There are also few lesser-known programs such as *tree-adjoining grammars*, as defined by Aravind Joshi, and *relational grammars*. Much of the critique against Chomsky can also be addressed to these other versions of generative grammar.

share with Chomsky a focus on grammar in the study of language.<sup>359</sup> Nevertheless,

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<sup>359</sup> Other grammatical frameworks since the Second World War, most of which also make the study of grammar central to linguistics, are (listed in order from 'grammar most important for language use' to 'grammar least important for language use'):

i) Functional Grammar: an approach to grammar originally developed by Simon C. Dik that seeks to replace grammatical functions (such as subject and object) with more general ones, and analyses linguistic utterances at three levels: 1) Semantic functions (Agent, Patient, Recipient, etc.), describing the role of participants in states of affairs or actions expressed; 2) Syntactic functions (Subject and Object), defining different perspectives in the presentation of a linguistic expression and pragmatic functions (Theme and Tail, Topic and Focus), defining the informational status of constituents, determined by the pragmatic context of the verbal interaction. It is quite similar to the earlier Role and Reference Grammar developed by William Foley and Robert Van Valin, Jr. in the 1980s; the Danish school of functionalism combines the work of Dik with the structuralist tradition of Hjelmslev.

ii) Systemic Functional Grammar: a semiotic approach to grammar developed by Michael Halliday and others. The word 'function' here indicates that the grammar is about meaning/use, the term 'systemic' places language within the broader theory of systemic-functional linguistics that regards language as 'a network of systems, or interrelated sets of options for making meaning; it is concerned primarily with the choices that are made available to speakers of a language by their grammatical systems, which are assumed to be meaningful, and with the relationship of speakers' intentions to the concrete appearance of a language: its content or the structure of the content, i.e. rather traditionally: its lexicon and its grammar. See 'a note of biosemiotics' (appendix 2) for more on the general approach of semiotics.

iv) Dependency Grammar: (subforms: Link Grammar and Word Grammar) a grammar framework in which structure is determined by the relation between a word (called a head) and its dependents. Dependency Grammar is well suited to languages with a relatively free word order as they don't define structure with a specific word order; *link grammar* adds directionality (natural left to right, i.e. chronological information flow in a sentence); *word grammar* relies even more strongly on lexical items and works within the tradition of cognitive linguistics, modeling language as part of general knowledge and not as a specialized mental faculty contra Chomsky.

v) Cognitive Grammar: a cognitive approach to grammar developed since 1976 by Ronald Langacker within the larger emerging field of Cognitive Linguistics, which assumes that linguistic structures are motivated by general cognitive processes and makes extensive use of principles of *gestalt psychology*. Cognitive grammar holds that human languages consist solely of semantic units, phonological units, and symbolic units (conventional pairings of phonological and semantic units), and extends this notion of symbolic units to the grammar of languages (Langacker 2008); syntactical structures are analyzed accordingly through conceptual and attentional structures, such as image-scheme, figure/ground, trajectory/landmark, frames and scripts, to show how our attention and experience of the world colors our syntax (Ungerer and Schmidt 1996). Its critics point to the many methodological problems in testing the claims of cognitive grammar empirically, as it is hard to exclude variables when focusing in research on the attention of research subjects to the 'structure' of pictures etc.

v) Construction Grammar: a name for the collection of theories, or models, of grammar that are based on the idea that the primary unit of grammar is the grammatical construction rather than the atomic syntactic unit and the rule that combines atomic units, and that the grammar of a language is made up of taxonomies of families of such constructions. One of the earliest studies was that of 'there-constructions,' by George Lakoff's *WOMEN, FIRE, AND DANGEROUS THINGS* (1987), arguing that the meaning of the 'there construction' was not a function of the meanings of the parts, that their odd grammatical properties followed from the pragmatic meaning of the construction, and that variations on the central construction could be seen as simple extensions using form-meaning pairs of the central construction; it is of a piece with his famous work on taxonomies of metaphors in *Metaphors we live by* (1980, coauthored with Johnson). It is akin to Longacker's functional grammar. Radical construction grammar, as the name implies, takes the ideas of construction grammar to the extreme by arguing that syntactic categories, roles, and relations are not only language-specific (instead of universal), but also construction-specific (thus wholly replacing syntax by semantics); embodied construction grammar adopts the basic ideas of construction grammar, but emphasizes the relation of constructional semantic content to sensorimotor experiences, using semi-popular material on embodiment cognition (in one of the many often rather vague senses of this popular research program tag, see Wilsons 2002).

vi) Stochastic Grammar: a collective word for statistical approaches towards grammar that usually don't judge grammatical productions as correct or incorrect, but assign a certain probability to the production of certain semantical combinations using complex computer techniques; one of the better-known approaches is the operator grammar of Zellig Harris 1991 which holds that each human language is a self-organizing system in which both the syntactic and semantic properties of a word are established

Chomsky is the most prominent and earliest figure in the field and he has carefully thought out the nature of the basic concepts of generative grammar and its relation to other disciplines. That is why we focus here on his views, although it is likely that no single linguist shares all his convictions. We will assume a basic knowledge of Chomsky's theory, but provide a summary of his views as they have changed over time in appendix 3. We start with the reasons that Chomsky's theory does not fit well with Darwin's theory, and then turn to a critical assessment of elements of his view that play a critical role in the debate around language origins. The critique of Chomsky will be substantiated further as we discuss the research of different branches of linguistics that have emancipated themselves since the sixties and seventies in §3.2.

### **The misfit between Chomsky's theory and Darwinian theories of the origin of language**

At superficial glance at Chomsky's theory seems to suggest that Chomsky's theory could easily be integrated in a Darwinian frame-work. At this superficial glance Chomsky's theory seems only to deal with what the grammatical correctness of strings of linguistic units amounts to; a Darwinian story about language origins could then show, if Chomsky's theory were true, how such grammatical correct strings could evolve. However, at a deeper glance many problems at multiple levels arise if one seriously wishes to integrate Chomsky's theory and the field of generative grammar into the Darwinian perspective (from strong to weaker): 1) the very idea of grammatical correctness and the normative claim that language is truly language if it displays the *static* deep structure of generative grammar forbid Chomskians to call proto-languages or other more simple languages that function pragmatically without having the deep structure of generative grammar true languages so that a Darwinian story about language origins would for Chomskians not be about *language* at all, whereas Darwinians miss in Chomsky's theory any *link* with *evolution* of linguistic forms, 2) Chomsky's theory is not just a normative/descriptive theory about correct language forms, but also contains a lot of theorizing and speculation about the *causes* and or *mechanism* how/why language has the structure it has and these causes and mechanism are not only problematic in light of new research in other fields of linguistic, but also hard to integrate into a Darwinian framework, 3) Chomsky's ideas about the essence of language and the mechanisms or causes behind the origin and workings of universal grammar gained so much current that language fields that provide key clues about the origin of language were long overshadowed in prestige and funds, so that a Darwinian theory about language origins could not really get off ground. We will list ten aspects of Chomsky's theory that substantiate these three problems further.

a) **No true empirical theory of learning:** for a Darwinian approach to language, the field of language acquisition is important for two reasons: i) what the stages

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purely in relation to other words, without any external system or metalanguage required to define the rules of a language. Instead, the theory presupposes unconscious learning through exposure to usage and participation, and is consistent with the idea that language evolved gradually, with each successive generation introducing new complexity and variation. It posits three universal constraints: Dependency (certain words depend on the presence of other words to form an utterance), Likelihood (some combinations of words and their dependents are more likely than others) and Reduction (words in high likelihood combinations can be reduced to shorter forms, and sometimes omitted completely). Together these provide a theory of semantic information. Dependency builds a predication structure; likelihood creates distinct meanings; reduction allows compact forms for communication.

children go through as they learn a language can teach us about how simpler languages can already be useful and can be analogous to the ways in which language gradually evolved over time, and ii) in a complete theory of the origin of language, it has to be shown how each generation of children has again been able to learn their mother tongue which gradually became more and more complex. Chomsky's theory and his approach to language acquisition do not teach us anything about these things. Although Chomsky postulates a pre-programmed language acquisition device to explain how, despite the supposed poverty of the stimulus (regarding quality, duration of exposure), children are able to learn a complex language with such amazing perfection in such a short time, he has no true empirical theory of the social process of language acquisition. He only focuses on what the *end result* of learning should look like (the adult language)<sup>360</sup>, on the basis of a contextless, not publicly or socially available universal grammar,<sup>361</sup> and then simply postulates that child language acquisition has to be constrained by an innate I-language. In practice, this meant two things: i) Chomskians neither observe the stages of language acquisition nor the pragmatic use of simpler forms of languages, whereas these provide valuable clues about language origins (§3.24), ii) the short-cut that as children learn language their language is constrained by an innate device meant that if one were to treat the topic of language origins, one would have to demonstrate how an innate language device could evolve, whereas as we shall see such a specific language device does not seem to exist.

b) **Operationally sterile rules:** Chomsky explicitly denies that his grammatical rules (the rules of competence) are the rules the brain actually uses when it forms correct grammatical sentences in actual *performance*; the rules of competence

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<sup>360</sup> Chomsky has no story to tell about how universal grammar is cast into language-specific grammars, how exposure to language triggers the universal language acquisition device and what the intermediary stages of language 'performance' look like. The most recent instantiation of the Chomskian program, the Minimalist Program, did not really change this. Note that Chomsky does not want to speak of the *end result* of language learning. He denies the *end result*, because it is not well delineated and because he denies the meaningfulness of the concept of public language (see below on general criticism), thus also the meaningfulness of the concept of 'the end result of the learning acquisition of a public language'. Chomsky's universal grammar is itself no end result, but the same unchanged entity from the beginning until the end of language learning: "What we say is that the child or a foreigner has a "partial knowledge of English," or is 'on his or her way' toward acquiring knowledge of English, and that if they reach the goal, they will then know English. Whether or not a coherent account can be given of this aspect of the common sense terminology, it does not seem to be one that has any role in an eventual science of language. (...) If Peter is improving Italian or Gianni is learning his (...) we gain no insight into what they are doing by supposing there is a fixed [external public] entity that they are approaching, even if some sense can be made of this mysterious notion (quoted by Millikan in *Chomsky and his critics* by Anthony and Hornstein 2003: 216)."

<sup>361</sup> Chomsky maintains that his universal grammar is contextless and that learning grammar does not necessarily need contextual info, although it may 'facilitate' learning (1965: 25) – just as "depth perception in lambs is considerably facilitated by mother-neonate contact, although again there is no reason to suppose that the nature of the lamb's 'theory of space' depends on this contact [33]." And indeed, the *attention* children pay to language is innate and (initially) independent of contact. However, children don't learn a language when they are not exposed to *child directed* speech (instead of random language noise from the radio or television), whereas lambs can see even when their mother dies directly after birth. Moreover, the language, i.e. German, French etc. the child develops depends on the *language community* in which it is raised, and even more specifically the intermediary stages of its language development depend crucially on the *mother* it has, whereas a lamb develops sight in any environment. Finally, although some special mechanisms for perception can be discovered in the brain, no such special language acquisition device can be located, further undermining the analogy between the lamb's perception development and children's language acquisition. See chapter §3.21 on the neurology of speech and §3.24 on first language acquisition.



are thus operationally sterile.<sup>362</sup> The rules are like a-temporal Platonic forms that can be gazed at by the *mind*. This is hard to integrate into a Darwinian frame-work as yet (and it seems hard to imagine how such rules might ever be fully integrated into a Darwinian frame-work): as yet there is no Darwinian explanation of the mind, mental intuition is not a proximate, mechanical mechanisms Darwinians normally use in their explanations and the static, non-variable nature of the language rules (with all humans having the same rules) is also hard to accommodate for within a Darwinian frame-work (see below e) no variation). Further, in practice Chomsky's distinction that his rules are not the rules the brain uses (a fact he elaborated upon only after his theory had long gained current) was forgotten by many of his followers as it was thought that the brain was programmed with the rules of universal grammar or that these rules were hard-coded in the genes. This meant that the rules were seen as the proximate mechanisms. Scientists thus started looking for these perfect, beautiful, mathematical rules. As such they sometimes overlooked the possibility that they do not exist and that in reality the true mechanisms behind language processing are 'messy' evolutionary bricolage products.

c) **Saltationist origin:** Chomsky insists that 'half a language' (half the set of grammatical rules) is 'no language', so that the complex phenomenon of grammatical language can only have arisen all at once (Chomsky 1988, §4.1), i.e. saltationistically, so that a true Darwinian *gradual* theory of language origins (including grammatical rules) seems to be excluded beforehand.

d) **No diachronic analysis:** Chomsky's almost exclusive concentration on *synchronic* analysis of language as an almost atemporal system of rules means that (perhaps not in word, but in practice) diachronic linguistics is for him not a part of linguistics proper; however, to evolutionary explanations of language, diachronic processes are central (§3.25).<sup>363</sup> The dominance of the Chomskian paradigm meant that research in this important area was long overshadowed.

e) **No variation:** Chomsky is quite explicit that "linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shift of attention and interest, and errors (random or characteristic) in applying his

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<sup>362</sup> See Chomsky 1965: 139f "Consider the simple case of a phrase structure grammar with no transformation (for example, the programming language, or elementary arithmetic, or some small part of English that might be described in these terms). It would be clearly absurd to suppose that the 'speaker' of such a language, in formulating an 'utterance'," first selects the major categories into which these are analyzed, and so forth, finally, at the end of the process, selecting the words or symbols that he is going to use (deciding what he is going to talk about). To think of a generative grammar in these terms is to take it to be a model of performance rather than a model of competence, thus totally misconceiving its nature. (...) [A] generative grammar as it stands is no more a model of the speaker than it is a model of the hearer. Rather, as has been repeatedly emphasized, it can be regarded only as a characterization of the intrinsic tacit knowledge or competence that underlies actual performance."

<sup>363</sup> One could even argue that in Chomsky's scheme language evolution, especially evolution of grammar, is impossible because there are no intermediate forms possible; innate grammar is fixed, the grammars of public languages are nothing but switch-settings of this grammar, and so if there is grammar evolution in public languages then this can only be imagined as *discrete* switching of some linguistic settings. We will see, however, that language (grammar) evolution is a *continuous* process. In similar vein, the historical linguist Larry Trask 2002 argues that the ergative case system of the Basque language is not a simple binary parameter as the Chomskian scheme supposes, and that different languages can have different levels of ergativity during their history.

Other problems from the theory of language evolution for the Chomskian scheme, such as the process of grammaticalization in which semantic elements gradually become grammar, are discussed in §3.24.

knowledge of the language in actual performance (1965: 3).” This *homogeneity* is guaranteed by an internal language acquisition device that is supposedly the *same for every human*. These presumptions immediately rule out an integration within an evolutionary dual-inheritance framework, as this frame-work assumes that there exists *synchronously present* variation in both genetic predispositions of the language skills of humans (§3.21) and in the linguistic forms themselves (§3.24), i.e. that there exist variation in the language acquisition device (whether it really exists or not) and in what sentences look like or what is perceived as grammatical. Thus, whereas Chomsky’s theory rules out variation, it is the *conditio sine qua non* of evolutionary theory.

f) **Primacy of descriptive sentences:** Chomsky (perhaps unconsciously) concentrates almost exclusively on one or two types of linguistic utterances: full, descriptive sentences and questions instead of wishes or orders. Due to this Chomskians can easily fall prey to the fallacy of equating language with a system of propositions, as many philosophers for over 2500 years have done. This places Chomskians far away from more use-oriented theories in linguistics,<sup>364</sup> which may focus on simple orders or ‘proto-sentences’. Such simpler utterances with clear uses are central to an evolutionary account that focuses on the actual *pragmatic* use of utterances, instead of the beautiful mathematical properties of sterile full-blown sentences. The dominance of the Chomskian paradigm may have caused less attention to them.

g) **Primacy syntax:** Chomsky concentrates on the abstract aspects of language instead of on semantics and pragmatics, as evolutionary accounts usually do. By focusing exclusively on grammar, one may start to idealize its system. It then becomes hard to see how this ideal, complex system *might have evolved at all* (so that one must resort to saltationistic stories). Fields that focus less on grammar but more on semantics and pragmatics do not have this problem; they observe how the current syntactical system (as well as all previous syntactical systems) have many loose ends,

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<sup>364</sup> On several occasions, Chomsky admits that language use can have many functions and that the stopgap idea that the function of language is communication (in general) is either devoid of meaning, or false insofar as the normal, everyday concept of communication implies the success of the communication which is not always the case for language use: “either we must deprive the notion communication of all significance, or else we must reject the view that the purpose of language is communication (...) It is difficult to say what “the purpose” of language is, except, perhaps, the expression of thought, a rather empty formulation. The functions of language are various (1980: 230)”, “What does it mean to say that language has [communication as] an “essential purpose”? Suppose that in the quiet of my study I think about a problem, using language, and even write down what I think. Suppose that someone speaks honestly, merely out of a sense of integrity, fully aware that his audience will refuse to comprehend or even consider what he is saying. Consider informal conversation conducted for the sole purpose of maintaining casual friendly relations, with no particular concern as to its contents. Are these examples of ‘communication’? If so, what do we mean by ‘communication’ in the absence of an audience, or with an audience assumed to be completely unresponsive, or with no intention to convey information or modify belief or attitude? (Chomsky 1980: 130)” and “I have yet to see a formulation that makes any sense of the position that the “essence of language is communication (1992: 215).”

Unfortunately, from the plurality of functions, Chomsky does not conclude that linguistic utterances come in many forms; on the contrary, he keeps focusing almost exclusively on descriptive sentences. Elsewhere, in his few remarks on the evolution of language, Chomsky regards language as a spandrel of our ability to understand and or use mathematical recursion (Hauser, Chomsky and Fitch 2002), thus as a byproduct of natural selection that has not been selected for its functionality. Chomsky’s insistence on the many uses of language can be understood to mean (in retrospect): no single use of language is primary, since they are all accidental uses of our innate mathematical system of recursively well-formed strings (universal grammar) that arose as an accidental byproduct with no primary evolutionary function of its own.

and that new syntactical rules slowly evolve when semantic sequences become gradually fixed (see §3.24). As such they hold the key to an evolutionary explanation. The dominance of the Chomskian paradigm may have caused that these facts, that actually upset Chomsky's theory, were long not well known or questioned.

h) **No public language:** Chomsky denies that there is such a thing as a common, public language: "It is a striking fact that despite the constant reliance on some notion of "community language" or "abstract language," there is virtually no attempt to explain what it might be (1994: 39)." Chomsky's arguments against this public language are that a) it is not well-defined since individuals differ too much in their language use to speak of a 'thing' they have in common (even the language use of individuals evolves during their lifetimes), and b) that all attempts to define it formally by a government or another organization, such as for instance the *Académie française* who mandated what correct French was, are but superficial declarations because the depth structure of a language is much more complicated than people think. However, denying that there is a common public language goes strikingly against common sense. People, lay and specialist alike, can consistently recognize that others are speaking the same language (or dialect) as theirs, or that they are speaking a different one (although there are of course borderline cases). In *first approximation* one can call groups of people that identify each other as speaking the same language as a language community, and their language as a common public language.

There is a beautiful parallel between languages and species in general. In §1.22 we encountered the species problem: Darwin showed that there are only individual animals and no species in the old sense of atemporal set entities. Nevertheless, it is still pragmatic to speak of animals as belonging to 'species' – in the new sense of groups of 'relatively similar' animals with a common ancestor that can interbreed, i.e. on the basis of i) typological, ii) cladistic criteria and iii) sexual contact. Chomsky with his denial that there are no public languages sounds like a Darwinist who says that there are no species because of all the variation, and who then refuses to look for pragmatic criteria that (although individual criteria are weak) can *in combination* (§1.22) be used to formalize our intuition that dogs are different from cats, although both are more similar to each other than to birds, let alone plants. The same three criteria for biological species can be transformed into criteria about different languages. Languages are the same if i) they satisfy certain typological properties, ii) speakers of a language form an ancestral chain (vertically by biological replication or horizontal by transmission) and iii) the people can identify each other as speaking the same language. All criteria again have problems: a) the properties are superficial (as with the definition of French by the *Académie française*, as Chomsky rightly noted), b) there exist dialect continuums in which people on either end of the continuum can't speak with each other, although people in between can,<sup>365</sup> and c) languages, although this is of course never observed, could arise multiple times (certain figures of speech or fixed expressions do arise multiple times). Nevertheless, the three criteria can be used to formalize our intuition that French is different from Spanish, although both are more similar to each other than to German, let alone

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<sup>365</sup> Chomsky interprets the languages of the dialect continuum as two distinct languages: "People who live near the Dutch border can communicate quite well with people living on the German side, but they speak different languages in accordance with the term Dummett argues is fundamental [for language] (Chomsky as quoted by Millikan in Chomsky and his critics 2003: 216)." Unfortunately, Chomsky doesn't say what he means by Dummett's term (probably a rather Fregian concept of language). When there is a true dialect continuum, *artificial borders* drawn by governments (although over time they can help the formation and isolation of two languages out of one common dialect) cannot suddenly create two languages.

Japanese. The fact that Chomsky regularly uses examples of different *E-languages* in his work, calling them phrases from ‘French’, or ‘Turkish’ etc., shows he respects the commonsense distinctions as well (and how could he do otherwise?).<sup>366</sup>

Be that as it may, the fact that Chomsky usually denies the existence of an external public language altogether or calls it a non-entity, makes an evolutionary, *dual*-inheritance account of language impossible, since only one component (the ‘language device’) is acknowledged by Chomsky to truly exist.

**i) Written sentences instead of spoken sentences:** in the last three decades, a great deal of research has been devoted to differences between spoken and written language. It has been shown how the first literary publications were rather close to spoken language literally transcribed, and that it takes quite some time for a genuine literary tradition and a writing style to evolve. This literary style can in turn influence the way people, especially educated people, speak. Nevertheless, written language will never coincide completely with the way people speak, because spoken language relies just as much on stress, intonation, topic-focus articulation and repetition as on ‘normal’ syntax. The linguistic tests in the tradition of Chomsky focus mainly on written language, which is much more systematic and mathematical (‘grammatical’) than spoken language; its results, therefore, can absolutely not be extrapolated to language use *in general*. As we focus in our evolutionary explanations on *spoken* language, Chomsky’s theory is of even less use than it is for written language.

**j) Mentalistic philosophy of the meaning:** Chomsky (implicitly) has a mentalistic philosophy of language, which is hard to include in the naturalistic framework of a Darwinian theory (see §3.13).

Our discussion of the results of other sciences (§3.2) and the results of the philosophy of language (§3.13) will show that these elements are all wrong.

### **General elements of Chomsky’s theory related to the question of language origins**

We conclude our discussion of Chomsky with a brief discussion of two topics, central to the debate on language acquisition, on which his theory has a clear stance and which cause a lot of stir in the discussion on language origins.

#### **Innateness**

From his earliest work onward, Chomsky insists that language (which is for him at heart its *grammar*) is innate; from the eighties onward he calls this innate grammar the I-language (see appendix 3). It is unfortunate, however, that this fundamental notion of I-language (and the concept of innateness in general) is such a vague concept in linguistics. Put two linguists together and they will mean something different when

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<sup>366</sup> As Millikan pushes Chomsky on this point, Chomsky seems to admit this too: “I hope it is also clear that I think that commonsense notions merit attention. We should attend to the fact that such notions are not uniform; even the word ‘language’ does not translate easily into closely related languages, and conceptions vary widely in other cultural settings (...). I think we should also take seriously the teleological aspect of commonsense notions: the fact that Gianni is a normal four-year-old child in Venice, we have no way of referring to the language that he has acquired, but can only say he is on his way to acquiring some version of what we here loosely call ‘Italian’ (...) All of this could fall within an interesting chapter of what I’ve been calling ‘ethno-science’ (Chomsky in Anthony and Hornstein 2003: 308).” However, ethno-science is for Chomsky the science that deals with questions of why people believe in fictions or why common sense (although false) is so common. We on the contrary don’t regard common-sense opinions about different languages as fallacious, just as the talk of animal species makes sense if understood correctly.

they claim that language is innate. Although almost nobody denies that *some* elements of language processing are innate (determined genetically), linguists are notoriously vague about *what* is innate: sound representations (phoneme systems), sound recognition patterns, speech stream analyzers, speech stream segmenters, word recognition mechanisms, word meaning recognition mechanisms, grammar recognition mechanisms, sentence meaning recognition, logical reasoning, the theory of mind (the ability to attribute intentions to some else), conscious or unconscious knowledge of grammatical well-formedness, sound production devices, word production devices? As long as linguists do not make explicit what mechanisms are innate and how they work, they would do better to eschew the vague word “innate.”<sup>367</sup>

The same can be said about Chomsky. Nowhere, as far as we know, does he detail what all the components of the I-language are (except perhaps that it contains the ideal templates or rules of grammatical well-formed sentences), and nowhere does he explain exactly what role the I-language plays in language acquisition and language use, except metaphorically as he speaks of the ‘mental intuition’ that a sentence is correct or that we ‘cognise’ unconsciously that a sentence is well-formed. In the discussion on language origins it is thus not surprising to find that different Chomskians have different ideas on what part of language is innate. Some see all the rules of competence pre-programmed, others a monolithic, laterally well-located and distinct language module in the brain and still others a central brain device used in understand or producing iterative structures in both mathematics and language.

### Language Universals

According to Chomsky, all languages have i) a common core and this is ii) due to a universal I-language. We will dispute both these claims.

#### i) A universal core of human languages:

One of the arguments for universal grammar is the supposed universality of certain language features (Chomsky 1988). Prime examples by Chomskians are the emergence of the same universal features in independently-formed creoles (Bickerton 1995, but see §3.25) and sign languages (Siegal 2004). However, all examples of universality have been seriously questioned. Sampson 2002: 100 concludes that “I have surveyed this literature [on language universals] as exhaustively as I could manage (...), and I concluded that no candidate for the status of contingent linguistic universals survives scrutiny.” This can be demonstrated by an analysis of the distinctions Chomsky makes himself.

*The basic subdivision of any sentence in any language is for Chomsky the division into Noun- and Verb-phrase. This works, of course, only for descriptive sentences and not for orders (Bread here!). This fundamental distinction is a) fuzzy (even for English), b) not universal for all languages and c) sometimes of no use. a)*

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<sup>367</sup> See also Deutscher 2005: 16 “Let five linguists loose in a room and ask them to discuss innateness – chances are you will hear at least seven contradictory opinions, argued passionately and acrimoniously. The reason why there is so much disagreement is fairly simple: no one actually knows just how much of language is an instinct. (...) Of course, there are some basic facts about innateness that everyone agrees on, most importantly, perhaps, the remarkable ability of children to acquire any human language. (...) The human brain is unique in having the necessary hardware for mastering a human language – that much is uncontroversial. But the truism that we are innately equipped with what it takes to learn a language doesn’t say very much beyond just that. Certainly, it does not reveal whether the *specifics* of grammar are already coded in the genes, or whether all that is innate is a very general ground-plan of cognition. And, this is what the intense and often bitter controversy is all about.”

Chomsky remarks that the phrase “their refusal to come” in which “their refusal” looks like a noun-phrase is actually a hidden verb-phrase, i.e. “they refuse to come” although in “their refusal surprised me” “their refusal” is a “noun phrase” (Chomsky 1965: 185). Cases like these, especially when the derivation of a noun from a verb is no longer clear, are extremely confused. b) Worse still for Chomsky, there are words that do not fit into either category (for example, participles have a verbal and a nominal ‘part’, they ‘share’ in both the verbal and the nominal realms). Take for instance the sentence ‘John walked home whistling’. Should the ‘whistling’ be placed in the Verb-phrase or to the Noun-phrase; is the way of walking qualified by the whistling or John? The answer is both: John is a whistler and the walking is a relaxed, care free type of walking. Hence the distinction of a Verb-phrase and a Noun-phrase is already violent towards the semantics of this sentence, because it forces an element that can be placed in both or in neither to fit into one category or the other.<sup>368</sup> c) A little joke with a serious undertone: Chomsky maintains that language is composed of sentences and that sentences contain a noun and a verb phrase, but the French author Michel Danzel wrote a 233-page novel without a single verb as an extreme example of so-called constrained writing.<sup>369</sup> Here is a whole novel to which the NP/VP-distinction does not apply!

But we can go further in demonstrating that the universal distinctions Chomsky proposes are not universal. For him, the Noun-phrase/Verb-phrase (NP/VP) distinction we just discussed is much more fundamental than the ‘traditional distinction’ between ‘Subject’, ‘Verb’ and ‘Object’ (Chomsky 1965:79). That is because in sentences such as “I urge John to see the doctor” John is both the object of “to urge” and the subject of “to see” (id, 72). Nevertheless, Chomsky formulates some rules of how to find, using NP and VP, the Subject(s) and Object(s) (id, 124). Therefore it seems that subject and object are universal features of a language, and indeed we see many Chomskians formulate a typology of languages based on Subject, Object and Verb, speaking of SOV, SVO, VSO, VOS, OSV, OVS languages. However, many languages cannot be categorised so easily using these criteria. Take for instance Basque, which is a so-called ergative-absolutive language (like Kurdish, Sumerian, Tibetan, Eskimo-Aleut, and Georgian, among others). Ergative-absolutive languages contrast starkly with nominative-accusative languages, in which (historically at least) the subject-object distinction was formulated. The distinguishing feature of an ergative language (formulated in our language) is that it maintains an equivalence between the object of a transitive verb and the subject of an

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<sup>368</sup> In languages that use the participle even more than ours, this violence is even more apparent. For instance, in classical Greek (or Hebrew for that matter), the relevant things in a sentence are often told using participles, whereas the main verb is irrelevant or even omitted. One can encounter sentences in Greek that literally look something like: ‘Having been beaten by his father, he sat [on a chair], shaming himself for his deeds yet thinking his punishment had been too harsh.’ A modern translator would translate this as ‘He had been beaten by his father. As he was sitting [on a chair], he shamed himself for his deeds, but also thought that his punishment had been too harsh.’ If one forced this original sentence into a verb and a noun phrase, it is clear that one would do a lot of injustice with this branching, since the word ‘sat’ is not the main verbal form in the sentence, i.e. the sentence is not about sitting down. Of course one *can* still do this – supposing one decides in which category to fit the participle – (option 1), but only if one also puts aside the claim that the branching tells us anything about the *meaningful* structure of a sentence (option 2). Neither option is attractive.

<sup>369</sup> The novel is called *Le Train de Nulle Part (The Train from Nowhere)*. A sample: “Quelle aubaine ! Une place de libre, ou presque, dans ce compartiment. Une escale provisoire, pourquoi pas ! Donc, ma nouvelle adresse dans ce train de nulle part : voiture 12, 3ème compartiment dans le sens de la marche. Encore une fois, pourquoi pas ?” “Fool's luck! A vacant seat, almost, in that train. A provisional stop, why not? So, my new address in this nowhere train: car 12, 3rd compartment, forward. Once again, why not?”

intransitive verb, while treating the agent of a transitive verb differently, whereas in our language the subjects of transitive and intransitive verbs are treated equally but distinctly from the object of a transitive verb.<sup>370</sup> If the Basque language had risen to world dominance, the basic categories in which we describe world *languages* would have been different, perhaps based on ‘self-doing’ and ‘being-part-of-an-action-with-another’.

Of course one can try to remedy the situation by positing a distinction, as Chomsky does in a different context, between a language-dependent surface ‘grammatical’ and a universal ‘logical or psychological’ subject [163] so that even in our nominative-accusative language the grammatical and logical subjects need not coincide.<sup>371</sup> But one should not forget that the distinction between subject and object still comes from our dominant European languages. The current *Minimalist program*, which claims to look explicitly at the consilience of a grammar with models of grammar of other languages, did not change this. In its original conception, only the Verb phrases in transitive and unergative verbs constitute phrases, and the Verb phrases in passives and unaccusative (if even present) do not (Chomsky 1998).

## ii) Universal structures due to a common innate language

The above discussion will have made it clear that language universals are unlikely to be found, because current proposed universals are not truly universal. However, even if universal features were to be found, these could easily be explained without universal grammar by pointing to a) epistemological or methodological shortcomings of the literature on language universals, such as that when one compares two or more languages or structures one will always discover multiple similarities at an abstract level (Tomasello 2003 and Sampson 1999), or b) general acquisition biases (Kirby and Christiansen 2003), semiotic constraints (Deacon 2003) or pragmatic constraints in mapping meanings in speech (Bates 2003). The most natural explanation for language universals, however, *if they were to be found*, would be to invoke their common

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<sup>370</sup> In an ergative-absolutive language, the agent of a transitive verb (**A**) is marked with the so-called ergative case, whereas the subject of an intransitive verb (**S**) and the object of a transitive verb (**O**) are both marked with the so-called absolutive case. Examples: Accusative English (as it is):

- I (**S**) have traveled.

- I (**A**) have invited her (**O**) to go with me.

- She (**A**) has invited me (**O**) to go with her.

Ergative English (if it were so):

- Me (**S**) have traveled.

- I (**A**) have invited her (**O**) to go with me.

- She (**A**) has invited me (**O**) to go with her.

In these last two cases (ergative) the declension for **S** and **O** is the same (**Acc**).

<sup>371</sup> There is some rationale for that, for instance in relation to the aforementioned sentence “I urge John to see the doctor,” where John is both subject and object. Linguists ever since the famous Paul 1886 and Jespersen 1924 have made this distinction. Furthermore, English itself also has a number of so-called ergative verbs, which allow the object of a transitive clause to become the subject of an intransitive clause. In English, most verbs can be used intransitively, but ordinarily this does not change the role of the subject; consider, for example, ‘He ate the soup’ (transitive) and ‘He ate’ (intransitive), where the only difference is that the latter does not specify what was eaten. By contrast, with an ergative verb the role of the subject changes; consider ‘He broke the window’ (transitive) and ‘He broke’ (intransitive), where ‘he’ is the agent in the first sentence and the patient in the second. Indeed, ‘He broke the window’ is more closely synonymous with ‘The window broke’, where the direct object in the transitive version has become the subject in the intransitive version. A particularly odd English ergative verb is ‘graduate’: ‘he graduated school’ and ‘school graduated him’ mean the same thing, although the latter usage has passed out of vogue. With the latter usage, the verb is ergative, but with the former, the verb is nonergative.

origin:<sup>372</sup> it appears quite likely that all human languages have a common origin if one goes back far enough in time (unless one assumes that proto-languages arose independently in several isolated groups of proto-humans). However, an innate universal grammar, the same for everyone, requires (to be compatible with Darwinism) a common origin; but “if a common origin has to be postulated anyway, why not let this common origin explain universal features – to introduce innateness at this point would seem to go against Occam’s razor (Johansson 2005: 180).”

### §3.13 Philosophical views of language

*Three topics that are studied in the philosophy of language are highly relevant in the context of a discussion of the origins of language: a) the status of mentalism, b) cooperation in language use and c) the relation of thought and language and the primacy (ontological and temporal) of one of them in the context of the question of the origin of language. We will see a) that we need not analyze any language, animal or human, too quickly from the perspective of mentalism. Further, we will observe b) that pragmatism, a highly valuable theory for explaining many types of language use using a few cooperative principles, gives a central role to intentionality, which will be relatively hard to integrate into a Darwinian framework. Finally, we will see c) that by disentangling components or elements of thought and language, it becomes clear that language in general is not a prerequisite for thought; yet, some elements of thought (such as memory) are necessary for language, and others (such as a theory of mind or self-awareness) are useful for language learning, understanding, and use, though not strictly necessary. This implies that we can disentangle an evolutionary theory of the origin of language from a more general evolutionary theory of the origin of the mind. We finish this section with a brief criticism of Chomsky’s theory, on the basis of the elements from the philosophy of language that we have distinguished.*

#### **a) The status of mentalism:**

Mentalism is an old position in the theory of meaning.<sup>373</sup> It is currently no longer popular among philosophers; some biologists, however, unconsciously use this model as they discuss the meaning of animal sign use, even though other interpretations of animal sign use are possible. As mentalism is not particularly compatible with Darwinism, a non-mentalistic analysis of animal signal use (or human language use for that matter) can further the cause of Darwinian language analysis.

The mentalist position was taken up by most philosophers of the classical modern period – such as the British empiricists Locke, Hobbes, Berkeley and Hume and the continental rationalists Descartes, Leibniz and the Port-Royal logicians. It is still current today in the work of Fodor on the *Language of thought* (1975) or *mentalese* (1994), and as we will argue, also in the work of Chomsky, who explicitly reverted

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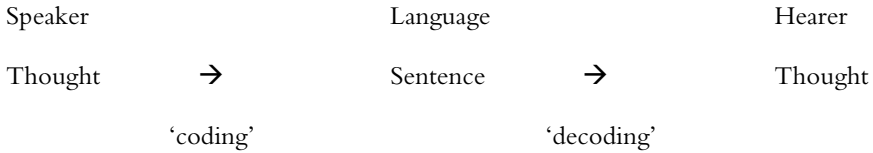
<sup>372</sup> Investigation of an original human language on the basis of current languages has restricted itself thus far to a reconstruction of elements of the supposed vocabulary of the original language (Ruhlen 1994); however, its critics have pointed out the enormous statistical uncertainties in this research and the rapid changes vocabulary undergoes, which makes tracing the original forms virtually impossible. Syntactic structures are a little more conservative, however, so one might expect to reconstruct them more accurately. However, see §4.2 on proto-language.

<sup>373</sup> When considering the philosophical problem of the meaning of linguistic utterances, we are not interested in the meaning of specific linguistic utterances (token question of meaning), but in what accounts for the meaning of *any* linguistic utterance (type question of meaning).



to the work of the rationalist philosophers. De Saussure’s classical distinction between a signifier (a sound) and a signified (primary a mental connotation) is also a version of mentalism.

Locke formulated the classical position of mentalism strikingly in his *Essay concerning human understanding*: “The use, then, of words is to be sensible marks of ideas; and the ideas they stand for are their proper and immediate signification (§3.2.1).” This position leads to a view of communication as a two-step process in which a speaker first codes his mental thoughts (composed of ideas) into material language signs, which are then decoded back into mental thoughts by the hearer, thus portraying language as an instrument to externalize our thoughts to others:



This scheme has the advantage of fitting well into our basic intuitions about communication and language use, and with the way we speak in everyday language about our own language use (also known as *folk semantics*):<sup>374</sup> “I told her what I thought and she understood me etc.” Another advantage is that it decouples meaning from too strict a relation with (external) reference. On the basis of this scheme (true or not), one can further understand the creativity of language use on the basis of cognitive association mechanisms. As words refer to mental ideas, new combinations of words can be seen as new associations of ideas into new thoughts.

This simple picture is, however, complicated by the fact that mentalism also needs to account for the relation of language to (external) reality. In the simple picture, only the elements of language and thought (the mental) are included, yet we also need the relation of these to reality because i) the thoughts need to be about something: an *extra-mental or externally existing reality*, or an *imagined world or inner reality*, and ii) for the thoughts to be inter-subjective<sup>375</sup>, the meaning of a word cannot be the private concern of an individual – as messages could then not be communicated to others – but needs to have some foothold in a shared supra-individual reality. As such, the sign of mentalism is an entity with a *double reference*: a name indicates a mental representation in the head (mind) of a person and that mental representation is a representation of a thing in reality, whereby the first referent (sometimes called *sense*) is instrumental in *indicating* the second referent (sometimes called the true *referent*).<sup>376</sup> This relation between the mental representation and that which it represents is sometimes called the *self-applying* character of ideas

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<sup>374</sup> Critics of mentalism, however, use this fact to argue that the faulty position of mentalism can be traced to a naive philosophical picture or abstraction of some innocent grammatical formulas we use in our everyday language. This abstraction is taken as a formal model for all forms of language use (orders, wishes etc.), whereas it is not even correct to describe the use of the innocent grammatical formulas we usually employ when we talk about language use. See Wittgenstein’s *Philosophical Investigations* §317 and §115.

<sup>375</sup> In fact, for even a single individual to understand his or her own linguistic utterances, these representations need to have some foothold in reality; otherwise they are devoid of meaning.

<sup>376</sup> Thus the sign as conceived by the father of semiotics, Peirce, is also a mentalist concept of the sign. Peirce defines a sign as something (A) that stands for something else (B) in a certain regard (C) for someone (D).

(Fodor) or the *self-luminosity* of ideas (Derrida). The mental representations or ideas make clear to the mind to which thing in external reality they apply.

One can of course wonder where these ideas come from, and how they can guarantee the intersubjective validity of their signification (i.e. their indicating the same true referents in reality). There are two classical answers. The rationalists hold that the ideas are the same for every human because they are innate (for instance, implanted by God); the empiricists hold that humans share a nature by which they abstract in the same way from sensations to simple ideas and from them to more complex ideas. The former answer lives on in the idea that natural selection has shaped us similarly, so that our innate 'ideas' or 'mental content' are the same (by whatever means supporters of the idea of *mentalese* or *universal grammar* think these innate principles have arisen<sup>377</sup>). The latter position lives on in, for instance, the work of Piaget with his mental constructionist approach to language acquisition.

The mentalists seem to be able to solve some naïve externalist problems relating to words that do not refer to anything in reality (such as 'Sherlock Holmes'), since mentalism allows words to refer to mental associations. However, mentalism creates quite a few problems of its own. These problems have been analyzed by Wittgenstein in great detail, and many of his arguments focus on the pragmatic redundancy of introducing private mental referents (varieties of the private language argument). We will not give an historical exegesis of Wittgenstein, which would be further complicated by the idiosyncratic nature of his writing style, the loosely connected fragments of the *Blue and Brown books* and the *PI*, and the many versions of his fragments in remaining manuscripts.

We simply mention three versions of this problem inspired by Wittgenstein: i) the problem of the comparison of external language and the language of thought, ii) the projection argument and iii) the pragmatic redundancy of mentalistic reference.

**i) The problem of comparison:** how do we compare our words with our mental ideas, or our sentences with our inner thoughts? How do we know that when we encode an inner thought in external words, it was this very thought we were encoding? Of course, there is a difference between what we say and what we think. Somebody can say he is happy, but we can tell from his face and behavior that he feels unhappy; of course, we can sometimes independently test someone's knowledge (or his thoughts if you like) and his use of words, as in the experiments on color perception to test the Sapir-Whorf thesis (§3.12). However, in this case such tests are impossible because the relation between mental thought and the words that encode is a strictly private and internal one. Thus the thesis that we encode mental thoughts as we speak becomes empirically untestable. What remains is phenomenological plausibility. Do not we all feel that we speak internally to ourselves all the time (in the language of thought) and that we vocalize this inner language when we speak to others? Perhaps, but can we really ascertain from our inner phenomenological perception that the inner thought of language and vocal language are two different entities? Make the following experiment: say and mean a sentence, e.g., 'It will probably rain tomorrow.' Now think the same thought again, mean what you just meant, but without saying anything (either aloud or to yourself). If thinking that it will rain tomorrow accompanied saying that it will rain tomorrow,

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<sup>377</sup> This approach was foreseen by Darwin: commenting on the claim that Plato thought our 'necessary ideas' arise from the pre-existence of the soul, Darwin famously wrote in his private notebook M "read monkeys for preexistence."

than just do the first activity and leave out the second. If thinking and speaking stood in the relation of words and the melody of a song, we could leave out the speaking and do the thinking, just as we can sing the tune without the words (Blue and Brown Books, Wittgenstein 1958).<sup>378</sup> When indeed the inner language of thought and its public manifestation cannot be disentangled, as Wittgenstein suggests, mentalism does not give an independent explanation for the meaning of words and only mystifies this with an extra dimension of ideas or mental senses.

**ii) The projection argument:** the projection argument is directed against the mystical character of the *self-application* or *self-luminosity* of ideas or mental senses. It runs as follows (see *PI* 90): presuppose the existence of a notion or concept for which the mentalistic case seems the strongest, such as a cube. The meaning of ‘cube’, according to the mentalistic position, is a mental picture of a cube. This picture of a cube thus has no use as yet. First of all, it needs to be recognized as a picture *of a cube* (as a picture as such, for instance on a blackboard, is only a collection of lines to be interpreted as a three-dimensional image rather than a physical object made of chalk). Moreover, to find out whether this picture fits a certain object in the external world, we need to specify how to read or interpret the picture. In this case of a picture of a spatial object, reading means specifying a projection method (as a picture can be read in infinitely many ways), to see whether it fits the object it pictures. However, this projection method is itself external to the mental picture, because if it were internal to the picture it would require an external projection method of its own, leading to an infinite regress. Hence, unless the proponents of mentalism explain how the projection can be internal to the mental sense (so that the internal regress is avoided), the whole concept of a mental sense seems questionable.

**iii) The pragmatic redundancy of mentalistic reference:** let us consider the case of the velvet monkeys. Velvet monkeys are animals that live on the African savannah, an environment with a great deal of grass and some trees. Their main predators are eagles and leopards. During the course of evolution, they have developed a system of communication to warn each other against these predators. When a velvet monkey sees an eagle, it will make a certain cry; when it sees a leopard, a different one. Upon hearing cries of the first type, all velvet monkeys immediately go under the trees where they are safe from eagles; upon hearing the second, they will immediately climb into the trees where they are safe from leopards.<sup>379</sup>

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<sup>378</sup> There is a parallel argument on the basis of comparison against reference in general, not only against mentalistic (sense) reference. This argument holds that it is impossible to compare words with their referents to see whether our words really cut nature at its joints. Two reasons are adduced in favor of this reasoning: 1) we supposedly have no word-free perspective on the world and thus in fixing a referent, words are somehow already used before their reference is fixed, 2) reference is fixed by social convention, but to reach such a convention (a definition of a planet for instance), we already need to talk and communicate and thus again we need words before reference is fixed. The first argument sees fixing reference as something done by an individual, the second by a community. Both fail to show that words do not have reference, only that we cannot ascertain or fix their exact reference (however, for some this is enough to conclude that reference does not exist, because anything of which we cannot be precisely certain is deemed nonexistent). Both arguments are, however, problematic. The first argument is a variant of linguistic relativism – which was a problematic position, as it clearly seems that we have a pre-linguistic access to reality (pre-verbal children have it, don’t they?); the second resembles the *gagavai* problem, and we saw why this philosophical problem is not a problem in real life.

<sup>379</sup> Seyfarth et al. 1980 also noted a third call, the snake-call; upon hearing this call the monkeys remained in the same position and started gazing at the ground around them for the snake.

A very mentalistically-inclined philosopher or linguist<sup>380</sup> would analyze this situation as follows. The first velvet monkey sees an eagle. This causes an image of an eagle to be projected in the head/mind of this monkey, which he interprets as danger. He decides to run into a tree and to warn the members of his community and uses the first cry. This cry is an arbitrary sound, but the monkeys agreed in the past that they would use this cry to refer to an eagle. The cry is heard by other monkeys; this causes the idea of an eagle to be projected in their head/mind too, and they too interpret this idea as a sign of danger and leap towards the ground.

This picture, based upon the picture of communication of the mentalistic position, seems too complicated to be true in this case. The details of the real communication system of velvet monkeys have been studied extensively by R.M. Seyfarth and his coworkers (Cheney and Seyfarth 1990, Seyfarth et al. 1980). They found that velvet monkeys react to tape-recorded cries from their childhood on, although older velvet monkeys took into account who was crying as they judged if and how fast they would run away. These findings indicate that the system of the velvet monkeys is probably to a large extent innate (i.e., 'fixed by birth'), a system that can be modeled by a simple pre-programmed 4-rule action-reflex system: when you see X, cry A; when you see Y, cry B; when you hear A, climb up; when you hear B, go to the ground, with some training on recognizing X and Y and vocalising A and B. The innateness of these rules in this model, however, does not mean that the velvet monkeys have an internal image of an eagle or a leopard in their minds.<sup>381</sup>

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<sup>380</sup> And they do exist. See Martin 1998: 72, who thinks that the alarm calls represent a 'mental image' that is activated not only by seeing an eagle, but equally by hearing the appropriate alarm call, in the same way that "hearing a word for us evokes a mental image of the word's referent." Martin realizes that the action following the alarm call can also be due to automatic triggering, but he thinks that he can in principle distinguish the case of a mental image from the case of automatic triggering by neurological brain scans, which seems unlikely given how little we know about the brain and the impossibility of comparing the brain and the mind directly. These brain scans are yet to be done.

Zuberbühler nevertheless tries to distinguish automatic triggering from other processes with his research on Diana monkeys, which react not only to their own alarm calls, but also to those of other species (notably guinea fowl which make the same alarm call for humans and leopards) (Zuberbühler 2000a), chimpanzees which make a leopard call but also eat Diana monkeys (id., 2000b) and Campbell monkeys which make distinct eagle and a leopard calls (id., 2000c). Interestingly, when Diana monkeys hear the alarm call of the guinea fowl they react as if a leopard were present, unless they already have independent reason to suspect that humans were in the vicinity, in which case their reaction is less alarmed. Furthermore, when a Diana monkey hears cries from a Campbell monkey, they react accordingly, just like the velvet monkeys; however, if an alarm call is followed after a brief time interval by playback of predator vocalizations, their reaction differs depending on whether the vocalization matches the alarm call or not. Finally, if a Diana monkey sees a chimpanzee, it hides; if it hears the chimp's leopard alarm call it takes the appropriate action. What should Diana monkeys do when they hear a chimp making the leopard call within earshot? Should they hide from the chimp or take the leopard action? Zuberbühler found that a population heavily hunted by chimps choose strategy 1, whereas another population that had experienced this to a lesser extent choose strategy 2. All this seems to suggest that *more is going on than just simple automatic triggering by vocal signals*, and that non-trivial cognitive processes are involved. However, these more complex reactions are also trainable and do not require a *mental image* to be explained.

<sup>381</sup> One can even argue that the signs of the velvet monkeys are without true reference. The velvet calls need not be interpreted as referring to eagles or leopards (which in turn imply a certain course of action). For their function, it suffices to interpret them (Occam's razor) as *orders* to climb up or climb down. Yet due to these order words, the world of the velvet monkey becomes *articulated* in two spheres: the world of the eagle and the world of the leopard. It is *as if* the signs are about eagles and lions. To distinguish the 'reference' of velvet monkeys from ours, Seyfarth et al. speak of *functional reference* compared to true reference.

Note that we don't have a problem interpreting the sounds of animals in humanese (the language of humans); when a velvet monkey cries at the approach of a lion, we take it as the (functional referential) signal 'lion!' or 'run away!' We don't think the problem of interpreting animal language is any

Just like a thermostat does not have an internal ‘mental’ image of the temperature outside world, so a velvet monkey needs no mental ‘image’ of eagles or leopards to react appropriately: such images are from a pragmatic standpoint redundant (i.e., such images might exist, but they are not necessary for the communication to function, hence (Occam’s razor) we argue that they do not (necessarily) exist).

The largely innate action-reflex system of the velvet monkeys is of course not a good model for human sign use, which seems free and creative. Nevertheless, some elements of human language use can be modeled analogously to the system of the velvet cries, and thus also would not require mental word senses. Take the famous language game of §2 of the *Philosophical Investigations*, where one person has to bring a slab to another upon hearing him shouting ‘slab!’

“We could imagine that the language of §2 was the whole language of A and B; even the whole language of a tribe. The children are brought up to perform these actions, to use these words as they do so, and to react in this way to the words of others. An important part of the training will consist in the teacher’s pointing to the objects, directing the child’s attention to them, and at the same time uttering a word; for instance, the word ‘slab’ as he points to that shape.

(I do not want to call this ‘ostensive definition’, because the child cannot as yet ask what the name is. I will call it ‘ostensive teaching of words’. – I say that it will form an important part of the training, because it is so with human beings; not because it could not be imagined otherwise.)

This ostensive teaching of words can be said to establish an association between the word and the thing. But what does this mean? Well, it can mean various things: but one very likely thinks first of all that a picture of the object comes before the child’s mind when it hears the word. But now, if this does happen--is it the purpose of the word?

– Yes, it can be the purpose. – I can imagine such a use of words (of series of sounds). (Uttering a word is like striking a note on the keyboard of the imagination.) But in the language of §2 it is not the purpose of the words to evoke images. (...)

But if the ostensive teaching has this effect, – am I to say that it effects an understanding of the word? Do not you understand the call “Slab!” if you act upon it in such-and-such a way? – Doubtless the ostensive teaching helped to bring this about; but only together with a particular training. With different training the same ostensive teaching of these words would have effected a quite different understanding.

“I set the brake up by connecting up rod and lever.” – Yes, given the whole of the rest of the mechanism. Only in conjunction with that is it a brake-lever, and separated from its support it is not even a lever; it may be anything, or nothing. (PI 6)”

The velvet monkeys do not need the idea of an eagle or a panther as long as they go up or down the trees upon hearing the right signals, just as A & B do not need the idea of a ‘slab’ as long as they obey the commands. During the training, the teacher does not try to elicit the idea of a ‘slab’ in the mind of the pupil, he tries to *direct* him

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different from interpreting that of human tribes whose language is completely unknown to us. This completely unknown language poses problems of radical translation similar to those involved in interpreting animal language. We can interpret unknown human language (think of adventurers or missionaries), because we humans know intuitively what other humans want and pursue. (We share a common life-form even though cultures can be starkly divergent). The same holds true for interpreting the utterances of animals: we can interpret mammal signals since we do know what mammals want and pursue. (We share a common life-form even though the actual things other mammals do can be very different.) Of course, in this act of radical translation of animal language one should take many precautions, as the signs of mammals in general are more difficult to interpret than that of unknown tribes. One should make proposals about their meanings as simple as possible. It is typical for our human languages that we make distinctions between subjects and predicates, and between imperatives and descriptive sentences, but we are not likely to find things like this in animal language. Acts of radical translation are also less problematic in human language because human language is so rich that the contrasts between different words are subtler. Animals often have such a small repertoire of sounds or language-like elements that proposals about the meanings of their signals will be rather vague.

towards the slab, to *let him bring* it. This teaching is not ostensive, since ostensive teaching is part of a different language game in which the pupil already knows a lot of language and can ask the name of something himself. Ostensive teaching thus already presupposes a different training in which many words have already been taught differently and in which the gesture of pointing and naming has repeatedly been used to identify something. In this more primitive language game, the words have a very different function within ‘the whole mechanism’ of calling for slabs, bringing slabs, taking them and asking for new slabs.

This primitive mechanism doesn’t need mental senses, but how far can the use of human language be extended without mental senses? Can mental senses only be forgotten in the case of simple cries that have only one function? How can we use representational signs that can reoccur in many new combinations? The first 100 paragraphs or so of the *PI* try to show that analysis of the language games without mental sense can be extended quite far, even in the direction of uses that would normally be seen as representational. Moreover, Wittgenstein keeps arguing against the problems of (mental) representationalism as the explanation of how language use works.

Nevertheless, a gap seems to remain in Wittgenstein’s analytical move from simple language games in which words function in *one unique way* (often as orders), to situations in which one word is freely used as a ‘building block’ in indefinitely many sentences. Many scholars claim that in animal language, signs are used like orders in which each sign is used only in one way, but that the threshold to human language *is* compositionality. This implies that one sign is used in many ways (feature 11 of §3.11). Some scholars elucidate this distinction with the Peircian tri-partition of signs (icon, index and symbols), claiming that animals signs can be iconic (i.e. similar to what they represent, like a butterfly with the eye of a predator on its wing) and indexical (i.e. causally related to what they are a sign of, like rabbit tracks to passing rabbits or like the signs in the causally evolved reflex-action system of the velvet monkeys), but that only human language is symbolic: it comprises *arbitrary representations within free combinations* (Hurford 2007).<sup>382</sup> Deacon even seeks to buttress this tri-partition with three hierarchical neurological layers of sign interpretation in the brain (Deacon 1997). We will return to this issue when we discuss animal language in more detail (§3.23). For now, we need to remind ourselves not to analyze animal language too hastily from the perspective of mentalism.

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<sup>382</sup> Or in Peirce’s own words: icons are based upon “a mere community in some quality (Works 1936: 2.56).” Indices are those signs “whose relation to their objects consists in a correspondence in fact (id.)” And finally, symbols are those “whose relation to their objects is an imputed character (id.)” Peirce originally developed his tripartition of signs to single out the symbols that are to be used in logic. Icon and index were only of secondary importance to him. Peirce originally only considered examples of human sign use; his early examples were portraits and the similarities between the letters ‘p’ and ‘b’ (icons), the weathercock and the relation between a murderer and his victim (indices) and the words “homme” and “man” sharing a reference (symbols). Although the tripartition plays a role from the beginning (the concise account of 1867-8 comprising the famous three signs) to the middle (10 sign 1903 account) till the end of his work (his speculative unfinished system of 66 signs), Peirce himself came to doubt the strictness of the distinction of 1867-68, the hierarchical nature of the division of 1903 (with a symbol also having an iconic and indexical side, an index also having an iconic side and an icon being just iconic). He held that an icon and an index also have a symbolic, arbitrary side (a portrait doesn’t just resemble someone, and only under a certain interpretation and history of iconography can this resemblance be seen; equivalently, every index requires an interpretation too). See also Short 2004 for the development of Peirce’s work. The fact that the founder of the tripartition found the distinction wanting casts doubt on any neurological story that tries to find an exact instantiation of the tripartition in the neurological wiring of the brain (which can only be speculative since as we know so little about it).

## b) Cooperation and language use

One can distinguish two main positions in the philosophy of language of what language is. First, portrayals of language as a system for storage of information; second, portrayals of language as a means to communicate and get things done. The first tradition is the dominant one in philosophy, but the second is a strong undercurrent. Grice, one of the founders of modern linguistic pragmatism, belongs to the second tradition. Pragmatism or pragmatics is the study of the ability of natural language speakers to communicate more than they explicitly state.<sup>383</sup> Grice's work arose as a reaction to linguistic structuralism by rejecting the idea that all meaning comes from signs existing purely in the abstract space of *langue* and the inner-linguistic relations of these signs to each other. Grice stressed the *extra-linguistic* context of use in explaining the meaning of utterances, and he introduced the notion of *conversational implicature*<sup>384</sup> in order to describe certain inferences from the context of use to the meaning of an utterance. Take the utterance "Mary became pregnant and got married," which strongly *suggests* that Mary became pregnant before the wedding. But the sentence would still be *strictly true* (from a logical point of view), if Mary became pregnant *after* she got married.<sup>385</sup> Another example is the sentence "We are out of fuel" uttered when near a gas station; it is not strictly true (as otherwise the car would no longer drive), but it implies "take the exit and fill the tank."

Grice distinguished natural from non-natural meaning (1957). *Natural meaning* is supposed to capture something similar to the relation between cause and effect as, for example, in "Those spots mean measles" and "That Michael is angry means that he takes the accusation seriously." This must be distinguished from what Grice calls *non-natural* meaning, as in "Those three rings on the bell (of the bus) mean that the bus is full," which is an arbitrary meaning based on a convention, that tries to communicate the intention of the person who broadcasts the signal – in this case the intention of the bus driver to signal that the bus is full.<sup>386</sup> The distinction between natural and non-natural meanings is one between signs that cannot be used *intentionally* and those that can.

An important category of non-natural meanings deals with linguistic phenomena. Grice discusses *speaker meaning* and *linguistic meaning*, whereby he

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<sup>383</sup> The field of pragmatics is hard to define (Mey 2001: Introduction) as can also be seen from the above characterization as the study 'of the ability of natural language speakers to communicate more than that which is explicitly stated.' This characterization is negatively dependent on a good definition of 'literal meaning' or of 'that what is explicitly stated'. The discussion of the other positions in the debate of meaning (which are concerned with literal meaning) shows that such a definition is hard to give. The extra problem of definition in the field of pragmatics is to make the notion of the deviation from literal meaning itself (i.e. 'the *more* than that which is explicitly stated') less intuitive and more explicit. Pragmatics usually takes a pragmatic approach to the definition of its own field by giving a few examples of what the field is about and which problems it deals with, without clearly demarcating its purview.

Important contributions to this field (apart from Grice and Austin's) are Brown and Levinson's 1987 analysis of politeness that says we generally act to save face, and Sperber and Wilson's (2004, 1995, 1987) extension of Grice's principle of conversational relevance.

<sup>384</sup> Grice used the word "implicature" instead of "implication" or "entailment," because these words had a strong connotation of logical deductions. That was exactly what Grice, with his focus on extra-linguistic content, tried to avoid.

<sup>385</sup> In temporal logic, a sub-branch of modal logic, people try to model this sentence from a strictly logical point of view by introducing temporal indices to the different propositions the sentence comprises (i.e. Mary got pregnant at  $t_1$  and Mary got married at  $t_2$ ); in that case the implicatures transform back into clear logical implications. Other conversational implicatures can probably also be reformulated using modal logic, but this comes at the cost of introducing more and more specific logical tools (because many sentences require their own logical analysis), which damages the supposed generality of logic.

<sup>386</sup> Compare Peirce's distinction between an index and a symbol in footnote 382.

analyzes the latter in terms of the former. According to Grice's initial suggestion, "A person 'A' meant something by utterance 'x' " is roughly equivalent to "A person 'A' uttered 'x' in context 'C' with the intention of causing effect 'e' by inducing a belief in person 'B' through the recognition of this intention".<sup>387</sup> This implies that A has, as it is now called, a theory of mind: A assumes that B has a mind just like his own, which is able to interpret the utterance 'x', i.e. A has the ability to attribute mental states – beliefs, intents, desires, knowledge – to himself and others, understanding that others have beliefs, desires and intentions possibly different from his own.

In order to explain how speaker meanings can be understood (which is a hard problem, since the meanings are not fixed by the words alone and can depend on many elements in the context of utterance), Grice postulated the existence of a *general cooperative principle* in conversation, as well as certain special maxims of conversation derived from this principle of cooperation. The general cooperative principle, as the name indicates, is the presupposition that speakers, at least in general, do not try to fool each other when they communicate. Grice's examples of the (partly overlapping) maxims are 1) the maxim of quality (do not say that which you believe to be false or that for which you lack adequate evidence), 2) the maxim of quantity (make your contribution as informative as is required for the current purposes of the exchange, and do not add more information than required), 3) the maxim of relation (give only relevant information) and 4) the maxim of clarity (avoid obscurity and ambiguity). These maxims make the interpretation of a speaker's meaning by the hearer a much easier task.

Price's position has many advantages. It can include many types of language use, pays systematic attention to the context and thus includes extra-linguistic factors in the interpretation of meaning. An important problem, however, is that even with the principles of cooperation it seems hard to *interpret* speaker meaning from linguistic meaning. Think of someone who believes that Fred is called Michael. This person utters the sentence "Michael is home" which linguistically just means "that Michael is home," but which was uttered with the intention of visiting Michael. How can the hearer understand that the speaker proposes to go to Fred? One solution is to assume a principle of charity on behalf of the hearer as well<sup>388</sup> (which parallels the speaker's principle of cooperation); the hearer knows what the speaker means because he knows what the plan is (to go and have a drink, which is a logical thing to do with Fred because he likes to drink) or because the speaker often confuses Fred's name with Michael. However, this doesn't really solve the problem. Even with the principle of charity, the meaning is not fixed (a hearer can interpret an utterance as such in as many ways as possible). The principle of charity only works when there exist general contexts of *socialization* due to which certain exchanges of linguistic utterances become regular: the speaker *often* confuses Fred and Michael, the speaker and the hearer *often* go to have a drink (with Fred). However, due to socialization, communication can lose some of its unique personal intentionality:<sup>389</sup> the exchange

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<sup>387</sup> This formulation is a contraction of the three conditions Grice 1957 develops to analyze speaker meaning in more detail. In his later work he refines his definition even further.

<sup>388</sup> Price puts emphasis on the meaning for the speaker instead of the meaning for the hearer. A Darwinian theory presupposes that historically signals were received before they were intentionally sent, putting more emphasis on the meaning for the hearer.

<sup>389</sup> Price's set-up is sometimes said to satisfy third level intentionality: I want (1) that you understand (2) that I want (3) to go to a bar and that you accompany me or more formally: "I intent that you intent what I intent with X" (Grice 1957). With semi-automatic triggering this third level intentionality can be argued



can now be seen as partly automatic, context-based use requiring little interpretation such as saying ‘hello’ or ‘how do you do’.

Related to this problem is the fact that the pragmatic theory of implicature requires for almost every case of language use a separate analysis, because the context can influence the meaning of linguistic utterances in indefinitely many ways. However, it can ultimately lead to pessimism about the possibility of a satisfactory theory of meaning (and indeed, pragmatic theories of meaning are known for their lack of theory). Solutions to this problem of almost indefinite possibilities of interpretation are the Gricean maxims,<sup>390</sup> theories of socialization or habitualization of the utterance of certain linguistic phrases in certain context. But then, the position of Grice (which places so much emphasis on intentionality) would degrade into the direction of behaviorism, with all its concomitant problems.

Two interesting challenges for integrating Grice’s work into a Darwinian framework are i) his emphasis on intentionality, as there is no good Darwinian account of consciousness or intentionality (and if one existed, it would probably seriously transform our intuitive notion of intentionality, see c) on the relation of thought and language) and ii) his emphasis on general cooperation which, an altruistic principle, requires within the Darwinian framework an analysis of *why* people benefit from cooperation and what its costs are. In chapter 4 we will see that some theories about the origin of language consider language evolution as a run-away process of cheating and detection, with lying and deceiving as primary forms of language use. However, there are many more Darwinian theories that see a role for social cooperation in the origin of language (hunting, education). These would clearly be compatible with Darwin.

In sum: pragmatism is a valuable theory for explaining many types of language use; the theory gives a central role to intentionality, which is relatively hard to integrate into a Darwinian framework.

### **c) The relation of thought and language**

One of the central questions in the philosophy of language is the nature of the relation of ‘language’ and ‘thought’. We have already touched upon some elements of this relation in our discussion of mentalism. One of the key questions about this relation regards

the primacy<sup>391</sup> of one over the other. Ancient and early modern philosophers usually opted for the primacy of thought, but modern philosophers since the linguistic turn in the 20<sup>th</sup> century often opted for the primacy of language.<sup>392</sup> In the intermediate

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to be reduced to first level intentionality, as the remark ‘beer now!’ can immediately lead us to go to a bar if we habitually go there.

<sup>390</sup> The Gricean maxims are, like all maxims, *normative*. If one holds that science should not prescribe how we use language, one can replace these with empirical studies of what strategies actual speakers employ to get their messages across.

<sup>391</sup> Questions of primacy are notoriously hard and ambiguous when what is meant by primacy is left undefined. “A is prior to B” can mean at least the following (with A and B statements, objects or events depending on the example): i) B can be derived logically from A (usually given a fixed set of other axioms) but not the other way round, i.e. logical primacy, ii) A is needed to understand B (or A is learned sooner than B) but not the other way round, i.e. some form of epistemological primacy, iii) A can exist without B or B can be a consequence of A but not the other way round, i.e. ontological primacy, iv) A can exist or happen earlier than B, i.e. temporal or chronological primacy. Regarding our question of primacy, we will only concentrate on iii) and iv) and will always try to make clear with what question we are dealing.

<sup>392</sup> The linguistic turn is not a homogeneous movement. Although it arguably originates in the philosophy of Johann Georg Hamann and Wilhelm von Humboldt, who stressed that language is not a transparent medium for thought as many earlier philosophers had supposed (an opinion also present in the work of the

period of the 18<sup>th</sup> and 19<sup>th</sup> centuries, language and thought were often thought to be intimately related (see Wells 1987).

Questions regarding the primacy of language and thought in the context of the origin of language were probably discussed for the first time by Rousseau (1755) in his criticism of Condillac's empiricist approach to the origin of language (1746). It appears to Rousseau that language requires non-negligible powers of thought before it can get off the ground, but that thought can only be rather primitive without language. This chicken-and-egg problem is further taken up by Herder (1784) and Müller (1866), but remained more or less forgotten (in this context) for over a century as questions of the origin of language were left almost undiscussed for more than a century since Müller's lectures (see § 4.1). Its study was only revived in the heyday of the linguistic turn, in which the question of primacy sometimes, especially on the Continent, seemed resolved in favor of language. This would ultimately mean

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philology-trained philosopher Nietzsche), its origin is usually traced to the work of Wittgenstein, especially by Anglo-Saxon philosophers (see for instance the popular anthology edited by Rorty: *The Linguistic Turn. Essays in Philosophical Method* of 1967).

Wittgenstein held in his earlier *Tractatus* that philosophical problems arise from a misunderstanding of the logic of language that had to be relieved by finding a clearer logic that could be used to improve everyday language. His later work in the *Philosophical Investigations* plainly departs from this, but in this work he also relates philosophical problems to the misuse of language, further arguing that their solution had to be envisioned quite literally as a solution (as 'dissolving'): philosophical problems are 'solved' not because an answer is found, but because the question has turned out to be a meaningless non-problem. The *Philosophical Investigations* gave rise to a whole school called *Ordinary Language Philosophy* with prominent proponents such as Gilbert Ryle, J.L. Austin, Peter Strawson and Norman Malcolm, whose approaches typically involved eschewing philosophical 'theories' in favor of close attention to the details of the use of everyday, 'ordinary' language. Sometimes these approaches turned out to be similar to the earlier approach dominant in analytic philosophy, now sometimes called *ideal language philosophy*: by analyzing the use of words (for instance the verb 'to know'), philosophers thought they had solved, instead of dissolved, a philosophical problem (for instance, the problem of what knowledge really is).

On the continent, the linguistic turn is usually associated with another movement: the humanities of the 1970s that started recognizing the importance of language as a structuring agent of (social) reality (known from the claim that 'everything is text'). Crucial to this movement were the works of the philosophical tradition of post-structuralism, with influential theorists such as Jacques Derrida, Michel Foucault, Gilles Deleuze, Jean-François Lyotard, Judith Butler, Luce Irigaray, Julia Kristeva (as well as the later work of Roland Barthes, Jacques Lacan etc.). While post-structuralism is difficult to define or summarize, it can be broadly understood as a body of distinct reactions to structuralism as founded in linguistics by Ferdinand de Saussure and in anthropology by Claude Lévi-Strauss. De Saussure distinguished the signifier and the signified as two sides of *one* sign, whereby the signifier refers to the material, phonological structure of a word and the signified to its mental denotation or meaning (a 'sense') within a *synchronic language system (langue)*, although often the relation between the signifier and the signified is pictured as a relation of a spoken speech sound (*parole*) to a thing in the world (a 'referent'), i.e., the traditional world-thing relation that De Saussure sought to replace with his own distinction. Lévi-Strauss held that the symbolic systems of (primitive) tribes are structured by dichotomies and that the social life of these tribes is structured accordingly. Post-structuralists criticized both the linguistic and the anthropological version of structuralism. Specifically, post-structuralism holds that the *study* of underlying structures is itself culturally and historically conditioned and therefore subject to myriad biases and misinterpretations. Post-structuralism further emphasizes the historical nature of all symbolic systems and thus criticizes any ahistoric essentialism, be it the fixed 'signifieds' of De Saussure's linguistic system or the fixed dualism of Lévi-Strauss's anthropology.

Two other varieties of the linguistic turn are i) linguistic relativism (see §3.12) and ii) semantic holism, a doctrine to the effect that a certain part of language, be it a term or a complete sentence, can only be understood through its relations to a (previously understood) larger segment of language (often understood to be the whole language). Quine's defense of it in the context of the problem of radical translation is well-known (Quine 1960). Because individual words don't (directly) refer to things in reality and their meaning is dependent on the meaning of all other words in the language, language is seen as a system whose contact points with reality are vague. This is analogous, but not identical to, the underdeterminedness of scientific theories by empirical results held by the Quine-Duhem thesis, see §1.33.

(ontologically prior) that there (could) exist people who can use language but not think, or (temporally prior) that there was a time when people spoke a language but did not yet think. And there is someone who actually defended a position that comes close to this: Julian Jaynes (1976) postulates that there existed non-conscious people as late as the Bronze Age who were perfectly capable of using language.<sup>393</sup>

Currently, the importance of the linguistic turn has faded somewhat. We still find people i) defending language as a prerequisite for (conscious) thought (Bickerton 1995, Spangle and Manzel 1991), ii) while others defend the primacy of thought or consciousness/self-awareness and intentionality for true human language use (Sinha 2004). Unsurprisingly, we also find people defending the view that the evolution of bigger brains was due to selection on language abilities, while others believe that selection on bigger brains caused or allowed language to evolve (see §4.3). We look at a few modern proposals about the relation of thought and language in some more detail, from which it will become clear that the way forward is to observe which *elements* of thought require or are dependent on which *elements* of language (use) and vice versa, thus no longer regarding thought and language as *homogeneous wholes*.

i) Peter Carruthers (2002) distinguishes four forms of the thesis that language determines thought:

- Strong forms:

a) language is conceptually necessary for thought

b) language is de facto used for all conceptual thought

- Weak forms:

c) language is necessary for concept *acquisition* (and acquisition only)

d) language serves as a scaffold for thought

But this classification scheme, whose classification criteria are not completely clear to us, is not exhaustive. Other forms of the thesis that language determines thought are still possible – such as, for instance, the thesis that language is necessary for having *beliefs* and other *propositional* attitudes that are so typical of our mind, as Fellows holds (Fellows 2000). A glance at the literature, especially in historical perspective, will reveal other examples not covered by Carruthers's scheme. Even Carruthers's own model of meta-cognition cannot be categorized under one of the four forms he distinguishes, as he presupposes that language is needed principally only for general non-domain specific thinking and that language bridges the different domain-specific modules in the mind.<sup>394</sup>

ii) But there are also strong critics of the idea that language is crucial for (elements of) thought, such as Zlatev 2001 (as quoted in Johansson 2005: 144): “language can *not* be the major cause of (self-)consciousness as is claimed by numerous contemporary theorists, since its acquisition presupposes (a degree of) intersubjectivity, which presupposes consciousness.”

Unfortunately, many of the above suggestions are still rather vague. Ideally, we would have an (independent) evolutionary account of thought (and the related phenomena of consciousness, self-consciousness, intentionality, belief), which we

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<sup>393</sup> But note that thought and self-consciousness are not the same, although for Jaynes the hallmark of true thought is self-consciousness. The Ancient Greeks were the first to achieve that, according to Jaynes.

<sup>394</sup> Carruthers's model clearly fits within the tradition of evolutionary psychology (with its many modules) and of computationalism (with mental and cognitive states analyzed as strings of symbols upon which computations are performed).

could use to observe how their evolution is related to the evolution of language.<sup>395</sup> But unfortunately, though some try to take a few steps in this direction (Donald 2005, 2001, 1991, Dennett 2005, 1992, Gärdenfors 2003), such an account does not exist yet – is not likely to come soon, as there is still great conceptual confusion and discussion about what exactly an evolutionary explanation of consciousness (or any *naturalistic* approach to consciousness) should explain. Think only of the notoriously difficult “hard problem of consciousness” (Shear 1999): the problem of the qualitative nature of consciousness or what it is like to be a being with a mind and to experience so-called qualia – a problem not only hard to conceptualize, but almost unsolvable within a naturalistic frame-work unless one bluntly denies the true existence of these phenomena in one way or another.

Here, we simply presuppose the existence of consciousness and thought and bracket questions about their evolution.<sup>396</sup> If the evolution of language and consciousness were inextricably intertwined in a co-evolutionary process, our discussion of the origin of language would be seriously limited. However, there are good reasons to suppose that thought evolved independently of language and that language evolution is dependent on it instead of the other way round. We will first argue from empirical results that thought as such is not dependent on language, and then briefly point to five elements of thought that are important for language learning and language use: memory, intentions, consciousness, self-awareness and the so-called ‘theory of mind’ (including the understanding of ‘false beliefs’). We ultimately conclude that many elements of the mind are either prerequisite or helpful for language acquisition, use and understanding.

The relative independence of thought from language:

There are quite a few observations that support some independence of thought from language:

- primates (and other animals) lack language, but few would consider them altogether mindless as does Descartes, or even deny that they have higher mental faculties such as consciousness and self-awareness (see below). Research on animal minds is plagued by the same problem as that on animal culture or animal language. Some researchers either easily attribute too much consciousness to animals, while others demand extremely rigorous tests that will hardly ever be satisfied. The best thing to do is to sail between

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<sup>395</sup> Of course if one believes in a strong version of entanglement between the evolution of thought and language, then such an evolutionary account is impossible to give.

<sup>396</sup> For naturalists, there is no other option than to presuppose that thought has evolved by natural selection. They find in the evolutionary tree animals or organisms without a mind and other, later evolved creatures with a mind (though it is uninteresting whether they attribute a full mind to all mammals, to all monkeys and apes, to all primates, to chimpanzees and bonobos and humans, to human ancestors such as *Homo ergastus* or *Homo sapiens* or only to the modern *Homo sapiens*; the fundamental problem remains the same). Since the only tool available to a naturalist to explain the origin of a (natural, biological) phenomenon is Darwinism, be it by explaining it as an adaptation, exaptation or spandrel, an explanation of the transition from mindless creatures to creatures with a mind has to be an evolutionary one. The discussion whether consciousness can be explained darwinistically is, however, complicated by the fact that there is no consensus on what consciousness is: an epi-phenomenon, a hard to describe feeling or sensation of being there, the ability to observe oneself etc. More or less broadly shared consensus on what consciousness is a precondition for a successful Darwinian theory of its origin. It is unclear whether such a consensus can or will be reached as current conceptions of consciousness differ substantially.

Note that we also bracket here the speculative philosophical question of whether other people truly have minds or whether we can ever completely be sure of this, and simply presume that other people have minds like us.

this Scylla and Charybdis by using the same standards we employ to attribute a mind to humans (other tribes whose language we do not speak, children, handicapped people) as to animals.<sup>397</sup> From that perspective, animals certainly have at least some mental faculties.

- pre-verbal children are not considered to be mindless and thoughtless (even though their mental life will be much less complex than that of grown-ups), see Hesperos and Spelke (2004), Clark (2004), Mandler (2004).
- aphasia patients too, even when suffering from strong agrammatic aphasia, are not considered to be mindless and thoughtless (even though again there may be some damage to the complexities of their mental life as well),<sup>398</sup> see Carruthers (2002), Bloom (2000). Hence the mind does not require language, but language use does not require full mental skills: seriously retarded Williams Syndrome patients often have language skills, although current research also indicates some language defects (see §3.21).
- people (introspectively) report that they think in images, schemes and numbers at least for some activities. Although some may argue that these modes of thought are still *symbolic*, it clearly shows that thought is not necessarily *verbal* or even *linguistic*.<sup>399</sup>

These examples show the independence of (some elements of) thought from language, even though we do not exclude the *possibility* that human mental life is seriously restructured by the existence of verbal language (a possibility we will not discuss further); we now turn to a more detailed analysis of what elements of thought are needed for language.

The dependence of language use on (earlier evolved) elements of thought:

The mind or human thought is composed of many, possibly interdependent elements, four of which we discuss here in relation to language and its evolution. If an element of thought already occurs in other species that do not use language, it is clear that it did not evolve for or was due to language use specifically, although it may have become exapted for language use.

**i) Memory** (and conscious access to it): it is clear that language learning and use requires memory and quick access to it: we use thousands of words and can form sentences in milliseconds. Being consciously aware of memories facilitates language learning (but not necessarily, because there are forms of learning on the basis of negative memories – such as avoiding fire after you have burnt yourself as a child – whereby we do not need to be consciously aware of our memories). Especially for correcting yourself, for instance when you correct your own text, such conscious access to memory is useful. It is clear that animals have memory too. Whether they have *conscious access to memory* is less clear. A clever experiment has been devised to

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<sup>397</sup> See also Boesch 2001: 526 “Isn’t it far more economical to assume that if two closely related species act in a similar way, the underlying mental process is also similar? If wolves and coyotes were being compared, there would be immediate agreement about that. Why should we adopt another logic when comparing chimpanzees and humans?”

<sup>398</sup> One may question this, arguing that the mind in these patients did initially ontogenetically develop or phylogenetically evolve because of or as a byproduct of language use, but that although language use is now impaired, the mind still functions. However, this would still prove that thought can *exist* without language.

<sup>399</sup> Some scientists will find this argument unconvincing, as it relies on the possibly unreliable method of believing introspective reports. However, most of the intuitive plausibility for a relation between thought and language also comes from introspective reports that we sometimes feel ourselves thinking in language.

test this,<sup>400</sup> but the subject remains controversial. If conscious access were to be demonstrated in non-human species, it would be clear that memory did not evolve for or was due to language use specifically, although it may have become exapted for language use.

**ii) Intentions:** some cases of language use may be almost automatic, such as the automated stimulus-reflex mechanisms in the language of some monkeys (think of greeting someone). However, as we discussed Grice's pragmatism we had to conclude that many forms of language use cannot be understood unless one takes the speaker's intentions into account. Animal behavior is usually explained without intentions, or at least with as few intentions as possible. It is treated as the result of genetic programming (and environmental constraints), or as the result of direct parental instruction (bird song) with no need for intentions, or as the result of (earlier) operant conditioning. Animal language (alarm calls) is also often treated as an automated-reflex system, without clear conscious communicative intent on behalf of the sender. We saw that some monkeys, like the Diana monkeys, adapt their call according to their circumstances, but again this need not be a sign of free, creative communicative intent; it can also be understood according to a more complex genetic architecture of rules. There exists, however, a good deal of anecdotal evidence that chimps (in the wild) convey information that could not have been indicated by situational cues and that cannot easily be understood from automated response systems (De Waal 2000, 1989, 1982). The experiment we discussed earlier (§3.12) of the chimps Sherman and Austin who choose a sign for a new food item with each ascertaining that the other understood the referent of the symbol seems to show that pre-verbal (although this time not pre-linguistic) animals have clear intentions (Savage-Rumbaugh and Lewin 1994). For another thoughtful argument that animal behavior displays signs of intentionality, see Kornblith (2002). Again, if intentions were to be demonstrated in non-human species, it would be clear that they did not evolve for or due to language use specifically, although they may have become exapted for language use.

**iii) Theory of mind:** the concept of a 'theory of mind' was coined in the context of ethological research (Premack and Woodruff 1978) to mean, roughly speaking, the ability to understand that other people or beings have minds too, including own mental 'states' such as beliefs, desires, feelings etc. (note that 'theory' in the phrase 'theory of mind' does not mean something like 'an explicit coherent set of laws and rules for conducting experiments', but only something like 'an implicit understanding of something', in this case: other beings' beliefs, desires and feelings). The absence of this ability is linked to autism (for instance Baron-Cohen et al. 2000, 1985), because autistic children fail theory of mind tests but not other complex cognitive tasks (though other explanations of autism and the lack of a theory of mind in autistic children remain tenable). The theory of mind is currently widely researched, with two standard paradigms: i) the theory which claims that people use a veritable method called folk psychology to reason about others and attribute mental states to

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<sup>400</sup> Hampton 2001 devised an experiment in which rhesus monkeys were faced with the task of remembering which set of test pictures they had seen before – but with the opportunity to refrain from taking the test. Incentives were arranged so that the wise course was to take the test when they remembered the set of pictures or refrain from it otherwise. In this case, the rational choice is only possible for beings which are aware of their memories. Hampton found that the monkeys did choose rationally, and thus inferred that the monkeys were consciously aware of their memories.

them to explain their behavior, ii) the simulation theory, which claims that people simulate ‘standing in someone’s shoes’. Given the current popularity of evolutionary psychology, some researchers postulate a neat ‘theory of mind’-module which they try to locate somewhere in the brain, although many criticize such attempts (Gerrans 2002) – in part because it has become more and more clear that there is no singular theory of mind ability that one can possess or lack. Instead, there are degrees of mind, with children developing a theory of mind through several stages at roughly predictable ages: from simple eye contact in the first weeks, to joint attention, gaze following and proto-declarative pointing (pointing in order to draw someone’s attention to an object) in the first year, an understanding of the wishes of others a little later (including an understanding of the distinction between animate and inanimate objects) and finally an understanding that someone may have a false belief, the recognition that others can have beliefs about the world that are wrong, from age five onward or so (see below) (Tomasello 2003).<sup>401</sup> From our description of child language acquisition in §3.24 it is clear that these elements are extremely important in learning a language. Since intentions are so important in language use (see above), understanding the intentions of others is relevant in language acquisition, understanding and use (Tomasello 1999). However, some autistic children, supposedly lacking a full theory of mind, are nonetheless able to learn language and use it more or less normally (Glüer and Pagin 2003). Moreover, in tests with autistic children whose verbal mental age was the same or higher than that of normal children (if tested by naming and description tasks, thus matching for verbal skills), the difficulties with a theory of mind tasks remained for the autistic children (Baron-Cohen et al. 2000); thus the link between a theory of mind and verbal skills is not complete in either direction.<sup>402</sup> It is also interesting that children deprived of social interaction have delays in the development of the theory of mind similar to those of autistic children, just as non- or under-exposure to language (as happens with hearing children who have deaf parents) leads to delays in language development, though this similarity need not have a common basis or background. Finally, ever since the publication of Premack and Woodruff’s 1978 paper (in which the notion was first postulated), the question of whether other animals have a ‘theory of mind’ too, has been hotly disputed. This is mainly because higher and higher standards are demanded to exclude any non-mind interpretation (see the 20<sup>th</sup> anniversary review of Heyes 1998); this might even be impossible if the standards are raised too high (higher than for the attribution of a mind to humans), because – from a philosophical

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<sup>401</sup> The most famous test for having a theory of mind is the Sally-Anne task: the child is shown two dolls, Sally and Anne, playing with a marble. The dolls put away the marble in a box, and then Sally leaves. Anne takes the marble out and plays with it again, and after she is done, puts it away in a different box. Sally returns and the child is then asked where Sally will look for the marble. The child passes the theory of mind task (this time a false belief task) if she answers that Sally will look in the first box where she put the marble. The child fails the task if she answers that Sally will look in the second box, where the child knows the marble is hidden, even though Sally cannot know, since she did not see it hidden there. In order to pass the task, the child must be able to understand that another’s mental representation of the situation is different from his or her own, and the child must be able to predict behavior based on that understanding. This test has been modified in numerous ways to ensure that the results really test for a theory of mind instead of verbal understanding, cognitively demanding tasks or the willingness to enact in pretense play by using human actors (as it is known that autistic children rarely engage in pretense play).

<sup>402</sup> Bloom 2000 further reports an aphasic patient with a full theory of mind but a complete lack of understanding and production of syntax in speech, further decoupling the theory of mind from language skills.

point of view – we never have direct access to another’s mind.<sup>403</sup> Animals, especially primates (but also dogs), clearly possess some elements of a theory of mind (eye contact, gaze following), but it is unclear whether these really prove the possession of a theory of mind or whether they are instead the result of trained or automatic programmed behavior: “The degree to which chimpanzees have a mentalistic interpretation of the gaze (...) of others is still an open question (Tomasello et al. 1999: 769, see also Povinelli 2000).” Animal tests for more complex elements of a theory of mind are hard to devise. There is a fair amount of anecdotal evidence from pet lovers and ethologists that animals understanding them and their conspecifics, but these have been notoriously difficult to reproduce under controlled conditions, although the negative results may be due to artificial setups or human-style social settings such as in the Sally-Anne test. The fact that at least primitive forms of a theory of mind are present in animals demonstrates that a theory of mind did not evolve for or due to language use specifically, although it may have become exapted for language use (for *telling lies*, for instance, which requires self-awareness, if lying were one of the key functions of language use).

**iv) Self-awareness and (self-)consciousness:** closely related to ii) and iii) is consciousness and or self-awareness. (Self-)consciousness is notoriously hard to define because it may also be a heterogeneous or graded phenomenon, but here we use it tentatively to mean: ‘understanding that you have intentions of your own’, and similarly self-awareness to mean ‘being aware of yourself’. Just as having intentions and understanding that others have intentions (a theory of mind) is important for language use, so is consciousness of your own intentions as well as self-awareness.<sup>404</sup> It is clear that as language learning, use and understanding all involve understanding intentions, so understanding one’s own intentions and thus being self-aware are important for language evolution as well. The standard test for self-awareness is the Gallup-mirror test proposed by Gordan Gallup in 1970.<sup>405</sup> Humans pass the test at age two or a little bit earlier; some chimpanzees (Van den Bos and De Veer 2000), other primates (especially those raised with intimate human contact, id.), bottlenose dolphins, killer whales and elephants also pass the test. However, from tests of self-awareness one cannot infer the existence of (other forms of) consciousness, and so the claim that animals have other forms of consciousness remains problematic. Thus it is clear that self-awareness did not evolve for or was due to language use specifically, although it may have become exapted for language use.

In sum (see also Johansson 2005: 154): language cannot be a prerequisite for thought, but some elements of thought (such as memory) are necessary and others (such as a

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<sup>403</sup> For arguments against this see Hauser 2001, Tomasello 2000; for arguments supporting it see Tomasello et al. 2003, Pennisi 1999. Tomasello is open about having changed his mind on this issue.

<sup>404</sup> Some include the understanding that you have a mind of your own in the definition of a theory of mind; other claim that being aware of the minds of others comes before being aware of your own mind (Gärdenfors 1996) – or vice versa, as in stronger mentalistic traditions.

<sup>405</sup> The test gauges self-awareness by determining whether an animal can recognize that its own reflection in a mirror is an image of itself. This is accomplished by surreptitiously marking the animal with two odourless dye spots. The test spot is on a part of the animal that would be visible in front of a mirror, while the control spot is in an accessible but hidden part of the animal’s body. Scientists observe whether the animal reacts in a manner consistent with it being aware that the test dye is located on its own body while ignoring the control dye. Such behavior might include turning and adjusting the body in order to better view the marking in the mirror, or poking at the marking on its own body with a limb while viewing the mirror.



theory of mind and self-awareness) are very helpful in acquiring, understanding and using language.

### **Criticisms of Chomsky on the basis of the philosophy of language**

On the basis of our above discussion, we are in a good position to criticize two aspects of Chomsky's implicit philosophy of language: i) Chomsky's mentalist or as he calls it internalist theory of word reference and ii) his internalist theory of knowledge.

**i) Chomsky's internalist theory of reference:** in the computationalist paradigm that Chomsky helped to inaugurate, the basic elements are rules and representations. However, Chomsky often denies that talk of 'representational content' makes sense in linguistics.<sup>406</sup> But without representational content, issues of word reference become very difficult. Most philosophers nowadays are proponents of an externalist theory of reference. Either they support a) the causal reference theory of Kripke and Putnam or some other theory that links words to objects, or they support b) a theory of community conventions in the sense of (for instance) Lewis's truth conditions. Chomsky is highly critical of both. He is critical of a) because objects and/or the reference to them are always *socially* conditioned so that one cannot fix reference uniquely by singling out an object in the world,<sup>407</sup> and of b) because the idea of conventions (and especially the idea of a public language it entails) is simply unintelligible to him.<sup>408</sup> Chomsky instead opts for an *internalist* theory of reference, with words referring to 'internal objects'.<sup>409</sup> By opting for this theory, in

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<sup>406</sup> See Chomsky 2003: 272 (in Anthony and Hornstein *Chomsky and his critics*): "I do not think that the notion 'representational content' has been clarified sufficiently even to have an opinion about the thesis ["that representational content serves an important explanatory function in computational theories"]."

<sup>407</sup> See Chomsky 1981: 324 where Chomsky gives the example of the word 'fault' having no reference: "If I say 'the flaw in the argument is obvious, but it escaped John's attention,' I am not committed to the absurd view that among things in the world are flaws, one of them in the argument in question. Nevertheless, the NP *the flaw in the argument* behaves in all relevant respects in the manner of the truly referential expression *the coat in the closet*." See also Chomsky 2003: 44 "From the natural language and commonsense concepts of *reference* and the like, we can extract no relevant 'relation between our words and things in the world'. And when we begin to fill out the picture to approach actual usage and thought, the externalist conclusions are not sustained." Chomsky even claims that the notion 'reference' is theory-laden (see 1992a: 225). Here Chomsky also discusses Putnam's famous Twin-earth thought experiment in which he says that speakers' intuitions about what counts as the *same liquid* are sensitive to contextual (social) factors. See also Chomsky 1975: 203 "We do not regard a herd of cattle as a physical object, but rather as a collection, though there would be no logical incoherence in the notion of a scattered object, as Quine, Goodman, and others have made clear. But even spatiotemporal contiguity does not suffice as a general condition. One wing of an airplane is an object, but its left half, though equally continuous, is not. (...) Furthermore, scattered entities can be taken to be single physical objects under same conditions: consider a picket fence with breaks, or a Calder mobile. The latter is a 'thing', whereas a collection of leaves on a tree is not. The reason, apparently, is that the mobile is created by an act of the human will. If this is correct, then beliefs about human will and action and intention play a crucial role in determining even the most simple and elementary of concepts."

<sup>408</sup> See Chomsky 2003: 48-49 (in Anthony and Hornstein *Chomsky and his critics*) "A notion of 'common, public language' that remains mysterious (...) unless for any form of theoretical explanation. (...) There is simply no way of making sense of this prong of the externalist theory of meaning and language, as far as I can see, or of any of the work in theory of meaning and philosophy that relies on such notions, a statement that is intended to cut rather a large swath."

<sup>409</sup> See Chomsky 2003: 271 (in Anthony and Hornstein *Chomsky and his critics*): "There is another way to interpret assertions about the technical notion *Refer*: one can take it to be a relation between an I-language expression and a 'semantic value' that is also internal to the mind/brain. That is, I think, the proper way to interpret the valuable and exciting work in formal semantics in recent years, though we should properly regard it as a form of syntax, not referential semantics, in the traditional sense of these terms: it is the study of symbolic objects and their properties – in this case, internal objects, linguistic expressions and semantic

line with the classical rationalist philosophers whose influence upon him he acknowledges (Chomsky 1966), Chomsky falls prey to all traditional challenges to mentalism (see above). For instance, just as in any mentalist position, it is uncertain how Chomsky's 'internal objects' are related to the outside world. To our knowledge, Chomsky has not made this clear. In fact, Chomsky dismisses 'external reference' as a question of complex, interwoven pragmatic relations (1995: 26) with which he as theoretical linguist does not wish to concern himself (id., p. 47).<sup>410</sup>

**ii) Knowledge of rules:** Chomsky often says that we *know* the rules of grammar, but it is unclear what kind of knowledge he has in mind. One thing is sure: it is knowledge of a peculiar type since it need not be conscious knowledge, because one can use language without *ever* being consciously aware of this knowledge. Of course a linguist can consciously spell out the grammatical rules a speaker is unconsciously following all his life, but that is again a completely different competence.

Chomsky realized that his use of the verb 'to know' is unusual; he suggested replacing this word in his work by the verb 'to cognize', but that does not bring much clarification.<sup>411</sup> Sometimes Chomsky equates 'knowing I-language' with simply being able to do computations. However, Chomsky then again reminds us that computations in his sense of the word are not the same as computations done by computers (footnote 663). Nevertheless, "computations" already sounds much less mentalistic than notions such as 'introspective knowledge', and indeed in more recent years Chomsky seems more eliminative regarding 'mentalistic talk' in general.<sup>412</sup>

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values. And postulation of semantic values faces the same challenges as postulation of other theoretical entities: phonemes, atoms, whatever." The internalism of reference is here similar to the categorization of lexical items in his earlier work, which Chomsky also wished to call part of syntax. See also Chomsky 1995: 49 "Though naturalism does not entail an internalist approach, it does seem to leave no realistic alternative." And 1994: 177 "[I advocate] an internalist semantics of the Cartesian variety that doubts the existence of external things." Many followers of Chomsky also work in the internalist tradition, see the work of Pinker 1999, 1994, 1992, 1989, Jerrold Katz 1963, Ray Jackendoff 1983, 1972.

<sup>410</sup> See also Chomsky 1992b: "Left to the side are the questions about the use of language to refer. [In earlier work] I've tried to indicate some ways in which, I think these questions can be profitably pursued, drawing on ideas of early modern philosophy (neo-Platonists, British empiricists). (...) This inquiry goes beyond syntax, but the study of internal objects does not, whether it falls under phonology or internalist semantics. Such work may be of considerable value as a preliminary to the study of language use, and may provide a correct account of internal processes and structures; but that is a different matter (2003 in Anthony and Horstein 2003: 271)." It is this last notion that we seem to be interested in when we speak of (external) reference. Chomsky seems to equate it with semantics as pragmatics, regarding it as "part of the interface level (1992b: 223)."

<sup>411</sup> See Chomsky 1980: 69f: "To avoid terminological confusion, let me introduce a technical term devised for the purpose, namely "cognize," with the following properties. The particular things, we cognize. In case of English, presented with the example, "the candidates wanted each other to win" and "the candidates wanted me to vote for each other," we know that the former means that each wanted the other to win, and that the latter is not well-formed with the meaning that each wanted me to vote for each other. We cognize these facts. Furthermore, we cognize the system of mentally-represented rules from which the facts follows. That is, we cognize the grammar that constitutes the current state of our language faculty and the rules of this system as well as the principles that govern their operation. And finally we cognize the innate schematism, along with its rules, principles and conditions. In fact I don't think "cognize" is very far from "know" (...) but this seems to me a relatively minor issue, similar to the question whether the terms "force" and "mass" in physics depart from their conventional sense (as they obviously do)." Despite this long explanation it is still unclear to me what 'to cognize' is but a less constrained use of 'to know'. When Chomsky complains a few pages later that "it is not at all clear that the ordinary concept of 'knowledge' is even coherent (82)", one might retort that his own use of the verb 'to cognize' is not coherent either.

<sup>412</sup> See Chomsky 1998: 45-7 "We can be reasonably confident that "mentalistic talk" will find no place in attempts to describe and explain the world (...). The notion "common store of thoughts" has no empirical

Finally, Chomsky always makes a distinction between how people perform and the true competence people have in speaking their language, which consists in the mentalistic knowledge of their grammar. And indeed, a distinction between (unreflected, direct) performance and competence is possible, for instance when someone corrects his own grammatical errors; in such a case someone rectifies his own (unreflected, direct) performance and thus shows he ‘knows’ or *masters* his grammar better than his initial performance might have suggested. We can call this ability to correct oneself ‘linguistic competence.’ However, this form of competence is different from the competence of which Chomsky speaks. This form of competence is just another type of *performance*, just as one can drive one’s car recklessly or carefully (two types of performance differing only in the attention paid to the task). In Chomsky’s picture, on the contrary, what stands behind the initial performance is not a better type of performance, but a competence consisting in mentalistic *knowledge*. However, the only way to gain empirical insight into this knowledge (as a scientist) is through performance. Occam’s razor then suggests that for all actual empirical tests one can drop the idea of mentalistic knowledge (competence) and simply speak of (different types of) performance. The role for the Chomskian kind of knowledge of the rules is then thus only a theoretical one to account for the performance, without being able to be tested independently. One might then be tempted to drop this vague concept of knowledge altogether as the performance can equally well be explained without mentalistic competence.<sup>413</sup>

## §3.2 Five domains of research

In this section we will discuss the empirical results, as found by different branches of science, to buttress and review ultimate Darwinian models of the origin of language with proximate mechanisms. The body of research in all these areas is far too big to do justice to here, but we discuss the literature only to the extent that an (high-level) ultimate explanation requires.<sup>414</sup> We start with *biolinguistics* in §3.21 (comparative anatomy, neurology, genes for language use), then turn to history: *paleontology and archeology* in §3.22, followed by a discussion of *animal language* in §3.23, *first language acquisition* in §3.24 and the closely related fields of *diachronic linguistics* and

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status, and is unlikely to gain one even if the science of the future discovers a reason, unknown today, to postulate entities that resemble “what we think (believe, hope, expect, want, etc.)” [Frege’s principle that there is such a storehouse] seems groundless at best, senseless at worst.”

<sup>413</sup> See also Deleuze and Guattari, *A thousand plateaus*, p. 102 in the context of arguing against universal constants: “[T]he distinction between competence and performance is entirely relative (a linguistic competence can be economic, religious, political, or aesthetic, etc; the teaching competence of a grade school teacher can be only a performance in relation to the judgment of an inspector or government regulations), linguists respond that they are willing to multiply levels of competence, and even to introduce pragmatic values into the system. Brekle, for example, proposes adding an “idiosyncratic performatory competence” factor tied to a whole constellation of linguistic, psychological, or sociological factors. But what use is this injection of pragmatics if pragmatics is in turn considered to have constants or universals of its own [hence there being some universal, possibly “mentalistic” *competence* behind them].”

<sup>414</sup> The devil is in the details: small discoveries in remote areas of science can have consequences for many other sciences, so that one cannot say a priori how extensive an overview must be in order to provide reliable proximate mechanisms to ultimate mechanisms. We are here content if we succeed in ruling out many ultimate explanations as implausible, given the general consensus on the nature of the most important proximate mechanisms involved.

*sociolinguistics* (focusing more narrowly on pidgins and creoles) in §3.25. The (relative) novelty of our approach is that we include both research on the evolution of linguistic abilities (§3.21, §3.22 and §3.23) and the evolution of linguistic forms (§3.24, §3.25); this is in line with the dual-inheritance model of section §2.3 where we argued that in cultural evolution we should look at both sides and their interdependence. We believe that the empirical results of these fields mutually constrain possible theories of the origin of language – as, for instance, the field of diachronic linguistics shows that language changes too fast for highly specific rules of grammar to be innate.

The fields have different advantages and disadvantages for the study of the (true, first) origin of language, which we list first in table 3.1. The material in the next sections will further substantiate and elucidate this overview.

<b>Field</b>	<b>Advantages</b>	<b>Disadvantages</b>
<u>Biolinguistics</u> a) Comparative anatomy b) Neurology c) Genes	a) Use of all the tools of physiological research, clear object of research b) Physically speaking, language use is ultimately caused by neurological processes c) Use of standard tools of evolutionary genetics	a) Much of the anatomy used by humans for speech is already present in our neighbors in the evolutionary tree b) General inchoate nature of the science of neurology, often leading to large speculations c) Many genes probably involved with complex interactions in language use, proteomic picture almost blank
<u>History</u> a) Paleontology b) Archeology	a) Bones of fossil records track evolution over macro-evolutionary time scales b) Archeological record tracks evolution over millennia For both: experimental material can be tested and sampled over long periods of time	a) Fossils often in bad condition, generally hard to reconstruct speech organs from bones (with vital speech organs not fossilizing at all), same disadvantages as anatomy b) First unambivalent records of language use (writing) only 6,000 years old, earlier clues of language use rather indirect
<u>Animal language</u>	Possibility to use all the tools of ethological research, comparison leads to clearer view of what <i>human</i> language is, comparison shows what design features had to evolve from primates to humans, lab studies demonstrate room for easy exaptation	Studies in the lab easily exaggerate the abilities of non-humans at learning language (though they might learn something else), whereas studies in the wild are hard to conduct and often rely on anecdotal evidence

<u>First language acquisition</u>	Possibility to apply recapitulation theory: phylogenetic language acquisition reconstructed on the basis of ontogenetic acquisition; the study of learning biases as guide to selection pressure on language ability; a simple language shown in action to guide us how proto-languages might have worked	Recapitulation is speculative (and refuted in normal Darwinism due to restructuring, exaptations); learning is dependent on complex parental language
<u>Evolution linguistic forms</u> a) Diachronic linguistics b) Socio-linguistics (pidgins and creoles)	a) Tracking changes in linguistic forms over centuries and discovering form-based principles behind this evolution. b) Tracking changes in linguistic forms over small time scales and discovering actual social (causal) principles of exchange behind them, pidgins/creoles show formation of whole new language	a) The processes of diachronic linguistics resemble for the most part neutral selection of molecular changes in biology, and do not reveal much about true increases in complexity/simplicity. b) Pidgins are formed as simple communication devices by people who already speak a complex language fluently, and thus have no true origin in evolution.

Table 3.1: Importance of areas of linguistics for study of the origin of language

### §3.21 Biolinguistics

Biolinguistics studies the biological basis of language use. Its subfields are a) (comparative) anatomy of language use and speech, b) neurological basis of language use and speech and c) genetic basis of language abilities. Nobody doubts that language use has an innate component, but ideas about the extent to which it is innate vary considerably (see §4.2d). Here, we will first focus on i) anatomy, then on ii) neurology and finally on iii) language genes.

#### **i) The (comparative) anatomy of language and speech**

A discussion of the anatomy of language organs and their evolution seems simple: just analyze how the organs for listening (ears) and the organs for speech (flexible tongue etc.) evolved. However, a little reflection shows that an actual discussion is rather complex for two partly contrary, partly overlapping reasons.

a) Many of the organs for speech are already present in neighboring species in the evolutionary tree, so that the evolution of language seems – on the *anatomical* level – more a matter of fine-tuning and exaptations of the use of already-present

organs than an evolution of true unique organs: almost all vertebrates have ears and almost all mammals, with rare exceptions, are capable of producing a variety of sounds often used for communication (Hauser 1997 and §3.23). Apes can even produce approximations of several human vowels, though some vowels are impossible for them and they have severe difficulties with consonants, particularly stops. Stories about fine-tuning and exaptations are, however, harder to verify than cases in which we find a new organ evolving 'from scratch'.

b) The use of language involves many, many organs in a direct and in an indirect fashion, so that it becomes hard to decide which anatomical elements to include in a story of the origin of language. For instance, human language use requires a large brain with expensive tissue; this brain probably co-evolved with a diet change towards protein-rich meat, which in turn required a smaller stomach and different gut bacteria. Are these gut bacteria part of the anatomy of speech or not? Another example: the human language use we consider is usually face-to-face communication, supported and or supplemented by facial and manual gestures, which require acute manual dexterity and visual acuity (even more so does true sign language) as well as an upright posture, which itself evolved for many reasons but certainly involved a major restructuring of the organs in the torso. Is this restructuring part of the evolution of speech? And what about visual acuity? A final example: our speech is composed of rapid sequences of precisely modulated sounds that are out of reach of most other mammals, including our closest living relatives (Savage-Rumbaugh et al. 1993); this sequencing requires an accurate control of breathing, which in turn is dependent on the way lungs function.

Although nobody would consider specific gut bacteria or the major restructuring of the organs in the torso parts of the anatomy of speech, the control of breath is part of this anatomy. But where to draw the line? The general, highly interdependent nature of biological organs makes the anatomy of speech hard to identify precisely. Our discussion of *Wright* and *Cummins* functions (§1.23) can be of use here. The *Wright* perspective would have us include all the elements in the anatomy of language that evolved or were exapted for speech *in the course of natural selection* (thus the chronological anatomical *changes* in organs used for speech from the time of proto-speech till now); here arises the usual problem of missing some exaptations and accidental or non-(directly)-functional current uses. The *Cummins* perspective would look at the functional decomposition of the speech system; here arises the usual problem of fixing the frame of the speech system (is the anatomical upright-posture system part of it or not?). In our approach to functions, which combines both perspectives, *we look at the system as a whole (like Cummins) and then include the elements that evolved or exapted specifically for speech.*

The biggest anatomical prerequisites are probably those placed on the brain's 'processing capacity' and the neural wiring (for voluntary control of the vocal apparatus and the rapid sequencing of motor actions in sound production), and the wiring for accurate sound perception and possibly phonological segmenting of the speech stream (see next section).

Here we focus on non-neurological prerequisites. We first focus on speech production, then on speech perception.

### **Speech production (speaking)**

The main non-neural element is the descent of the larynx, which we discuss in more detail. Other anatomical indicators of language are summarized in the table below.

The larynx of chimpanzees and humans does not differ greatly to that of other primates (Hauser and Fitch 2003). Only its position is changed: it is further down the throat compared to other primates<sup>415</sup> and is bent almost 90 degrees, with the bend dividing it into two almost-equal pieces, unlike the fairly straight tube of most other mammals. This is usually thought to broaden the spectrum of articulatory possibilities (Aiello 1998).<sup>416</sup> Babies are born with the larynx in the ape position, which is better suited for breastfeeding (Lieberman and McCarthy 1999); the larynx descends as the baby starts to babble (Fitch 2000), and the larynx descends even deeper in male throats during puberty.

The descent of the larynx is seen as a true adaptation for speech by Lieberman and Carthy 1999 and Aiello 1998. Others relate its descent to sexual selection, favoring a deeper voice in males (Randerson 2001); although this is a plausible explanation for the additional descent in males, it cannot account for the whole descent. Still others regard the descent as an adaptation to drier habitats (Reeves et al. 1996) or as the non-functional by-product of the transition to bipedalism (Pearl 2000), which in both cases became later exapted or simply used for speech. For other, less clear indicators of language use we find similar disagreements as to whether the trait is an adaptation, exaptation of an older adaptation or functional use of an old by-product. The stretching and bending of the supralaryngeal tract is sometimes seen as an adaptation (Lieberman et al. 2000), but is also explained as a byproduct of increased brain size, change to bipedal posture and/or the shrinking of our jaws (Spoor 2000). Other indicators are below in the table 3.2.

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<sup>415</sup> In our nearest ancestors, the chimpanzees, the larynx is also lowered compared to other primates, but not as much as in humans (Nishimura et al. 2003). Fitch 2002 presents other non-primate mammals with a lowered larynx and warns that many mammals can already temporarily lower their larynx during loud vocalization, so that a lowered larynx is thus not a unique human feature.

<sup>416</sup> Early experiments in teaching an ape human language, such as the famous chimp Nim Chimpsky (raised by Herbert Terrace like a human child [Terrace 1979]), focused on vocal language. Chimps were, however, very bad at learning this (or the researchers were bad at recognizing their attempts at human words, see Hess 2008), because the anatomical impossibility of achieving rapid velopharyngeal closure (closing off the connection between nose and mouth) means that apes cannot produce oral consonants, since without this closure air is not forced into the constrictions that we use to make consonants (Savage-Rumbaugh and Lewin 1994). Nevertheless, the vocal apparatus of apes and monkeys does seem capable of producing a non-negligible number of sounds that are usable for communication (if subtle to human ears), with a richer structure than is often believed (Rendall et al. 1999, Fisher 1998).

<b>Body part</b>	<b>In favor of anatomical indication of language</b>	<b>Against anatomical indication of language</b>
Brain (size and shape)	Size and shape indicate early appearance of capacity for language.	Size changes more complex and related to stature; brain asymmetry widespread in nature; shape arguments depend on assumptions on innateness and fixed functions of cerebral regions; brain not only used for language.
Throats	The shape of the base of the skull indicates speech ability.	The shape and base of the skull is a product of complex developmental processes affecting many parts of the skull, and is unlikely to be principally related to speech production.
Hyoid	Dimensions of earliest surviving hyoid within range of modern humans.	Dimensions of earliest surviving hyoid within range of modern humans and many modern non-human primates.
Hypoglossal canal (tongue)	Dimensions of canal, related to the nerves controlling the tongue, within range of modern humans for 400,000 years (Kay et al. 1998).	All hominid specimens and most apes within range of modern humans (DeGusta et al. 1999).
Vocal ligament structure	The shock-absorbing ligament attachment structures, and the multiple layers with fine-tuned elastic properties in the human vocal cords, seem designed for heavy use (Sataloff 1993).	Full development is not achieved until puberty, indicating sexual instead of natural selection.
Laryngeal air sacs	Absent air sacs (which play a role in other ape vocalization), possibly due to enhanced breathing control (Hewitt et al. 2002).	Absence of organ used for vocalization at first sight an argument <i>against</i> natural selection on speech.
Nerve canal enlargement	Humans have more motor neurons going down to the spine (presumably for breathing control, so important for speech (MacLarnon and Hewitt 1999).	Enhanced breathing control has been linked to bipedal walking too, as walking on four feet requires close coordination between breathing and stride.

Table 3.2: Non-neurological indicators of speech<sup>417</sup>

<sup>417</sup> First four rows after Davidson 2003: 147 with serious modifications.



## Speech perception (listening)

The main conclusion from research on sound perception can be summarized briefly: “the auditory apparatus as such is essentially identical in humans and other primates, and very similar in all mammals” (Johansson 2005: 86). This holds even for the details of the auditory system, as we can know from our daily experience; dogs and horses respond well to voice commands, and can discriminate individual voices of humans and conspecifics (the latter ability being widely present amongst mammals, see overview Johansson 2005: 87). Even the finer details of the phonetic system are not human-specific, as research on phoneme discrimination shows that monkeys (Sinnott and Kreiter 1991), birds (Mueller 1996) and even guinea pigs (McGee et al. 1996) are able to do it, which led Savage-Rumbaugh et al. (1993) to question the popular view that humans have a unique ‘phonetics decoding module’. Even such an apparently language-specific feature as the human categorical perception of voice onset time is based upon the time resolution necessary for speech perception (around 25 ms) in mammals in general (Simos et al. 1997, Mueller 1996).

Further, it is well established that humans, even pre-verbal infants, are very good at perceiving speech and learning to segment it into (formal) language-relevant units (Kuhl 2000); this learning process is even said to occur during sleep (Cheour et al. 2002). From the earliest work onward, Chomsky has invoked this ability as proof of massive innate language specialization (Chomsky 1957). However, the sound-pattern analysis abilities that infants use have been found to be useable for the analysis of non-speech sounds such as music (Saffran et al. 2001) and also to be present in monkeys (Hauser et al. 2001). This indicates that these abilities are not language-specific.

One of the few human-specific features of speech perception is its optimization for analyzing sounds in the range 2-4 kHz, whereas the sensitivity of ape ears has a minimum in this range. This optimization is due to minor changes in ear ossicles (tiny bones that conduct sound from eardrum to the inner ear), and does not make the human ear unique in the sense of radically different from other mammalian ears. Larger changes in speech apparatus have to do with neurology, and are the subject of the next section.

The story behind the evolution of the ear, from jawbones which also conduct sound in early vertebrates to complex mammalian hearing devices, has been described in detail in many textbooks, for instance Cowen (1995). Clack (2002) focuses on the earliest history of the ear and Fritz et al. (2002) focuses more on the inner ear, the structural gene of which has been subject to selection in the human lineage (Olson and Varki 2004). Given our discussion of the small differences in speech perception between humans and other animals, we can safely conclude that human hearing in general (as well as phonetic discrimination and possibly even segmentation in particular) have not evolved especially for speech perception. Fine-tuning in the range of 2-4 kHz may have been selected as our hearing is optimized for these frequencies, although the adaptive reason for exactly this frequency range is not clear. The absence of strong evolution of human hearing during the evolution of speech seems logical from a Darwinian naturalistic perspective, which demands that signs are received before they are broadcast,<sup>418</sup> since there is no one to communicate

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<sup>418</sup> Only non-naturalistic rationalist or idealist accounts hold that an understanding of meanings and ideas comes before the learning of words, so that the problem of the evolution of language would be how to map a priori meanings that everybody privately understands into words. Thus the evolution of hearing devices can be imagined to co-occur or leap behind the evolution of speech organs. The rationalist or idealist account of meaning was, however, shown to have many problems.

with or to manipulate before he or she has evolved the ability to understand signals first. More specifically, this means that the ‘choice’ of phonetic distinctions to be used in communication was guided by the pre-existing discriminatory capacity of our hearing system, and not the other way round.

## **ii) The neurological prerequisites for language**

As we have mentioned, most evolutionary changes will have occurred in the brain. We start with a general introduction of the evolution of the brain as a whole (regarding size and shape) and its importance to the evolution language, then we briefly discuss the early state of development of the methods of neurology for studying the inside of the brain and some of the *areas* in the brain that are linked to language use. Finally we turn to the neural mechanisms behind speech production and hearing, behind the more general linguistic abilities such as word and syntactical knowledge, and behind language acquisition. This discussion will be relatively brief, as the area of neurology is still in its infancy compared to other branches of science.

### **Brain size**

Brain evolution starts very early in history, in Precambrian times, as the ancestor of all bilaterian animals most likely possessed something that might be called a proto-brain (Nielsen 1999). But a true brain, homologous to ours, is a *vertebrate* characteristic, one that has evolved almost without exception towards increasing encephalization (the degree to which an animal’s actual brain size exceeds what would be predicted for a typical animal of its size). The reason behind this unique trend may be a feedback loop in which increasing complexity in the environment favors selection on bigger brains, which leads to behavior that again increases the complexity of the environment (both social and natural), which in turn leads to selection on bigger brains (see Ng 1996).

The brain enlargement in the lineage of the vertebrates takes place through enlargement of already existing structures, also called the ‘reptilian’ part of the brain (Lieberman 2001), as well as through the addition of new structures on top of old ones with novel organizational principles. The mammalian top part is called *neocortex* and *isocortex*; its structure is more or less the same for our mammals, but its evolution is not exactly known (Kaas and Reiner 1999, Nishikawa 1997). Our human brain is not noticeably different from the brain of other primates, apart from being much larger than would be predicted by allometry (see §1.33), and with a more convoluted surface (see Roth 2002 for more details).

The different parts of the human brain have not increased equally in size during evolution. Enlargement has been thought to be biggest for the neocortex, particularly the prefrontal lobes, which have traditionally been regarded as a human specialization (Deacon 1997). Others such as MacLeod et al. (2003) report that the cerebellum, rather than the neocortex, has increased in size in hominoids. However, still others hold that all the main parts of the human brain have evolved in tandem (Nishikawa 1997), and the most detailed multivariate analysis thus far finds multiple differences between the brains of humans and other primates, with both neocortex and cerebellum enlarged in humans, as well as some other parts. The lack of consensus is mainly due to statistical problems in analyzing part-whole ratios, and the presence of humans as an extreme outlier in the dataset and therefore skews the results (Johansson 2005: 91), as well as difficulties in identifying homologies between brain areas of humans and apes (Deacon 2004).

It is also remarkable that the large increase in human brain size compared to other primates does not occur during embryological development, but during early childhood. At birth, human babies have a brain size that is not larger than that of other ape babies (when allometric measures are taken into account). But their brains keep growing as they did in the womb (a process sometimes called neoteny) until they are four times the size of a chimp brain, and neoteny may favor parts that develop late in ontogeny, such as the neocortex (Finlay et al. 2001). This research, as well as more detailed measurements of the relevant data (Leigh 2004), however, suffers from the general methodological pitfalls described above.

Despite these many uncertainties, nobody doubts the existence of the massive, quantitative growth of the brain in humans as compared with other primates – along with the enormous costs involved not just in prolonged parental care, but also in maintaining the growth of the metabolically expensive tissue, which is calculated to cost up to 20% of our daily energy intake, whereas primates only spend around 5% of their energy intake on the brain (Raichle and Gusnard 2002, Nilsson 1999, Dunbar 1998).

These huge costs clearly indicate that our brain has been a major adaptation, but the question is: for what? The reasons adduced are intertwined with the reasons for language evolution and causal relations between the two in both directions (with language needs driving brain enlargement such as Deacon 1997 or brain enlargement as a vital exaptation for language such as Chomsky et al. 2005). Here we concentrate on the current proposals for the (adaptive and non-adaptive) reasons for brain growth (overview in Johansson 2005, Martin 1998 and Dunbar 1998).

- 1) As a byproduct of larger body size (Sacher and Staffeldt 1974); however, this is unlikely given the expensiveness of brain tissue, and it does not explain the high degree of encephalisation in humans compared to other primates.
- 2) As a byproduct of bipedalism and the narrowing of the pelvis (Ragir 2001), but this is counter-intuitive as the narrowing of the pelvis would sooner predict that babies would be born smaller.
- 3) As a byproduct of weaker jaw muscles as the result of a diet change (Stedman et al. 2004), which freed the skull from heavy-duty chewing and allowed it to grow; however, *allowing* something is not the same as causing it.
- 4) As a cooling device for the blood, as Aristotle thought and which has most recently been defended by Falk (1990); however, the brain is very inefficient at cooling (being isolated by bone and hair), and it costs far too much to regard this non-cognitive purpose as its primary function.
- 5) As an energy-driven extra growth, as supplied by the mother to the fetus increased (Martin 1998) due to a diet change from grass to fruit (and with maternal energy the limiting factor in fetal growth); however, why would the surplus energy be spend solely on brains?
- 6) As a shortening of the energy-costly gastrointestinal tract (again by a change in diet) so that more energy was available for the brain; however, the gastrointestinal tract is vital for providing the body with energy, so that it can only be shortened as the diet becomes both easier to digest and richer in energy. This does not tell us *why* the brain evolved, but only *how* there would be energy free for it to develop.

- 7) As a means of keeping track of when fruit is ripe and ready to be picked, an activity considered more complex than grazing or browsing, as the available fruit can vary temporarily and spatially (Potts 2004);<sup>419</sup> however, some primates such as gorillas are not fruit-pickers but leaf-eaters and are smart nevertheless. And it is not clear why carnivores wouldn't need a big brain as well (to track herds, or to time and prepare an attack).
- 8) As a means of navigating in trees, which is far from trivial for a heavy ape (Byrne 2000); this does not explain the extra brain increase of bipedal man.
- 9) As requirement for complex binocular vision that was needed to find fruit (Barton 2004), but see 7.
- 10) As requirement for tool-making, but most primate use of tools is accidental and seems rather crude (see also §4.3f).
- 11) As a device to live in complex groups in which interactions and manipulations of conspecifics play a major role, which has been called the social brain hypothesis (Dunbar 1998, 1996) or 'Machiavellian intelligence'.

This last hypothesis is currently the one most often supported. It is buttressed by the fact that – while in most social mammals, a rather straightforward dominance hierarchy is the rule – politics and coalition-building are important for a primate's success (Seyfarth and Cheney 2002, De Waal 2000, 1989, 1982). Since humans live in larger and more complex groups than other primates, this selective force may have been unusually strong for humans, even leading to a runaway arms race for brain power (possibly on the basis of sexual selection). The work of Dunbar (1996), who found that the group size in primates is not related to brain size proper but only to

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<sup>419</sup> This *variability* hypothesis, as it has come to be known, is also the favorite explanation for the brain increase found in the work of supporters of dual-inheritance theory. See for instance Heinrich & McElreath 2003, who claim that this hypothesis is further supported by unusually strong climatic variability over the past 14 million years. However, we do not have enough data about the climate of other epochs to conclude that the variability has been more than unusual in the last millions of years. Further, one can doubt whether there is adaptation on this variability (which is non-periodic and chaotic, and is sometimes absent for thousands of years or hundreds of generations). Finally, the same problem that plagues the original proposal of Potts 1998 seems to be present in the work of Heinrich & McElreath, as one can argue that being a carnivore would require an even bigger brain.

The attractiveness of the variability hypothesis to supporters of dual-inheritance might be that part of the package of coping for climatic variability is the evolution of social learning: either directly, as selection on social learning to find more food as a group increasing the brain size, or indirectly, as (later) exaptation of selection on a bigger brain to individually cope with climatic variability. The first possibility is the one proponents of dual-inheritance support (Richerson and Boyd 2005) because it seems to make cultural learning clearly adaptive – as they intended to prove to people who were skeptical of the adaptive value of culture. However, it seems unlikely – for the reasons outlined above and because it relies on some form of group selection, and thus needs to take manipulation from within into account too, which will make the explanation rather complex. The second option with cultural learning as an exaptation of larger brains is better insofar as it frees the theory of dual-inheritance from the direct link to climatic variability or any other theory about why the brain grew. This exaptation can itself still be pictured as adaptive, as can specific (non-genetically coded) cultural customs, so that the reason proponents of dual-inheritance probably support Potts's suggestion can nonetheless be upheld (although cultural learning changes from an adaptation to an exaptation, but that is not a real problem either, as the boundary between an adaptation and an exaptation is fluid, see §1.23). A good candidate for a function on which cultural learning is exaptive is probably a cognitive ability specifically involved in living in a complex social group. This might seem strange, because living in a complex group seems to imply cultural learning, but it isn't. Cultural learning (as adaptive cumulative learning), as proposed by Richerson and Boyd, is a scarce trait and absent in many complex social animal groups. Thus group living certainly does precede cultural learning. Cultural learning nonetheless presupposes group life and probably also some (implicit) understanding of the nature of the group; the further idea that cultural learning is exaptive upon some cognitive, functional mechanisms involved in group life has the advantage that group dynamics come in more or less for free.

the size of the neocortical part of the brain, also supports this hypothesis; the neocortex is, as we saw, often assumed to be the brain part increased most during primate and human evolution. However, an open problem for this explanation is how social complexity originated in the first place (see §4.3).

### **Methods of neurology, brain areas involved in language use**

Only recently have techniques (such as CAT, MRI, fMRI, ERP, PET, SPECT, and DOT) been devised to investigate the inner workings of the brain. A vast body of research concentrates on correlating specific human abilities or functions with brain regions. This research is, however, in its infancy. And the models of the brain based upon it are often toy-models (some even argue that brain scans only show which areas are used for a certain task and nothing more than that, revealing practically nothing about how the brain works).<sup>420</sup> A major methodological problem with this type of research is that it is hard to ascertain that a brain is doing one thing and one thing only.<sup>421</sup> But an even bigger problem with matching brain functions to specific regions of the brain is that the same piece of brain is generally used in many different tasks, and any given task typically activates several, disjoint parts of the brain (see the meta-analysis of brain scan experiments by Stowe and Haverkort 2003). There is also some variation among people's brains, and in case of brain damage new parts of the brain may take over old functions, further breaking down one-to-one-correspondence between brain functions and brain regions. Ironically, the same people with damaged brains who challenge the one-to-one correspondence are also essential research subjects for experiments designed to uncover the workings of the brain.<sup>422</sup>

General questions about (human) brain anatomy ask how: i) lateralized, ii) modular and iii) plastid it is. These question cannot be answered in general terms, and here we will concentrate mainly on language processing in the brain, but even then the picture is complex and current research far from conclusive. It is impossible to discuss all current research with brain-probing techniques, and the picture will probably look very different in a few years.

i) Lateralization concerns the functional asymmetry of the brain. This rose to prominence when Paul Broca (1861) discovered that language is mainly handled by the left hemisphere of the brain, with damage to the left side commonly leading to aphasia<sup>423</sup> (now called Broca's aphasia, see below); right-sided damage had little effect on language. The brain is of course not the only asymmetrical feature of anatomy (think of the inner organs and the general preference for the right hand also present in sign language, Bonvillian et al 1997)<sup>424</sup>, nor are humans the only animals with a

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<sup>420</sup> There is only one organism whose brain anatomy is known in adequate detail to determine its exact function: the nematode *Caenorhabditis elegans*. A map of every single neuron (all 302 of them) in its nervous system, including the brain, is known (Buller and Hardcastle 2000).

<sup>421</sup> As such, non-invasive brain probing experiments can only be comparative, measuring the difference in brain activity between the task of interest and some control task.

<sup>422</sup> It has sometimes been claimed that, with the advent of fMRI and other brain monitoring devices, the previous dominant role of lesions in brain research would be a thing of the past. However, for a recent defense of the enormous importance of lesions for current brain research, see Rorden and Karnath 2004.

<sup>423</sup> Aphasia is a loss of the ability to produce and/or comprehend language, due to injury to brain areas specialized for these functions.

<sup>424</sup> The right hand is controlled by the left hemisphere (the supposed language side), since the brain is cross-connected. Some therefore argue for a *connection* between handedness and lateralization (Natsopoulos et al. 1998), but this is generally rejected on the basis of too little empirical proof (for instance Ross and Bever 2004).

lateralized brain. Any major textbook on (animal) neurology can tell what brain functions have been linked by brain-probing techniques to specific hemispherical sides. The left side localization of language is also not absolute; if the left hemisphere is damaged very early in life, well before language acquisition, nearly normal language development can still follow, with language largely handled by the right side (Stow and Haverkort 2003, Müller et al. 1998). This demonstrates iii) the plasticity of the brain.

ii) Modularity: we already discussed brain modularity in general in §2.2, where we touched on evolutionary psychology with its claim of massive brain modularity. We concluded that it is not a question *whether* but only *how much* of the brain is modular, and that although it is hard to define in general whether something is modular (as it depends on a definition of generality or domain specificity which cannot be given *a priori*), the question of modularity is only answerable by extensive empirical testing.

iii) Plasticity: plasticity is the question of how malleable different areas of the brain are at different stages of life – whether an area is genetically predetermined to serve a certain function, or if it is only susceptible to doing so. This question is also only answerable by extensive empirical testing. Generally, the infant brain is considered highly plastid, but the older someone gets the less flexibility there is (Buller and Hardcastle 2000).

### **Specific neural findings concerning language use**

We first discuss the neural mechanisms of (physical) production, then those of (physical) perception. Next we turn to neural mechanisms involved in the production and understanding of the lexicon and syntax. Our account has profited greatly from Johansson (2005), chapter 5.

#### *Speech production:*

The easy vocal learning, imitation and innovation of humans appears to be unique amongst primates (Fitch 2000). This might be due to the fact that humans have conscious control of their vocal cords, unlike apes that produce more or less non-voluntary sounds similar to human laughter (Deacon 1992). This conscious control is thought to be related to neural pathways from the neocortex to the vocal cords, which other primates lack as their vocal cords are connected to older parts of the brain, which in turn are thought to be related to automatic innate vocalizations. For conscious control to evolve, the latter connections first had to be partly removed, leaving room for vocal learning (Deacon 2000). They are, however, not completely removed; we have a few vocalizations that are controlled in the old way, such as laughter and the sounds we make when sexually aroused. Another neural pathway to the vocal cords that had to be removed was that pathway controlling the procedures of swallowing and breathing – these are precisely synchronized in mammals to prevent choking on food. This removal was necessary for breathing control, which required that breathing could be controlled independently of swallowing, though it did create a huge risk of suffocating for humans (Deacon 1997, Pinker and Bloom 1990). Breathing control for speech has also been invoked as adaptation for the significantly increased number of motor neurons going down the spine to the thorax (MacLarnon and Hewitt 1999).

A further important element in speech production is the control of rapid sequencing, i.e. an ability to produce rapidly complex sequences of motor actions necessary for speech (tongue, lip, vocal chords, lungs etc.). These actions sequence

with an extreme degree of precision, proceeding too rapidly for sensory feedback, thus implying that sentences must be pre-planned. For instance, voice-onset time needs to be produced with an accuracy much better than any conceivable feedback loop time (Cho and Ladefoged 1999). Before we receive any feedback on our speech production (via our ears through the brain, with the intrinsic timing jitter in individual neuron firing in the order of 10 ms<sup>425</sup>) we are several sounds further ahead, as we manage to produce streams of dozens of sounds which require several separate motor actions to be coordinated within 10 ms or so. The location in the brain of this ability is unclear,<sup>426</sup> and so is its origin. Calvin (2003, 1993) saw this ability as an exaptation of a general ‘sequence handler’ evolved for throwing, an activity that also requires the coordination of many muscles with a timing precision of milliseconds (also too rapid for sensory feedback), and at which apes also perform miserably compared to humans. However, such speculation would require a further explanation of how this ability became exapted and why the sex difference in throwing is not present in speech production. Calvin’s strong claim, that a large chunk of the ‘language cortex’ on the left side actually is this sequence handler, finds few other supporters (see for instance Hickok et al. 1998).

#### *(Physical) Speech perception*

Rapid sequencing, although less complex here than in production, also occurs in speech perception. It requires the detection of incoming features in incoming sounds with millisecond precision, and the decoding of them on multiple levels in a time about equal to the time it takes to say a sentence (Carr et al. 2001). Little is known about it, just as little is known about the sequencing involved in speech production and about speech perception in general. Kaas and Hackett (2000) have shown that the processing of sounds in the brains of humans and other primates appears similar, at least for non-linguistic sounds. It is worthwhile to remember that the processing of linguistic sounds, though similar at first, activates different neural areas than do other sounds (Müller 1996).

#### *Further language processes in the brain*

We already mentioned that brain damage in early life occasionally has few consequences for language development. However, damage to the brain in later life can cause highly specific forms of aphasia; these tell us more details about how the brain processes language, especially when they are as peculiar as some of the cases

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<sup>425</sup> It can be demonstrated mathematically that the timing accuracy of the brain can be improved when the signals from multiple neurons are averaged. However, the required number of neurons to reach an n-fold degree of precision grows exponentially; in rhythmic tasks, precision can nevertheless be increased more easily (Ivry and Richardson 2001). Hence prosody in speech?

<sup>426</sup> See, for an overview, Johansson 2005: 84. Some parts of the brain thought to be involved in sequencing (often buttressed by claims about specific language impairments due to brain damage in specific areas) are Broca’s area (Calvin 1993), the subcortical basal ganglia (Lieberman 2001), and the cerebellum (Desmond and Fiez 1998) – Richards and Chiarello 1997 even propose a right-hemisphere role for it, as it involves higher-level action planning. We can conclude that there is little consensus and that much further research is required. What would help is defining the vague notions of sequencing better: many tasks involve a planned succession of actions, so that it is not strange that many parts of the brain are thought to be involved in it. However, can we really assume there to be one unique sequencer involved in all these many tasks, like a data sequencer and operation sequencer in a serial computer? Would it not be better to presuppose that many tasks are simply performed in a certain order (planned or not) and that the sequencing is local to this succession of tasks?

reported in Pinker (1997), with impairments affecting either regular or irregular verb production only.

The best-known cause of language impairment is damage to Broca's area<sup>427</sup> (located in the frontal lobe of the cortex), which can give rise to Broca's aphasia (also called expressive aphasia or nonfluent aphasia). It is traditionally contrasted with Wernicke's aphasia, which results from damage to Wernicke's area (the more posterior regions of the left hemisphere in the superior temporal lobe). People suffering from Broca's aphasia exhibit slightly reduced comprehension skills (especially regarding syntactically complex sentences), but strongly reduced syntax production skills. Their verbal utterances are rather telegraphic, containing almost nothing but so-called content words. People suffering from Wernicke's aphasia, on the other hand, speak with correct syntax, yet their use of words is roundabout, vague or meaningless. As such, the difference between syntax and semantics is thought to be present in a difference between brain regions. However, this cannot be a justification for an absolute distinction between syntax and semantics. It is hard to detect syntactical structure in the often-meaningless combinations of words uttered by people suffering from Wernicke's aphasia, just as the 'content words' of people suffering from Broca's aphasia still have some orderly syntactical structure. More detailed studies of aphasia in general, (Bates 2003) and Broca's aphasia in particular (Lieberman 2002), show that the picture is more complex; it involves more regions and partial aphasia in many possible aspects of language use (even specific languages in case of bilingualism), with the loss of producing syntactical sentences being only one aspect, affected to different levels in different patients (Mueller 1996); moreover, Broca's aphasia also involves impairment to other brain areas such as motor control (Binkofski and Buccino 2004). These results break down the *simple modular* Broca-Wernicke picture of language processing (see also Poeppel and Hickok 2004), a picture that may have inspired the idea of a modular language acquisition device in the brain.<sup>428</sup>

The simple picture of language use being lateralized on the left side of the brain has also broken down. It has at least been complicated by findings that suggest a right-hemisphere role for some aspects of language – notably prosody, but also phonology (Simos et al. 1997), word learning (Sabbagh 1999) and other elements (Poeppel and Hickok 2004).

The breakdown of the simple lateralized and modular picture of the brain does not rule out more complex modularity and requires more extensive testing. Empirical questions relevant to the issue of language modularity include the following:

- a) normal child development: what correlations are there between language acquisition and other developments?
- b) development defects and delays in children: are there any defects that affect language use uniquely, or are language acquisition problems always accompanied by other problems?

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<sup>427</sup> Broca's area is believed to be the main section of the human brain that is involved in language processing, speech production and comprehension. Decreased activity in Broca's area is, for instance, linked to stuttering.

<sup>428</sup> See also Lieberman 2002: 36 "The traditional theory equating the brain bases of language with Broca's and Wernicke's neocortical areas is wrong." Supporters of the modular nature of language processing (Fodor 1987) may object that the functional modularity they posit is different from anatomical modularity, but in practice the ideas are often closely linked.



- c) brain lesions in adults: what are the correlations between language defects and other problems caused by brain lesions?
- d) brain activity: do brain scans reveal areas that are used uniquely for language processing? And if so, does everybody use the same areas for it?

**a)/b) normal and abnormal child language development:** Levy and Kave 1999 give an extensive overview of specific language impairments (SLIs) in children, on which our account is mainly based. SLIs are discrepancies between children's linguistic and non-linguistic development, without such an obvious cause as hearing problems.<sup>429</sup> Although there are quite a few proposals for such forms of impairment, "normal language development is seen as a major predictor of neurological intactness in children (Levy and Kave 1999: 103),"<sup>430</sup> which seems to rule out massive modularity of an isolated language device.

It is hard to diagnose a SLI. It might well be that a certain child has many problems, but that these problems are simply most prominent in the area of language use; the causes of these problems also need not be neurological (they may be social problems, hidden physiological problems etc.). Most proposed SLIs deal with syntactical problems and the absence of (rather complex) syntactical principles in language use such as binding and coreference (work obviously inspired by the wish to find innate components of the Chomskian universal grammar); explanations on the basis of processing deficits rather than grammar per se are, however, also possible. Tomblin and Pandich (1999) further argue that children with grammatical forms of SLIs are just at the extreme low end of the normal distribution of language abilities in general – which is likely insofar as proposed examples of SLIs are usually about aspects of language use that normal children also acquire rather late in their development. Since SLIs are never characterized by a complete lack of syntactical structure, their study seems to rule out again the existence of an isolated general language acquisition device.

**c)/d) Brain lesions in adults and language areas in the brain:** we already discussed the research on the most famous case of language-related brain lesions (Broca's aphasia), which indicated that any strong claim of modularism is probably wrong. In general cases of adult lesions (which focus on syntactic impairment), we find two competing types of explanation (reviewed in Levy and Kave 1999), just as in the case of impairments in children: a) explanations based on *structural deficits* where some principle of grammar – such as the supposed trace-deletion – is violated and b) explanations based on *processing deficits*, where syntactic knowledge as such is unharmed, but working memory or other processing resources

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<sup>429</sup> There are also non-language problems associated specifically with an SLI, such as impairments of timing and rhythmic ability (Alcock et al. 2000), as well as syndromes standing in stark contrast to SLIs (Levy and Kave 1999). Some of these syndromes involve severe general retardation but little effect on language – such as the autistic Savants, Spina Bifida and the well-known Williams syndrome. Williams syndrome is a genetic disorder resulting from the deletion of a contiguous sequence of genes along a certain chromosome, with consequent malformations in many brain areas but not affecting speech (and thus an ideal candidate to prove modularity involved in speech, Pinker 1999, Deacon 1997). However, subtle deficits in language use in Williams patients have recently been noted, see Johansson 2005: 105.

<sup>430</sup> Pinker 2003 also claims that it is never a matter of language being completely lost with other cognitive functions spared, and argues that this favors his view of language as the computational language of cognition. However, one need not believe in an inner language of thought (a view with many problems, see §3.13) if one believes that language-development problems are not isolated from other cognitive problems.

are diminished so that language use is limited to fairly short sentences (Stowe and Haverkort 2003).

The first group of explanations is clearly Chomskian, the second not necessarily so. Levy and Kave (1999) conclude that we have insufficient evidence to decide between the two types of hypotheses; however, more economic explanations such as the second type are to be preferred, especially when one has independent reasons to be highly critical of grammatical rules such as trace deletion.

We also already reported some findings of language abilities in areas other than the left hemisphere – such as, for example, an area in the auditory cortex specifically sensitive to the human voice (Belin et al. 2000). An overview of studies about picture viewing, picture naming, lexical processing, and syntax encoding can be found in Johansson (2005: 109f). Much is yet disputed and unknown, partly because of the already-stated general fact that the same piece of brain is generally used in many different tasks and any given task typically activates several, disjoint parts of the brain.

In sum: *it seems difficult to find a clear correspondence between the functional subsystems identified in the brain and the logical components of the language faculty identified in linguistic theory* (see also Poepel and Hickok 2004).

#### *Mirror neurons and imitation:*

Another region that has been linked to language skills is the *putamen*, which plays a role in reinforcement learning. The putamen is coextensive with the insula, which has been found to contain mirror neurons. Mirror neurons are considered by some to be one of the most important findings in neuroscience of the last decades; they are neurons that fire both when an animal acts and when it observes the same action performed by another (especially conspecific) animal. Some believe that mirror neurons might be very important in imitation and language acquisition, even suggesting that they indicate that vocal speech started as gesture language. Gesture language (manual and facial) is thought to be more iconic (thus lending itself more easily to imitation) than vocal speech.<sup>431</sup>

Mirror neurons were first observed in monkeys (Fogassi and Gallese 2002, Gallese et al. 1996), and some claim they have also been identified in humans and other primates (Rizzolatti et al. 2002). However, there are major differences between the imitative learning of humans and other primates (let alone monkeys), with apes – despite the name – being notoriously bad at ‘aping’, at accurately mimicking the behavior of another ape (Whiten et al. 2004, Tomasello et al. 1993 and see also §2.3 on animal culture). Moreover, the assumption that gesture language is more iconic (as assumption upon which the argument for the role of mirror neurons and imitation in language evolution is built) is not completely true. Although some manual and facial gestures are easier to understand than most vocal sounds we use to convey meanings, either because they have an innate component like facial expressions or because they already have a meaning in certain contexts (such as signs for throwing by pretending one is throwing something), in general gestures can be just as arbitrary. Think of the

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<sup>431</sup> This was originally proposed by Rizzolatti & Arbib in 1998. See also Skoyles 2000 and Théoret and Pascual-Leone 2002 and some of the work of the popular science writer Vilayanur S. Ramachandran. See for instance the latter's article at [http://www.edge.org/3rd\\_culture/ramachandran/ramachandran\\_p1.html](http://www.edge.org/3rd_culture/ramachandran/ramachandran_p1.html), retrieved on 1<sup>st</sup> August 2007, entitled *MIRROR NEURONS and imitation learning as the driving force behind 'the great leap forward' in human evolution*.

Bulgarians, who move their heads horizontally when they mean no and vertically when they mean yes, or the people in North Africa who make (from our point of view) a ‘come-here’ signal when they mean that you should go away (on gestures versus speech, see also §4.3). Further, even if mirror neurons function as it is claimed, they would only function as a useful exaptation for language without accounting for the origin of language; monkeys have mirror neurons, but no trace of language. Finally, to date no plausible neural models have been put forward to describe how mirror neuron activity supports cognitive functions such as imitation. Without such a *causal mechanism*, the suggestions about mirror neurons are nothing but speculative analogies.

In sum: *until better techniques and models are devised, we can put the field of neuroscience aside, as important as it will probably turn out to be in a complete theory of the evolution of language.*

### iii) Language genes

We can be relatively brief about genetic changes that gave rise to *human* speech. Not much is known about them, because the changes to the organs involved in speech in the evolution from apes to humans are mainly neurological. About the genes controlling neurology even less is known than about neurology itself.

Nonetheless, some people speak about ‘language genes’. This expression is rather unfortunate. For speech, many organs are needed: all the genes for human ears, the genes for decoding, the genes for speaking (such as the genes for control of the tongue), the genes for memory, the genes for abstraction, the genes for categorization, the genes for planning and control etc. It is unclear what people mean by “language genes” in general. They probably do not mean the genes for hearing or memory, but what they do mean is unclear unless they clearly define what they mean by language, and even then the trait is probably too complex to have a simple and unique genetic basis (see our general critique on linking phenotypical properties one-to-one with genes and the other way round in §1.23). And even if we found genetic loci linked with language use, we wouldn’t know whether these genes are typical of human language use only; such loci can likely also be found in our nearest relatives, probably with some minor mutations, which could be neutral or could have something to do with the evolution of speech. Only proteomics, the immense project of finding out the (multiple) functions of all the enzymes in our body on the basis of the genomics project, can give us definitive answers. This project is, however, in its infancy and exponentially more difficult than genomics. That is why we can be relatively brief about *specific* genetic loci related to language use.

The best-known examples of genetic loci that are sometimes dubbed language genes are the so-called FOXP2 genes.<sup>432</sup> FOXP2 genes are members of the FOX-group of proteins that play an important role in embryology, regulating the expression of cell elements involved in cell growth, proliferation, differentiation etc. FOX genes are thus not very specific; they regulate large groups of very basic phenotypical properties. The most famous are the FOXP2-genes. They rose to fame because several cases of developmental verbal dyspraxia in a human family, and in an unrelated boy with a similar SLI, have been linked to mutations in the FOXP2 gene.

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<sup>432</sup> Pinker 2003 mentions two other loci linked with SLIs. A good, yet already slightly-dated overview of the body of literature on FOXP2 since the discovery of its relation to speech defects in 2001 is Marcus and Fischer 2003.

These individuals have little or no cognitive handicaps, but are unable to perform the coordinated movements required for speech. There is evidence that this is not just a motor control problem, and that the impairments also include difficulty in comprehension. However, FOXP2-genes (just like other FOX-genes) are not unique to humans at all. They can be found in the genomes of other primates, other mammals such as mice, and even birds and reptiles.<sup>433</sup> This is not strange, because FOXP2-genes are not only involved in speech production (even not in humans) but also in the development of tissues such as brain, lung, and gut. There are only two or three amino acids different in the sequence of human FOXP2 compared to primate FOXP2 (Enard et al. 2002), and only seven compared to zebra finch birds. This led some researchers to postulate that these mutations in FOXP2 form the basis of the evolution of human language (Enard et al. 2002). However, as long as there is no exact causal mechanism that shows how the FOXP2 protein is involved in the embryonic development of brain structures used in speech (which? and how do they work?), skeptics will not be convinced. FOX-genes are involved in general embryological processes that can influence linguistic skills in numerous ways. Of course, since the same proteins can play a role in many different processes and since one gene need not code for just one protein, FOXP2-genes may be involved in both general embryological processes and highly specific linguistic skills. But one needs to show exactly how. It is much more likely that many different genes are involved in the evolution of functions as complex as linguistic skills. Only the proteomics project can bring answers, albeit in the distant future.

Until then, evidence for a genetic basis of language use in general (the existence of which nobody doubts) has to come from family and twin studies, since identifying individual genetic loci with such a complex trait as language use is as yet untenable.<sup>434</sup> Given the fact that after the human genome project we know we possess no more than 20,000 to 25,000 genes that have to account for the entire body and brain, most of which we share with non-speaking apes and even other mammals, it seems unlikely that there will be genes for highly specific innate syntax modules in a putative language organ (Müller 1996).<sup>435</sup>

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<sup>433</sup> See Webb and Zhang 2005.

<sup>434</sup> See Stromswold 2001 for a meta-review of studies on the heritability between normal language abilities and defects. Stromswold finds strong evidence of a heritable component in the existing variation in language skills, even between people with no evident language abnormalities. Others have invoked the non-negligible heritability of verbal IQ (Alarcón et al. 1999), but it is unclear whether verbal IQ is a good measure of language skills and whether it is independent of cognitive skills in general. Stromswold finally warns us that we should not regard language as one uniform device (as the Chomskians do) for which one can imagine that just a few genes code; he stresses that in the case of language, the relation between *genotype and phenotype is not simple* (2001: 705).'

<sup>435</sup> There is nonetheless a heated discussion in the literature about whether Chomsky's universal grammar in its latest form of the Minimalist Program (or earlier versions) can be realized in the brain by genetic hard-coding, a discussion in which Pinker and Bloom correctly remark that often "such arguments depend on inaccurate assumptions about biology or language, and usually both (1990: 707)." Supporting hard-coding are Lorenzo and Longa 2003, who claim that because the minimalist program proposes but a very simple core grammar with basically just a single rule (the iterative application of NP, VP decomposition), it could easily be genetically encoded; however, this rule is just in appearance one rule (more a type of rule), and is in its application quite complex (§3.13). On the opposite side is Worden 1995, who calculates how few *kiloBytes* of genetic information humans have won since their separation from chimpanzees, arguing that this amount of *kB* is far too small to program these rules. However, the translation of genetic strings into digital informational bits is problematic (see §1.32), and thus Worden is also against a pre-programmed grammar. Deacon 2000 further notes that with almost the same genes as mice, humans build a brain three times as large, and that the largeness of the brain – not genes – is the main factor in accounting for human abilities such as the use of syntax.

## §3.22 Paleontology, archeology and the origin of language

Describing the evolution of language and linguistic skills is very difficult, in part because older languages did not leave any written traces until 6000 years ago. Language back then was already as complex as it is nowadays, and it thus does not inform us about the earlier stages of language. The clues of (complex) language use at an earlier date will thus always be indirect. The advantages of the clues used by paleontology and archeology involve their relative incorruptibility. Bones and other remains can be tested and sampled over long periods of time without a problem. These traces can teach us something about the linguistic abilities of men, but of course not much about the *actual* languages spoken.

The traces fall into two categories: hominid skeletons and bones on the one hand, and other remains (such as stone artifacts, wall paintings and fire and burial places) on the other hand. The first category is the topic of paleontology, the second of archeology. Sometimes traces can fall in the purview of both sciences: bones found on islands can tell us about the distribution of hominids around the world (paleontology), but also about archeology. In the time of these bones, people already had to have boats to travel to islands, and such bones can even provide clues about linguistic abilities if one assumes – which seems plausible – that building boats would require at least a rudimentary form of language. We start with the clues of archeology, then we discuss the results of paleontology for the evolution of speech.

### Archeology

The traces of archeology always give *indirect* clues for the existence of language. We clearly find remnants of phenomenon X, phenomenon X requires language, thus in the time of these remnants people spoke a language. The archeologist Iain Davidson provides a good skeptical overview of the many indirect clues that have been adduced by archeologists for language use, and the far-fetched conclusions some researchers have drawn.<sup>436</sup> We will mainly use his synthesizing work and comment critically upon it.

Amongst the adduced archeological clues for language use are i) the remnants of early migrations out of Africa (1.7 million years ago), ii) stone artifacts as supposed remnants of early sea crossing on the island of Flores (0.84 million years ago), and iii) evidence for the use of fire in pre-modern sites and for changing patterns of foraging. However, Davidson points out that i) “hominids have moved out of Africa many times without any sign of symbolic communication”, that ii) “short sea crossings may be possible in some environments without the use of language”, and that iii) it is very hard to demonstrate “hominid making and control of fire from remnants” – and “interpreting the ability to plan hunting activities from the evidence of bones at archeological sites” is virtually impossible (Davidson 2003: 152). It is unfortunate that Davidson does not define here what he understands language to be. He might understand it to be a verbal system of words and syntactical rules, so that cries, gestures and other one-word utterances will not do for him. And indeed, all the adduced phenomena are perfectly imaginable without such a verbal

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<sup>436</sup> Ian Davidson in: Morten H. Christiansen and Simon Kirby (eds.), *Language Evolution*, Oxford UP 2003, chapter 8.

system,<sup>437</sup> although it becomes hard to picture them without some system of gestures or cries. During the hunt, which is a much more complex process than previously thought, chimpanzees give each other many signals. Why would one refrain from calling this a language? (see our discussion in §3.23 on animal communication). However, if one narrows language down to our modern verbal, syntactical language as we did in §3.1, it is clear that archeological evidence will never convince the skeptic that certain hominids already had language.

Nevertheless, although clues from archeological *artifacts* regarding man's linguistic abilities cannot demonstrate *syntactical* sequences of *spoken* words, they can at least indicate the use of symbols (such as decorations on tools, ochre use, early art and burial sites). Even our skeptic Davidson accepts them as "indicators of the sort of displacement and reflexivity made possible by the use of symbols".<sup>438</sup> Unfortunately, it is hard to say when scratches on stone tools (some from 230,000 years ago) are random or meaningless, and when symbolic. Archeologists propose a criterion to distinguish them: "we can infer a convention or code through which meaning might be reorganized if we find depictive or non-depictive marks on objects in repeated patterns, restricted in time and distribution (153)." Regularity is thus taken as an indicator of symbols. However, is regularity really a sufficient<sup>439</sup> condition for meaning? Does every row of strokes mean something? And is meaning here not regarded as representation, whereby the perspective of use is overlooked? (Marks on a jail wall can 'represent' the number of days of imprisonment, but *only* because someone wants to keep track of time and to claim certain rights from it). And even if meaning is thought to be related to the use of the symbols, what was their use? Identification of similar objects with the possibility of determining who owns it? But what if the strokes were simply considered beautiful; would that imply meaning? (If these strokes did not represent anything nor have any use, can we still speak of meaning?) The answers to these questions can sometimes be given: if we regularly find that the number of strokes on an object is 30 or 360 we can infer with some certainty that we are dealing with lunar calendars. But in most cases their meaning, if any, remains unknown.

Evidence for ochre use, art and burial sites is less controversial; they are dated back as early as respectively 70,000 years ago, 32,000 years ago and 43,000 years ago (idem, 155). We can ask the same questions as we did about strokes on stone tools: can we infer their meaning from their mere existence? Ochre can be used to put marks on bodies, and these marks can express many things (tribal membership, rank etc.), but ochre can also be used to mark the possession of certain items. Can we be sure in what context it was used and how it functioned? Is not the world, the context of meaningful relations in which the ochre played a role, long gone - making it impossible to say anything about the function of the ochre? Can we even be sure that ochre use always had a meaning (again: if it does not represent anything or have any function, does it have meaning)? Nevertheless, many uses of ochre are probably

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<sup>437</sup> Henshilwood et al. 2004 nevertheless speculate that art implies subtle thought which in turn is argued to imply syntax, so that art is seen an indicator of *complex* language use.

<sup>438</sup> But see also McBrearty and Brooks 2000: 486 "[a]bstract and symbolic behaviors imply language, but it is doubtful that the point at which they first can be detected coincides with the birth of language." That is, the absence of fossilizable art and the like does not imply absence of language.

<sup>439</sup> It is not even a *necessary* condition: if we found *just one* very clear hieroglyph, would we regard it as a symbol? Or do we indeed need more than one 'copy'? A 'sign' used only once is no sign. When we cut open a tomato and the inner fruit looks exactly like a portrait of someone we know, we don't think of it as a sign (although it can start to function as such).

symbolic, so that ochre use would indeed signify that humans had mastered ‘language’ in the broad sense of the use of symbols.

However, although some people still regard the evolution of language as something that happened in the recent millennia – even holding it responsible for the Neolithic revolution as a whole some 40,000 years ago<sup>440</sup> – most researchers believe that language use evolved much earlier, possibly more than half a million years ago, on the basis of paleontological evidence to which we now turn.

**Paleontology**

The field of paleontology is huge, with every old hominoid skeleton (and especially every old hominin<sup>441</sup> skeleton) being researched thoroughly. Roger Lewin and Robert Foley’s *Principles of human evolution* (2004<sup>2</sup>) provides an excellent introduction to the paleontological side of human evolution. We first address human paleontology or paleo-anthropology in general, along with the stages of human evolution, then we turn briefly to fossil findings related to language use.

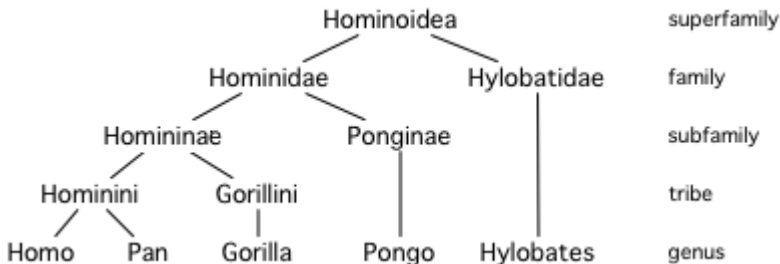
**Paleontological storytelling and the stages of evolution**

Paleontology seeks to interpret fossils. One of the easy traps this field (and evolution as a whole) can fall into is story-telling, describing developments as a journey from there to here, with purposive storylines relating the temporally isolated

<sup>440</sup> See, for a recent defense, Li and Hombert 2002 and the reference therein. Li and Hombert even suggest a biological mechanism (the famous hox-genes) to buttress their saltationistic view of the origin of language. We will argue in §4.3 that this picture is biologically naïve. Note that the existence of the Neolithic revolution, the *sudden* appearance of advanced art and technology in the caves of Europe about 40,000 years ago that supposedly indicates a cognitive leap, is currently under serious criticism: some think that the supposed leap is just an illusion caused by the predominance of European sites in the fossil records, and possibly by Eurocentrism amongst archeologists (Henshilwood and Marean 2003). Since earlier non-European archeological sites have been recently investigated (McBrearty and Brooks 2000), the new arising picture is one of a gradual evolution of artifacts and works of art slowly becoming more complex. See also Bar-Yosef 2002.

<sup>441</sup> Modern research on the ancestors of man has complicated and revised the names of human-like mammals multiple times. In the traditional scheme, based upon typological research concerning adaptations, Humans (Homo) were considered so unique that they were put in their own family of *Hominidae*; Chimpanzees, Gorillas and Orangutans were put in the family of *Ponginae*.

Modern cladistic research using molecular systematics has shown far more similarities between humans and other apes, leading to the following picture of the *Hominoid* superfamily, including the *Hominids* and the Lesser Apes such as Gibbons (formally known as *Hylobatidea*). The family of *Hominids* includes the subfamily of the *Hominines* (with the tribes of the *Gorillas* and *Hominins*) as well as that of the Orangutans (*Ponginae*). Finally, the tribe of the *Hominins* contains the *Homo*s (humans and their direct ancestors) as well as the chimpanzees and bonobos of the genus *Pan* (all names include extant and extinct ancestors). See Lewin and Foley 2004: 15 and 114, with on page 17 a modern picture of the complex, cladistic *species* tree of the genus *Homo* with multiple *Homo* species coexisting for most of the time (such as *Homo Sapiens* and *Homo Neanderthalis*, but also the earlier *Homo Habilis* and *Homo Ergastus* etc.).



developments. The anthropologist Misia Landau (1991) has famously analyzed this tendency, present not only in popular science, but also in the work of professional scientists.<sup>442</sup> Landau has shown that paleo-anthropology is particularly susceptible to being cast in narrative form, because fundamental stages in the evolution of men are regarded as episodes of a single story, giving such theories a degree of plausibility.

Traditionally, the main stages in human evolution have been identified as terrestriality (coming to the ground from the trees), bipedality (upright walking), encephalization (brain expansion in relation to body size) and social life or civilization. These four stages were ordered in multiple ways by paleo-anthropologists before the Second World War, and were staged as the drama of a hero (man) being challenged but finally reigning undefeated (which suited the orthogonalist idea of evolution) – a drama in which each event was but a preparation for the next. For instance, Henry Fairfield Osborn tells us that our ancestors came to the ground so they could walk on the ground where “they became bipedal in order to make and use tools and weapons (...) tool use enabled brain expansion and the evolution of language [manual sign language] (...) thus endowed, sophisticated societal interactions were finally made possible (quoted by Lewin and Foley 2004: 22).” Other early paleo-anthropologists made up a different story on the basis of a different ordering of the stages (see table 3.3).

<b>Proponent</b>	<b>Stage 1</b>	<b>Stage 2</b>	<b>Stage 3</b>	<b>Stage 4</b>
Darwin (1871)	Terrestriality	Bipedalism	Encephalization	Civilization
Keith (1915)	Bipedalism	Terrestriality	Civilization	Encephalization
Elliott Smith (1915)	Encephalization	Bipedalism	Terrestriality	Civilization
Wood Jones (1916)	Bipedalism	Encephalization	Terrestriality	Civilization
Osborn (1916)	Terrestriality	Bipedalism	Civilization	Encephalization
Gregory (1934)	Terrestriality	Civilization	Bipedalism	Encephalization

*Table 3.3: Proposed stories of the main stages (after Landau 1991)*

There are five things wrong with these accounts. Firstly, one thing is sure: not all orderings can be right. Secondly, modern research acknowledges more anatomical, neurological and enzymic transitions that are left out by the four stages (although these still form useful guides). Thirdly, the stories wrongly presuppose that teleological goals are present in advance, that human evolution is proceeding toward them, and that adaptations are accordingly seen as pre-adaptations for future developments. Fourthly (but related to the previous point), these stories see the stages as transitory, whereas in reality there is nothing transitory about them as they can last for millions of years in more or less stable fashion. Fifthly, the story pretends that there is a causal link between the stages, whereas in reality we can doubt whether there exists a true causal link between them at all; the only relation between the stages is then that structures evolved in a previous stage can be exaptations in future stages.

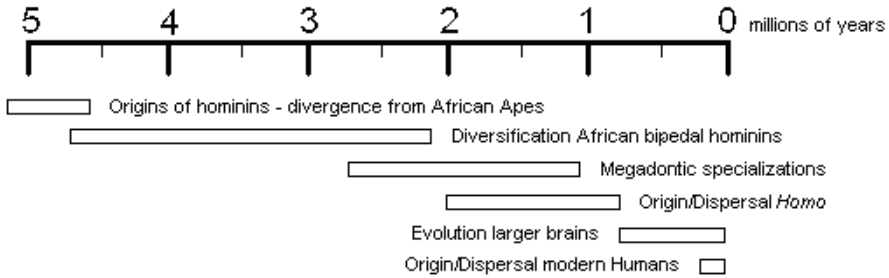
Instead of story-telling, paleontology should craft a correct chronology of events based on empirical evidence, and should be critical of supposed causal links.

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<sup>442</sup> The conclusion of Landau 1991: “Scientists are generally aware of the influence of theory on observation. Seldom do they recognize, however, that many scientific theories are narratives.”



The timeline of human evolution based on modern research would look something like this (after Lewin and Foley 2004: 23):



The many hypotheses as to why bipedalism evolved were already discussed in §1.33. The evolution of mega-dontic specialization deals with the specialization of teeth, which are crucial for the study of human evolution. Teeth are one of the best-preserved parts of the skeleton; they provide information about diet changes (in the direction of being more carnivorous), which in turn relates to changes in energy management in the body and the length and nature of the gastrointestinal tract (which becomes shorter in more energy-rich meat), which has serious implications for the organization of organs in the torso. The adaptive reasons behind larger brains and their relation to the evolution of language were already discussed. In this section we consider paleontological clues for language evolution, about which we can be brief.

Nobody doubts Deacon’s suggestion of ‘coevolution between the symbolic ability and human biology’ (Deacon 1997). Thus once our ancestors did possess speech in any form, they would likely have been under a fair bit of selective pressure for improving the vocal apparatus (Donald 1997), with some claiming co-evolution between the shape of the vowel tract, its motor control system and the human vowel system (Honda and Kusakawa 1997). Nevertheless, the fossil indicators of language use are highly disputed. We can again distinguish between a) fossils indicating speech production and b) fossils indicating speech perception.

**a) Speech production:** in the paragraph on the anatomy of speech production, we discussed the various proposed anatomical indicators of language use and their problems, which indicated that it would be hard to put a clear upper or lower date limit on the origin of language use. There is no clear upper limit<sup>443</sup> because new skeletal findings can easily show that we have to revise our view; there is no clear lower limit because one can argue that the obverted skeletal indicator was adapted for another function first, before it became adapted for speech. The best indicator would certainly be the inner brain structure, but that doesn’t fossilize. The

<sup>443</sup> The cases of the upper and lower limit are asymmetrical. A fossil can be found with a clear skeletal indicator telling clearly that at that moment language use was present, thus fixing a lower date. This fossil, however, doesn’t tell much about how much earlier language use had evolved, thus leaving us in the dark about the upper limit. Another problem for fixing the upper limit on language use is that the skeletal indicators are indicators of speech, whereas it can be imagined that earlier language use was gestural in nature, which would certainly be missed by a study of the indicators discussed thus far. And even if language use has always been vocal, there still remains another problem for fixing an upper limit: proto-speech probably didn’t use the full capacity of the modern speech system with its many producible sounds. Proto-speech could make do with a simpler and more primitive speech system, as in fact no modern language uses the full capacity of human speech (Vanechoutte and Skoyles 1998 point to languages that use as little as 11 sounds out of the hundreds that we are capable of producing).

second best indicator, the descent of the larynx (even though we saw that adaptations other than speech were imagined for this descent), suffers from a similar problem: it doesn't fossilize easily. Until some time ago, many researchers even doubted that Neanderthals (the closest relative of our species *Homo Sapiens*) had a lowered larynx or were capable of speech (Lieberman and Crelin 1971). This fitted nicely with speculation that the sudden extinction of Neanderthals 30,000 years ago was due to their being replaced by the smarter language-using *Homo Sapiens*. And although some people still defend the view that Neanderthal vocal tracts differed substantially from ours (Schwartz and Tatterstall 1996, Laitman et al. 1996), most people are – since the discovery of a well preserved Neanderthal skeleton with a clear hyoid bone (Arensburg et al. 1989) – convinced that Neanderthals had a vocal tract similar to that of humans (Boë et al. 1999), and that they were, anatomically at least, capable of human speech (id., d'Errico et al. 2003, Heim et al. 2002). This implies that language organs were already present in the most recent ancestor of *Homo Sapiens* and the *Neanderthal*, *Homo Erectus*, which would put the lower limit on language use at 500,000 years ago (see also Davidson 2003) – though complex human speech with all its articulatory capacity need not be much older than 100,000 years.

**b) Speech perception:** as there have been but few changes to speech perception, let alone speech anatomy, the skeletal indicators of changed speech perception are also minimal. We noted that one of the few changes to the primate speech perception in humans was its optimization for sounds in the 2–4 kHz range, which is dependent on minor changes to the ear ossicles, the tiny bones that conduct sound from the eardrum to the inner ear. Interestingly enough, these changes to ossicles were discovered in 400,000-year-old fossils from Sima de los Huesos in Spain (Martínez et al. 2004), long before *Homo Sapiens* conquered Europe. These fossils are probably bones of Neanderthal ancestors, the *Homo heidelbergensis*. This discovery sets a firm lower limit on the age of speech at 400,000 years ago; if it is true that the fossils are part of the Neanderthal lineage, this again pushes the origin of speech at least back to the common ancestor we shared with Neanderthals 500,000 years ago (see also Johansson 2005: 88).

### §3.23 Animal language

*In this section, we will discuss the uses animals make of signs and the different ways in which linguists and ethologists have generally looked at them. We discuss three genera of animals in more detail: bees, birds and primates. We summarize the results in a table that shows how many of Hockett's design features of human language can be distinguished in the 'language' of other animals, taking into account the differences between animal communication in the wild and animal communication as learned by humans in the lab.*

Animals use signs. A swallow uses the polar star (as a 'natural sign') to direct itself to the north. Many rodents are brown or otherwise camouflaged, so they are hard to detect in sandy environments. Some beetles in the Amazon have telltale bright colors that, anthropomorphically speaking, send a signal loud and clear ("do not eat me, you will regret it") – whether they are truly poisonous or not. A butterfly wing can imitate the eye of a large mammal to scare off small predators, a phenomenon known as mimicry. Chameleons actively adapt their color to their surrounding to remain

unnoticed. A horse stamps his feet to scare away his enemies. A velvet monkey effectively tells his fellow monkeys which predator is closing in. A forager bee signals to other foragers where to find flowers and other food sources, and this signal contains information about the direction, quality and distance of the food source. Male birds sing to attract a mate, varying their songs using complex mathematical algorithms. Higher primates can be taught to use ideograms, and some argue that they can even combine these symbols in original ways.

The first example of the swallow seems to have much less in common with human language than the final example of the ideogram-using ape. First animals use *external objects as signs* to guide themselves (the swallow). Then they *passively broadcast true signals* (the poisonous beetles). After that come animals that *passively broadcast false signals* (the non-poisonous beetles, the butterflies). The chameleon seems a bit more active, since it is not broadcasting the *same* signal (color) all the time, but changes it depending on the surroundings. He does this to remain *unnoticed*. The horse, birds and bees, on the other hand, want to be *noticed*. The horse always uses the same (collections of) signals, and so do velvet monkeys that functionally refer to entities in the world; birds, on the other hand, vary their songs. And the higher primate's use of ideograms (see discussion below) seems to be a partly free, creative use in which different ideograms are combined into new formations.

The above series of examples suggest a common axis along which the different forms of animal sign use can be placed, from passively receiving and using signals to actively using signs (first not to be noticed, then to be noticed), using more-refined signs for referential and other relations. However, there doesn't exist a *single* criterion needed for such an axis to compare all forms of sign use. The forms of sign use can be distinguished in many ways, and two forms may be equal on one criterion but radically different on another. This is already clear from the above series, that distinguishes i) receiving signals from also broadcasting signals, ii) passively broadcasting signals from actively broadcasting signals, iii) sending signals to be noticed or sending signals to remain unnoticed, iv) having a fixed repertoire of signals or not, v) using signals for selfish purposes (defense etc.) or to help conspecifics. In section §3.1 we discussed C.F. Hockett, who gave criteria to distinguish human and animal languages. We will use his account to compare the signal use of three genera of animals whose signal or language use has been studied most extensively, distinguishing between their communication in the wild and in the lab. With the help of Hockett's work, we attempt to cut across the two paradigms on animal language, that of a) linguists and that of b) ethologists, with the former focusing more on syntactic properties of language and the latter more on pragmatic, functional properties of language use.

**a) The paradigm of linguistics:** many *linguists and philosophers* deny that animals can speak a *language*. For them a "definition of language that rests on its social or functional aspect is unacceptable (...) [and language should instead be defined] independent of the purpose it serves, (...) equating language with grammar – the mental system that allows human beings to form and interpret the sounds, words and sentences of their language (Drobovolsky 1997: 576)". Chomsky is of course one of them, and indeed he empathetically dismisses all research on animal language.<sup>444</sup> Other linguists who take a more pragmatic or social-functional approach to language,

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<sup>444</sup> See Chomsky, quoted in Belsack et al. 1999: 35, linking 'speaking a language' with the necessary and sufficient condition of 'having a language faculty': "But the question whether it [an ape] has a language faculty is a meaningless question and therefore nobody should talk about it."

however, are willing to speak of animal language; their position comes closer to that of the ethologists.

**b) The paradigm of ethology:** *ethologists* take the social and functional aspects of language or sign use as central to their discussion. Hauser (1997) gives the best and most thorough review of animal communication from an evolutionary perspective. The research in this field has found many species, including most mammals, that have their own means of communication. Early ethology regarded these forms of communication as simple emotive utterances such as ‘back off’ or ‘come here, I’m ready to mate.’ In 1978, Dawkins and Krebs published a famous article that changed this perspective by emphasizing the costs of broadcasting signals: they emphasized not only the cost of maintaining organs for communication and possible learning of signals, but also the fact that an animal reveals valuable information about itself (location, condition) to predators and conspecifics by sending signals – information it might have done better to keep to itself. It seems it is better to remain silent unless one can balance the costs of signaling with fitness-enhancing benefits. Dawkins and Krebs call this *manipulation* using signals.<sup>445</sup> Since this paper, more complex systems of communication have been found, systems that cannot be interpreted as simple emotive utterances. There is for example the velvet monkey system of ‘functional referential’ alarm calls, which was discovered in 1980 and which we already discussed in §3.13. Since Seyfarth’s work on the velvet monkeys, such functional calls have been discovered in numerous other species such as chickens (Hauser 1997), passerines (Marler 1998), marmots (Marler 1998), squirrels (Blumstein and Armitage 1997) and many species of primates (Marler 1998, Hauser 1997).<sup>446</sup> There is also some research on the ways in which signals are *combined* in the wild or in the lab, and on whether we discover syntax-like properties; the results thus far are, however, inconclusive (see below).

The perspectives of ethology and of traditional linguistics both have a point: language use is pragmatic, but human language also has a complex inner structure that has no direct pragmatic value. When we speak of animal language, we will be much more likely to find pragmatic elements than elements of a complex inner structure. It then becomes a matter of definition whether one wishes to call animal languages real languages. Hockett’s list, which contains both linguistic and ethological criteria, tries to steer a middle course. Using his list, we will see that certain animal

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<sup>445</sup> The exact details of Dawkins’s and Krebs’s account, refined by Dawkins in his *Extended phenotype* (1982), are complex. Dawkins tries to reinterpret signaling from the perspective of genes (the genes ‘manipulate’ animals to warn their conspecifics, even if this puts the callers into danger, because this benefits copies of the same gene in the gene pool of the population, as in kin selection), or from the perspective of reciprocity at the level of individuals (as Trivers 1971 had already done).

Noble 1999 similarly argues more thoroughly that under reasonable assumptions behavior will only evolve if both sender and receiver benefit from it (not necessarily directly, but through its iterative use). A serious problem for cases of the evolution of signal use by kin selection or reciprocal altruism is how to keep callers honest (Gouzoules and Gouzoules 2002). Why not cry ‘eagle’ when your conspecific is sitting near a rich food patch, and steal it when he runs away? The standard solution in the animal kingdom is to make sending signals costly (just as in sexual selection, see §1.5), so that cheating will not benefit the caller.

<sup>446</sup> The alarm calls nevertheless differ in how automatically or voluntarily they are uttered, and in how they function exactly: the velvet monkeys seem to ‘functionally refer’ to eagles and leopards (with some degree of voluntariness); chickens, on the other hand, use calls for foxes and hawks depending on predator elevation. A hawk on the ground usually elicits the ground-attack call normally used for foxes rather than the air-attack call normally used for hawks, and researchers have even (automatically) elicited the air-attack call from chickens by flying a stuffed raccoon overhead. This makes it dubious whether anything remotely resembling semantic labeling is involved in chicken calls.

languages resemble human languages to a certain degree, with some animal languages closer than others. We will concentrate on bees, birds, monkeys and apes.

### **Forager bees**

*Forager bees*, and only these bees, communicate with each other by making special movements, which humans call *dancing*. This dancing conveys information about the location of a food source, its quality and its distance from the hive. “Distance is conveyed by one of three different dances performed on the wall or floor of the hive (some species have only two different dances, and so may be said to have a different ‘dialect’). In doing the round dance, the bee circles repeatedly. This indicates a food source within five meters or so from the hive. The sickle dance indicates a food source from five to twenty meters from the hive. It is performed by the bee dancing a curved figure-eight shape. The tail-wagging dance indicates distances farther than twenty meters (see Dobrovolsky 1997: 561).” The location is communicated only in the last two dances, where it is needed most because of the larger distances involved. The angle of the “open side of the sickles dance’s figure eight or the angle of the wagging path during the tailwagging dance indicates the direction of flight (idem).”<sup>447</sup> And finally, the quality of the food source is indicated by the intensity of the dancing and the number of repetitions. Young forager bees make more mistakes than others (about direction and quality), and they are corrected and thus trained by older bees; bees that keep giving incorrect information are killed (Riley et al. 2005).

Dancing is not the only way in which bees communicate: they also carry back traces of food odors, which aid other bees in their search. Moreover, a forager bee leaves a trace of a pheromone, specific to her hive, at the site of the food source; this will direct her fellow foragers to the exact location (a small minority of ethologists still see these odors as the main or even the only signs guiding bees). Different pheromones also have a role in other forms of communication in the hive – for example, about the number of available forager bees and the number of new bees that have to be hatched. Some researchers distinguish as many as 15 different pheromones and related *actions* (Free 1987). Finally, in a process called trophallaxis, bees exchange food in order to convey information about the quality of a food source, as well as about the temperature, the water demand and the condition of the queen (Thomas Sebeok 1990).

### **Birds**

Different species of birds have very different ‘language’ abilities. It used to be thought that parrots were stimulus-bound mimics, merely ‘parroting’ responses to external stimuli as a result of repetitive conditioning. However, I.M. Pepperberg (2000) was able to train her parrot Alex to make true requests: the parrot indicates that he understands the words he utters by rejecting rewards he has not requested. He is able to identify colors, numbers up to six, and shapes based on the number of corners an object has. For instance when he is asked about an object “what color?,” he can respond “green,” “blue,” etc. He has also demonstrated understanding of the

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<sup>447</sup> Actually, this is the case when the dance is performed on a horizontal wall. When the dance is performed on a vertical wall, the angle of the dance relative to the sun – by whose angle bees ordinarily orient themselves in flight – indicates the location of the food source. The exact details depend, moreover, on the species of bee. See the outdated, but still useful Karl von Frisch, *The Dance Language and Orientation of Bees*. Cambridge, MA: Harvard Univ. Press 1967. Von Frisch was awarded the Nobel Prize in physiology of medicine in 1973, together with Konrad Lorenz and Nikolaas Tinbergen, for their work in founding ethology. See for more recent work Dyer et al. 1991 and Riley et al. 2005.

concepts ‘same’ and ‘different’. For example, when presented with a blue triangle and a green triangle and asked “what same?” Alex is able to respond “shape.”<sup>448</sup> Alex is thus able to label individual objects, use kind words (blue, round), use hierarchical concepts (color → blue, green, red, etc.), and use complex predicates for comparison tasks. Especially interesting is that Pepperberg and her associates do not use standard conditioning techniques (as earlier, less successful parrot experiments did), but instead try to mimic the social model-rival interactions of these parrots in the wild.

However, what captive birds can learn should not be equated with behavior in the wild. The vocalization of birds in the wild can roughly be divided into *calls* and *songs*, although this division is somewhat arbitrary. Some authorities focus on the *structure* of the sound sequences (Catchpole and Slater 2008, Marler and Slabbekoorn 2004, Kroodsma 1997), others on their functionality (Howell 1994). From the first perspective, calls are short burst of sounds or simple patterns of notes, whereas songs are lengthy, elaborate patterns of mostly pitched sounds. From the second perspective, calls tend to serve as alarms or to keep members of a flock in contact, whereas songs are used to claim territory or attract a mate (Howell 1994: 231). Note that songs are usually limited to males (perhaps one could even distinguish songs from calls by searching for the exclusively male vocalizations).

An example of a call is the crow’s cawing, which is used in general to signal danger. When a crow hears it, it flies up to a tree if it is on the ground, or else flies higher in its tree. In some other birds, specific calls are associated with specific activities. The danger calls of small birds are usually thin and high-pitched – sounds hard to locate – so that they are safe from predators when uttering them. Flight calls, on the contrary, are short and crisp and easy for other group members to locate. *The ‘form’ of the bird sound (high-pitched or low-pitched) is thus not arbitrary, but evolved to fit its meaning.*

Calling is a year-round phenomenon; singing does not happen in the winter, since it functions to establish a territory to attract a mate. It follows that bird song is unique from species to species, and even varies to some degree from bird to bird within the same species, since its purposes require species and individual recognition. Again the ‘form’ of the bird song thus evolved to fit to the meaning (an individual’s claims), albeit less obviously than in the case of calls. There are birds that know as many as 3000 kinds of songs (such as the brown thrasher), but individual songs are, as far as is known, not related to different meanings or actions. In many species, it further appears that although the basic song is the same for all members of

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<sup>448</sup> See the website of Irene Pepperberg, with some excellent videos of Alex in action: [www.alexfoundation.org](http://www.alexfoundation.org). Alex died in November 2007; the New York Times published an obituary for him.

Pepperberg’s tests exclude the possibility of the so-called Clever Hans effect, named after a horse called Hans who supposedly mastered arithmetic. Hans’s owner, Wilhelm von Osten, gave public demonstrations of his horse’s abilities; at such demonstrations Von Osten gave Hans a question from arithmetic (for instance 3+5), and Hans would tap out the correct answer with his hoof. Hans even did this when his owner was turned with his back towards him and only the public was looking at him. Experimentation by a skeptical scientist, however, proved that Hans was not so much a clever thinker as a careful observer: the horse perceived cues, either from his owner or from the public (even if he or they did not intend to send them) when the number of taps was equal to the answer of the problem in arithmetic. Amongst the cues were subtle head nods (von Osten involuntarily moved his head very slightly when the answer had been reached) and an increased sense of expectation in the audience. Animals can observe movements far outside the perceptual range of humans, movements they can learn to associate with the correct answer. Note that when Hans was blindfolded he could not perform his miracles.

This Clever Hans phenomenon is also known under the name of *dressage*: a type of interaction between trainer and animal that depends on the animal’s interpreting subtle cues given by the trainer.

the species and thus innate (Whaling et al. 1997), young birds learn the details of their songs from their fathers, and this allows variations to build up over generations, in a form of dialect (Baker 1996, Wiener 1986). Finally, birds have in common with man a so-called *critical period*: if children or birds do not hear language in the first phases of life, they will never acquire more than rudimentary language skills. Some researchers try to model patterns in the song of birds by grammar-like rules, as Okanoya (2002) did for Bengalese finches. However, as we have no common ancestor with birds since the dinosaurs, the singing of birds can only provide a *comparative* perspective on the origin of language. It does not reveal much about the evolution of our own language abilities; for that we turn to apes.

### Monkeys and apes

The literature on monkeys and apes and their language skills is abundant. The results for each species are a little bit different, and the greatest difference is found between monkeys and apes.

We already described the case of the velvet monkeys. Their functional referential system of cries is different from most other monkeys, in that it needs training and input from the parents to be perfected. These cries are not iconic, as they do not mimic the sounds of the predators, but instead seem more or less arbitrary. In most other monkeys, the cry system appears to be determined genetically – a fact established by raising newborn monkeys in isolation. This system can include as many as 25 different basic patterns, as in the case of the Bonnet macaques. Scientists give descriptive labels (such as whoo, rattle, growl, whistle and bark) to these vocalizations. These basic patterns are described as grading into each other. It is also claimed that they occur in combinations. There is no undisputed evidence, however, that these combinations mean anything novel when they occur.<sup>449</sup>

Apes, such as *gibbons*, *orangutans* and *chimpanzees*, use many manual and facial gestures. Even untrained laymen can easily interpret them correctly: these gestures are highly similar to our own (and have thus remained the same in evolution). The primate's vocal communication, however, does not seem to resemble human communication much.

The *gibbons*, who live in family groups, are known among primates for their duetting – singing between two members of the species – which serves to maintain 'spacing among territories'. Solitary orangutans make as many as 16 different distinct vocalizations, amongst them the male's loud call (lasting for up to three minutes) to proclaim his territorial interests. High-ranking males approach these calls, while low-ranking males stay away from them.

The few studies on chimp vocal behavior in the wild have not been able to find much evidence of *discrete* vocal signs resembling the discrete human words that can be made into novel combinations (see Arcadi 2000 and references therein). For the most part, the communication system of chimps is made up of groups of graded vocal signs called grunts. In the wild, chimpanzees are known to make three different *types* of grunts, which make for as many as 34 distinct calls: a) grunts made in the presence of a chimpanzee's favorite food source, b) grunts uttered when greeting or when a chimp is excited about something (called pant hoots) and finally c) pant-

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<sup>449</sup> Zuberbühler 2002 reports that he has found evidence that Diana monkeys react to certain combinations as if one call were a modifier of the other; the monkeys reacted quite differently to the 'main' call depending on whether it had been preceded by the modifier call. However, to claim that non-human primates thus can comprehend "simple syntactic rules (2002: 298)" is much too strong, as the calls may simply be fixed combinations.

grunting by lower-ranking animals in the presence of higher-ranking animals, apparently to acknowledge a subordinate status in the hierarchy. Thus grunts appear to display a certain level of sign-symbol correspondence, but some also controversially claim that compound calls have some syntax-like structure (Ujhelyi and Buk 2001). There are, however, major methodological problems involved in testing whether a species uses a language-like communication system, especially because we cannot take for granted that it would resemble our vocal speech.

The projects raising the most controversy, however, have been those in which primates are taught human language using vocal sounds, gestures or iconograms. The most famous projects are:

- Chimpanzees: Rumbaugh et al. with Lana, Sherman and Austin (1975), Gardner and Gardner with Washoe (1984, 1982), Terrace et al. with Nim Chimsky (1979);
- Bonobos: Savage-Rumbaugh and associates with Kanzi (1986);
- Gorillas: Patterson and associates with Koko (1990, 1981).

Pioneering work was done by Allen and Beatrice Gardner between 1965 and 1972, with the chimp Washoe. The Gardners were his 'adoptive parents,' teaching him American Sign Language from an early age. Washoe learned to reproduce a large number of signs and appeared to use them for communicative properties. The Gardners claim they took all sorts of precautions before acknowledging Washoe's gestures to be true signs: the use of a sign had to be reported by three independent observers and had to occur in an appropriate context without prompting. It, furthermore, had to be used at least once a day for 15 consecutive days before it was deemed to have been acquired. The signs Washoe learned were not just for specific objects (this specific doll), but also for – what we would call – similar objects or categories. The Gardners' most important claim was, however, that Washoe communicated intentionally with signs in a creative manner. Her first novel creation is reported to be WATER DUCK on seeing a bird.<sup>450</sup>

At the same time Sue Rumbaugh, now Sue Savage-Rumbaugh, taught three chimps (Lana, Sherman and Austin) how to use abstract tokens on a computer keyboard. These tokens, called lexigrams, allowed the animals to communicate with her with a degree of success similar to Washoe's; the apes learned to produce large numbers of more or less appropriate strings of tokens, which was taken as evidence of their linguistic prowess.

These early experiments were, however, harshly criticized by Herbert S. Terrace, who argued that they were prey to Clever Hans effects; Terrace has a point, as early experiments were rather lax in their methods and optimistic in their interpretations. Terrace tried to do more rigorous testing (blind controlled tests etc.) with his own experiment: teaching sign language to the chimp Nim Chimsky (whose name is of course a pun on Noam Chomsky). Nim learned many ideograms, just like Washoe. Attention was particularly focused on Nim's ability to make different responses to different *sequences* of signs ('dog bites man' versus 'man bites dog') and to produce different sequences in order to communicate different meanings. However,

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<sup>450</sup> Another ape, the gorilla Koko, made novel combinations such as (amongst many others) MILK CANDY for a rich tapioca pudding, NOSE FAKE for a mask, PICK FACE for a tweezers (Patterson and Cohn 1990, Patterson 1981). Unfortunately, the story of Koko's language acquisition is not well-documented, so that Terrace's critique (see below) still applies. There are reports of gorilla groups in captivity that spontaneously develop a system of gestural communication (Pika et al. 2003).



the results were here not as impressive as had been reported in the Washoe project (Fouts 1997). Even though Nim did learn 125 signs, Terrace concluded that he hadn't acquired anything the researchers were prepared to designate worthy of the name 'language' (as defined by Noam Chomsky).<sup>451</sup> Although he had learned to repeat or 'parrot' his trainers' signs in appropriate contexts, Nim's use of language was said to be strictly pragmatic; it was a means of obtaining an outcome, unlike a human child's language, which can serve to generate or express meanings, thoughts or ideas. There was nothing Nim could be taught that could (supposedly) not be taught equally well to a pigeon, using the principles of operant conditioning. Therefore Terrace strongly discredited the results of the Gardners' Washoe project.

New experiments by Sue Savage-Rumbaugh and associates (1994, 1985) took the lessons of Terrace to heart by insisting on more rigorous testing methods. Two of her new innovations were a) to teach chimps language in a social setting, with chimp pupils Austin and Sherman living together with other pupils and b) focusing on learning and using signs instead of producing signs, as comprehension always extends further than production (see §3.24). Savage-Rumbaugh was in her turn highly critical of Terrace. She claimed that his experiments based upon operant conditioning used a double standard, as nobody expects children to learn a language by operant conditioning alone.<sup>452</sup>

The most impressive experiment of this time period (again with lexigrams) is the one in which Austin and Sherman are given a novel food item and one of them chooses a token to use for this item (Savage-Rumbaugh and Lewin 1994: 79f); pointing back and forth between the token (on a keyboard) and the item, they establish jointly the referent and symbol, and from then on both of them used the agreed-upon token for this food item. This is hard not to interpret as a case of jointly assigning reference. Later experiments also showed that chimps could use the ideograms to communicate about absent objects, including objects not recently seen, or outside the context in which they were last seen, which is clearly different from the typical cued, context-based productions of animals (especially in operant conditioning experiments). This breakthrough was, however, criticized for not being proof of the linguistic abilities of apes; it still did not show that apes can understand syntax.

Another breakthrough came by accident, also in the lab of Savage-Rumbaugh (1993, 1985) but this time with bonobos, a species of primates just as closely related to us genetically as chimpanzees but with a stronger social structure. Rumbaugh tried to teach a bonobo female the lexigrams, but the animal failed to learn anything notable. However, her adoptive son Kanzi (who accompanied his mother to the sessions) did learn, and in a rather surprising way: although he lacked explicit instruction, Kanzi began competently using the lexigrams on a day when his mother was away, thus becoming the first observed ape to have learned aspects of language naturalistically rather than through direct training.<sup>453</sup> Within a short time, Kanzi had mastered the ten words that researchers had been struggling to teach his adoptive mother, and he has since learned more than 300 more. The symbols refer to

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<sup>451</sup> Pinker and Bloom 1990 still maintain this position upon the basis of the work of Terrace.

<sup>452</sup> See for instance Pinker "Though artificial chimp signaling systems have some analogies to human language (...), it seems unlikely that they are homologous. Chimpanzees require massive regimented teaching. (...) This contrasts sharply with human children (1994: 3)."

<sup>453</sup> Later experiments with *infant chimps* (to exclude species-specific effects), in learning environments similar to Kanzi's (Brakke and Savage-Rumbaugh 1996), showed similar good results with learning in an environment with conspecifics instead of in more formal environments.

familiar objects (yogurt, key, tummy, bowl), favored activities (chase, tickle), and even some concepts considered fairly abstract (now, bad).<sup>454</sup> He, however, *understands* over 3000 spoken English words, about ten times as many as he can produce. When he hears a spoken word (through headphones, to filter out nonverbal clues)<sup>455</sup>, he points to the correct lexigram.

A spurt in the number of words learned came about when Kanzi learned some more general categories such as ‘fruit’ instead of ‘apple’ or ‘pear’; having mastered the lexigram for ‘food,’ he understood new lexigrams for food items such as ‘berries’ much faster (a process similar to the one we often find in human child language acquisition, see §3.24) – even without visual contact with the object (Lyn and Savage-Rumbaugh 2000). Kanzi also learned to understand the meaning of regular combinations of symbols, called ‘proto-grammatical’.<sup>456</sup> In tests, he demonstrates by his action that he understands novel combinations such as ‘get the carrot from the fridge’, including sensitivity to word order reversals (‘put the juice in the egg’ versus ‘put the egg in the juice’) with a level significantly higher than chance. This is marginally *better* than the performance of a two-year old child in the same environment (Savage-Rumbaugh et al. 1993), which is also true of Kanzi’s production (Belsack et al. 1999).

Deacon (1997) and others argue that the experience with Kanzi is evidence of a critical period in language acquisition of apes, similar to that of humans (see §3.24). This idea is buttressed by the fact that of the nine apes reared by Savage-Rumbaugh, the four that learned language best were exposed to language before the age of 2.5 years. Deacon constructs a perplexing problem for the Chomskians on the basis of this assumption. The critical period is thought by Chomskians to support the idea that an innate language acquisition device takes time to learn the correct settings of the language of the community in which a child is raised. If apes display a critical period, this would be evidence of their having a language acquisition device as well. However, why would they possess a language acquisition device “dormant for millions of years until human scientists came along (Johansson 2005: 133)”, as they do not seem to learn a language in the wild? From an evolutionary point of view, the existence of such a device would be nonsensical. Therefore, it seems reasonable to argue that the common ancestor of chimps, bonobos and humans already had (some) general language learning abilities and/or a plastic brain, and that language learning does not imply a (human-specific) language acquisition device.

Many linguists nevertheless still hold that bonobos and chimps are simply very skilled at getting what they want, and that their abilities do not constitute language use.<sup>457</sup> This might be a true conclusion regarding chimps’ or bonobos’

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<sup>454</sup> The Smithsonian magazine’s November 2006 issue offers a detailed article (‘speaking bonobo’) on Kanzi’s life and current linguistic abilities. ([www.smithsonianmagazine.com](http://www.smithsonianmagazine.com))

<sup>455</sup> It is already remarkable that Kanzi recognizes voices through headphones as human commands.

<sup>456</sup> Proto-grammatical two-word sentences (see §3.24) do not rely on abstract word order (such as that of subject and predicate-sentences) – with, for instance, object words first regardless of their thematic role. Instead, their word order is determined by constituent roles (as by agent-action, action-patient patterns etc). Skeptics should visit the website [www.greatapetrust.org](http://www.greatapetrust.org), North America’s largest great ape sanctuary, with free videos of Sue Savage-Rumbaugh and Kanzi. Kanzi shows by his actions that he correctly interprets orders such as ‘put the soap in the water’ – sentences he has never heard before, according to Savage-Rumbaugh.

<sup>457</sup> See, for such a categorical denial of language abilities of animals, Smithsonian magazine November 2006: “[linguist Geoffrey Pullum:] I do not believe that there has ever been an example anywhere of a nonhuman expressing an opinion, or asking a question. Not ever. (...) It would be wonderful if animals

syntax *productions* that are often random, such as Nim Chimsky's longest recorded utterance: "give orange orange me give eat orange me eat orange give me eat orange give me me give give me you (Terrace 1979)". However, it does not seem to be true of the latest experiments on *understanding*. What more could constitute speaking a language than understanding (proto)-grammatical sentences and reacting accordingly? Are linguists looking for something that is not there, equating 'understanding' with 'mental intuition'?

Linguists seem to raise their standards higher and higher as they distinguish human language from animal language. The latest proposal by Chomskians is that human language is unique because it can produce embedded sentences (also confusingly called 'reflexivity').<sup>458</sup> To open a gap between human and animal language, Chomskians have to call this the *fundamental* feature or *essence* of human language. That is quite unnatural: it is perfectly imaginable that humans might never utter relative clauses. Of course, from our point of view, this situation would be a little uncomfortable and we would lose some of our communicative power; yet losing the possibility of embedded clauses is much less problematic than losing the use of grammatical tense, let alone the use of adverbs – although we can still imagine a language without grammatical tenses<sup>459</sup> or without adverbs. That losing embedded clauses is not such a major problem is made even more clear by the fact that children (until the age of 10 or 12, on average) do not generally understand relative clauses. They are confused by sentences such as "the general saw the soldier, who wore a blue hat" by being uncertain who wore the blue hat: the soldier or the general? Moreover, pidgins (see below) do not use relative clauses, and many creoles and non-Western languages do not mark them; yet creoles and non-Western languages are full-fledged languages. In some languages, relative clauses were only introduced due to writing. Hence, by postulating that the fundamental or essential aspect of language is its possibility to generate embedded clauses, one excludes a great deal of linguistic communication that we would usually regard to be human language. Finally, (almost) any relative clause can be reformulated into two separate sentences with no or minor changes in meaning: "the soldier wore a blue hat and the general saw him".

It thus seems that Chomskians, by desiring to stick to their credo that human language is incomparable to any animal communication, have sought a feature unique to human language (indifferent to how inessential and remote it is), in order to postulate that this feature makes human language unique among all communication. This practice resembles that of an ancient biologist troubling himself to find the essence of man, and finally finding it in the remote characteristic that "man is (the sole) biped without wings".<sup>460</sup>

The results of this comparison can be summarized in table 3.4 below, using Hockett (1968) (see also Drobovolsky 1997: 583) plus an added 17<sup>th</sup> feature of proto-

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could say things about the world, as opposed to just signaling a direct emotional state or need. But they just don't."

<sup>458</sup> Some philosophers hold this position too, since embeddedness in the sense of relative clauses ('that sentences') seems to imply an understanding of what full sentences or propositions are – the truth-bearing properties of which are seen as the hallmark of human language, see §3.13.

<sup>459</sup> Instead of 'I went to the station' we would have 'earlier I go to the station', etc. The origin of grammatical tense markers often comes from grammaticizations of temporal or content words.

<sup>460</sup> In the language of the Schools this equals using a *supinum* to name an *essence*. Of course Chomskians can say that not embeddedness *sec* (which can be resolved by separate sentences and adverbs etc.) but *indefinite embeddedness* is a key characteristic of human language. However, in actual practice the embeddedness cannot be infinite: a sentence with a level of embeddedness of 4 or more (which is a long way from indefinite) is already hardly understandable.

grammar, between square-brackets, i.e. [], exceptions for lab experiments compared to behavior in the wild:

Design feature	Bees	Birds	Nonhuman primates	Humans
1. Vocal/auditory channels	No	Yes	Yes, but limited (no for lexigrams)	Yes
2. Broadcast transmission and directional reception	Planar broadcast, unidirectional reception	Directional broadcast, directional reception	Directional broadcast, directional reception	Directional broadcast, directional reception
3. Rapid Fading	No	No	No (Yes for lexigrams)	Yes
4. Interchangeability	Yes, between foragers	No, only males sing	Yes	Yes
5. Feedback (in the process of learning)	Yes, in learning process	?	Probably	Yes
6. Specialization	Yes	Yes	In part	Yes (No for sign language)
7. Semanticity	Yes, very limited	Yes, limited	Yes	Yes
8. Arbitrariness	Yes, for expressing distance	Yes, though highly adaptive	Limited confirmation, selective adaptivity	Yes
9. Specialisation	Yes	Yes	In part	Yes (No for sign language)
10. Displacement	Yes	No for songbirds, possibly yes for parrots	Yes	Yes
11. Productivity (creativity)	Yes, very limited	Possibly	Possibly	Yes
12. Duality of patterning	Yes, but dual patterning of form only	No	No	Yes
13. Tradition	Possibly, but highly limited	Yes, limited	Possibly	Yes
14. Prevarication	No	Possibly	Possibly	Yes
15. Learnability	No	Possibly	Yes	Yes
16. Reflexiveness (embeddedness)	No	No	No current evidence	Yes
(17. Fixed combinations/proto-grammar)	Maybe fixed combinations	Maybe fixed combinations, understanding proto-grammar by parrots	Maybe fixed combinations; maybe understanding proto-grammar	True syntax

Table 3.4: Animal language in relation to human language

Given this table, it makes little sense to categorically affirm or deny that animals speak a language. One could of course opt for the one and only factor in this table unique to human language, the embeddedness, and say that animals do not have a true language because their language shows no sign of embeddedness – but we just criticized this idea. Of course, no ape will ever produce anything like *Hamlet* (partly because there are no or almost no real traditions in primate culture, see §2.3), but animals come closer to our language abilities than one might have previously thought.

## §3.24 First language acquisition

If Ernst Haeckel's formula 'ontogeny recapitulates phylogeny' were true, looking at the way in which children acquire language might teach us a great deal about how the human species gained their linguistic abilities over the course of many years (just as studying the phases of a human embryo would teach us something about the evolution of ancestral mammals towards us). However, the formula (also known as the recapitulation theory) is of course not generally true. New evolution does not simply build upon old elements (like building an extra floor to an intact building), but actively transforms old elements (like restructuring the foundations and driving new piles into the ground, upon which the building will henceforth rest); when a new entity develops it starts from a new basis which has no longer everything in common with the old structure (i.e., when builders now only build houses by driving piles into the ground instead of older ways to lay foundations). Nevertheless, first language acquisition can give us some clues about how language evolved. This long section intends to show *what biases exist in language acquisition (possibly due to evolutionary path dependencies or to selective pressures), how a simple language can work (so that we need not explain how all at once a complex language evolved but can start from a simple proto-language in Darwinian evolution), and how we should be careful not to attribute our grammatical categories to child speech (or similarly proto-linguistic forms in language evolution)*. We will start with a general overview of the field of first language acquisition, and then turn to a chronological discussion of empirical results about the phases of actual acquisition, critically assessing them in light of philosophical accounts of meaning.

### **The history and the paradigms of the study of first language acquisition**

Apart from some isolated 'studies' of (for instance) parents writing down the linguistic progress of their own children, research on first language acquisition started after the Second World War. It was first a sub-branch of developmental psychology, a field that had recently established itself as an *empirical* discipline, separate from psychology proper and from pedagogy (Piaget 1955, 1953); later (from the seventies onward) it became its own discipline.<sup>461</sup> The work of Chomsky, even though he did not have a true theory of language *acquisition* (see §3.12), has long dominated the research agenda in this field. Researchers have tried to find innate components of language learning, or have sought to demonstrate that language learning could proceed without an innate universal grammar by stressing the richness of child-directed speech in actual iterative learning contexts (more critique of Chomsky below).

Work in the field of language acquisition is currently very empirical, with relatively little theorizing behind it. Most research concerns experiments with children of a certain age (from a couple of months to their teenage years), testing whether they can produce and understand certain linguistic distinctions (minimal pair phonemes, active and passive word use, syntactic structures etc.). Four paradigms can nonetheless be distinguished.

**Neo-nativist accounts:** work in the tradition of Chomsky (e.g., 1965) and his current minimalist program (1995), such as Goldberg (2006), Boeckx (2006) and Yang (2006), or more modularized versions of universal grammar such as Pinker

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<sup>461</sup> A group of papers documenting the history of the field of first language acquisition can be found in Lust and Foley 2004 and Tomasello and Bates 2002.

(1999, 1994), or specialised universal grammar for children such as Bickerton's 'bioprogram' (1995).

**Cognitivist theories:** theories that (more or less in the tradition of Piaget) regard language acquisition as co-evolving with cognition – or, more usually, as simply dependent on and emergent from general cognitive development (Bates 2003). Mental schemes (Arbib and Hill 1988), and the approaches of functional and cognitive linguistics to language acquisition, also fall under this paradigm (Evans and Green 2006, Croft and Cruse 2004, Taylor 2003, see also footnote 358). In their most crude form, these theories hold that a child develops a mental picture of the world. This picture contains a mental space of meanings, which are mapped unto the words of the community as a child learns to speak a language. Their main advantage is that they make maximal use of everything babies and young children already 'know' (in some sense of the word 'to know') before they learn to speak a language. Their main disadvantage is their often-mentalistic models, and our general lack of knowledge of the pre-verbal infant's mind.

**Socio-cultural interactionist accounts:** this approach is the most common in the field, with different accounts stressing different elements of the interaction: the context of use (Walczak 2002), the actual use (Tomasello 2005, 2000), shared perceptual attention (Zukow 1990) and the influence of the mother in educating the child.

**Hardcore empiricist<sup>462</sup> approaches:** a) *connectionist networks* that are trained to learn (for instance) phonological rules or elements of syntax such as number agreement, verb-argument structure and discerning relative clauses (Elman 1996, see also the overview in Rispoli 1999, and for the evaluation of the pros and cons by the community following this paper, see also Rispoli 2002). Connectionist models do not work as yet, or work only very modestly for any real languages (Palmer-Brown et al. 2002); yet they do work for 'toy languages,' displaying some patterns of over- and under-generalization interestingly similar to those displayed in normal child language acquisition (Kirby and Christiansen 2003). One of the main advantage of these models is that processes in the connectionist networks they use seem to mimic our brain's neural net well. There are also b) *probabilistic and distributional approaches*, such as the closely related study of word collocations and stochastic grammar (see Mintz et al. 2002 and overviews of Reddington and Chater 1997 and Plunkett 1997, and footnote 358). A standard problem of these approaches is that true grammatical rules are not statistical in nature but normative exceptionless law-like rules (except for a fixed list of exceptions...). However, one can argue that although grammatical rules claim to be normative, they are only the received wisdom of a dominant social group of the statistical syntactic rules of their own speech and to which this group wants to conform everyone who speaks their language. This conclusion would be supported if it were found, as it indeed is, that time and time again there exist exceptions to the highly likely correlations that have the appearance of a grammatical rule (Seidenberg et al. 2002).

Many researchers pragmatically combine approaches; cognitivist and interactionist accounts in particular can often scaffold each other. The neo-nativist accounts may seem to lie furthest away from the others, especially from hard-core empiricist accounts. Although this is usually the case (Elman 1996), we nonetheless see some approaches that try to present a formal (generative) grammar within a connectionist

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<sup>462</sup> Empiricist here in its usual contrast with empirical.

account (Smolensky 1999); all other pragmatic combinations of work from the four paradigms can also be found.

There are few book-length monographs on first language acquisition. Worth mentioning are Lust (2006) at a more elementary level, Guasti (2004) who focuses on syntax acquisition, and the older Ingram (1989). We follow Clark's thick standard reference book (2005), because her account covers all aspects of first language acquisition, including a great deal of statistical empirical research. Moreover, she points to the different views that linguistic schools have on certain issues, assessing these views in as unbiased a manner as possible. The few minor problems with Clark's account are that it emphasizes language learning from parents too strongly (as does Catherine Snow), instead of focusing on other adults such as teachers and peers (especially children a few years older). It may also rely heavily on language acquisition in modern Western cultures, where there is constant speech interaction and correction between parents and children, a phenomenon which may be absent in other cultures.<sup>463</sup>

### **Chomsky and language acquisition**

Only of Chomsky and nativism in general is Clark very critical. She falsifies three assumptions on which Chomsky based his ideas of an innate language acquisition device with a universal grammar: "1) natural language syntax is too complex for children to learn, because 2) adults offer such a distorted and imperfect source of data; and 3) children learn their first language so fast that they must be relying on an innate capacity, specifically for syntax (399)." The second assumption is wrong because adults offer easy, highly 'grammatical' speech to children with a lot of repetitions – sometimes even to the point of ungrammaticality (Chafetz et al. 1992) – called 'motherese' (Pinker 1995), 'parentese' (Chafetz et al. 1992), or just 'child-directed speech' (Rivero 2004). The third assumption is wrong because children take considerably longer to acquire syntactic structure than originally estimated: children still learn details of the syntax of their language well into "their early teen years (405)," and they also spend much more time practicing their linguistic skills: "three-year-olds devote as much as 70 hours a week to using a new language. By comparison, adults learning a second (or third or fourth) language may spend only four or five hours a week. It is hardly surprising that they fail to learn as much as a child tackling a first language (408)."

With the second and the third assumptions gone, the basis for the first assumption is undermined too, but Clark also offers independent criticism of this first assumption.<sup>464</sup> First of all, as noted earlier, the idea of innate grammar is very vague.

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<sup>463</sup> This might be true for corrections by parents in explicit language game interactions typical of middle-class Western child-raising. Moerk 1994 conducted a meta-analysis of 40 studies on conversational analysis (of conversations of adults with children), and found substantial evidence that corrections do indeed play a role, in other cultures as well. Moreover, these corrections were not only abundant but also contingent on the mistakes of the child (and its age) and thus really helped it improve its language use.

<sup>464</sup> The best discussion of the poverty of the stimulus can be found in the special 'poverty of the stimulus' issue of *Linguistic Review* 19, edited by Ritter 2002. One of the fundamental elements in the argument for innateness is the lack of negative evidence (how language does not work) as evidence against learnability (Pinker 1995). However, some researchers give examples of the use of negative evidence in production (Strapp 1999) – and, more importantly (as comprehension comes before production), of extensive negative feedback in comprehension (Savage-Rumbaugh 1990). Others show that in other forms of learning, such as in scientific discovery, there is no shortage of negative evidence either; yet nobody argues that scientific results are innate (Sampson 2002). (But see our discussion of the supposed asymmetry of verification and falsification in §1.33).

Clark distinguishes two versions: the continuity and the maturational version. The *continuity* view holds that universal grammar is present from the start, and that children only have to set a fixed number of parameters by listening more and more to the speech delivered to them by their parents. The *maturational* version holds that the parameters in universal grammar are set based upon a biological schedule. We put this maturational version aside, because it throws away the few virtues of the universal grammar approach – i.e., the constancy and universality of the grammar – by postulating an innate shadow structure that mimics the stages of externally observable linguistic competence. But the continuity version suffers from many problems too: “Just what belongs to universal grammar? How many parameters are there? How much exposure to a language and what kind of evidence do children need to set a parameter? Is it reasonable to assume that learning syntax is quite separate from learning the rest of language – the lexicon, phonology, morphology, and all the pragmatic conditions of use? All these are part of acquisition from start (403).” And “how do innate categories and linguistic forms actually get linked (404)?”. “One should be cautious before assuming that anything is innate, already “given,” in the process of acquisition (180).”<sup>465</sup>

As our discussion of anatomy and neurology showed, one need not deny that the brain and other elements of the human phenotype are fine-tuned to learn language<sup>466</sup>; nevertheless, the claim that *grammar itself* is innate is a very hard claim with little evidence. When we look at the stages of language acquisition, we will see that these stages do not support the presence of a universal grammar at all. This provides one of the best critiques of the Chomskian paradigm from the perspective of first language acquisition, since other critiques (such as the poverty of the stimulus and the complexity of language) are rather subjective. What counts as too poor or too complex?

### **The principles of language acquisition**

Before we discuss the stages of language acquisition, we turn to its principles and basic ideas according to Clark (and many other researchers in this field), along with their relation to the philosophies of meaning we discussed above. One of the first assumptions or principles is that children from different backgrounds are all equally able (individual differences notwithstanding) to acquire any human language: if placed in the right environment at a tender age, children of Chinese or Basque

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<sup>465</sup> Despite her critique, Clark sometimes unconsciously relapses into some sort of Chomskianism. Near the end of her book, just after dismissing Chomsky, she seriously and quite positively discusses Dan Slobin’s idea of a universal Basic Child Grammar “that is later modified as children become attentive to specific features of the language they are acquiring (423).” But what is this “natural set of basic grammatical distinctions (424)” but Chomsky in another disguise?

<sup>466</sup> As Lenneberg, in his *Biological Foundations of language*, put it as early as 1967: “No features that are characteristic of only certain natural languages, either particulars of syntax, or phonology or semantics, are assumed (...) to be innate. However, there are many reasons to believe that the processes by which the realised, outer structure of a natural language comes about are deep-rooted, species-specific, innate properties of man’s biological nature (1967: 394)”. A further question: to what extent can these innate biological properties be disentangled from ‘cognition in general’ (whatever that may be)? One way to answer this question is, according to the dominant paradigm in neuroscience, to look for accidents – that is, syndromes in which either cognition in general still functions but linguistic skills do not, or the other way round. Famous examples include language disorders in which some very specific rule-governed aspects of language are impaired. A lesser-known example is Williams syndrome: children who suffer from this disease are reported to have normal linguistic skills yet subnormal scores in nonverbal areas (Clark 2005: 405). Yet a closer critical look at Williams syndrome makes this picture of disentanglement much cloudier, see §3.21.



descent are equally good at acquiring the language of their ancestors or some other human language.<sup>467</sup> Moreover, it is generally assumed that children learn a human language only when exposed to it in early childhood during the so-called *critical period*. Children like Genie (Goldin-Meadow 1982), who are not exposed to language use in early childhood, fail to learn a human language except for some rudimentary, proto-grammatical utterances; it is a matter of controversy, however, how long this critical period lasts and whether even primary language is unlearned when no longer used.<sup>468</sup>

Another important assumption is that *comprehension always reaches further than production*,<sup>469</sup> which is for Clark not the *sine qua non* of any theory of human language learning and evolution (as in our anti-mentalistic philosophy of meaning), but an empirically testable assumption (which is verified again and again). In rejecting Chomsky's almost automatic, solipsistic process of language learning as learning a system of abstract rules on the basis of minimal exposure, she places her sympathies with a pragmatic use approach to language, with acquisition coming about in a social setting of "joint attention and updating of common ground (6)" and in which there is "feedback" from the parents to the child [28].<sup>470</sup> In such a setting, basic conventions that one easily overlooks such as *turn-taking rules* (when you say something, when can I say something in return?) have to be acquired through training; these rules ('when you are asked something, you respond') still belong to the most fundamental rules of our behavior.<sup>471</sup>

Other principles are less in line with our philosophy of meaning. Clark speaks of language as a) "strictly conventional" (8)<sup>472</sup> and of words as b) "representations of what children know and see (*idem*)," and further claims c) that

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<sup>467</sup> This seems to plead in favor of a Chomskian universal grammar, but Lieberman 2003 presents an argument against a universal grammar device on the basis of this fact. Lieberman argues that if there were an innate language device, it would be subject to variation and selection to better fit the language spoken, especially in communities of some language families that have been isolated from the bulk of humanity for millennia (just as Tibetans are genetically adapted to living at great heights). However, since there seem to be no signs of innate predispositions to acquire any *specific* language (apart from probably incorrect speculations about tone and absolute pitch, see §4.3e), the hypothesis of an innate language device is to be discarded.

<sup>468</sup> This is usually thought to be related to the plasticity of the brain in childhood (§3.21). Yet some argue that there is no real critical period, and that older children raised without a language don't learn a language fully only because, in their later life, they are not sufficiently exposed to child-directed speech that is easy to understand and learn. As systematic experiments in raising children without a language are clearly unethical, it will be hard to convince the few remaining sceptics of the critical period. See also Grimshaw et al. 1998, Batali 1994.

<sup>469</sup> See Clark 2005: 16 "Comprehension, throughout this process [of language acquisition], tends to be far ahead of production. (...) This asymmetry between comprehension and production is lifelong." For instance, we can understand many dialects; however, we are often unable to imitate them perfectly. See also Burling 2000 and 2005, who stresses this asymmetry multiple times in his evolutionary account of the origin of language.

<sup>470</sup> Clark adds against Chomsky that "merely overhearing [instead of social interaction] does not appear to promote acquisition (2005: 28)."

<sup>471</sup> See Clark (2005: 35): "The general notion of reciprocity and alternation in 'exchange games' seems to be established early through a variety of interchange types, notably games of give-and-take and peek-and-boo that emerge around nine months of age." These notions of reciprocity are very strong: try ignoring the street sellers in any part of the Third World that force themselves upon you by continually asking you questions.

<sup>472</sup> See Clark 2005: 12 following Lewis "Language is used to convey *meaning*. Words, suffixes and prefixes all carry meanings that are *conventional* [Lewis 1969] (Clark's italics and square brackets – MH)." And 26: "Conventionality covers all the agreements members of a language community tacitly adhere to in using their language."

children have to find out “which meanings there are words for (...) and how they have to map each meaning to the right word (idem)” so that in the end “children represent their experiences in words”. The first principle is not so problematic: the word ‘conventional’ has many meanings. Although words are not conventional in the sense of an explicit contract-like agreement (see our critique on Lewis in §1.13), (individual) words are to a large extent conventional in the sense of arbitrary, even though not all aspects of them are arbitrary (see footnote 343).

However, the second principle (which Clark shares with most *cognitive linguists* who seek to explain language acquisition) is more problematic; moreover, it overlaps with what is problematic about c). It assumes that we know freely floating meanings (or ‘senses’) beforehand, which we then have to map onto conventional words – which is *in general*<sup>473</sup> problematic. We argued at length against this view in §3.13.

Although Clark supports this mentalist view,<sup>474</sup> it is relatively harmless in her work, since with conventionalism she accepts contrastivism: “each conventional word differs from all its neighbours. Each word reflects a choice made by the speaker to convey one meaning rather than another and so contrasts with all others (15).” With these contrasts, Clark introduces something that is not completely conventional into the mentalistic (and supposedly conventional) task of mapping meanings into words. These contrasts (as between *man and woman, homme et femme, Mann und Weib*) are present in the language into which one is born, and are not just the results of conventions (or choice, as Clark says), but are the very “joints of nature”.

When we now take a look at the different stages of a child’s language acquisition, the reader may be disappointed in the explanatory value of the work on first language acquisition, as it seems only to give a description of the sequence of observed stages in which children’s linguistic fluency increases and increases. In this sense, the explanation has at heart a lot in common with Wittgenstein’s sketch of language acquisition in the *PI*: the teacher repeats and repeats the same words and corrects the utterances of the student, until his (situational) *use* is correct (which can be a sudden occurrence or a continuous process). One cannot give an ‘ultimate’ explanation of why the student uses the words correctly; *it just happens after some time*.<sup>475</sup> Anti-naturalistic, metaphysical philosophers will of course be tempted to say that this student suddenly sees the meaning of the word or knows which meaning should be mapped onto this word, but this intuitive shadow-world of mentalistic meanings raises many problems of its own (such as the private language argument) and doesn’t have any extra explanatory value (see §3.13). The only extra explanatory power provided by the description of the stages of language acquisition comes from the many small steps this process goes through (steps of imitation, steps of feedback when something is wrong etc.). The many steps, and the enormous amount of training time that goes into them, make language acquisition more understandable.

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<sup>473</sup> But of course not in *specific* cases, as the case of correct and incorrect derivational morphology shows: someone who tries to map the meaning of ‘a person who does something’ into a single word may speak of a *doer*, only to be corrected since in English the correct word is *actor*.

<sup>474</sup> At the end of her book, Clark speaks positively about ‘connectionist models’ (413) that may cure her of the idea that mental representations as connectionist network can learn without explicit representations. See also Elman 1996.

<sup>475</sup> Of course there are also differences between this empirical work and the work of Wittgenstein: Wittgenstein’s scheme is an idealization, and does not contain any statistics on the actual stages of acquisition and the errors that are made. In Wittgenstein’s scheme, the correction by the teacher is also much more explicit than in actual first language acquisition.

They also allow us to criticize views that would predict a different, faster, more discrete pattern of acquisition. It is helpful to compare language acquisition with learning how to walk: how do children learn to walk? Do they first grasp the meaning of walking and then set themselves to imitate this? No, of course not; by training with the help of their parents they simply *do* learn to walk (by falling and standing up again). Let us now turn to the actual stages involved.

## The stages of language acquisition

### Babbling and segmenting

Language acquisition starts early on. Directly after birth, children already show a preference for the voice of their mother (62).<sup>476</sup> This might be due to early priming (just as ducks follow the first living being they see) or to pre-natal *in utero* exposure. “Infants also seem to be able to distinguish their own language from another (foreign) language from as young as four days from birth (63).” The differences between sounds are minimal, as for instance with ‘b’ (closure of the lips with voicing) and ‘p’ (closure of the lips without voicing), which differ categorically only in the so-called *Voice Onset Time*. “Infants from birth or soon after appeared to make categorical discriminations for speech sounds. This ability, present so early, strongly suggests that human infants were specialised in some way for the perception of speech (61).” Clark further adds that “young infants can discriminate nearly every phonetic contrast on which they have been tested, including the ones that do not occur in their language learning environment. (...) After 6 or 12 months, however, this ability decreases (64).” Sounds that are not used need not be distinguished.

The biggest early learning challenge is formed by the segmentation problem: “how come infants to segment what they hear [a near continuous stream of sounds] into the appropriate units for that language, whether segments, syllables, words, expressions, clauses?” This problem becomes even harder when we realize that sounds (for instance the initial and ending letters of words) differ depending not only on the speaker, but also on the place of a word between other words. During this process, children have to learn which parts of sounds belong to speech and which do not. Children solve this segmentation problem by relying on several clues the stream of sounds provides: “By ten to eleven months, English-speaking infants seem to be using several kinds of information about word-boundaries: prosodic (word-based stress patterns),<sup>477</sup> phonotactic (legal sequences at the beginning and end of words),<sup>478</sup> and allophonic (positionally conditioned variants of sounds segments) (69).” However, learning the segmentation is not an autonomous process induced by mere listening: “overall it was found that for recognition of sound sequences as well as for

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<sup>476</sup> Some argue that language acquisition starts even earlier. Clark 2005: 62 “Subsequent studies have supported the possibility [of *in utero* exposure – MH]. DeCasper and Spence 1986 showed that infants preferred to listen to a story already heard prenatally to a story never heard before. But *in utero* exposure to speech is limited. Frequencies above 1 kHz are attenuated by transmission through tissue, whereas intensity and spectral properties are very similar whether inside or outside the uterus. This suggests that newborn preference for maternal voices and for familiar stories could be based on *prosodic* information in speech. In fact, when newborns, exposed to a story prenatally, hear either an unfiltered recording of their mothers’ voices or a low-pass (1 kHz) filtered version (...) [our italics],” they display the same preference for both.

<sup>477</sup> Children recognize the word ‘bóttle’ easier than ‘awáy’, since the first syllable in ‘away’ is unstressed (Clark 2003: 65) and young children often do not recognize it as *part of the word*. In languages that use tone instead of stress patterns to differentiate words, children will take clues from tone.

<sup>478</sup> Think for instance of the NeReiS-rule of Greek. See footnote 343.

associations of sound and meaning, people need more than speech alone: they need a visible reference field, and they make active use of repeated sequences of sounds and the highlighting of objects in conjunction with specific sound sequences (67).” Hence, even the segmentation problem could not be solved without a social context of *use*. We also see that comprehension comes before production.

The first contexts of use ease learning by being highly repetitious.<sup>479</sup> The older children get, the more their parents expect them to contribute to linguistic interactions (in which the fundamental element of turn-taking is taught as well): “As we saw in the exchange between three-month-old Ann and her mother, turns are imposed on very young participants. An adult talking to a two- or three-month-old will count a burp, a smile, a leg kick as a turn; in fact, adults typically say something, then wait for the baby to do something, and then resume talking. But as babies get older and extend their repertoire of actions, adults tend to up the ante. A four- or five-month-old must smile or kick; a six- or seven-month-old must vocalize; an eight- to ten-month-old must babble. That is, adults wait for an appropriate level of reaction before going on talking. Once infants begin to produce their first words, adults raise their expectations still further: now only a word (or perhaps a babble-sequence) will do (35).” The child-directed speech in these contexts is “higher in pitch” (39), the “pitch range itself is wider” (1.5 octaves instead of the normal 0.75) (40), the “tempo is lower” and it has “a greater rhythmicity”, its “amplitude is higher” (41), the “pauses between utterances are longer”, the speech is furthermore “stream-lined” (43), i.e. characterised by “a relative absence of speech errors” (44): “overall, both mothers’ and fathers’ speech showed “greater prosody” to infants than to adults (51).” This facilitates learning too.<sup>480</sup> Overall it must be said that parents “do not teach young children language” but “set out to make themselves understood”: they do not teach them (just) ‘meanings’, but try to get their children to *do* something.

Children produce crying sounds from birth on, and babies can use these in an instrumental way to get the attention of their parents. They start to make cooing sounds from around two months of age, sounds that are sometimes characterised as “happy sounds (102).” Babbling starts a little later – between six and eight months – and its earliest form “tends to consist of a single ‘syllable’ repeated, for example ‘babababa’ or ‘gagagaga’, where the syllable consists of a consonant-like sound (here a b or g) with a vowel-like sound produced with an open vocal tract, some kind of ‘a’. Canonical babbling consists of short or long sequences containing just one consonant-vowel (CV).” These sounds are easy to make and consist of combinations of a ‘closed’ and ‘open’ mouth position, as a layman would put it. Interestingly enough, the CV-structure is still the structure of words in many languages (for instance Japanese), and is the dominant pattern some say we find in new creole languages (but see §3.25). The ‘arbitrary’ form of words might thus be dependent on

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<sup>479</sup> Clark 2005: 31 “The daily routines during the first two years of life and the stereotypical adult verbal routines that accompany them are both highly repetitive (Ferrier 1978) and very frequent [bye bye, down you lie, in you go, spoon, one more, shall I tickle you etc.]”

<sup>480</sup> It is easy to see that the characteristics of child-directed speech make it clearer and more recognizable. There is, however, controversy about the reasons for this. For instance, high pitch in child-directed speech is not a universal feature, since in some cultures a high pitch is used when speaking to a high-ranking person and does not occur in child-directed speech (39). It may be that high pitch functions as an ‘attention getter’: “it may set off speech directed to the infant from other conversations and background noise by making that speech more audible (39).”

prosodic babbling (and babies that babble better are able to learn to speak words more easily, so there might be a selection pressure on babbling).<sup>481</sup>

### Early word production and over-extensions

Then after a year or a bit more, children start to produce their own words (or at least words that can be recognised as such by their parents, and often at first *only* by their own parents).<sup>482</sup> These words have “one syllable” (81). Children recognise their own (recorded) words and those of other three-year-olds less well than the words produced by their parents (74); again, a clear sign that comprehension comes before production. Their own productions tend to evolve, the word for *dog(gy)* can for instance go from “‘do’, to ‘dodi’, ‘gogi’ to ‘doggy’ (108).” Some children experience a spurt in the number of words they master, others show a more continuous development (83).<sup>483</sup> Clark speaks of a child’s “mental lexicon of recognisable words (57)”, “based on adults’ productions of words” that differ from the “representations of their own productions (77).” The word ‘mental lexicon’ is a bit misleading, suggesting that children recognise sounds as *words* with a *meaning*; but it is unlikely that children do so. Children’s ‘words’ are not exactly the same as the multi-functional ‘words’ that we use in the novel combinations of our everyday speech, let alone dictionary items in a mental lexicon. “The first words are at first monotonous *order* words for people [‘mama’, ‘baby’], food, body parts, clothing, animals, vehicles, toys, household objects, routines [bye, bye] (81).”

There have been quite a few speculations about the early vocabulary of children. Some, like Genther (1982), have tried to demonstrate that children learn ‘object words’ (‘nouns’) more easily than ‘action words’ (‘verbs’). Others have even connected the type of words children use with the context of use: “when children read books with their parents, they produce a higher proportion of object words, but more action words when playing with mechanical toys.” Some even start to correlate the proportion of these word types to different cultures!<sup>484</sup> However, is the word ‘choo choo’, uttered when a child plays with or points at a train, an action word or

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<sup>481</sup> There is a research field called *phonotactics*, which deals with a language’s restrictions on the permissible combinations of phonemes. It defines permissible syllable structures, consonant clusters, and vowel sequences by means of phonotactical constraints. These constraints are language-specific: the sounds /kn/ and /gn/ are not permitted at the beginning of a word in Modern English, but are in German. These rules are much more complex than a layman can imagine. Word forms can be said to be arbitrary, given the language-dependence of the phonotactical constraints. Yet given the fact that these rules are used to segment and decode the speech stream, we suddenly see that in another sense they obey some highly specific unconscious rules. These unconsciously-used rules are needed, for otherwise the speech stream would become one long garble of flowing words.

<sup>482</sup> See for an extreme example Clark 2005: 101 “When children start to talk early in their second year, it is often difficult to identify their first words. Their earliest attempts at word production typically fall short of the adult forms. Take one one-year-old’s first production of the English word *squirrel*: ‘ga’. Upon reflection, it is easy to trace the processes that led to this simplified pronunciations. First, children typically omit liquid sounds like ‘l’ and ‘r’; they simplify consonant clusters, usually retaining only the stop if there is one, so in the initial cluster ‘skw’, they drop both the glide w and the initial s-, keeping only the velar stop ‘k’; and they often voice initial stop consonants, here changing the ‘k-’ into a ‘g-’. Finally, vowels in children’s earliest attempts at talking are typically produced with little or no narrowing of the vocal tract, as in ‘a’. In this case, we know what the target word was [do we really? – MH]. But in children’s earliest attempts at talking, it is often hard to discern the intended targets.”

<sup>483</sup> This spurt can be attributed to a child’s ‘symbolic understanding’ that “everything has a name (83)”, as some researchers have done, but also to their “increased motor skills,” which make parents recognize their children’s attempts at word production more consistently. The second alternative seems more conservative in attributing knowledge to children, and is to be preferred from the perspective of Occam’s razor unless other evidence surfaces. Notice that some children show no sign of a sudden spurt at all.

<sup>484</sup> See Clark 2005: 86f for an overview of the relevant literature concerned with object and action words.

an object word? And what about ‘doggy’? (Doggy come here!) Let alone the word ‘mama’ – is that the name of a certain “object”? No, of course not. It is nonsensical to project our linguistic categories onto children’s language.<sup>485</sup> Clark gives the illustrative example of some children who had “picked up the word *door* and used it consistently for opening things and for getting access to things – opening boxes, taking lids off jars, removing nuts from bolts, taking clothes off dolls. Other children the same age used *open* in the same range of contexts to express the same range of meanings (...). That is, some had chosen what for adults was a noun, and others had chosen a verb, but the child uses for the two terms appeared identical (87).”

Or put differently, the use of the word *door* or *open* becomes so over-extended that our categories of action words and object words make no sense anymore. These over-extensions happen often; “they commonly appear in children’s speech from about 1.5 and 2.5 years and may affect as many as 40% of children’s early words until they reach a production vocabulary of about 100 (88).” Under-extensions also occur, but less often.<sup>486</sup> Clark takes it for granted as an empirical fact that over-extensions occur more often than under-extensions. We may, however, wonder why this asymmetry between over- and under-extensions for early words exists. Given our understanding that the meaning (use) of words depends on contrasts with other words, this follows almost logically. A child masters only a few words early on; the fewer words, the fewer contrasts, and thus more over-extensions.<sup>487</sup>

The “majority of over-extensions” are over-extensions on the basis of “some similarity of shape”,<sup>488</sup> but are also “sometimes based on other physical similarities, most often characteristics of movement, sound, taste, size or texture. (Color is notably absent, perhaps because it is less reliable as an indicator of category membership)<sup>489</sup> (88).” An example of an over-extension based on shape: ‘tick tock’ used for a watch, but also for a clock, a gas meter, a fire hose on a spool, and a bath scale with a round dial. A second example: ‘baw’ for balls, apples, grapes, eggs, squash, bell clappers and anything round. And a final example that French philosophers will like: ‘nénin’ (in French) as the word for breast, also used for a button on a garment, the point of a bare elbow, an eye in a portrait, a face in a portrait and a face in a photo (89). An example of over-extension based on sound: ‘sch’ (in German) for a train, but also for all other moving machines.

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<sup>485</sup> See also Clark 2005: 88 “to label single-word utterances as consisting of nouns or verbs attributes additional knowledge about such terms to very young children, knowledge they are not likely to have.”

<sup>486</sup> See Clark 2005: 83 “The first words children learn to produce tend to be used in highly restricted ways, often in very limited contexts. They may say *hi*, for example, only when standing in a particular doorway or *shoe* only for shoes inside a cupboard. These uses have been characterized as context-bound, but they rarely last more than a few weeks and rarely effect more than two or three words.”

<sup>487</sup> Another possibility is that under-extensions are more difficult to observe, because they are always included in the set of appropriate adult uses. Note that the word over-extension is metaphysical, insofar as it may suggest that there is a whole field of meanings out there and that one word covers too much of it; such a field does not exist. Instead of ‘over-extended’, we should adopt the term ‘under-contrasted’ (in use). Here, though, we will keep using the familiar words.

<sup>488</sup> Interestingly, in Greek philosophy the word for the *ousia*, the substance of a thing (what makes an apple really an apple) is connected with shape – most explicitly in *morphè* (form) but also in *eidos* (the ‘image’ of a thing to the eye). Thus Platonic philosophy can be said to be based upon an ‘over-extension’ of over-extensions on the basis of shape.

<sup>489</sup> Colors are no more properties than ‘[being a] dog’, ‘[being a] cat’ etc. If people were colorful and the rest of the things in the world were either black, white or grey, we would use words like ‘green’, ‘blue’ and ‘purple’ as substantives for human types, as we sometimes already do when we speak of ‘the blacks’ and ‘the whites’ etc.

It can be doubted, however, that Clark's examples really prove that children over-extend the use of words by clues regarding *shape*. Is 'tick tock' really used as an extension by shape, or are functional clues and sound clues involved here as well? And how do we disentangle shape clues from size clues? Are we not again projecting one of our categories onto children's language usage? Is it not that the case that *we* see a child over-extending the use of a word, and then try to find a higher category into which all these uses fit? But what guarantees that the child's use has anything to do with such a category? We cannot ask a child *about* his use (because he does not know the names of these 'higher' categories yet), and so we seem to be on thin ice here.<sup>490</sup> This problem is of course a modified version of the *gavagai*-problem (§3.13). And how is it solved, since in actual life we do not have much of a problem understanding each other? The simple, general answer we get from the field of language acquisition is this: the problem is solved (i.e. over-extensions no longer occur very often) when children learn to use *more words*, i.e. when they learn more *contrasts*. These contrasts are formed in part by the prototypes children learn as they are taught the meanings of the words 'dog' and 'cat' (children's use of these words will be truly different when they are first familiarized with a Chihuahua instead of a Dachshund, or a hairless cat instead of a furry one).

Note that the over-extension of words is far less dramatic in comprehension than in use: "take a one-year-old who overextends a word like ball, to refer to round objects like doorknobs, lamps, pieces of soap, apples and oranges. The same child, presented with a picture of a ball and a picture of an apple, and asked, 'Show me the ball,' points to the picture of the ball. That is, in comprehension, children show a much closer match to adult uses than they do in their production of the same target words (90)." This is the same asymmetry between understanding and production that we have encountered many times before.

### **More complex words and concept formation**

There have been many speculations about what happens when children expand and refine their vocabularies. Do they simply learn more words? That seems unlikely, because then they would need to learn how not to over-extend every new word (when you learn the word 'lemon,' you also need to learn that you cannot use it for fruit generally, for all sorts of yellow objects, for all spherical objects etc.). It seems that as you learn more words, you learn *categories* ('fruit', 'food') as well. It is with these categories that over-extensions are narrowed down dramatically. The traditional definition of the meaning of a word as the *genus proximum et differentia specifica*, makes use of them as well, since the *genus proximum* can be called the higher category.

It is around these categories, however, that heated controversy has arisen. It is clear that the words for categories have to be *trained explicitly*: 'lemons, apples and pears are *fruit*; carrots, cabbage and broccoli are *vegetables*; fruit and vegetables are *food* etc.' And it is also clear that if one has learned these categories, new meanings can be learned more easily by giving definitions. The controversy, however, surrounds certain "a priori constraints on the hypotheses [of word meaning] (133)" to which children are subject. Some researchers have postulated that "by relying on these constraints children simplify the task of ascertaining meanings to forms in the early

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<sup>490</sup> See Clark 2003: 93. Clark goes in a very different direction here, by supposing that the categories *we* use in describing the over-extensions of children are the conceptual categories of the children themselves. As she refutes some sort of "symbolic insight" (85) as an argument to account for the vocabulary spurt, she should also refute what I would call a 'conceptual insight' to account for over-extensions.

stages of acquisition”, also called the “mapping problem (133).” These constraints are constraints on *implicit* categorization.

Among the proposed constraints are a) the “whole object assumption (133),” which presupposes that children already have the category of ‘object’, so that objects “can be represented as whole entities distinct from their locations or their relations to other objects or places”; b) the “taxonomic assumption,” according to which speakers “use words to pick out categories of objects rather than associated clusters of objects that is, children appear to assume that a term like squirrel picks out just squirrels and not complexes of squirrels-on-branches or squirrels-eating-nuts (...) (134)”; c) the “basic-level assumption,” which captures the fact that the categories of whole objects favored by one- and two-year-olds tend to be categories at what psychologists have identified as the basic level;<sup>491</sup> and d) the “equal-detail assumption,” which postulates that children “who already produce the term dog for dogs should look for a category at the same level of specificity when trying to map the newly heard term *cat* and so are more likely to assume that *cat* refers to cats in general (as experienced so far) and not just to some subgroup of cats (135).”

All these assumptions, typical of the mentalistic nature of *cognitive linguistics*, are *highly* problematic. All of them arise from the mapping problem, which presupposes that a pre-given field of meanings is to be mapped out in words that refer to things; we criticized this presupposition already. But the assumptions can also be criticized individually. The first constraint can only hold for ‘nouns’ and not for the many ‘action words’ that children use; yet children’s ‘nouns’ do not necessarily function as nouns, as we saw while discussing children’s use of the words ‘mama’, ‘door’ and ‘choo choo.’ Furthermore, what is a whole object? Something that can be moved independently? Something whose parts are materially interconnected? The proponents of the first constraint owe us an explanation of what counts as a ‘whole object’.<sup>492</sup> Moreover (in an actual falsification of the whole object assumption), the first ‘nouns’ children learn include many ‘part-objects’ such as ‘eye’, ‘nose’ and ‘leg,’ as well as ‘mass nouns’ such as ‘food’, ‘milk’, and ‘drink.’

The second constraint begs the question: how do children learn our use of the word ‘squirrel’ (as the name of that little animal)? They just *do* it like us. There is no need to invoke an innate squirrel or innate animal or innate whole object as such, whose self-luminosity incites children to see whole objects everywhere by mapping or projecting their internal categories onto the world.

The third constraint is also somewhat question-begging. What is the basic level? Is it fruit or food or apple? And does this not differ depending on the pragmatic context of use? Furthermore, does not the ‘basic level’ owe its existence to the *word* that is used *most often*, instead of the other way round?

And the final constraint: if one knows the word ‘dog’, how does that help to learn the word ‘cat’? The word ‘cat’ might just as well have been the word for ‘animal’ or ‘living being’. Furthermore, is not the child’s use of the word ‘cat’ (that we use for so many different cats) initially bound by the individual cat with which the child gets acquainted? And finally, does not the ‘detail’ of the use of the word ‘dog’ shrink when a child learns the word ‘cat’? It often happens that children initially

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<sup>491</sup> Conceptually, members of such categories are thought to be easier for adults to categorize than objects either above or below this level. Compare ‘dog’ at the basic level to ‘animal’ (above the basic level) or ‘Dachshund’ (below the basic level).

<sup>492</sup> The recent wave of philosophic literature on meriology and ontology in general shows that this is far from an easy question.



call all animals at home ‘dog’. This implies that equal detail arises *only* because the word ‘cat’ was learned *after* the word ‘dog’.

The biggest problem with these constraints, however, is their vagueness. Are they learned? But do not we learn individual things before we learn about categories? Or are they innate? But then how did they evolve? And if they evolved, then in the past they came into existence from selection pressures on the way we perceive external stimuli; but why then not presuppose (Occam’s razor) that exactly such processes occur during the lifetime of each child? Furthermore, the constraints are “intended to hold only for a limited amount of time (137).” But when do they take effect and when do they stop functioning? And finally, how exactly do they work? How does an equal-detail constraint fix the meaning of ‘cat’? The constraints raise more questions than they solve, and we can just as well do without them. Of course it might be that, due to our internal constitution, we pay more attention to changes in the environment than to things that remain constant (which seems logical from a Darwinian point of view). Things that we call whole-objects, animals etc. are things that can *move as wholes*; thus perhaps our perception apparatus incites us to point to (what we now call) ‘whole objects’ and name them. As such, our ‘knowledge’ of the world (our experience with it) can influence the way we go about learning words when we are still pre-verbal. If cognitive linguistics would focus on such cognitive biases instead of mentalistic, vague categories that aid the acquisition process, its results might help us to better understand this process. Comparative research on non-verbal primates’ experience of the world might help us to find these biases.

Two approaches to concept formation<sup>493</sup> that do not rely on constraints are the popular *prototype learning of concepts* and the related *exemplar learning of concepts*. The prototype view of concept learning holds that people abstract out the central tendency (or prototype) of a few experienced examples, and use this – rather than a set of definitions – as a basis for their categorization decisions. According to prototype learning, this leads to a *hierarchical* inventory of one or more central examples of a given category, followed by a penumbra of decreasingly-typical examples. Exemplar learning is more or less similar, though it places a stronger emphasis on the individual object that was experienced; it argues that no abstraction is made in evaluating other instances of the type of this individual object. Prototype learning can best be illustrated with an example. Think of a child who learns the word ‘bird’ by observing sparrows and crows. It will easily recognize blackbirds as birds; eagles, hawks and finches less easily; and even less easily geese or ducks or chickens. Penguins are hardly recognizable for it as birds, just as bumblebees and airplanes may provide false positives and accidentally be called birds.

Prototype learning fits well with Wittgensteinian family resemblance, and is useful for overcoming traditional accounts of learning word-meaning on the basis of essences or definitions; it also predicts patterns of over-extension that we encounter in the actual practice of language acquisition.

Finally, Clark mentions three important moments (outside her main line of argument) that can teach us how children learn meanings and categories.

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<sup>493</sup> Concept learning is a field of studies in its own right; in its modern form it started with the work of Bruner et. al 1967, who defined it as “the search for and listing of attributes that can be used to distinguish exemplars from non exemplars of various categories.” Other versions of concept learning are *rule based theories of concept learning* (something like definitional learning, when for instance infrared waves are defined as X-ray with a certain wavelength), *Bayesian approaches* (this time not of theories versus evidence and background information, but of concepts versus individual instances and background information), and *machine learning approaches*.

1) The first step is that children learn deictic and general-purpose terms early on, on which they soon “rely heavily. They use deictic terms like *that* to pick out all kinds of objects and events, and they make use of general-purpose verbs like *do* to pick out different kinds of situations (97).” An attentive parent can then easily teach children words.

2) More important, however, is the second step: “As children add to their vocabularies, they have more options available as they decide to talk about a particular object or event. That is, they can take different perspectives on the object or event and mark the perspective chosen through the words they decide to use. For example a cat can be *the cat*, *the animal*, *the Siamese*, *the family pet*, or *the scratcher*. While children still have only a small vocabulary, they may only have a few choices of perspective. The cat might be just cat or animal; a doll might be doll or toy; and an apple might be apple or fruit or even food. Speakers, adults or child, can have different reasons for choosing one perspective over another as they present an object to their addressees (98).” These different ‘perspectives’ on the same objects (calling an apple ‘food’ or ‘apple’ can make a lot of difference in achieving a goal) can be quite different: a chair may be a chair or a horse, etc. However, they do provide a step towards categorization, which happens more explicitly when words of the same ‘category’ are juxtaposed: cats and dogs, the green (one) and the red (one).<sup>494</sup>

3) Due to these many perspectives, a restructuring of the use of words takes place (the third step). Only by restructuring can so-called late errors be accounted for: “children may make apparently error-free use of some forms for as much as a year or two, and then start to produce occasional errors. [It is suggested] that such errors arise as children analyze the meanings of the relevant forms in more detail and, as a result, start trying to reorganize parts of their lexicon (157).” An analogous case in syntax (which comes chronologically later) is the well-understood phenomenon of over-grammaticalization that happens when children start to use the rule of past tense formation instead of learning individual forms of the past tense one by one. Some forms of irregular verbs that were learned one by one are for a short time replaced by regular forms: i.e. ‘to sleep, slept, slept’ instead of ‘to sleep, slept, slept’ etc.

Clark concludes, and this cannot be stressed enough: “so one has to be careful not to attribute full adult-like knowledge of word and construction meanings to children too soon. Appropriate uses in one context do not necessarily connote mastery of the adult meaning (157).” One can never immediately grasp the meaning of a word at once, especially not when one does not yet understand the use of explicit definitions. Word use involves *experimenting* in different contexts and constantly being corrected by other, more mature users. And even in adulthood, the use of a word is never fixed, but depends on (implicit) mutual correcting. Because of the open character of word use, semantic shifts can occur (see §3.25).

### **Word combinations and proto-grammar**

Soon after learning their first words, children start to produce word combinations. There is no clear boundary between “individual words uttered some time after another” and “word combinations.” In some cultures, such as the Italian, the

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<sup>494</sup> See Clark 2005: 147 for a beautiful example: “Explicit contrast was invoked in much the same way when Carey and Bartlett (1978) introduced four- and five-year-olds to the unfamiliar word *chromium*. They relied on requests like ‘Give me the chromium tray, not the red tray, the chromium one.’ This told the children that chromium belonged in the same domain as red and that they must therefore pick out some color, even if they were unsure which one.”

production of word combinations is preceded by an *increase* in word-gesture<sup>495</sup> combinations (166), but in most languages word combinations are more central. Children are quite conservative in producing word combinations (203): at first they only produce combinations they have heard from their parents (or teachers and peers etc.). The production of word *combinations* brings the meaning (use) of *individual* words uttered by children more in conformity with that of their parents; for instance, the word ‘open’ becomes more often ‘verb’-like and the word ‘door’ more often ‘noun’-like (adult words are designed to be part of larger word-sequences). The combinations of children are, however, not just noun-verb combinations, but of all sorts.<sup>496</sup> Some examples of these combinations, with Clark’s analysis in parentheses: ‘doggie bark’ (verb + agentive), ‘see Kendall [K sees]’ (verb + experiencer), ‘writing book’ (verb + goal), ‘play bed’ (verb + locative), shoe off (verb + objective), ‘Kendall book [is reading]’ (agentive + objective), ‘Kimmy bike’ (experiencer + objective), ‘towel bed’ (locative + objective) (Clark 2005: 169). The noun-verb combinations are often related to descriptions, but other combinations also occur and form different ‘speech acts’. Some examples, with Clark’s analysis of the character of the combination in parentheses: ‘more milk’ (request), ‘there book’ (locate), ‘no wet’ (negate), ‘hit ball’ (describe), ‘mama dress’ (possess), ‘big boat’ (modify) (Clark 2005: 167). We find these analyses quite arbitrary when the context of utterance is not described as well, and we doubt whether assigning *grammatical* categories to these two-word combinations is not an over-projection. Clark seems to agree, as she later asks: “to what extent do children use word-order for grammatical relations?” and answers “that for children, there is little evidence<sup>497</sup> that word-order has any grammatical role at this stage (171).” A better way to analyse these combinations is either as “learned formulas” or as “pragmatic responses to conversational factors (171).”<sup>498</sup>

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<sup>495</sup> Children use gestures alongside of speech to aid communication, for instance pointing and reaching gestures. The use of these gestures starts a little before first word productions. Some children also use what Clark calls “representational or referential gestures to be used to refer to objects and events in much the same way that words refer (95).” With these gestures, children can get something *done*. Examples: a sucking action to request the pacifier, or dancing to request that the radio be turned on. “As they grew older, however, the two-year olds replaced their representational gestures by words.”

<sup>496</sup> Note that full canonical sentences are rare in child-directed speech; parents commonly use isolated words and sentence fragments rather than full sentences (Cameron-Faulkner et al. 2003). These ‘sentence fragments’ (possibly a misnomer, because they presuppose the whole sentence, and they can also be seen as meaningful pragmatic word combinations in their own right) ease later learning of more complex full sentences. Note that the word combinations children learn are often examples of sentence fragments parents provide.

<sup>497</sup> Since children do not produce syntactic markers (such as the plural -s) at this stage, finding evidence for this claim looks even more problematic.

<sup>498</sup> See also Clark 2005: 312. Clark points to the research of Greenfield (on single-word utterances) in which he demonstrates that children ‘encode what is uncertain’. The advantage of this approach is that it does not assume that children have “identified such [grammatical] roles as agent, location, and so on for each event.” Greenfield’s proposal can also be made fruitful for longer utterances. Some “studies have suggested that [in longer utterances] children place given information first and follow it, within the same utterance, with what is new.”

Note that the question of information transfer is itself a matter of pragmatics. On page 37, Clark raises an important question: “how soon can we be sure that children are intent not just on achieving some goal, but also on making sure their addressees have understood them?” From a pragmatic Darwinian perspective, this question seems a bit strange, because reaching your ‘goal’ is the same as being understood. Yet Clark’s pragmatic examples demonstrate what she means: from the age of two, children ‘repair misunderstandings,’ and they tend to give more *information* when their mother doesn’t know where an object is. If they were interested in the ‘goal’ as such, then they would give the same amount of information in both cases.

These learned formulas are still very important to adults' own language use. Learned formulas contrast with free compositionality. *Free compositionality* is seen as the hallmark of human language (see §1.11): we can use the same words to form new combinations that have new meanings. In the sentences *The pit bull bit our dog* and *Our dog bit a sausage*, the word *dog* occurs twice (once as subject and once as object), but the word *dog* seems to 'refer' to the same animal in both cases; there seems to be no end to the novel combinations that basic fixed units of language like 'dog' can form. Our dictionaries are in a certain sense exponents of the view of compositionality, since they list the meaning of *individual* words which, once fixed, can be combined into indefinitely many sentences. However, some languages do not have an analogous word for 'word', which might make us sceptical about viewing words as the basic units that can enter freely into novel combinations. An argument that 'word combinations' instead of individual words are often fundamental is provided by the BBI and by learner's dictionaries (such as the Oxford Advanced Learner's Dictionary). These dictionaries list the so-called 'word collocations'<sup>499</sup> one can form. For instance, under the word 'question' (noun) one finds that one can *raise* a question, *drop* a question, *put* a question, *answer* a question, *return* a question, *throw up* a question – but not *uplift* a question, *let fall* a question, *place* a question, *bring back* a question, *dive up* a question etc. As children learn word combinations, they learn *fixed collocations*, and they tend to be very conservative in making up their own combinations. Free compositionality is not a basic feature of human language, but rather one of the most advanced features of human language. Even adult speech seems much more strongly constrained by fixed collocations than we might think.

Nevertheless, in learning formulaic word combinations, children slowly learn to use syntactic rules. This is the basis of true or free compositionality (as constrained as it is in reality). It is a long process, involving many faulty constructions:<sup>500</sup> children are still learning details of their syntax as late as their early teenage years. In learning the syntactic rules (for instance 'past tense = root + ed'), children often display a phase of over-regularization (i.e. *slept* instead of *slept*). A process of restructuring can account for this. Nevertheless, the process of learning syntactic rules is continuous and not abrupt. "English-speaking children tended to use *-ing* (indicating ongoing duration) on activity verbs like *run* but not on accomplishment verbs or achievements verbs like *break* and *build* or *finish* and *reach*; nor did they ever use *-ing-* on stative verbs like *want* or *know* (...). Children make use of *-ed* on both accomplishment and achievement verbs, but the meaning seemed to be 'completed action' rather than the adult 'past tense'. (...) And children rarely used the past tense *-ed* on activity verbs. That is, they appeared to be sensitive to whether a verb denoted an ongoing activity or an activity with a clear end-point and tended to observe compatibility of meaning between the verb-stem and inflection (203)."<sup>501</sup> Only much later do children learn the adult use of *-ing* and *-ed*. Semantics

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<sup>499</sup> Word collocation is defined in linguistics as a sequence of words or terms that co-occur more often than would be expected by chance. Word collocations are thus a highly common phenomenon, as word combinations are hardly ever random.

<sup>500</sup> One of the arguments for an innate universal grammar is that children supposedly never make certain errors, forbidden by the constraints of universal grammar. However, Drozdz 2004 presents several types of errors in child speech and child grammaticality judgments that appear to violate these constraints. Samson 2002 even points to such violations in adult speech. Nonetheless, the relative lack of such violations may be ascribed to learning syntax through fixed formulas; this does not presuppose a universal grammar either.

<sup>501</sup> Note that in this case, linguistic 'ontogeny' thus seems to follow linguistic 'phylogeny'. In Shakespeare's time, the *-ing* form was only used on activity verbs. Later it became grammaticalized (see §3.25). Hence the child's acquisition of the *-ing* form follows its historic evolution.

takes precedence over syntax, and syntax only arises as a streamlining of individual word combination rules.

We already criticized the Chomskian idea of an innate grammar that helps in restructuring. It is a complex, problematic, unnecessary assumption. We saw in our analysis how language use developed gradually by repetitious external stimuli (linguistic utterances) that invoked action in the child, and from the continuous feedback from more mature language users. Since Darwinism postulates that any natural innate ability must ultimately have come from the outside (confrontation with selection pressures from the environment), Chomsky needs an account of basic reactions to external stimuli too if he ever wants to explain how his internal grammar evolved by natural selection. He might just as well opt for the steps we outlined above, but that would mean giving his theory up; the above steps did not use the innate language acquisition device to describe or explain ontogenetic linguistic development ('evolution').

### §3.25 Evolution of linguistic forms

In our overview of the history of linguistics and our explanation of why research on the origin of language was virtually non-existent for over a century, we already pointed out that historical linguistics was the key discipline of linguistics in the 19<sup>th</sup> century. However, it lost its preeminent position to synchronic, formal (structuralist) studies of language and grammar (in the 20<sup>th</sup> century in particular). In this section, we describe how diachronic linguistics made a comeback at the end of the 1960s. This time it was closely associated with sociolinguistics, which showed how much variation was synchronically present in a speech community and even in the idiolects of the individual speakers; this buttressed theories of diachronic linguistics with a natural background against which to explain historical processes. We first discuss a) diachronic linguistics in isolation from social processes and then discuss b) the social processes behind diachronic linguistics. As we discuss social linguistics, we also discuss the results of the study of pidgins and creoles.

#### a) Diachronic Linguistics

The advantage of looking at diachronic linguistics is that we look at actual language evolution in progress. The downside is that we need records that tell us what a language looked like in an earlier period; before the advent of audio-recorders, that means we are dependent on *written* remnants of a language. Since writing has been around for only 6000 years (whereas people have used language for a vastly longer amount of time), we cannot go back to the original language using diachronic linguistics. We can only document evolution within languages that are already fully developed.

The study of diachronic linguistics really took off in the nineteenth century, and culminated in the work of the Neogrammarians. We will briefly describe this field, then discuss its demise, and then address the 1960s resurrection of diachronic linguistics; for this last, we will turn to the famous article by Weinreich, Labov and Herzog (written for the 1968 symposium *Directions for Historical Linguistics*) that gave the field a new impetus. We will then briefly show how language evolution, though a social process, is nevertheless relatively independent of other elements of cultural

evolution. We will then look first at intra-linguistic factors that explain diachronic linguistic change, then (in the next section) to the social processes behind it.

A general remark: diachronic linguistics is not an exact science, and its explanations of particular diachronic changes are often contested. Often the initial form and the end form of a process of linguistic change are well attested, but the intermediary stages are not; moreover, the most direct route towards the end form is not always the route actually taken.<sup>502</sup> There are, however, some general laws or rules that changes often obey, and one can use them to make reasonable suggestions about the evolution of linguistic forms.

As mentioned, the heyday of diachronic linguistics was the 19<sup>th</sup> century. Philology and anthropology were interested in deciphering the ancient texts of other cultures, and linguistics was generally under the spell of historicism; the chief goal of linguistic investigations was the description of a language's historical changes. The culmination of this era was the work of the Neogrammarians, a German school of linguists originally at the University of Leipzig, who in the late 19<sup>th</sup> century proposed the Neogrammarian hypothesis of the regularity of sound change. Well-known examples are Grimm's Law and Verner's Law, which famously solved a problem with the generality of Grimm's Law. Although the Neogrammarians were perfectly aware of the contingent nature of their sound laws, these laws are often caricatured as 'immutable', eternal laws (Weinreich et al. 1968: 133). In reality, the Neogrammarians were the first generation of linguists to be well aware of issues of falsifiability according to the scientific method (§1.33), and they regarded the laws or rules as guiding principles (just as linguists today do) – such as:

- 1) Sound change has no memory: sound change does not discriminate between the sources of a sound. If a previous sound change causes X,Y > Y (features X and Y merge as Y), a new one cannot affect only original X's.
- 2) Sound change ignores syntax: a sound change can only have phonological constraints (such as X > Z in unstressed syllables), not syntactic constraints (such as dropping an 'h' only from adjectives).<sup>503</sup>
- 3) Sound change is exceptionless: if a sound can happen at a place, it will. It affects all sounds that meet the criteria for change. Apparent exceptions are possible, due to analogy and other regularization processes, or another sound change, or an unrecognized conditioning factor.<sup>504</sup>
- 4) Sound change is unstoppable: *all* languages vary from place to place and time to time, and nothing prevents this change.<sup>505</sup>

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<sup>502</sup> Diachronic linguistics is a bit like chemistry, where the reactants before and after the reaction are well-known, but the intermediary stages and the intermediary reactants are often not known. However, in the case of chemistry, we do have the clear mathematical rule that the number of atoms remains unchanged. In linguistic evolution, phonemes can easily be lost or gained.

<sup>503</sup> Today it is known that a sound change may or may not recognize word boundaries, and is in principle even independent of prosodic clues (though often not in practice). Also, sound changes may be regularized in inflectional paradigms (such as verbal inflection); in that case, however, the change is no longer phonological but also morphological in nature.

<sup>504</sup> In past decades, it has been shown that sound change does not necessarily affect all the words it could all at once. However, when a sound change is initiated, it usually expands to the whole lexicon if given enough time.

<sup>505</sup> Neither writing nor media can stop this, as we know today, even though these can slow down the developments.

These principles clearly indicate the Neogrammarian focus on the autonomy of the sound level, which they considered (in accordance with their philosophy of science) to be the most important level of linguistic description – because *it is the most observable one*. It can even be said that ‘language change’ was for the Neogrammarians equivalent to ‘sound change’. In describing the sound level, the Neogrammarians focused on idiolects (and generalizations of collections of idiolects), partly because language localized in the individual is most directly observable.<sup>506</sup> However, in contrast with diachronic linguistics in the 20<sup>th</sup> century, they did not focus on the social mechanisms behind the transmission of linguistic changes (except in the very general sense that people might adapt their idiolect to that of people around them, Paul 1888: 34).

Interestingly enough, Neogrammarian linguists in the 19<sup>th</sup> century saw similarities between the evolution of species and the evolution of languages (see for instance August Schleicher<sup>507</sup> and Max Muller). Darwin himself noted in the *Descent of Man* (1871: 465–466) that the principles philologists were using in their taxonomy of Indo-European languages were Darwinian in nature: “The formation of different languages and of distinct species [are] (...) curiously parallel. We find in distinct languages striking homologies due to community of descent, and analogous due to similar processes of formation.” Another strong analogy is the gradual nature of both biological and linguistic evolution: “[in evolution] there is always a *continuous series of infinitely numerous sounds* (Paul 1888: 52f).”

Moreover, it was the Darwinian tree of life, with its many branches, that the linguists could use as a picture of the Indo-European roots that evolved into the many branches of European (and Indian) languages. Some of these languages exist today, such as German, French, Italian, Spanish, and the lesser-known Frisian, Catalan and Gaelic. Some have died, such as Latin, Dalmatian and Cornish. Darwin had opened the eyes of biologists to the huge variation in languages and the slow accumulation of changes in them; linguists could use this model of variation and slow

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<sup>506</sup> Implicitly, they also assumed an individual’s language was homogeneous, so that within the idiolect a regular system could be found. This is already a step in the direction of the structuralist and Chomskian assumption of a perfectly homogeneous speech community.

<sup>507</sup> See August Schleicher (1821–1868) in his essay *Darwinism tested by the science of language* (1863): “Languages are organisms of nature; they have never been directed by the will of man; they rose, and developed themselves according to definite laws; they grow old, and died out. They, too, are subject to that series of phenomena which we embrace under the name of ‘life’. The science of language is consequently a natural science; its method is generally altogether the same as that of any other natural science (quoted by Law 2003: 269).” Note that Schleicher claimed that he himself had been convinced of the natural descent and competition of languages before he had read Darwin’s *Origin of Species* (1859). It is true that Schleicher is commonly credited as the inventor of the famous (graphic) representation of a family tree to depict the relation and descent of languages, as early as 1853; he had taken the metaphor of a language as an organism with periods of development, maturity, and decline from Hegel and Humboldt. It can even be argued that Schleicher’s appreciation of Darwin’s work was only a superficial flourish to give his theory a more respectable scientific character. Darwin’s work did not change Schleicher’s views, as Schleicher simply overlaid the fundamental features of his prior evolutionary project (in the tradition of Hegel and Humboldt) with Darwinian ideas. Instead of a story about progressive evolution as in Hegel’s scheme, Schleicher believed in progressive degradation. This was common in 19<sup>th</sup> century linguistics, as people had only studied the causes of the ‘destruction’ of linguistic structures, and not the causes of structures being formed. Thus they portrayed the languages of the Golden Age as Proto-Indo-European, highly complex (‘rich’), and regular. (See Deutscher 2005: 110–112 and §3.24 for the reasons that this picture needs serious revision.) What Schleicher did take from Darwin (who took it from Lyell), though, was the assumption of uniformitarianism, which according to both Schleicher and Darwin is the *conditio sine qua non* of doing science instead of making speculations: Schleicher presupposed that the same principles of language change that were operating today (and only those principles) were also the causes of language change in the past.

changes. And, using another widely shared metaphor of ‘languages as organisms’<sup>508</sup>, some explicitly spoke of a struggle for existence amongst languages: “certain people, such as the Indians of North America, are unfitted for historical [i.e. civilized – MH] life on account of their endlessly complicated languages, bristling with overabundant forms, (...) [and these] can only undergo retrogression, even extinction.”<sup>509</sup>

These Darwinian linguists were soon criticized by linguists who had more interest in a newly-emancipated academic discipline: psychology. Steinthal, one of the more psychologically-oriented linguists, criticized the Darwinian-minded linguists for taking “only into account the organism – the lifeless body of language – and not its life; they dissect its corpse.”<sup>510</sup> That is, Darwinian linguists only looked at the evolution of external word forms and forgot about the inner life of languages. As a philosopher, one might expect the ‘inner life of a language’ to comprise the meaning of words and the intention of speakers as they interact, but in reality the psychologists were not interested in this; they actually sought “sound laws that proceed *mechanically* and admit of no exception (Law 2003: 271).” In that sense, even the psychologists saw language “as an organism independent of human volition (272).” And although they had some interest in the societal nature of language, they did not differ much from the Darwinian-minded linguists.

As we showed in §3.1, diachronic linguistics lost its central place in the field of linguistics to the study of grammar, as structuralism became dominant at the beginning of the 20<sup>th</sup> century; Darwinian metaphors of language evolution were by then forgotten. Since structuralists viewed language primarily as a system of layers, and diachronic processes only destroyed the beautiful nature of the ideal system, they paid little attention to diachronic linguistics; the same went for the Chomskians with their fictitious ‘homogeneous speech community’. Only in the sixties and seventies did diachronic linguistics regain ground, this time in close affiliation with the field of sociolinguistics. Only they came up with a model in which “variability and systematicity did not exclude each other (Weinreich et al. 1968: 50)”. What they showed is that variability is always present in the speech community, which is thus not homogeneous but highly heterogeneous, and that part of being able to speak as an individual is mastering multiple heterogeneous styles on all levels from phonology – from semantic choice of words to syntactic structures. Moreover, they showed that variability is the source of new systematicity. What they demonstrated, contra the 19<sup>th</sup> century speculations of an ideal and perfectly regular *Ursprache* that gradually became corrupted, is that every natural language has always been a bricolage product with lots of loose ends and inner variation. Time and time again, new general

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<sup>508</sup> Famous linguists of the nineteenth century who used this metaphor were Franz Bopp, August Schleicher and William Dwight Whitley. The latter wrote the successful, popularizing work *The life and growth of language* (1875).

<sup>509</sup> August Schleicher’s *On the meaning of language for the natural history of man* (1865), quoted by Law 2003: 270. See also Herman Paul 1888: 32 “In the development of Language Custom, purposiveness (*der Zweck*) plays the same role as that which Darwin attributed to it in organic nature: the greater or lesser usefulness (*Zweckmässigkeit*) of the resulting patterns (*Gebilde*) is decisive for their preservation or extinction (as quoted by Weinreich et al. 1968: 110).” It is an open question whether language forms are truly selected for their usefulness (easier and faster to pronounce); often when a change seems to simplify a language, it also has consequences at other levels that may make the language more complex. So the net result of the change is more or less neutral. For instance, the elimination of gender from English forced a stricter word order upon English, and led to other devices to prevent ambiguity. See also McWorther 2001, who sometimes claims that language changes make language easier, but in general claims that all language changes are ‘neutral fluctuations’.

<sup>510</sup> Steinthal quoted by Law 2003: 270.



structures are imposed that actually use the existing variation (Janson 1983: 18 “synchronic variation is intimately connected with diachronic change.”). This will become clearer when we discuss some examples of linguistic change.

Weinreich, Herzog and Labov, in their reinauguration of the field of diachronic linguistics, formulated seven principles that were as true in 1968 as they are today:

“1. Linguistic change is not to be identified with random drift proceeding from inherent variation in speech [all people having slightly different speech organs etc. speak a little different – MH]. Linguistic change begins when the generalization of a particular alternation in a given subgroup of the speech community assumes direction and takes on the character of orderly differentiation.<sup>511</sup>

2. The association between structure and homogeneity [of the crystal clear rules of the ‘ideal language’ of the structuralists and Chomskians – MH] is an illusion. Linguistic structure includes the orderly differentiation of speakers and styles through rules which govern variation in the speech community; native command of the language includes the control of such heterogeneous structures [e.g. in the speech repertoire of an individual, multiple variants (including bilingualism) are present – MH].

3. Not all variability and heterogeneity in language structure involves change; but all change involves variability and heterogeneity.

4. The generalization of linguistic change throughout linguistic structure [i.e., when for instance a sound change in a group of words spreads to all many other words] is neither uniform nor instantaneous; it involves the covariation of associated changes over substantial periods of time, and is reflected in the diffusion of isoglosses [maps of dialects – MH] over areas of geographical space.

5. The grammars in which linguistic change occurs are grammars of the speech community [contra structuralists and Chomskians, who consider language primarily as an abstract structure instead of a public medium – MH]. Because the variable structures contained in language are determined by social functions, idiolects do not provide the basis for self-contained or internally consistent grammars [contra Neogrammarian diachronic linguistics with its focus on idiolects – MH].

6. Linguistic change is transmitted within the community as a whole; it is not confined to discrete steps within the family. Whatever discontinuities are found in linguistic change are the products of specific discontinuities within the community, rather than inevitable products of the generational gap between parent and child.<sup>512</sup>

7. Linguistic and social factors are closely interrelated in the development of language change. Explanations which are confined to one or the other aspect, no matter how well constructed, will fail to account for the rich body of regularities that can be observed in empirical studies of language behavior (Weinreich et al. 1968: 187f).”

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<sup>511</sup> For example, linguistic changes do not take place due to random fluctuations, but as certain variants are generalized or extended to larger domains. Compare neutral molecular evolution (as in drift) with directional molecular evolution (as in the case of true natural selection).

<sup>512</sup> Earlier linguists often naively assumed that language is transmitted from a generation of parents to a generation of children. In reality, children learn much of their language from their peers rather than their parents or teachers (as Labov has demonstrated in many papers). Traditionally it was assumed that language change is caused by children who, as they learn the language of their parents, make small mistakes that accumulate over generations (Paul 1888: 34). However, in his most recent work, Labov 2007 has stressed that this picture is also wrong: children go to great lengths to imitate the language of their parents. The parents themselves are the cause of change, as they start to use their own language in a sloppy way (which is then imitated by children during language acquisition).

Before we turn to concrete examples of language change, we will add four principles to Weinreich's list: 8) language change is an unconscious process, 9) language change can happen at any level and can influence other levels, 10) language change can proceed both gradually and abruptly, 11) language change is relatively independent from other forms of cultural evolution.

8. More or less unconscious language change: when we read old books or when we speak to older people, we may feel that their word use is somehow old-fashioned. At such a moment, we realize that language changes. However, language change is in general an unconscious process, which we usually experience without controlling.<sup>513</sup> Hermann Paul acknowledged this fact (1888: 18), and it still holds true today.<sup>514</sup> This is important because dual-inheritance models, which we will employ to describe diachronic linguistic change, can only describe processes of change that are relatively unconscious or unintentional (§2.3).

9. Language change can happen at any level:<sup>515</sup> language change happens not only at the sound level (as the Neogrammarians focused on), but also at the lexical or syntactic level – as when foreign words or even foreign syntactic structures are incorporated into one's own language. Moreover, changes on one level can have consequences for other levels: sound changes can have consequences for the morphology of words (which is part of syntax). And in a process called grammaticalization, lexical content words can become part of syntax. For example, the word 'pas' in French – which once meant simply 'step' ("je ne marche *pas*," "I do not walk another *step*") – became the general word for negation. On the other hand, when grammatical distinctions are used less and less, they may live on only in fixed lexical combinations with meanings of their own. Take the Dutch 'heer des huizes,' with an old genitive construction of 'des' + 'es,' which once simply meant "lord of the house" or 'landlord,' but which is now only used ironically.

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<sup>513</sup> See Trask 1996: 12 "The lesson to be drawn is (...) that language change is ceaseless and remorseless."

<sup>514</sup> Note that, although for Paul it was a fact that linguistic change was unconscious, this posed a real problem for him. He wondered why an individual did not notice as his language started to change. In retrospect, we can see that Paul implicitly assumed that the idiolects of the individual speakers were homogeneous, but according to the second principle of Weinreich et al. this is not the case.

Of course, there are some cases in which language change proceeds consciously – as in the adoption of words from street language, or the formalization of a grammar. But the evolution of deep (syntactic) structure, when it comes to the street language of children of immigrants from very different countries, is largely unconscious.

<sup>515</sup> See Trask 1996: 1 "One of the fundamental things you need to understand about languages is that they are always changing. Every language that is spoken continues to change, not just by century, but day by day. The language that you speak is not just different from your parents' language: it's different from the language you were speaking last year or last week, even if you don't notice changes occurring on such a small time scale. In fact, most people don't notice the language changing at all: at best, they are merely aware that young people speak a little differently from the old folks. Even then (...) the conclusion drawn is likely that young people are 'sloppy' or 'lazy'. Even those few who are perceptive enough to realize that language is genuinely changing will often regret this fact and yearn for a world in which languages never change, or at least for one in which changes are carefully and thoughtfully introduced by suitable authorities after protracted deliberation. They can yearn all they like, but they're not going to get such a world." See also McWhorter 2001: 12 "everything about language is eternally and inherently changeable, not just the slang and the occasional cultural designation, but the very sound of basic words, and the word order and grammar."

10. Language change can proceed both gradually and abruptly. We can illustrate this best by considering sound change. Sound change can proceed through two means: i) because the sounds of one's language gradually change or ii) because words and their pronunciations from neighbouring sounds and dialects (with sounds that are not present in one's own language) are incorporated into one's native language (a process also known as lexical diffusion). The first process is traditionally viewed as a gradual form of sound change, the second as a discrete form of sound change. However, Janson (1983) shows that lexical diffusion can also be gradual and that phonemes can also change abruptly (as is the case for the shift of apical to uvular (r) throughout Western Europe). At other levels, such as the lexical or syntactic, similar abrupt and gradual changes can happen; in general, however, they are more gradual (grammaticalization is a slow process, but the formation of a creole syntax is a fast one).

11. Language change is independent from general cultural evolution: some cultures may have more words for things that are relevant in their own culture, although the famous story of the Eskimo and his many words for snow is a myth: Eskimos have "only 4 words for *snow*, and even this must be seen within light of the fact that we have *snow*, *slush*, *sleet* and *blizzard*, despite English speakers having traditionally had neither any particular fascination with nor any significant cultural rootedness in snow (McWorther 2001: 49)." It is, however, true that in different languages words and word combinations exist that are not exactly translatable into one another. In an English-French dictionary, one may find 'bread' translated as 'pain'; yet when a Frenchman has 'pain' in mind, his thoughts are different from an Englishman's, because a French *baguette* is very different from English *sandwich* bread. And it is also true that not only semantic issues, but also grammatical issues can be related to culture: "Japanese [for instance] has different forms of many verbs depending on the power relations between people in exchange. To discuss eating with a peer, one uses the verb *taberu*; however, to talk about eating in a conversation with a superior personage – or even to talk to someone else about having eaten in the presence of the said personage – the verb is *itadaku*, whereas in referring to that higher personage's eating (whether that person is present or not) one uses a different verb, *meshiagaru*. Obviously, these verb forms are manifestations of the hierarchical nature of traditional Japanese culture (id., p. 49)." Finally, it is also true that some cultural inventions, such as writing, may introduce more formal style elements into speech (such as relative clauses etc).<sup>516</sup> Nevertheless, for many other typical features of a certain language, such cultural differences do not play any role. The German word *Fleisch* is not equivalent to the English *flesh*, because in English the word *meat* exists as well, and the uses of the German word are more or less divided by these two English words. But these things have nothing to do with different English and German cultures. "[N]or is there anything inherently 'French' about 'taking' a beverage (*je prends une bière* "I take a beer" for "I'm drinking a beer") instead of drinking it or inherently English about 'taking' a nap, whereas the French 'make' one (*faire un petit somme*) (49)." Or on a syntactic level: the English, French and German syntax have a

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<sup>516</sup> McWorther 2001 shows that relative clauses were only introduced into the language of the Eskimos after they had learned to write. Another example of a cultural phenomenon strongly influencing language is an Indonesian tribal practice that, when somebody has died, his name (or words similar to his name) may no longer be pronounced. When the name of the dead person is akin to a word or morpheme with a key role in syntax, this may result in significant changes to a language in a short span of time.

different system of definite articles. English has one general ‘the’, French has a male and female version and German has a male, female and indeterminate version. But these fundamental differences (it takes a lot of time to learn the correct article/gender with every German and French word) cannot be related to any difference in culture (why is the moon male in German [der Mond] but female in French [la lune]?). Even within one language, the gender marker can evolve: knowledge (*Erkenntnis*) was sometimes still neutral (*das Erkenntnis*) in the age of Kant, though it later became female (*die Erkenntnis*).

This clearly shows that the evolution of language is in general independent of the evolution of other cultural elements;<sup>517</sup> this is important because it justifies our treatment of the evolution of language as isolated from cultural evolution in the next chapter.

We will now look at more detailed examples and indicate any parallels between diachronic linguistic evolution and Darwinian evolution. As mentioned, language change can happen at many levels: i) the sound, ii) the lexical or morphological and iii) the syntactical. Some changes, as we will see, cannot be uniquely attributed to one level.

**i) Sound change** (Trask 1996, chapter 3 and 4): as we just saw (point 10), sound change can happen discretely and gradually through an accumulation of phonetic changes or through lexical diffusion. The case of the accumulation of gradual phonetic changes, which was central to the view of the Neogrammarians, is best known. We will briefly discuss sound erosion here because of its supposedly *adaptive* nature; after that we will discuss rebracketing, which will bring us to lexical change.

**Sound erosion:** there is a strong tendency for sounds to erode or disappear over time, especially when the accent does not fall upon them. This supposedly happens because of the wish to speak *quickly*, due to which sounds are pronounced less distinctly (McWorther 2001: 18). In the next generation, less-distinct sounds become the default, and so the sound becomes even less distinct until it disappears in a few generations (whether parents or children are the first to pronounce words more sloppily, see footnote 512). The endings of French verbs provide a good example.

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<sup>517</sup> See also McWorther 2001: 16 “All human speech varieties are always in a constant process of slow transformation into what eventually will be so different as to be a new language entirely. This change is certainly *influenced* by historical, social, and cultural conditions but is not *caused* by them alone; the change would continue apace even without these things. Human speech transforms itself though time just as vigorously, and even more so, in isolated hunter-gatherer societies where cultural change of any kind has been minimal for millennia. (...) [L]anguage is a fundamentally mutative phenomenon.’ See also p. 51: “Because all languages are and have always been in a state of continual transformation, anything we see in a language today is the result of change. These clicks [the click sounds in certain sub-Saharan African languages], then, are products of sound change, but that evolution cannot have had anything to do with ‘culture.’” And p. 52 “Thus language change is hardly independent from culture, yet it is in no sense collapsible into a larger category of cultural change. Language overlaps with culture but is not subsumed by it. More to the point, most aspects of any human speech variety’s sounds, sentence structures, and word meanings are determined not by culture but by the cumulative effect of countless millennia of transformation proceeding through structured chaos. Although Japanese developed its hierarchical vocabulary alternatives because of culture, in the mean time myriad sound changes, extensions, grammaticalizations, reanalyses, and random driftings in word meanings were taking place. The result of these processes constitute about 98.5 percent of the task of learning the language and had not more to do with Japanese culture than the rise of (...) [‘present perfect’ constructions in German] did to the German “soul.” This is, and has always been, the case in all the world’s languages.”

We learn ‘je chante, tu chantes, il chante, nous chantons, vous chantez, ils chantent’, but the pronunciation of the first three forms and of the last form is just ‘chant’. The endings have become fixed in writing, but they are no longer spoken. Their erosion becomes even more dramatic when one remembers from which Latin endings the French endings evolved (*canto, cantas, cantat, cantamus, cantatis, cantant*).

The opposite of sound erosion is *epenthesis*, whereby sounds are inserted into a word to *ease pronunciation*. Examples are Proto-Greek *amrotos* which became *ambrotos* in *Ancient Greek*, the Old English *thun(o)r* which became *thunder* in English (examples in which consonants are added), and the Latin *stupidus* which became *estúpido* in Spanish (an example in which a vowel is added). Words only become shorter due to sound erosion; sometimes a few unpronounceable sounds then collide with each other, so that a new sound is inserted to make words more *pronounceable*.

Thus, there are adaptive reasons for the opposed process of sound erosion and epenthesis: speaking more quickly with less energy on the one hand, and speaking more clearly on the other hand. These reasons are not newly invented, but can already be found in the Neogrammarian literature.<sup>518</sup> However, it is not certain that these processes really make speech and language use more efficient. Take the example of the French verbs: the endings were simplified, but at a cost of having to use personal pronouns all the time. These costs were not taken into consideration, as it was claimed that one could speak more quickly due to sound erosion. Another example of hidden costs: sound erosion can make different word forms start to look very similar, so that it becomes impossible to discern them. An ‘invention’ to overcome this situation is tone. Two words that look alike due to sound erosion can be marked as distinct if they are pronounced in different tone; the first in a high, the second in a low tone.<sup>519</sup> What one wins in the speed of pronouncing is lost in the complexity of tone.

Thus, although sound erosion may make languages simpler or faster to produce, it can also make languages more complex and can even have consequences for syntax (as the necessary introduction of personal pronouns in French showed). Any conclusions that linguistic change is adaptive thus need to make very careful calculations about the actual complexities involved. It could be that some forms of erosion do make language use faster, but certainly not necessarily so. Some students

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<sup>518</sup> See Paul 1888 who argues similarly. See also Trask 1996: 56 “Naturally, speakers, being human, prefer to make less effort rather than more.” In the context of the phonetic change induced by the famous English Great Vowel Shift, Trask further remarks: “All chain shifts, though, however they start, can be plausibly interpreted as continuing in order to maximize the use of the available phonological space: the farther away contrasting segments are in phonological space, the easier it is to tell them apart when listening to speech, and the less likely are misunderstandings (1996: 87).”

<sup>519</sup> There are more tones than two; in Cantonese, for instance, there are by most counts six tones: ‘high and level tone’, ‘high and rising tone’, ‘middle and level tone’, ‘low and falling tone’, ‘low and rising tone’ and ‘low and level tone.’ See also McWhorter 2001: 195f “Tones emerge in a language as an accident of sound erosion rather than out of any communicative imperative. Specifically, a language becomes tonal as a kind of ‘desperate measure’ when sound erosion has left this as the only way to distinguish one word from another. In Vietnamese of roughly two millennia ago, for instance, words were distinguished by sounds as we are accustomed to. (...) Technically also tonal differences distinguished the words. [But] these subtle distinctions in tone were essentially background noise when the (...) [sounds] were still there to distinguish the words; to not pronounce the tones would simply create a cute but comprehensible ‘accent’. However, when these final sounds eroded, they left behind the tone differences on the vowels before them. At this point, for better or for worse, the tones were all that was left to mark the distinctions in meaning.”

Note that McWhorter possibly wrongly assumes that tone was not a part of proto-language. Apes are better at singing than speech, hence why not assume that meaningful word-sounds in proto-language were distinguished by tone? See §4.3.

of diachronic evolution call language change neutral because all changes do not change the efficiency or adaptiveness of language, just as Kimura's molecular evolution was neutral (§1.23). That may be true, but as such it is only a speculation that requires the same calculations of all the effects involved.<sup>520</sup> So far, nobody has done such calculations.<sup>521</sup>

**Rebracketing:** rebracketing occurs when the spacing of a stream of words is consistently misinterpreted. Examples: a 'nickname' started as "an *ekename*; in earlier English *eke* meant 'also' (McWhorther 2001:28)." Or the other way round: an orange comes from *narangi* (*idem*). Another example in which rebracketing meant that no brackets were left: *Goodbye* comes from *God be with you* pronounced very quickly. This latter case is sometimes called contraction. Rebracketing can occur not only on the level of words and word combinations, but also on the level of syntax, often accompanied by grammaticalizations (see below). A semantic shift occurs in most cases of rebracketing, even in the seemingly pure case of rebracketing that 'nickname' offers: the character of *eke* (also) in 'nickname' had to wear down to reinterpret an *ekename* as a nickname. This process of rebracketing is similar to gene transcription, in which a base pair is wrongly transcribed and added to the next gene. It can have similar radical consequences for the gene, as it may start encoding for a very different trait. Gene retranscription can be neutral, negative or once in a while lead to new adaptations. Similarly, rebracketing can be neutral, make syntactic rules more arbitrary or the other way round: lead to new linguistic innovation or more consistency in the use of the syntactic rules.

**ii) Lexical change** (Trask 1996 chapter 1): the example of rebracketing involved semantic shifts, which are part of lexical change. We will first discuss semantic change and then compound word formation.

**Semantic change and the expressiveness cycle:** semantic change is a phenomenon with which we are all familiar, as the meaning of words changes gradually over the ages. The word 'silly' provides a nice example. "In old English the word that became silly meant 'blessed'. Just as wanting to do something implies that one will do it, blessedness implies innocence. That kind of implication led people to gradually incorporate innocence into their conception of the word, and through time innocence ended up becoming the main connotation rather than the 'definition 2' one. (...) If one is innocent, one is deserving of compassion, and this was the next meaning of the word (a 1470 statement: *Sely Scotland, that of helpe has gret neide*), but

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<sup>520</sup> Trask describes sound evolution itself (in isolation from the semantic or syntactic effects that a sound changes may have) as an everlasting struggle between syntagmatic phenomena (affecting the sequence of speech sounds within words and phrases such as sound erosion, epenthesis, rebracketing etc.), and paradigmatic phenomena (the system of sounds and the consequences that changes in one type of sound have for the rest of the system, as is observable in chain shifts like the English Great Vowel Shift): "A crucial observation has been that there are always competing phonological pressures, both syntagmatic and paradigmatic; these can never be satisfied at once, and a great deal of phonological change can be understood as endless attempts at satisfying these competing pressures, with each resulting change introducing new strains into the system (Trask 1996: 95)." This everlasting battle can be seen as a type of neutral evolution.

<sup>521</sup> One could in principle calculate how much faster one can speak due to sound erosion; one could, for instance, calculate from physiological models how much less energy a less distinct vowel costs to produce. However, the other costs of sound erosion, such as the necessary introduction of pronouns or tone, cannot be calculated so easily.

because the deserving compassion has a way of implying weakness, before long the meaning of *silly* was ‘weak’ (1633: *Thou onely art The mightie God, but I a sillie worm*). From here it was a short step to ‘simple’ or ‘ignorant’, and finally *silly* came to mean ‘foolish’ – having begun meaning ‘sanctified by God!’ (McWorther 2001:32)”

Due to semantic changes, words can even shift their categories. In the process of changing, *silly* remained an adjective in the category of personal character traits (which could by extension be said of other things). But consider this sentence: ‘Plato hopefully travelled to Syracuse.’ Around 1600 this meant *Plato travelled full of hope/in a hopeful manner to Syracuse*, although from 1800 onward this started to mean *I (really) hope or it is (really) hoped that Plato travelled to Syracuse*.<sup>522</sup> In this case, the literal meaning of *hopefully* as ‘full of hope’ did not change very much, but the attribution of this hope shifted drastically from a concrete person mentioned to some person(s) not explicitly mentioned. ‘Hopefully’ also remains an adverb, but now it modifies the sentence as a whole rather than elements in the sentence. Thus semantic shifts can have syntactic consequences.

The expressiveness cycle provides a special case of semantic change. Slang and other fashionable words often fall prey to this. When an expressive word is used too often, its effect gradually wears down. A good example from slang is the word ‘groovy,’ which is not particularly groovy these days. A good example from everyday speech is the word ‘terrible’. It was once used for truly horrifying things, but through the years it gradually became used so often that it now designates things less grisly – like unsavoury meals or unsightly architecture. Its use has worn down.<sup>523</sup> Just as viruses should not become too successful because they kill all their hosts, words for ‘extreme’ or ‘rare’ situations should not become too commonly used or their use wears down. This puts a very pragmatic pseudo-Darwinian limit on use of slang and fashionable words.

**Compound word formation:** word formation is a field of study that sometimes comes close to etymology, but that also touches on morphology. It analyzes such interesting phenomena as backformation (the formation of a word by adding a suffix from another, such as ‘writer’ by adding ‘-er’ to the verb ‘to write’) and re-analysis (hamburger originally indicated a citizen of Hamburg, but became re-analyzed as Ham-burger, after which the new morpheme -burger could be added to other words to form cheeseburger and veggieburger). One other way of forming a new word or phrase, which also illustrates lexical diffusion, is to construct a calque. A calque (or loan translation) “is a new word or phrase constructed by taking a foreign word or phrase as a model and translating it morpheme-by-morpheme (Trask 1996: 21).” An example is the German *Mit-Leid*, which is a literal translation of the Latin *compassio*. Interestingly, however, with such calques new morphological (and ultimately syntactic patterns) can be introduced into a language. The Germans started using not only the word *Mit-Leid*, but many similar words as well.

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<sup>522</sup> This sentence is still ambiguous. It can mean: it is hoped that Plato traveled to Syracuse (instead of Aristotle), it is hoped that Plato traveled to Syracuse (instead of teleporting to Syracuse), it is hoped that Plato traveled to Syracuse (instead of staying at home) or it is hoped that Plato traveled to Syracuse (instead of Troy).

<sup>523</sup> Similarly, when someone always calls for help, even in case of the slightest troubles or misgivings, nobody will help him when some real problem arises; his calls for help have worn down.

Note that, apart from the expressiveness cycle, there are no real principles behind semantic change.<sup>524</sup> Elizabeth Traugott (1989) proposes some, such as the tendency of external descriptions to become internal descriptions of perceptions and evaluations (words for ‘soul’ in many languages once denoted just ‘air’), or external descriptions to become textual meanings (the word ‘while’ once indicated only a period of time, but now structures discourse as in ‘while you wait, I call Margie’). However, these tendencies are not unique: we start to attribute inner states to machines (the computer does not want to cooperate today), and the evolution of textual meanings may just be a side-effect of the fact that our culture has become literate – and literary structure is in need of clear text-structuring words. The tendencies found in semantic change will probably never turn into more than rules of thumb. Similarly, it will never be possible to indicate with absolute certainty how genes will change and what consequences this will have for the phenotype. In genetic evolution this is one of the causes that not all elements of the phenotype are adaptive. Similarly again, not all elements of syntax or language will likely be adaptive or functional.

### iii) **Syntaxis** (Trask 1996: chapter 5 and 6):

**Over-extensions in syntax:** there is a tendency for occasional patterns in a syntax to generalize into exceptionless across-the-board rules. An example: in Latin the plural for sisters (in nominative) was ‘sorores’, an ending with *-es*, whereas the plural for women was ‘feminae’, the ending being *ae*. In French the endings both became ‘s’: ‘soeurs’ and ‘femmes’. The rule: plural +s spread like a virus all over the French language. Other examples are the regularization of verb conjugations in English and French compared to Latin. The details of these processes require a great deal of empirical research; the spread of the *-s* in French as a plural marker may have involved many other intermediary steps that had nothing to do with the extension of a plural pattern. However, the end-result was such an overextension.

**Grammaticalizations:** grammaticalizations happen when semantic elements become regularised into certain fixed patterns, called grammar. We already mentioned the example of the French word ‘pas.’ This initially meant ‘step,’ occurring in sentences such as “je ne marche pas” and “je ne promène pas” next to “je ne marche” and “je ne promène” (meaning literally *I do not set another step, I do not stroll a step* versus *I do not walk, I do not stroll*). These existed alongside combinations such as ‘je ne mange’ and ‘je ne mange goût’: I do not eat versus I do not eat another piece. But ‘pas’ became grammaticalized to mean ‘not’; in contemporary spoken French, if you want to say ‘I do not eat,’ you say formally *Je ne mange pas* and informally *Je mange pas*. Linguists think that almost all “grammatical twisters and bells” that are “developed far beyond the call of duty” (McWhorter 2001: 190)<sup>525</sup> are the result of past grammaticalizations. These twisters and bells

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<sup>524</sup> See also Trask 1996: 45f “You will note that my discussion of semantic change has been somewhat anecdotal, and you may be wondering whether there are any principles involved. Certainly there are some interesting observations to be made (...) [but they are such] we are only beginning to understand.”

<sup>525</sup> See McWhorter 2001: 178 “In any language on earth, the structure of the grammar and the subdivisions of meaning it can convey far overshoot what would be necessary to even rich and nuanced communication. All languages are provided with the equipment to convey sine qua non fundamentals of communication. They all have, for example, nouns, verbs, pronouns, words to indicate the position of an object in relation to others etc. However, there are more potential ‘spaces’ that a language can fill, all of



include the tenses, modalities and aspects of the verbs and their conjugations, the system of gender and category markers as well as the declinations of the nouns.

A nice example of grammaticalization (in which a semantic shift and a case of rebracketing can also be observed) is the origin of our *perfect tense* construction, as in ‘has seen’, which feels so natural and elementary to us.<sup>526</sup> Latin, however, does not distinguish between imperfect and perfect tense: if we translate the word ‘vidit’, we sometimes have to opt for ‘saw’, but in other contexts must choose ‘has seen’ (when it is clear that the ‘activity of seeing’ is completed but still has consequences for a later time).<sup>527</sup> The perfect tense construction, which is absent from Latin and which feels so natural to English speakers, evolved out of Latin through many stages.<sup>528</sup>

“As Latin began to become French (and other Roman languages like Spanish and Italian), a new construction arose using the verb for *have* with a past participle. At first, though, the Latin construction didn’t have the meaning of our have-perfect. In saying “*Eam habeo visam* [her I-have seen]” a late Latin speaker was using *have* in its full meaning of “possess.” What the speaker meant was “I possess her in the state of having been seen.” This isn’t as bizarre a sentiment to express as it may first appear – an equivalent in English is when we say *But I already have the dress sewn up – why not wear it?* Just as we have the dress sewn up, the Roman had the woman in the state of having been seen – that is, “She is in my sight.” This difference in meaning was reflected in how the case endings worked: *Eam habeo visam*. The ending of the verb form for ‘seen’ agrees with *eam* ‘her’ because it describes something about ‘her’ – what you have is her, all nice and seen, just as you might have a dress all nice and sewn up. (...) However, if you have something in the state of having been seen, then this implies that in the recent past you saw it – just as if you have a dress nice and sewn up, then it pretty much follows that in the recent past you sewed it up. That kind of looming implication has a way of transforming constructions in languages into new ones. The assumption that something you have in your sight is something you came to see in the recent past led to a gradual reinterpretation of the sentence as meaning just that (...). As part of this transformation, *have* evolved from concretely meaning ‘to possess’ to just indicating the pastness. *Have*, then, went from being a good old-fashioned ‘real’ verb into being a mere helping verb situating ‘to see’ in time – another instance of *grammaticalization* like the evolution of *pas* in French into a negative marker. Under this new interpretation, the sentence (...) was now about you having seen her – in other words, *see* was now *taking* an object instead of being *part* of the object ‘her in the state of being seen’. As such, it no longer made sense for

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which a native speaker of a language might suppose “must” be filled but which in fact could not be, with no detriment to communication.”

<sup>526</sup> See McWhorter 2001: 178 “Ludwig Zamenhof dutifully included it [the past/perfect distinction] in creating his artificial language Esperanto, his European linguistic roots making it naturally seem to him that even a language designed to be maximally “simple” and “universal” must certainly have a past-perfect distinction. Yet a great many languages do not make such a distinction overtly and usually just say *I found the money* in both cases, the nuance conveyed by *I have found the money* simply left to the context.”

<sup>527</sup> For example, ‘I have eaten lunch’ implies both that a previous action happened (‘I ate lunch’) and that a current state resulted (‘I am full’). This differs from the plain ‘I ate lunch’, which implies only that an action happened, with no relevance to the present.

<sup>528</sup> The evolution of the perfect tense is a very complex process with many intermediary steps, which themselves need not involve evolution towards a perfect tense construction, and which require much further analysis. The outcome of the evolution, however (that a verb that marks possession evolved into a marker of past tense), is undisputed. This is again just like a chemical reaction: the reactants and the end-products are often known, but the intermediary stages of the process are often not known.

*visam* to be marked as an object along with *eam*. Accordingly, speakers gradually stopped marking *visam* with the ending agreeing with *eam* and instead gave it the default participial ending *-um*, which agreed with nothing: *eam habeo visum*. (McWhorter 2001: 29f). Here, then, was a rebracketing (...) on a large scale. The expression began as: ‘habeo [*visam eam*]’ and evolved into “habeo visum [*eam*]. It is the third person singular version, *habet visum* ‘has seen’, that further developed, through erosions of sounds, into the *a vu* in our French *La femme n’a même pas vu*. This kind of reshuffling occurs with various constructions in all languages at all times (McWhorter 2001: 31).”

With these ingredients of language change, we can look back at the image of the tree of languages that linguists of the 19<sup>th</sup> century used, aligning themselves with Darwin. According to this old image, languages slowly branch from a common ancestor. In biology, the image of the hierarchical tree has been replaced in the 20<sup>th</sup> century by that of a tree *growing in upon itself*, as we encountered in the work of Margulis (see §1.23). Bacteria from different species exchange DNA; the same RNA viruses can reinscribe themselves in the genome of unrelated species (such as cats and humans), without being found in the genome of other animals descending from the common ancestor of cats and humans; sometimes whole organisms with their whole DNA get merged into one another, such as the chloroplasts in plant cells and the mitochondria in general eukaryotic cells. An analogous image now holds in the area of diachronic linguistics: languages not only change due to the splitting of a common ancestor (a hierarchical tree), but also because sounds, words and even whole syntactical structures are imported cross-languages. The hierarchical tree of languages is also transformed into a *language tree that grows in upon itself*,<sup>529</sup> and no parts of the tree are excluded from this mingling ingrowth.<sup>530</sup>

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<sup>529</sup> In the 19<sup>th</sup> century, the popular tree model of languages was already being criticized. Linguists such as Johannes Schmidt and Hugo Schuchardt stressed that languages change not only by vertically splitting from a common ancestor, but also due to horizontal exchange (e.g. lexical diffusion). A new model was constructed, the wave model, that did justice to the dimensions of horizontal exchange between languages. In the wave model, new features of a language spread from a central point in continuously weakening concentric circles, similar to the waves created when a stone is thrown into a body of water. This should lead to convergence among dissimilar languages. Unfortunately, in the wave model, descent from a common ancestor (so clearly visible in the tree model) could not be marked. Our model of the tree that grows in upon itself tries to combine the tree and wave models. See Labov 2007, who also proposes combining these two models.

Note that if one likes to stress the inclusion of the wave model in the new model of the ‘tree that grows in upon itself’, one could also call it the bush model.

<sup>530</sup> See McWhorter 2001: 128f “I have analogized mixture to the mating of a horse and a mule, but one problem here is the implication that language mixture is rather exceptional. Also, because mules are sterile, another problem is the possible impression that hybridicity in language results in speech varieties somehow broken. Modern developments in biology [however] suggest a richer analogy. (...) As Margulis puts it: “In reality the tree of life often grows in on itself. Species come together, fuse, and make new beings, who start again. Biologists call the coming together of branches – whether blood vessels, roots or fungal threads – anastomosis. (...) Anastomosis, although less frequent, is as important as branching. Symbiosis, like sex, brings previously evolved beings together into new partnerships.” (...) So it is with language, not only between languages in one contained area like Kupwar or even between several languages occupying a larger region such as the Balkans, but sometimes between languages by the continent. All of the world’s languages as they evolve carry along words, and often even structures, from other languages, such that some languages are even fifty-fifty hybrids of both parents. In broad view, the world’s languages comprise tens of thousands of dialects harboring evidence of symbiotic matings in the past.” See also p. 93 “So far, in describing how the first language split into thousands of subvarieties, I have implied that speech varieties have developed like a bush, starting from a single sprout and branching in all directions, each branch then developing subbranches and so on, culminating in a dense web of a plant whose outer layer is crowned

The parallel of the evolution of linguistic forms and the evolution of DNA can be made even stronger. In DNA, fundamental switches (Hox-genes) are sometimes recoded to become simple genes or the other way round; this corresponds to syntactic combinations becoming obsolete, and to lexicalized standard formulas being used in exception cases (on the one hand) and semantic items becoming grammaticalized (on the other hand).

Thus we find strong *formal* parallels between the evolution of DNA and biological organisms on the one hand and the evolution of linguistic forms on the other hand. In the next chapter we investigate whether these suggestive parallels are due to similar forces of selection at work leading to adaptive complexity or whether the parallels are only accidental. We will show, within the frame-work of the dual-inheritance theory that this is not just accidental, but that the processes at work are not directly identical either.

### **b) Sociolinguistics: pidgins and creoles**

The name *sociolinguistics* might look like a clear pleonasm. Every language is a social ‘instrument,’ insofar as true private languages either do not exist at all or can be dismissed as meaningless. The name was coined in the seventies, due to the efforts of William Labov<sup>531</sup> and others to distinguish their discipline from the popular formal linguistics of Chomsky and others. Sociolinguistics studies the effect of all aspects of society, including cultural norms, gender expectations and context, on the way language is used.

One important claim of this discipline is that language use is much more diverse than we usually think. People from different regions and different social classes use not only different words, but also different accents, different lexicons, different syntactical structures, different sentence lengths, and different intonations. Some of this variation can be explained by taking context, social class, gender norms and the relation of the speaking partners into account. There are thousands of examples like these. Professor Henry Higgins from *My Fair Lady*, for example, boasted that he could identify the street where someone was born in London simply by hearing that person speak. What sociolinguistics thus shows is that words are over-coded: single words have more than one function. Nouns, for instance, are never just names of things; they reveal something of the speaker’s background, social status, gender, birth place etc. and thus function within systems of social, or Darwinistically speaking, power relations. Just as certain pieces of genes code for enzymes that function in more than one chemical reaction and are thus responsible for more than

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with thousands of leaves, symbolizing languages or dialects. (...) Indeed linguists have traditionally taken the “family tree” model as central to how language has developed. Yet particularly in the past 20 years, language change and classification specialists have come to realize that this model actually only takes us so far in describing the reality of what has really happened to the first language as it spread across the planet. Just as it is inherent to languages to change gradually into new ones, it is equally inherent for them to mix with each other. Moreover, just as language change is an unbroken process along which no lines of demarcation can be drawn, language mixture is along a continuum of degree. Viewed close up, not only are “languages” clusters of dialects but, in the past 150.000 years, the dialects of languages have been constantly adopting words, sounds and sentence structures from neighbouring dialects of other languages, spoken by peoples encountered through migration [etc.]. (...) The result is that we can only preserve our family tree or bush analogy by making it a flowering bush, with each dialect not a leaf but a flower, with the various flowers cross-pollinating one another.”

<sup>531</sup> In his 1966 *The Social Stratification of English in New York City*, he (as his title suggests) presents the results of his collection of data on the various dialects of English spoken in New York City and their relation to social class.

one phenomenological trait, the same words function within more than one system of relations.

Some linguistic variation cannot be attributed to these social factors, but is due to random drift. The branch of sociolinguistics that deals with *random* fluctuations is dialectology, which studies geographical distributions of different language use. These random differences, however, can start to reflect social differences, since speaking a dialect is seen as a characteristic of backward people from isolated areas. Sociolinguists tend to see the major language of a country as a dialect amongst others, a dialect that only happens to be spoken by the powerful elite of that country. In the words of the Yiddish linguist Max Weinreich: “a language is a dialect with an army and a navy (Weinreich 1945: 13).” Often this elite resides in the capital of a country; one of the rules of thumb of sociolinguists is that the major language (‘dialect’) of a country is less and less commonly spoken the further away one gets from the capital.

With nationalism, and with general education in which a standardized writing system is taught to all children in a country,<sup>532</sup> dialectical variation tends to become smaller. These effects continue despite the self-conscious revitalization and cultivation of dialects by people who speak them (which sometimes even gives rise to new over-generalization of the rules of the dialect.) Another rule of sociolinguistics is that people from higher social classes and with higher education tend to speak more similarly to each other than people from the lower classes do. This may be due to the various reasons outlined above: higher education, membership by birth to the higher social class that already speaks this language, as well as the imitation of this class by those ambitiously striving for power.

This huge *variation* in language use is important for a Darwinist conception of language; it implies that there is something to be selected for, both regarding linguistic forms and regarding different linguistic abilities (although these are probably closely related to general cognitive abilities). This variation is often continuous, as in the famous case of the Dutch-German border dialect continuum: Dutch and German are not mutually intelligible languages (i.e., when you speak or understand one you don’t thereby speak or understand the other), yet people living near either side of the border speak such dialects that they can easily understand each other.

### **Pidgins and creoles**

An even more important topic in sociolinguistics for research on the origin of language is the empirical study of *language birth* of pidgins and creoles. Language birth is here of course not concerned with the *absolute* birth of human language, but with the origin of languages in communities where languages are already spoken. Pidgins are languages that arose as people of very different linguistic backgrounds had to communicate with each other. The prime example is that of slaves from different parts in Africa, who for communication amongst each other and with their masters were forced to devise a very simplified language. A creole is a language that evolves out of a pidgin, because for some people (notably the children of slaves) the pidgin is their first language. In the process of creolization, a pidgin becomes more complex, richer and at the same time standardized. The advantage of the study of pidgins and creoles is that we can actually see how a simple language evolves into a full-blown language, but again this process should not be confused with the evolution of language as such, since the creation of a pidgin already presupposes people who have

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<sup>532</sup> Hence note the effect of writing on speaking! Writing is not just coding the primary form of language: as speech, it can influence speech itself.

mastered another language. General accounts of pidgins and creoles are Holm (2004, 2000), De Graf (1999), and Sebba (1997). Here, Sebba's detailed, balanced and very informative monograph *Contact Languages – pidgins and creoles* will be used.

Sebba warns us from the beginning that the common “notion of pidgins as a mixture of two or more languages underlies many of the ways of talking about the origins of pidgin grammars (25).” One language is often seen as the lexifier, the source from which the majority of a pidgin or creole takes its lexicon. This is often a European language; another language is seen as the *substrate*, the grammars of the indigenous languages with which the lexifier comes into contact. The lexifier is sometimes also called the *superstrate*. This idea of “a ‘top layer’, a superstrate of European vocabulary resting on a ‘bottom layer’ (substrate) of indigenous grammar bears more than a passing resemblance to the colonialists’ model of a (European) social elite resting (literally!) on a socially inferior indigenous underclass.” It also supports the idea of grammar as the form and words as the content of a language, and we will shortly see what consequences this has for the understanding of these languages. In reality, however, the grammars of pidgins are so minimal that we can hardly recognize the grammar of an indigenous language.

The ‘grammars’ are reduced to bare bones, and supposedly have the following features in common: I) syntactic features: a) no definite and indefinite article, b) no copula *to be*, c) tense, aspect and modality are marked externally to the verb (‘Yesterday he go to’ etc.), d) no complex sentences with relative clauses and e) no passive forms, II) morphological features: a) very few or no inflections, e.g. no morphologically marked number, case, tense, gender or grammatical agreement, b) analytic constructions used to mark possessive, e.g. X of Y rather than Y’s X, III) phonological features: a) avoidance of difficult<sup>533</sup> sounds, b) simple syllabic structure (i.e. consonant vowel consonant vowel), c) tone is seldom used to distinguish words, IV) semantic features: preference for semantic transparency, V) lexical features: a) small vocabulary, b) very small inventory of pre- or postpositions, c) preference for short words and d) small number of compounds (Sebba 1997: 39f). Interestingly, due to its small vocabulary (V.a) there is no room in a pidgin for two words with the same ‘meaning.’ And the words that do exist seem, from an outside point of view, to lack specificity: for instance in “tok pisin [a certain pidgin] the word *ston* could be used to translate not only English ‘stone’, but also ‘boulder’ and other near-synonyms like ‘rock’. Similarly, the word *lek* can mean ‘leg’, ‘food’, ‘hind leg’, ‘footprint’ (53).” These word extensions become so huge that we can sometimes speak of “multifunctionality of lexical items, when the same word functions as two or more different syntactic categories, for example the word *kaikai*, can either mean ‘food’ or ‘to eat’ (54).” However, is this multifunctionality not the projection of our own categories on a word that is in fact both or neither? A word that functions by avoiding the categories ‘verb’ and ‘noun’ that are so fundamental to us?

In creoles, these things change a little; creoles have often definitive/indefinitive articles, a few inflections and more complex sentences with reflexive pronouns. An interesting example is that adjectives behave like verbs: “In many creoles, the adjective has been analysed as a subcategory of stative verbs (i.e. verbs which describe a state rather than an action). This means that adjectives take verbal TMA [TenseModusAspect] markers and do not require copula (p. 174).”

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<sup>533</sup> But what is a difficult sound? To us, Serbian words with their concatenations of consonants and relative absence of (clear) vowels sound very hard; but if a pidgin were formed between Serbian and another similar language, the supposedly difficult sounds would of course be included.

Again we can wonder whether we are not projecting our categories onto the creoles, with the two (often non-distinguished) forms of predication that we can find in Plato and Aristotle: 1) the ‘subject-predicate’ (thing-state) scheme versus 2) the ‘subject-action’ (agent-action) scheme, with their different metaphysical suppositions. Maybe ‘static verbs’ and ‘adjectives’ (or that which corresponds with them) are not really felt to be different in creoles; is it possible that we project our categories onto creoles because we regard them through the lens of our own grammar?

What we can learn from pidgins (and creoles) is that a language *can be much simpler and almost devoid of syntactical structure, and still function properly* (although in a creolization by the children of pidgin speakers, the language quickly gains in complexity). The initial language could thus have been much, much simpler and still have functioned; thus the Darwinist need not explain how a complex language full of complex syntactical structures evolved all at once, but only how a very simple language evolved and how this language gradually became more complex.<sup>534</sup>

In the context of pidgin and creole research, we again encounter Chomskian-like attitudes in the search for language universals – but also many criticisms of these attitudes. Linguists found that the pidgins (some 63 of them are known, Sebba p. xiii) not only share many of the above features, but also have a standard SOV word order, with a negator just in front of the verb as well as the temporal words: “I not yesterday go market (71).” There are many hypotheses about this apparent order. Some simply deny that it exists, maintaining that it is based on a very poor induction from a limited amount of data, and presenting pidgins that falsify any found regularities. Others assume that the order arose because all pidgins are linked directly or indirectly “to an earlier Mediterranean pidgin (72)”, probably used among different groups of European colonizers themselves before it was used by colonizers and their slaves. Others hold that, even though pidgins arose multiple times, these languages always developed under the heavy influence of European languages that have the same deep structure, or point out that (a group of) pidgins developed out of related West-African languages and that the similarities are due to the substrate structure of these languages.<sup>535</sup> Some even combine these two opinions by postulating a common syntactic core to the languages involved in the process of pidgin genesis. Still others assume a universal linguistic device or universal tendencies to simplify a language when speaking to foreigners (in so-called ‘foreigner talk,’ people tend to systematically simplify their sentences by removing syntactic elements).

The idea of a universal linguistic device in the context of *sociolinguistics* was mainly defended by Bickerton (1981, ch. 3). He postulated the Language Bioprogram Hypothesis (LBH), according to which the observed regularities are due to some ‘program’ in the brain that makes a few basic semantic distinctions that have structural consequences for language. Bickerton does not say where they come from;

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<sup>534</sup> The only point one can make against this claim is that people understand the simple pidgin because they already have a much more complex language of their own. The fact that children can learn the pidgin as a first language speaks against this (although children probably also learn the language of their parents alongside of the pidgin, and partly use the syntax of this language in creolizing the pidgin).

<sup>535</sup> If this view is right, then conclusions drawn from pidgins regarding the original settings of a universal grammar should be evaluated skeptically; the ‘grammar’ of the pidgins is not that of the original settings of the universal grammar, but is related to the actual syntax, for instance that of the language of the dominant group – e.g. the slaveholders who spoke Indo-European languages (Dutch, English, Spanish, Portuguese etc.). The convergence of evidence for settings of a universal grammar (such as SVO), as given by a comparison of the Indo-European languages on the one hand and the genesis of pidgin and creoles on the other hand, is thus a false convergence based upon language mixing within the same language group.

he simply postulates them. They are somewhat reminiscent of Kant's categories. These distinctions are: a) specific versus non-specific. Consequence: the article system. b) state-process. Consequence: differential markings of statives and non-statives, c) punctual-nonpunctual. Consequence: universal marking of nonpunctual verbs in creoles, and d) causative-noncausative. Consequence: the existence of 'passive equivalents'.<sup>536</sup>

More recent literature, and Mark Sebba himself, opt for the view that supposed regularities are the result both of bad empirical work (because many exceptions can be found), and the idea that the mixing always involves dominant Indo-European languages that have a common structure by descent. Bickerton's thesis cannot be upheld: some creoles do not have an article system, some languages do not have passive constructions at all (not even passive equivalents) etc. Just as in the case of diachronic linguistics, we find few universals. As a rule, one can say that a feature absent from the languages involved will not be introduced; hence two languages without an article system that form a creole for communicative purposes will create a pidgin without an article system.

Pidgins and creoles are just an extreme example of the mixing that happens all the time between languages that are in contact with each other. Relative geographical isolation of linguistic communities creates language differences due to linguistic drift. But linguistic communities are of course never completely isolated, and that is why mixing between languages (or dialects) always occurs. Most of the mixing occurs within neighbouring, often relatively similar linguistic communities. We already saw an example of this in the Dutch-German dialect continuum. But language mixing can also occur between less similar languages. One case of this is simple linguistic borrowing. In recent years many languages borrowed words from the *lingua franca* of the modern world: English. This development was so strong that linguistic purists started to complain about their native French or Spanish turning into 'franglais' or 'spanglais'. But these purists often forget that modern English came into existence through heavy linguistic borrowings from Old French (and thus indirectly from Latin, the root of Spanish), and earlier by incorporating Scandinavian/Viking vocabulary (and Latin, for that matter). The lexical influence of the Old French in particular was so strong that one has to suppose that a situation of diglossia prevailed, in which a substantial part of the population was bilingual in both English and Norman French.<sup>537</sup> In English, we still find many more or less synonymous words, one with a Germanic and another with a Latin root, such as 'woods' (German: Wald) and 'forest' (French: forêt), 'flesh' (German: Fleisch) and beef (French: boeuf), 'eat' (German: Essen) and 'have a meal' (French: manger), 'king' (German: König) and 'monarch' (Latin: monarchus) etc. There are several speculations about these differences. It is supposed that the less formal word is generally Germanic, and the

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<sup>536</sup> Linguists speak of passive equivalents when 'the passive voice is not marked morphologically, but by a pattern where an objectless active verb may be interpreted as passive, e.g. Jamaican Creole /di tri plaan/ 'the tree plant', i.e. 'the tree was planted' (174).' However, we can ask again if it not we who already distinguish between active and passive, between actions and the objects of actions and thus, failing to find our own categories, have to postulate certain patterns that function as passive equivalents. Why can't we say that, when a Jamaican says /di tri plaan/, he simply 'links' the words 'tree' and 'planting'?

<sup>537</sup> At the start of the Norman dominance, the Norman overlords who dominated Britain knew only French. But "in the course of a few generations the French-speaking nobility first became bilingual in French and English, and then monolingual in English. (...) Later, French became in effect a foreign language though it still had a role as an official language in England (Sebba 1997:11)."

more formal of French/Latin heritage; it is further speculated that British women, excluded from public life, were responsible for the conservation of traditional English words (related especially to the domain of cooking and other household jobs), whereas men were responsible for the integration of French words related to the public sphere.<sup>538</sup> The case of bilingualism is much more common in history than the current monolingual situation in the US particularly (of course many people in the US have Spanish instead of English as their native language, but these people often speak English almost as badly as the English speak Spanish); Papua New Guinea, just a little smaller than Spain, is already home to around “800 languages (McWhorter 2001: 132)”. Bi- or trilingualism is the norm, monolingualism the clear exception. Clark puts it very strongly: “Most people grow up speaking two or more languages (Clark 2005:364).”

A stronger example of mixing is that of language convergence: this “involves languages within a community changing and adjusting their structures so that they all become more similar to each other. (...) It was found that in Kupwar, a village in India, the languages Marathi, Urdu and Kannada had been in contact for 400 years. The Kupwar varieties of these three languages had grown so similar that there were virtually no differences in their grammatical structure. Although their vocabulary remained separate, a sentence in one of the three languages could be translated word by word simply [looking up] the appropriate items of vocabulary [which of course is normally never the case - MH] (McWhorter 2001: 13).”<sup>539</sup>

The pidgins and creoles we discussed in this section can be seen as extreme cases of language mixing, in which it is thought (correctly or not) that one of the languages forms the substrate language and another the lexifier. But linguists distinguish an even stronger case of mixture, simply called ‘mixed languages,’ in which two languages are completely blent. An example of mixed language is Mishif, “a language with French nouns, numerals, articles and adjectives, Cree [an Indian Language] verbs, demonstratives, postpositions, question words and further possessive prepositions and negative elements from both languages. Not only is there a fairly neat distribution of the two languages across these grammatical categories, both languages lost nothing or little of their complexities (266).” We are thus confronted with a *continuum* of related forms of language mixture: lexical borrowings, heavy lexical borrowings in a situation of diglossia, code switching<sup>540</sup> and language

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<sup>538</sup> See Shay 2008 for a detailed evaluation of these speculations.

<sup>539</sup> At the end of our discussion of diachronic linguistics, we discussed the similarities between natural evolution and linguistic evolution and their tree-models. The case of the Kupwar demonstrates another principle of natural selection in linguistic evolution: things that go through the same replication channel (such as mitochondria and eukarotic cells, symbiotic species such as hippos and the birds that clean their teeth) become more and more *adjusted* to each other.

<sup>540</sup> A case that has something in common with both lexical borrowing and language convergence is known as *code switching*. Code switching is the phenomenon wherein people who are both bilingual will switch between languages during a conversation, even in the middle of one sentence. It can often be seen amongst second generation immigrants who speak both the language of their parents and that of the country in which they were born, but it is attested in written (!) documents from as early as the days of Henry IV (Sebba 1997: 12). It is only recently that code switching was recognized as a systematic phenomenon following certain rules, instead of a linguistic mishmash. Code switching differs from language convergence in that switching is something done by an individual speaker who knows both languages, whereas convergence takes place over time, in a community with a high degree of bilingualism, but in which the languages retain their separate vocabularies and individual members of the community may or may not be bilingual.



convergence, pidgins and creoles, creoloids,<sup>541</sup> and the exceptional true mixed languages.<sup>542</sup> This conclusion dovetails nicely with the image of the tree at the end of the previous section on diachronic linguistics: languages are branches of a tree that grows in upon itself. Selection forces are present to streamline these branches and to defend them against the strong winds of globalization, although many branches will break in the process.

### Summary

We saw many proposals for the presence of a force of selection in linguistic evolution, with some candidates more plausible than others: a) words for things that have disappeared, disappear too because the memory capacity of young children is the bottleneck through which each word has to go in every new generation, b) difficult sounds erode, and sometimes some extra sounds are added, to make speech more efficient, c) small linguistic communities have the most difficult languages just as small human communities accumulate genetic defects due to inbreeding, d) languages used by large groups (such as English and Spanish) become slightly pidginized – or, one can say, streamlined to be learned more easily by groups of immigrants with vastly different backgrounds. These are processes no single person can control.

Aside from these forms of selection, we found other (Darwinian) analogies between the evolution of languages and the evolution of organisms:

- Languages vary in almost all of their aspects, just as organisms do;
- Languages can go extinct, just as species do;
- Linguistic symbols are dual encoded (meaningless phonemes make meaningful words), just as DNA is dual encoded (meaningless nucleotide bases code for meaningful traits);<sup>543</sup>
- The biological model of the tree of life was replaced by the model of the tree of life growing in upon itself, just as the hierarchical tree of languages was combined with the wave model into a new model of a tree of languages growing in upon itself (the ‘bush’ model);
- Versions of punctuated equilibrium (Dixon 1997) exist for languages and organisms;
- Languages change by descent and horizontal transmission just as bacteria and plants do;
- Words undergo expressiveness cycles, just as many organisms with certain traits are subject to frequency-dependent selection;

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<sup>541</sup> A creoloid is “a language which originated through language contact and which has creole-like features but apparently is not a full creole (Sebba 1997: 292)”. One example is Afrikaans. The concept of a creoloid is interesting, because it again shows the existence of all sorts of in-between cases in language classification. Before a pidgin is stabilized, one sometimes speaks of a tertiary hybridization of a language (102), and between a pidgin and a creole one sometimes speaks of an expanded pidgin.

<sup>542</sup> See also McWhorter 2001: 115 “Finally, like language change and the distance of one speech variety from another, Linneaus and Aristotle again fail us when it comes to parsing language mixture, because it is an inherently graduated process. There are no discrete lines on the way from minimal borrowings of words (...) through structural influence to outright language fusions. There are innumerate cases that fall between the prototypical ones in degree of mixture.”

<sup>543</sup> This is a tricky analogy, which might be based on an ambiguity in the word *meaningful*. See appendix 2: a note on biosemiotics.

- Organisms sharing the same replication channel co-adopt, just as different languages spoken in the same community will become structurally more similar;
- Glottometrics uses the same dating methods as biologists using the biological clock; both suffer from the same problems, as their clocks are species/language dependent and cannot be supposed to tick uniformly over large time-scales;
- Linguistics and biology both use cladistic methods to determine ancestral lineages;
- Many diachronic linguistic developments are like genetic drift (or like Kimura's theory of *neutral* evolution at the molecular level).<sup>544</sup>

These analogues clearly show similar structure in the evolution of linguistic forms and the evolution of organisms. The evolution of linguistic forms is, however, only part of the story of the evolution of language, which also includes the biological evolution of language skills. In the next chapter, we will use the dual-inheritance framework to analyze Darwinian explanations of language with more rigor. We will then see how well the evolution of language can really be modeled analogously to a normal Darwinian process.

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<sup>544</sup> See McWhorter 2001, p. 190 "Like the other aspects of language change, the benign overgrowth of language has an analogy in the evolution of plants and animals. Not every aspect of a life form is an adaptive advantage created by natural selection. As Richard Lewontin puts it: "(...) Random fixation of nonadaptive or even anti-adaptive traits because of limitations of population size and the colonization of new areas by small numbers of founders; the acquisition of traits because the genes influencing them are dragged along on the same chromosome as some totally unrelated gene that is being selected; and developmental side effects of genes that have been selected for some quite different reason." The analogy here is not perfect. (...) Nevertheless, Lewontin's observation is valuable. (...) The bulk of any grammar of a language does not lend itself to this kind of analysis [of cultural adaptations] – the features usually evolved because of overzealousness of language change."



# Chapter 4: Darwinian explanations of the origin of language

*In this chapter we focus on Darwinian explanations of language; as Darwinian explanations explain the origin and evolution of complex design traits, this means that this chapter focuses on Darwinian explanations of the origin and subsequent evolution of language. As we will see the phrase ‘origin of language’ has multiple meanings. Conform the two sides of dual-inheritance theory, we focus on the origin and evolution of all organs involved in language use and learning on the one hand and on the origin and evolution of (proto-)linguistic forms on the other hand (§4.1). In the latter case we stress two important characteristics of human speech: dual patterning (each new string of sounds having a new arbitrary meaning) and compositionality (the combination of existing words into new sentences).*

*We restructure the field of existing Darwinian explanations of language (§4.2) using the 8 models of cultural evolution we distinguished in §2.3, something that has not been done before. We show that all models except the dual-inheritance model are insufficient to analyze language evolution. To substantiate the explanation of the dual-inheritance model further, we comment on 6 key aspects of the evolution of language: was the evolution of language a) early or late, b) gradual or sudden, c) speech first or gestures first<sup>545</sup>, d) innate or learnt, e) functional or non-functional, f) by natural or sexual selection. Using the material of chapter 3 we demonstrate that it is likely that language is an early, gradual, speech first, learnt (functional) adaptation which evolved primarily by natural selection (§4.3). We recognize the fact that there need not have been one unique adaptation for which language evolved; in different time periods different functions may have been key to its evolution. We nevertheless show that language is most likely and primarily an adaptation for socio-politics, but that language can have secondary adaptation for hunting behaviour, tool use and child care amongst others. We analyze the positive interdependencies between these adaptations for the evolution of language: hunting provides humans with an energy richer diet, which could cause the growth of bigger brains, which in turn implies better socio-politics, learning skills and tool use and thus a bigger need for language, which leads to better coordinating in hunting and so on.*

*After a discussion of the general aspects of language evolution we turn to explanations of the origin of language organs and the origin of linguistic forms, thus focusing on the two sides of dual-inheritance model. We analyze further whether there existed any form of coevolution between language organs and language use. We show that compositionality (of sound-words) evolved earlier than dual patterning and analyze a plausible scenario how dual patterning might have evolved, given the nature of our speech organs. We conclude in our final evaluation (§4.4) that many elements of the explanations of the origin of language require further empirical research, but that the dual-inheritance theory provides a frame-work that is likely to stay. In line with our conclusion in chapter 3, we do acknowledge, however, that some important aspects*

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<sup>545</sup> Current gesture language is usually dependent on verbal language in one way or another: the sign languages of Bonet, Abbé de l'Épée and Gaillardet which lie at the basis of most national sign languages such as ASL (American Sign Language) presuppose the verbal alphabet for translating words into gesture signs; but even if a current gesture language does not depend on the verbal alphabet, the production of the gestures presupposes normal brain mechanisms that have evolved for the verbal language that we use today. However, in the discussion around ‘gesture or speech first’ the gesture language under discussion is considered to have originated earlier and independent of existing verbal signs and (brain) mechanisms for verbal speech comprehension and production. Only later would this original gesture language and the mechanisms behind it be exapted for verbal language use.

of language use, such as the intentional use, are not likely to be integrated in any Darwinian theory soon.

## §4.1 The origin(s) of language

In this section we briefly discuss pre-Darwinian explanations of the origin of language to provide some context to the discussion of the evolution of language. Then we argue shortly why an explanation of the origin of language today should be a Darwinian one. We start by emphasizing the multiple meanings of the phrase ‘origin of language’.

Philosophers have reflected on language since Antiquity. Sophists like Gorgias and Protagoras taught about the power of words to manipulate and convince people in court and politics; Plato wrote on the relation of words and ideas in the *Cratylus* and on truth-bearing utterances like ‘Theaetetus flies’ in his *Sophist*, and Aristotle devoted a lot of work to the phenomenon of language in his *On Interpretation* and other works of the *Organon*. A few centuries later, Grammarians such as Dionysios Thrax and Varro introduced some categories into linguistics that are still used today, as they tried to systematically describe the structure and grammatical elements of their language.<sup>546</sup>

With reflections on language came reflections on the *origin* of language. The meaning of the phrase ‘origin of language’ is ambivalent: it can for instance refer to the origin of meaningful speech as such (from gestures and or cries to meaningful utterances), the origin of (proto-) linguistic forms, the origin of names and meanings (etymology), the origin of language structure, the origin of the plurality of languages, the origin of logic or reasoning, the origin of speech organs and the origin of brain mechanisms involved in language use, to name but a few. Our basic division (§4.3) will be the *origin of our (biological) ability to speak* versus the *origin of linguistic forms as such (from proto-linguistic forms or cries to our current abundance of forms)*.

In the philosophical literature we find little or no reflection on biology-related questions of the origin of language, but much more on the other question(s).<sup>547</sup> In Plato’s *Cratylus* we already find a pseudo-etymological account of the origin of words that seeks to find the basic, smallest elements (‘stocheia’) of *meanings* out of which the meaning of all other words can supposedly be composed (think of etymological roots, syllables or letters). The aporetic conclusion of the dialogue, however, seems to be that there exist no stable, basic element of *meaning* in language itself, but only outside of language in a-temporal, fixed ideas; with this suggestion Plato paved the way for classical mentalism and essentialism that are hard

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<sup>546</sup> A detailed overview of ‘linguistics’ in antiquity can be found in Everson 1994; subsequent developments in linguistics until the beginning of the 20<sup>th</sup> century are well-covered by Law 2003 and the first of the multi-volume selection of primary sources by Harris and Taylor 1989 is also useful to gain a good overview of the earlier history of linguistics. Some of the developments in linguistics in the 20<sup>th</sup> century were already covered in §3.12.

<sup>547</sup> For two reasons: a) being able to speak was often not (primarily) seen as a matter of biology (speech physiology, let alone speech neurology), but as the *mental* (non-physical) act of mapping words unto ideas or meanings clear to the mind so that if a story on the origin of language use (in any form whatsoever) was given, this simply did not involve biology, b) although in Antiquity theories existed of the temporal evolution of biological species such as those by Empedocles and some atomists, mainstream thought was occupied by Platonic and Aristotelian essentialism which denied the existence of temporal evolution and thus also of our biological, species-specific ability to speak.

to integrate into naturalistic or Darwinian accounts of the origin of language. Nevertheless, in Antiquity we also find naturalistic accounts of the origin of language such as the theory of Empedocles which we can anachronistically label a stimulus-response theory, as Empedocles believed that external events caused people to utter certain sounds, a specific noise in response to each stimulus; as people supposedly shared the same pairing of external stimuli and noise-responses this explains how joint reference could emerge.<sup>548</sup> Empedocles's views were, however, forgotten and Platonism remained long prominent in the theory of meaning and any thinking on the origin of language.

In the Christian era we find a lot of reflections on the basis of the story of the Tower of Babel on the origin of the *multitude of languages* and the problems of communicating with other people, reflection partly given in by problems missionaries experienced in translating the message of the bible to make it understandable for barbarians.<sup>549</sup> Only in the Age of the Enlightenment, however, did reflections on the origin of language rise to true prominence, especially in France. The best known of the many Enlightenment accounts on the *Origin of language* are the long essays of Condillac (1746), Rousseau (1781, 1755), Herder (1772) and Lord Monboddo (i.e., Burnett 1773).<sup>550</sup> In these theories we already find discussions on whether language use can be explained by Platonic innate ideas (Condillac 1746), how the first signs could be understood by their receivers (id, Rousseau 1755), what the primary mode of communication is: gestures or cries (id.), and what the advantages of each mode of communication is (Rousseau 1781), how syntax could evolve, how to deal with the chicken-and-egg situation of language requiring thought and thought requiring language to get off the ground (Rousseau 1781), how human language opens up a whole world that animals lack (Herder 1772) etc. These works also contain strange suggestions. Lord Monboddo for instance believes that just as people learnt to spin from spiders, they learnt to sing and speak from birds. And there were much stranger theories such as John Webb's proposal (John Webb 1669 in Yaguello 1991) that Chinese was the primitive language of mankind, happily preserved by Noah and his family in the Ark. When confronted with such speculative and odd proposals one is almost tempted to think that the famous 1866 ban on papers on the origin of language by the Linguistic Society of Paris was justified.

But this ban was not really needed as the emerging scientific discipline of linguistics in the 19<sup>th</sup> century, although ironically focusing on diachronic or historical linguistics, practiced self-censorship in excluding speculative proposals on the origin of language. Typical for this ban is Max Müller's *Lectures on the Science of Language* from the same year as the ban, in which he discusses possible ideas for the origin of language, giving them funny names<sup>551</sup> such as the *bow-wow* theory (basing language

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<sup>548</sup> See Everson's reconstruction of Empedocles' views in Everson 1994.

<sup>549</sup> An accurate historical overview of these reflections, researched with German *Gründlichkeit*, provides Arno Borst's six volume enterprise *Der Turmbau von Babel* (Borst 1995).

<sup>550</sup> A good review of these theories and a defence of the current importance of the Enlightenment 'rational reconstructions' of language origins can be found in Wells 1987 who also discusses lesser known texts.

<sup>551</sup> Other authors such as Mario Pei (Pei 1965) have given other approaches similar funny names. We list these ideas as a backup to demonstrate that Darwinian theories that assume functional selection pressures behind the evolution of language are often not original in their proposal for the original functions of language as these can also be found in pre-Darwinian literature.

1. **The mama theory:** language began with the easiest syllables attached to the most significant objects. An example is Roman Jakobson.

2. **The ta-ta theory:** Spoken language was preceded by gestures, eventually unconsciously imitated vocally, like the way a child's mouth will move when they use scissors. Sir Richard Paget, influenced by

on onomatopoeia) and *pooh-pooh* theory (also known as the *ouch*-theory basing language on emotional cries), as he rejects, like Herder and explicitly *contra Darwin*, the possibility of evolution from animals to humans: “Language is our Rubicon, and no brute will dare cross it (...) [N]o process of natural selection will ever distill significant words out of the notes of birds or the cries of beasts (Müller 1866: 354).”<sup>552</sup>

For almost a century Müller’s view was dominant and the ban on speculation regarding the *absolute* origin of language (in the sense of language abilities and proto-linguistic forms) more or less effective.<sup>553</sup> Interestingly enough, however, Darwin’s theory did gain prominence in linguistics in the second half of the 19<sup>th</sup> century: not as a theory to explain the *absolute* origin of the first language use, but to describe how one (already fully complex, non-*proto*) language evolved from another

Darwin 1872, believed this. Although many theories hold that gestures preceded vocal language, this theory is unique for holding that vocal sounds arose as *imitations* of gestures.

3. **The yadda-yadda theory:** language originates from social grooming. A modern proponent is Dunbar 1996. See §4.3.

4. **The ding-dong theory** (also known as sound symbolism): language arises from mysterious connections between certain meanings and certain sounds. Small, sharp, high things tend to have words with high front vowels in many languages, while big, round, low things tend to have round back vowels. See for the supposed sound symbolism of *kn\** and *sp\** sounds (see footnote 343). The theory gets its name from the analogy that different objects make different sounds when you hit them. A famous proponent was Max Müller who took it from Oken and Heyse: “everything which is struck, rings. Each substance has its peculiar ring. We can tell the more or less perfect structure of metals by their vibrations, by the answer which they give. Gold rings differently from tin, wood rings differently from stone; and different sounds are produced according to the nature of each percussion. It may be the same with man, the most highly organized of nature’s work. (...) We are still very far from being able to identify roots with nervous vibrations, but if it should appear hereafter that sensuous vibrations supply at least the raw material of roots (Müller 1877: 211).”

5. **The sing-song theory** (also known as the *la-la* theory): language comes out of song, dance and the like (and often play a role in sexual selection as in the scenario of Darwin 1871). Famous proponent is Danish linguist Jespersen who even suggested that contrary to other theories, perhaps some of our first words were actually long and musical, rather than the short grunts many assume we started with.

6. **The yo-he-ho theory:** language began as rhythmic chants, perhaps ultimately from the grunts of heavy work (heave-ho!). The linguist Noiré 1877: 331 suggests that these possibly evolved during collective efforts when rhythmic gestures and calls were produced to coordinate common projects involving many individuals.

7. **The hey you! theory** (also known as the contact theory): the linguist Geza Révész suggested that we have always needed interpersonal contact (Révész 1946), and that language began as sounds to signal both identity (here I am!) and belonging (I’m with you!). Other “hey you! cries” are cries uttered out of fear, anger, or pain (help me!).

8. **The ah-ha theory:** language evolved from insight (Herder 1772).

9. **The goo-goo theory:** language began when man repeated sounds purposely and these were understood by others just as babies automatically mimic their parents’ sounds. Proponents of this view can be found in §4.3.

<sup>552</sup> Despite this critique Müller himself does formulate his own theory of the origin of language: following German romanticism, particularly Schelling, he tries to relate the history of languages to the history of cultures and the supposed most important and central element thereof: religion. I.e., for Müller the origin of language lies in ‘religious awe’ (a little bit similar to the *ah-ha* theory), with the first names for things evoking *numina*. The funny names he gave to other theories of the origin of language are to debunk non-religious, naturalistic accounts of a phenomenon that according to him could only be explained from the vastly superior perspective of religion.

<sup>553</sup> Darwin briefly discussed the origin of our ability to speak (with singing, evolved by sexual selection, as an intermediate stage) in his *The descent of man and selection in relation to sex* (1871) and even briefer in his *The expressions of emotions in man and animals* (1872). In this earlier account Darwin places emphasis on the role of music in language evolution (as musical abilities are clearly sexual selected traits); in the later account the role of gestures (facial/manual etc.) are more prominent as in the book. Rare book length accounts of the origin of language in this period are Geiger 1868 and Wundt 1921, both working from a non-Darwinian perspective.

(already fully complex, non-*proto*) language, i.e., how French evolved from Latin. Thus Darwin's theory was used to explain the *relative* origin of languages.<sup>554</sup>

The first author who did this was the prominent linguist Friedrich Müller<sup>555</sup> who just after Darwin used the latter's theory to elevate the science of linguistics to a higher level of scientific prestige. Darwin himself in turn based his account of the origin of language in the *Descent of Man* (1871: 465–466) on Müller's theory, acknowledging that the principles philologists are using in their taxonomy of Indo-European languages were 'Darwinian' in nature: "The formation of different languages and of distinct species [are] (...) curiously parallel. We find in distinct languages striking homologies due to community of descent, and analogous due to similar processes of formation."

It can even be argued that at a certain period in the nineteenth century Darwin was more popular amongst linguists than amongst biologists; the biological community was quite divided by disagreement upon the foundations of their discipline – many were wedded to the notion of catastrophism, whereas the linguists (although not building a monolithic community either) were content to compare their work to the work of Darwin, given their recent success of reconstructing parts of Indo-European.<sup>556</sup> It was the picture of the tree of life with its many branches that was used as picture of the Indo-European roots that evolved into the many branches of European (and Indian) languages that still exist today such as German, French, Italian, Spanish and the lesser known Frisian, Catalan, Gaelic, or that have died as Latin, Dalmatian and Cornish. Darwin had opened the eyes of biologists for the huge variation in languages and the slow accumulation of changes in them; linguists could use this model of variation and slow changes and some explicitly spoke of a struggle for existence amongst languages: "certain peoples, such as the Indians of North America, are unfitted for historical [in this racist theory the word 'historical' means 'civilised' – MH] life on account of their endlessly complicated languages, bristling with overabundant forms, (...) [and these] can only undergo retrogression, even extinction."<sup>557</sup>

These Darwinian linguists were soon criticized by other linguists with more interest in a newly emancipated academic discipline: psychology. Steinthal, for instance, criticized the Darwinian minded linguists for taking "only into account the organism – the lifeless body of language – and not its life; they dissect its corpse."<sup>558</sup> I.e., the Darwinian linguists only looked at the evolution of external word forms and forgot about the inner life of languages. One might expect that the 'inner life of a language' meant to psychological minded linguists the meaning of words and the intention of speakers as they interact, but in reality the psychologist weren't really interested in this; they actually sought "sound laws that proceed *mechanically* and admit of no exception (Law 2003: 271)." In that sense, even the psychologists saw language "as an organism independent of human volition (id, p. 272)." And although they had some interest in the societal nature of language, they didn't differ much

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<sup>554</sup> This is remarkably parallel to Darwin's own account of the origin of species which does not explain the absolute origin of life or species, but only how relatively complex forms of life species could evolve into other species.

<sup>555</sup> Not to be confused with the contemporary linguist and comparative religion scholar Friedrich Max Müller commonly known as Max Müller whose opposition to Darwin we just discussed.

<sup>556</sup> See footnote 507.

<sup>557</sup> August Schleicher's *On the meaning of language for the natural history of man* (1865), quoted by Law 2003: 270.

<sup>558</sup> Steinthal quoted by Law 2003: 270.



from Darwinian minded linguists.<sup>559</sup> Nevertheless, this critique was effective, and slowly Darwinian explanations of languages evolving disappeared from linguistics, just as discussions of the absolute origin of languages had.

Other reasons why the subject of the origin of language fell from the agenda were already mentioned in §3.12 and included De Saussure's and other structuralists' adherence to essentialism at the beginning of the 20<sup>th</sup> century, and after the Second World War the predominance of the view of language as learnt and generated by a strictly human, monolithic, innate language acquisition device (Chomsky 1994) (Chomsky 1966) and the return of the blank-slate model of the mind (like that of Locke and other empiricist) in naive behaviourism which made language origins a problem like all others and which left little room for biological evolution of behaviour (see also Newmeyer 2003). Only in recent years has "*the study of language origins returned to respectability* (Pinker 2000: 441, original emphasis by Pinker)" as can also be seen by a glimpse at the literature list with most papers from the last or last two decades.

Even today many linguists are skeptical about the idea of language *evolution* (in many of its senses), and especially of *Darwinian* evolution. The best known example is of course Chomsky: "Evolutionary theory is informative of many things, but it has little to say, as of now, of question is of this nature [such as the origin of language]. (...). In the case of such systems as language or wings it is not easy even to imagine a course of selection that might have given rise to them (1988: 167)."<sup>560</sup> But others are also critical, such as Bickerton (1998, 1995, 1981), Fodor (1998), be it not always for the same reasons. Rudie Botha gives the most detailed critique of possible Darwinian stories and analogies regarding language evolution in nine thick papers: Botha 2002ab, 2001ab, 2000, 1998ab, 1997ab. We will discuss these in footnote 600.

### **Why a Darwinian theory of the origin of language?**

In this chapter we turn to a discussion of the origin of language using Darwinian theory. a) One can wonder why we should use Darwinian theory at all to tackle this problem and b) if we use it, *to what extent* the problem can be solved using Darwinian means. An answer to these questions will also show why the Darwinian theory needed will be a *cultural* Darwinian model.

a) In §3.13 we saw that language use presupposes thought and intentionality and that a theory of mind (understanding the intentions of others) is helpful or even almost obligatory in understanding communicative intent and acquiring a language. As Darwinism has yet to find ways to deal effectively with intentionality and consciousness a Darwinian approach to the origin of language seems like a complete false start in dealing with this problem. However, (natural) scientists *generally assume*

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<sup>559</sup> Another early critique on Darwinian theories of linguistics comes from the American linguist William Dwight Whitley in his *Darwinism and Language* (1874) and the popularizing account *The life and growth of language* (1875). Although he criticized Darwinian accounts, the freely spoke of languages as living organisms.

<sup>560</sup> Quotes like these can be found in many works of Chomsky. Only recently has Chomsky changed his position slightly in the famous co-authored paper with Hauser and Fitch which claims "firm support for the adaptationist program (Hauser, Chomsky & Fitch 2002: 1574)." However, this paper feels like old wine in new bottles as the adherence to the adaptationist program doesn't mean Hauser et al. give a functional explanation of language as they regard language a spandrel which developed rather sudden as the ability to understand mathematical recursion (which is thought to be adaptive and or functionally evolved) was in place. As such Hauser et al. don't give an adaptive evolutionary explanation of language as such, but only use the loophole in the adaptationist program to call some elements you consider non-functional spandrels on other adaptations.

that we humans have evolved from other primates by natural selection and within their naturalistic frame-work the *only* useful theory to explain this transition is Darwin's theory and thus a naturalist's approach could only be to see how far his or her approach brings him or her in tackling this problem; some of the results of for instance sociobiology and evolutionary psychology demonstrate that Darwinian principles can be applied successfully to understand elements of the transition from primates to man, and the naturalist hopes (*as he or she has no other viable (biological) theory of evolution*) that Darwinism can elucidate at least some elements of the origin of language as well.

Thus, even though a naturalist would admit that currently no complete Darwinian explanation of language is possible, he would nonetheless want to look to what extent Darwinian mechanisms can explain the evolution of language. In practice, the naturalist would (consciously or not) presuppose the existence of intentionality, whose origin is as yet not explained successfully or uncontroversially, and then continue the Darwinian analysis of the origin of language. This would mean bracketing a Darwinian analysis of the origin of intentionality,<sup>561</sup> possibly hoping that future empirical research and conceptual refinement can elucidate this problem. This is not atypical for research: one acknowledges the problems with a certain presumption and the future research that is needed, but on the assumption that the assumption is true one analyzes what will follow. However, one further needs to assume that the existence of intentionality does not fundamentally *change* the *evolution* of a certain phenomenon, which certainly need not be true for all phenomena, i.e. the existence of intentionality lies at the basis of the *evolution* of science as without it no good experiments can be set up and science could not 'progress'. In other words: to give a good Darwinian explanation of the origin of language one need not only presuppose the existence of intentionality on which language use depends, but for this explanation to work one also needs to assume that the *evolution* of language itself is not seriously changed by the existence of intentionality (which was first needed to explain many forms of language *use* and language *acquisition*). Fortunately, this assumption can be made since we saw that natural selection of biological speech organs and many processes of diachronic linguistics are unconscious processes that don't require intentions. Which brings us to the second problem: the extent of the Darwinian explanation.

b) If one starts discussing the problem of the origin of language by Darwinian means, one can ask *which* elements will have to be explained by Darwinian means. But few people will doubt that the evolution of the biological speech organs can be described by means of natural selection, but many people will be more skeptical about the evolution of linguistic forms from a Darwinian perspective. However, the speech-organs co-evolve with linguistic forms and their evolution as speech organs was even *kickstarted* only when the linguistic forms started to evolve (because otherwise there was no evolutionary reason for better speech organs) so that a complete Darwinian explanation of the speech-organs *includes* the evolution of (proto)-linguistic forms (whose evolution need of course not be

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<sup>561</sup> This move is made by for instance Johansson 2005: 145 in his discussion of the role of the origin of mind for the origin of language. See also Dennett 1995 who argues that even if one assumes true intentionality to exist (which he doesn't) Darwinian explanations don't become useless as they can explain the evolution insofar it is not 'interrupted' by intentional acts. It is then an empirical question to what extent the evolutionary process is intentional and to what extent not and thus which elements can possibly be explained Darwinistically and which not.

completely analogous to that of biological organisms nor completely Darwinian).<sup>562</sup> As linguistic forms are not (completely) genetically encoded but cultural forms, an explanation of the origin of language using *Darwinian* means should involve one of the cultural Darwinian models. This is even more true as a selection pressure on the biological organs of hearing, let alone on those of speech (involving natural selection), is only imaginable within the Darwinian scheme that presupposes i) that there don't exist pre-adaptations, and more specifically in the context of language that ii) receiving signals comes before production and that understanding will always reach further than actual production, if the (initial) evolution of linguistic forms *precedes* it. Because of that Darwinian explanations of the origin of language should start with the evolution of proto-linguistic forms and then discuss the selection pressure on biological organs involving language use. As such the general explanation of the origin of language will have to be treated within the frame-work of a cultural Darwinian model that takes this co-evolution serious.

## §4.2 Darwinian stories about language

*In §1.23 we stressed that to give a good Darwinian explanation implies doing a lot of work: though Darwinian just so stories are easy to come up with, real Darwinian explanations require a lot of thinking on all the levels involved, on the correct application of the 3 fundamental principles and on the functionality of the trait and its intermediary stages as well as many empirical results on the proximate mechanisms involved to buttress the explanation and to rule out alternative (Darwinian) explanations. In this § we make this point again, but this time not for Darwinian explanations in general but for Darwinian explanations of language, using some of the results of the previous chapter. We then discuss 8 types of just so stories on the origin of language, corresponding to the 8 models (2 ontogenetic, 6 phylogenetic) we distinguished in chapter 2 on cultural evolution. After that we stress that Darwinian theories of language can only be substantiated when they live up to the ideal formulated in §2.4.*

Darwinian sounding stories about the origins of language are easy to make up, but most of them are 'just so stories'. Such 'just so stories' run along the lines of: since language allows people to communicate fast and instruct each other about a great many things, it offers a lot of advantages to its speakers; hence there existed a strong selective pressure on the linguistic abilities of our primate ancestors, so that people with better linguistic abilities survived better and that's why even the simplest man speaks such a complex language as ours fluently. Yet this explanation (and similar ones) leave a lot unexplained. First of all: if language abilities are so advantageous, why did only humans evolve these abilities, or at least to the extent they have evolved it that seems to create a qualitative difference between human and animal

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<sup>562</sup> It is helpful to compare the evolution of shoes to that of linguistic forms. The evolution of shoes need not be part of the story of feet evolving for bipedal walking as the evolution of shoes *postdates* the evolution of the feet. In contrast: the evolution of linguistic forms antedates the evolution of speech organs for speech as only the existence and broader use of 'cries' and other proto-linguistic forms forced the speech organs to become much more effective and efficient for speech.

language?<sup>563</sup> And what are the real instead of the supposed *obvious advantages* of (verbal) language? There are individual deaf people that live in small communities with people without this handicap, and these deaf people function properly within the community:<sup>564</sup> hence, what is the *need* for language? Further, what does the Darwinian explanation presuppose what language itself is? If language is regarded as the material carrier of a Platonic idea or a spiritual meaning, one could devise an evolutionary story about selection pressures on ever better, more transparent carriers, yet the very notion of platonic ideas or spiritual meanings seems anti-mechanical and thus hard to fit into any Darwinian explanation (with the proximate mechanical mechanisms it presupposes) beforehand, so that in such a case the evolutionary story would break down before it gets off ground. And does the proposed explanation distinguish between the evolution of linguistic abilities and the evolution of linguistic forms? And if so, on what elements does the explanation of the evolution of linguistic forms concentrate? Words? Syntax? Intonation and intonation patterns? Melody, rhythm? Further, what were the intermediate stages of language evolution (or of the evolution of the supposed universal grammar behind it): language is too complex to arise out of one macro mutation as Derek Bickerton once fiercely defended, yet linguistic structures are so interdependent that some (like semantic holists) claim that

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<sup>563</sup> This is of course a question one cannot truly ask in a Darwinian frame-work: things did evolve simply *as they did*, we can look back and give an explanation why this happened and what advantages certain organisms enjoyed. However, if linguistic skills were really so advantageous as described, we would expect cases of *convergent evolution* for linguistic abilities, just as some researchers (correctly or not) suppose to have happened in the case of seeing, with eyes popping up multiple times in different branches of the evolutionary tree.

<sup>564</sup> Robbins Burling 2005: 223f. describes the case of a single deaf girl in a small agricultural community in India that functioned properly without oral language using only a couple of gestures: "Once, many years ago, I lived for six weeks in a poor agricultural village near the city of Varanasi in northern India. A young woman who must have been about 18 years old and who appeared to be totally deaf lived in the house next door. Like everyone else in the village, her parents were farmers of modest means, and I do not believe that she had ever had a single day of schooling. She had no regular contact with other deaf mutes, so she had never had any opportunity to learn an established sign language. Of course she used gestures to communicate with other members of her family, but everyone else in her household was able to hear and to speak, while their communication with the deaf girl was limited to gestures. In spite of her deafness and her resulting isolation, I was continually astonished, as I watched her and the other members of her family, by her apparent normality. She dressed as other young women of her age dressed. She worked in the kitchen and around the house doing a young woman's work. She went to the fields and shared in the labor of agriculture. She squatted as other women squatted, and she used the appropriately self-effacing postures of a young village woman of her age. (...) Watching her was one of the experiences that has made me skeptical of the importance of language for pre-industrial technology. Here was a person who had no language at all, but who appeared to participate fully in the technical tasks of her family and community. She had learned everything by watching and imitating [and I would add: being corrected by others – MH], and she had needed no language to do so." Of course, this single case cannot claim to have shown that language has no value, but it sheds doubt on our usual assumption that language confers many advantages to man without whom he might not survive. However, what if the girl had been a man? Had he been able to join the hunt? And were others not able to help and correct this girl because they themselves spoke a language, whereas a community of deaf people would never reach the level of society this pre-industrial society had reached (but on the contrary, think of the rich language that develops spontaneously amongst a group of deaf people). Further: were not some complex relationships present in this society that were vital to keep it running such as marriage customs, family relationships, a leadership hierarchy, religion etc. of which the deaf girl could never gain a full understanding? And finally, might not the cognitive abilities of the deaf girl have evolved just because of the evolution of linguistic abilities and thus presuppose them?

Also see McGrew 1992, a landmark study synthesizing evidence for chimpanzee behaviour which shows how much these animals can achieve without language, particularly in the production and use of material tools.

it is hard to imagine how language could evolve if not at once?<sup>565</sup> Since, what is the function of ‘half a language’? Learning a language costs a lot of energy and if this language functions only partially it might not be advantageous at all to have it. Language can thus only arise if the ‘intermediary’ stages it went through were advantages of their own, but what were these advantages? And can there be an explanation why we have an oral and not a gesture language, given the fact that sign language can be just as effective (think of American sign language)? Finally, selfishness is something that is taken for granted, altruism needs to be explained in Darwinism; communication seems altruistic because communication involves sharing information that one might better keep to oneself, hence can free communication be the function of language and the reason why it evolved? Or is the function of language some form of manipulation? And how could this manipulation/free communication get started in the first place, especially when language use is not or not strongly under genetic control as in the case of the almost automated reflexes of alarm cries which probably evolved by genetic kin selection? If you are the first to send a signal, how can anyone else understand you when understanding is not genetically encoded? Or is this a wrong question and do we need the counter-intuitive principle that understanding signals comes before transmitting them, as the Darwinists Deacon (1997) and Burling (2005) keep stressing? And how to make the step from passively understanding signals to actively sending them?

This initial survey suffices to remind us of the complexity of a Darwinian explanation of the origin of language and that any good Darwinian explanation should take the ideal as formulated in §1.4 and §2.3 into account. This ideal will be substantiated for the specific problem of the Origin of language as we systematically discuss some of its main aspects in the next section. In this section we will systematically discuss some proposals for evolutionary and Darwinian accounts of the Origin of language (in its many senses) that can fit one of the models of cultural evolution outlined in §2.2. This gives us a good first overview over the field of Darwinian explanations in linguistics, which shows us that many are pseudo-explanations or just so stories, which is not very surprising as most of the general models of cultural evolution were already found wanting too and a lot of our critique on the 8 general models of cultural evolution is also useful to criticize the theories of the origin of language based upon them.

### **8 models of cultural evolution applied to language evolution:**

In §2.2 we distinguished 8 models of cultural evolution, 2 ontogenetic and 6 phylogenetic models. We will discuss here examples of:

#### **Ontogenetic models**

**I) Operant conditioning:** operant conditioning was proposed by Skinner as an alternative theory to classical conditioning for explaining how individual animals could learn something (Skinner 1938). Skinner applied it also to human language use in his book *Verbal Behaviour* (1957) which we discussed in §3.12, where we pointed to the many problems of linguistic behaviorism. Skinner was also the first to fit operant conditioning within the wider frame-work of Darwinism (Skinner 1984), a

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<sup>565</sup> Derek Bickerton defended his macro mutation idea exactly with this argument (Bickerton 1981). Nevertheless, in organisms the different elements (organs etc.) are also highly interdependent and following Bickerton’s line of reasoning one could wonder how they could have evolved at all.

perspective recently defended by Glenn (Hull et al. 2001). Interestingly enough Skinner combined his ideas on the application of operant conditioning to language use with his ideas on Darwinian models of operant conditioning to formulate a Darwinian model of language evolution on the basis of operant conditioning (Skinner 1984). Since in Skinner's operant conditioning model of verbal behaviour the ontogenetic evolution of the language use of an individual depends on the verbal reaction he or she receives from the environment which includes other speakers, one can easily extend this model to a model of *reciprocal* operant conditioning involving two speakers conditioning each other. As the two speakers can be from different generations the reciprocal model based upon the ontogenetic operant conditioning then turns into an intra-generational or phylogenetic model of language evolution. However, Skinner's model fails for various reasons: a) all the problems with verbal behaviour analyzed as an operant conditioning process (see §3.12), b) all the problems with (the evolution of) operant conditioning analyzed as a Darwinian process (§2.1) and c) by ignoring of both the results of the field of first language acquisition and those of diachronic linguistics (including sociolinguistics) which deal in an empirical way with the onto- and phylogenetic evolution of language (linguistic forms): Skinner sees language evolution and linguistics not as a unique process with its own principles, but just as an example of learning by reciprocal operant conditioning. As such it does not tell us anything about language evolution in particular.

**II) Neural Darwinism:** neural Darwinism is a stricter ontogenetic model than Skinner's model as it cannot be extended phylogenetically and is only concerned with the formation of neural connections within the individual brain. Some major proponents of Darwinian explanations in linguistics (Deacon 2000, Calvin 1996) use neural Darwinism to describe how neural connections for language use are formed within the brain. They don't change the theory of neural Darwinism to suit the problem of the origin of language better, except perhaps by locating the neural selectionism in areas believed to be involved in language use (so that if one would replace the reference to language in references to mathematics, reasoning, cognitive development etc., the explanation would look the same). As such, the general problem facing neural Darwinism also hold for explanations of language origins using neural Darwinism. We demonstrated that the models of neural Darwinism are not true applications of *Darwinism*; as such the description and/or explanation of the proximate neural mechanisms in terms of ultimate Darwinian mechanisms is clearly wrong. Whether the identified proximate neural mechanisms are useful for the description of the neural mechanisms in general or for the mechanisms behind language outside of a Darwinian frame-work is another issue. It presupposes that we can isolate these mechanisms from the pseudo-Darwinian frame-work, which is highly questionable. Further, that these mechanisms would be true is also unlikely, given our knowledge, as little as it is, of neurology in general and of the neural basis of language in particular (§3.21).

### **Phylogenetic models**

**III) Ideational selection:** ideational selection is not a true Darwinian model, but it is an evolutionary model which we discuss nonetheless because it is or was so prominent in the social sciences. For linguistics, the model of ideational selection would mean that an idea or general cultural program determines/seriously constrains what linguistic forms look like. Such a model is not popular since the field of

linguistics presupposes that linguistic change is more or less independent of the evolution of other cultural factors (see §3.25).

Theories that nevertheless presuppose correlations between the evolution of language(s) and the evolution of culture usually antedate the emancipation of diachronic linguistics (and of modern ideational selection for that matter) such as Humboldt’s famous correlations between cultures (‘Weltanschauungen’) and linguistic forms (1820), or the racist linguistic pseudo-theories in the 19<sup>th</sup> century that correlate the primitiveness of cultures with the ‘chaotic’ complexity or un-usefulness of their language etc.

The best example of a theory that relates a culture (or cultural idea) to language is Oswald Spengler’s application of his idiosyncratic and highly speculative cultural model to the phenomenon of language (a model that is heir to the German idealistic and morphological tradition of Humboldt, Goethe and others). Spengler (1922, 1918) believes that there have existed 8 high cultures (the Egyptian, Chinese, Greek, Western etc.) that to a large extent have evolved independently of each other over a period of a thousand years following a fixed order of phases of birth, growth and bloom, civilization and decline; in this independent evolution the culture’s unique ‘Ursymbol’ makes itself manifest in all the cultural elements (artefacts, art, science, politics etc.). One can compare this to the Platonic or Platonistic idea of, for instance, a cow which is held to be manifest in all the details of an individual cow in the sublunary world. Examples of Spengler’s Ursymbols are the shining statue of Apollo for Greek culture and the Faustian strife for infinity for our own western culture; these symbols somehow ‘determine’ that in Greek culture reality is perceived as things that are directly ‘shiningly’ present before one’s eyes whereas in Western culture people feel that they are in an infinite stride to mentally grasp the whole of reality including objects that are somehow far away (from the senses).<sup>566</sup> Spengler

<sup>566</sup> This vague metaphor becomes a little more concrete if we just list some of the examples Spengler gives (table 4.1): they share a vague analogous quality although it can seriously be doubted whether this quality is unique (instead of a family-resemblance quality) and whether the examples are historically accurate.

<b>Cultural element</b>	<b>Graeco-Roman, Apollonian culture</b>	<b>Western, Faustian culture</b>
Math	- restriction to simple geometric objects - rejection of the number zero	- infinite dimensional bodies - zero accepted, even infinitesimal small quantities count as numbers
Physics	- finite space - physical objects are conceived as simple objects of concrete material (even atoms) - method: observation and generalisations	- infinite space - physical objects are hardly observable centers of force, infinitely small and infinitely big - method: experiments as rational plans to force nature to reveal its secrets
Art	- main form: architecture, sculpture - theatre: unity of time, place and action	- main form: music which is less sensually present - theatre: infinite reflections on the inner states of the soul, the neverending quest
Economy	- mostly barter trade, objects are their value, nominal and real money coincide	- from early on a complex system of credit, representations of value
Nature	- ideal nature: olive tree	- ideal nature: dark, infinite forests
Politics	- small polis, voting by call	- complex interwoven cabinet diplomacy, intricate systems of representation
History	- little attention paid too, family history often interwoven with myths, short periods	- exact research on all cultures over vast amounts of time, natural history describing periods of geological time

*Table 4.1: Examples of cultural dependency of different cultural elements*

believes that our western culture has entered the stage of civilization and will rapidly enter the stage of decline.

This model, when applied to linguistics, would result in the idea i) that linguistic forms too are determined by an *Ursymbol* and ii) that in the phase of civilisation the languages are always in a state of decline. Accordingly, older (ancient) Greek and older German are much more perfect and complex than later Greek and current German.

And indeed, we do find that Spengler claims that cultural or race characteristics determine the form of languages (with race for Spengler a non-biological, spiritual property indicating a relation with the *Ursymbol*),<sup>567</sup> and that (western) languages currently are in a state of decline. However, i) he does not give concrete examples of how the language(s) of people of different high cultures are determined by the *Ursymbol* (the few examples he gives almost only involve a comparison of different languages within a *single* culture<sup>568</sup>) and ii) in his model of language evolution languages in high cultures are *always* in a state of corruption,<sup>569</sup> as

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<sup>567</sup> Spengler, *Untergang des Abendlandes*, second volume (1922), Chapter 2, subchapter II: *Völker, Rassen, Sprachen*: “Es liegt also schon ein Rassezug in der Art, wie das Mitzuteilende in Sätze gefaßt wird. Sätze sind bei Tacitus und Napoleon nicht dasselbe wie bei Cicero und Nietzsche. Der Engländer teilt den Stoff syntaktisch anders auf als der Deutsche. Nicht Vorstellungen und Gedanken, sondern das Denken, die Art des Lebens, *das Blut* bestimmen in den primitiven, antiken, chinesischen, abendländischen Sprachgemeinschaften [es bestimmen] (§11).”

<sup>568</sup> See Spengler’s empathetic or programmatic ‘it has to be the case, although it has not yet been investigated’ that the evolution of all western languages is determined by a common *Ursymbol*: “*Es muß* in der Entwicklung des Deutschen, Englischen, Italienischen, Französischen, Spanischen von 900 bis 1900 einen gemeinsamen Zug geben und ebenso in der Geschichte der hellenischen und italischen Sprachen einschließlich des Etruskischen von 1100 an bis zur Kaiserzeit. Aber was ist hier, unabhängig vom Verbreitungsgebiet der Sprachfamilien und der Rassen, allein durch die *landschaftlichen Grenzen der Kultur* zusammengefaßt? Welche Veränderungen haben das Hellenistische und das Latein seit 300 gemeinsam, und zwar in der Aussprache, im Wortgebrauch, metrisch, grammatisch, stilistisch, welche das Deutsche und Italienische seit 1000, das Italienische und Rumänische aber nicht? Dergleichen ist noch nie planmäßig untersucht worden (id.).”

<sup>569</sup> Spengler refuses to speak about the human origin of language as animals supposedly already use ‘language’ in the broad sense of the word (and as an anti-Darwinist Spengler refuses to take theories about the origin of man from apes seriously as he regards them as culturally determined fables of a late, over-scientific age with Darwin’s idea of an infinite quasi-progressive evolution in particular fitting our own Faustian culture); nevertheless, in his view there are three key stages in the evolution of language: 1) the origin of names out of religious awe and fear (a theory akin to Müller 1866), 2) the origin of syntax, 3) the corruption of stage 2;

1) Spengler attributes the origin of verbal language to a mentalistic change of perception which originates in fear: “Aber das halte ich im Gegensatz zur modernen Forschung für das Entscheidende: nicht eine Veränderung des Kehlkopfes oder eine besondere Art der Lautbildung oder sonst etwas Physiologisches, das in Wirklichkeit Rassemerkmal ist, liegt hier vor – wenn dergleichen damals überhaupt eingetreten ist – und auch nicht eine Steigerung der Ausdrucksfähigkeit der vorhandenen Mittel, etwa der Übergang vom Wort zum Satz (H. Paul), sondern eine tiefe Verwandlung der Seele: mit dem Namen ist ein neuer Blick auf die Welt entstanden.” This new vision stems from “tiefe Angst (...) um das Rätselhafte der Welt zu benennen. (...) Mit dem Namen ist gleichsam *der Sinn* des Wachseins und *die Quelle* der Angst angerührt worden. Die Welt ist nicht nur da; man fühlt ein Geheimnis in ihr. Man benennt über alle Zwecke der Ausdrucks und Mitteilungssprache hinaus das, was *rätselhaft* ist. (...) *Mit dem Namen ist der Schritt von der alltäglichen Physik des Tieres zur Metaphysik des Menschen vollzogen*. Es war die größte Wendung in der Geschichte der menschlichen Seele (§11).”

2) The second important step of the origin of syntax is again explained mentalistically: “Indem zum Namen der Satz, zum Wortzeichen die Wortverbindung tritt, wird das Nachdenken – das Denken in Wortbeziehungen, nachdem man etwas wahrgenommen hat, wofür es Wortbezeichnungen gibt – für den Charakter des menschlichen Wachseins bestimmend. Es ist eine müßige Frage, ob die Mitteilungssprachen vor dem Auftreten echter Namen schon wirkliche ‘Sätze’ enthielten. Der Satz im heutigen Sinne hat sich zwar aus eigenen Bedingungen und mit eigenen Epochen innerhalb dieser Sprachen entwickelt, aber er



the origin of language (once a supposed perfect system of grammatical forms) has supposedly taken place *before* the origin of all the high cultures.<sup>570</sup>

These conclusions are understandable i) from the German tradition of idealism and linguistic relativism in particular and ii) from the perspective of 19<sup>th</sup> century linguistics that knew only the causes of the corruption of linguistic forms and didn't understand how new syntactic constructions were continuously evolving (partly even due to the corruption) so that the original language was envisioned as perfect systems that gradually broke down during history,<sup>571</sup> whereas in fact it is just as much a transitory patchwork as our modern languages nowadays (see §3.25). However, because of empirical tests that show how cultural evolution in general and linguistic evolution in particular are relatively independent (see §3.25) and because of our earlier general critique of linguistic relativism (see §3.21), we can put Spengler's proposals and similar, earlier ones aside. Spengler's theory did show us, however, what a theory might look like that works from the assumption that an 'idea' and mentalistic mechanisms determine the evolution of language.

**IV) Behavioural ecology:** behavioural ecology tries to correlate environmental selection pressures with phenotypical properties, without taking the specific evolutionary histories of species (replication lineages, existing properties, constraints and variation in the genome) into account. In linguistics parallel models for human languages are rare: not many people try to correlate language abilities or linguistic forms with environmental variables.<sup>572</sup> An exception is perhaps Skinner's problematic, *ontogenetic* model of the evolution of verbal behaviour by operant

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setzt dennoch das Dasein der Namen voraus. Erst die geistige Wendung, die mit ihrer Geburt eingetreten ist, macht Sätze als gedankliche Beziehungen möglich (§11)."

3) The third step of degradation starts due to a too secure feeling of language mastery which ends in a mechanical exchange of messages between minds. This third step ultimately leads to a written language, which changes languages into even more 'mechanical' instruments for thinking, that, ironically, turn out to be culturally dependent (Spengler's version of linguistic relativism): "Nun [greift] die Geschichte der hohen Kulturen ein, die mit einer ganz neuen 'Sprache der Ferne', der Schrift, und durch die Gewalt ihrer Innerlichkeit dem Schicksal der Wortsprachen eine plötzliche Wendung gibt (...) [D]ie Zeichentechnik [hat] sich inzwischen in eine Denktechnik verflüchtigt hat, welche der verkürzten, aber mit Bedeutungsgehalt durchsättigten Zeichen gleichsam nur zu Anspielungen bedarf, die ausschließlich ein Eingeweihter der betreffenden Sprachgemeinschaft versteht. Dies ist der Grund, weshalb ein westeuropäischer Mensch (...) dem Verständnis der Urworte jeder anderen Kultursprache [entsagt ist], dem logos, der archè im Griechischen, dem atman und brahman im Sanskrit, die auf eine Weltanschauung hin weisen, in der man aufgewachsen sein muß, um ihre Zeichen zu begreifen."

<sup>570</sup> See §12: "Es erscheint die Blütezeit der Grammatik, für die man - mit großer Vorsicht - vielleicht die zwei Jahrtausende vor Beginn der ägyptischen und babylonischen Kultur ansetzen darf (...) Die äußere Sprachgeschichte ist für uns gerade in den wichtigsten Abschnitten so gut wie verloren. Ihre Frühzeit hegt tief im primitiven Zeitalter."

<sup>571</sup> See also §12 "Das indogermanische System kennen wir nur im vollsten Verfall. Von den Kasus des Altvedischen - um 1500 - sind in den antiken Sprachen ein Jahrtausend später nur Trümmer erhalten. Seit Alexander dem Großen ist in der hellenistischen Umgangssprache der Dual aus der Deklination und das ganze Passiv aus der Konjugation verschwunden. Die abendländischen Sprachen, obwohl sie von denkbar verschiedenster Herkunft sind, die germanischen aus primitiven, die romanischen aus hochzivilisierten Verhältnissen stammen, verändern sich in gleicher Richtung: die romanischen Kasus sind bis auf einen verschwunden, die englischen mit der Reformation sämtlich. Die deutsche Umgangssprache hat den Genitiv im Anfang des 19. Jahrhunderts endgültig eingebüßt und ist im Begriff, den Dativ aufzugeben."

<sup>572</sup> The only example we know of a researcher directly linking the nature of linguistic forms (of animals) is Nicastro 2001 who proposes that habitat plays an important role in the *evolution* of alarm calls, arguing that the visual affordance of open environments invites more nuanced alarm calls, as well as decreasing the risks to the caller. This might be true for the vervet monkeys, however, it does not apply to the case of the Diana monkeys discussed in §2.33 and their calls were the most complex of all monkey calls (Zuberbühler 2000b).

conditioning which relates language use (almost) directly to environmental variables and which he later turned into a *phylogenetic* model as we just saw (Skinner 1984).

Research on the origin of language that comes closest to behavioural ecology (especially if we include the social environment into the ecological environment) is work in i) comparative anatomy that for instance correlates survival in highly social groups with complex (political) interactions (also between non-genetic relatives) with language abilities and ii) work in animal language that relates animal vocalizations with their function.

i) The correlation of language use and living in highly complex social groups is supported by the fact that attempts at teaching language to animals are only successful (at least to some extent and especially when it takes place in *social* exchange contexts) in species such as primates, parrots and dolphins (see §3.23) whose social life is highly complex, although we do find active sign use (*sign broadcasting*) in non-social animals as well such as the mimicry of butterflies or the use of colour by a chameleon and we can find highly social groups such as hyenas or African wild dogs that seem to lack the ability to use *complex* signs, so that the correlation is thus not perfect. The phenotypic gamble of behavioural ecologists is in this case thus only a gamble (which is exactly one of the problems of the explanations of behavioural ecology in general, see §2.3). We come back to the proposal of language as an adaptation for complex social life in §4.3.

ii) Examples of correlations between animal vocalizations and their function are alarm calls which are generally lower pitched than mating calls as these lower pitched calls are harder to locate than higher pitched ones and alarm calls should not reveal the location of the sender whereas mating calls should. However, in practice research works the other way round: one observes what calls certain animals make and then starts analyzing what function they may have instead of that one starts analyzing what calls would benefit certain species and then starts looking for them as behavioural ecology would imply. Thus behavioural ecology has little to say on the evolution of language.

**V) Memology/memetics:** in §2.2 and §2.3 we discussed a number of problems of memology. If we apply memology to language evolution we will find the same problems back, although some of the problems apply to a lesser extent such the specific issues of: a) heterogeneity of memes, b) the discrete nature of memes, c) the parallel with genes and their phenotypes, e) the autonomous *evolution* of memes, intentionality and natural selection, f) the error prone transmission, g) identification through general transformation or transformation in other media, are not as strong as in the general case. Problems d), h) and i) still hold without qualification. We will analyze the mitigation in detail, and outline several reasons why a memological theory of linguistic forms is nevertheless not a fruitful theory.

a) Heterogeneity of memes: although linguistic forms have many different uses, the linguistic forms *as forms* don't differ much or are at least vastly less heterogeneous than the collection of all supposed memes.

b) Discrete nature of memes: linguists use the phrase dual patterning (see §3.1) to express the fact that most linguistic forms are composed of meaningless units: the phonemes. This is of course evident from the everyday experience that we can write down all words with only a few basic phonemes (whether we use the standard alphabet or a subset of the more advanced international phonetic alphabet); and although one can question the truly discrete instead of continuous or overlapping

nature of different phonemes in phonological space, it is clear that the linguistic forms can systematically be distinguished from each other as more or less discrete forms.

c) The parallel with genes and their phenotypes: dual patterning of linguistic forms provides a clear analogy with the dual nature of genes, whose individual nucleotide-bases don't 'mean' anything, but are only meaningful insofar they code for phenotypes. This analogy is used by many biosemioticians (see appendix 2) and is also central to Daniel Dennett's memetic model of the (genetic) Library of Mendel as a sub-part of the Library of Babel in which all replicators including DNA are modeled upon the basis of the dual patterning of linguistic forms, with some strings of phonemes (the ones forming understandable word-strings) or DNA-letters (the ones that are transcribed) being meaningful and others not. But the analogy fails: the phenotypical effects of genes that are transcribed can in principle be observed by looking at the mechanic processes in the cell, but the memotypical effect of linguistic memes cannot: what should they be as we cannot observe them by looking at the mechanic processes in the human body? One could argue that the memotypical effect of linguistic memes are the uses that can be made of certain word memes? But are those not indefinite? Well, yes, but so might be those of a genetic sequence. However, the actual uses vary much more than those of a genetic sequence: genes correspond more or less with a phenotype (though of course not in a clear 1-to-1 fashion), but linguistic memes have no clear memotype.

e) The autonomous *evolution* of memes, intentionality and natural selection: we saw that although the picture of linguistic change that the Neogrammarians supposedly offered of autonomous, exceptionless, irresistible linguistic change (at the phonetic level) is false, it can be argued that diachronic linguistic *evolution and change* proceeds more or less automatic without human intentions having much influence upon it, even though much actual human language *use* may involve intentions (see §3.13).

f) The error prone transmission: we saw that pronunciation and linguistic forms change continuously; however, often this change is so slow that we are sometimes able to identify linguistic forms as similar to ours that are thousands of years old (think of the Indo-European roots) and which have accordingly been used billions of times;<sup>573</sup> the errors are too big to conclude what the original words may have been like, but going back thousand of years is still a sign of relative error free transmission.

g) Identification through general transformation or transformation in other media: linguistic forms can be identified across media, from sign language to speech to written language so that this general problem of memology seems to apply less to linguistic memes than to memes in general. However, some transformations are less predictable, especially when we look at inner-linguistic memetic transformations such as synonymy, translation or paraphrase, whereby linguistic forms change drastically but their meaning not.

Two other general problems with memetics remain as strong in the case of memology as in the case of linguistics: d) the dependency of the evolution of memes on the nature of the brain and h) the identification through replication from multiple

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<sup>573</sup> Let's consider the word 'to know' and assume that people use this word on average 5 times a day, is about 1500 times a year, which is about 50.000 times from the time when a new generation is born (if children are born when their parents are on average 30). Over 200 generations (6000 years or less) this would mean about 10.000.000 uses! This would mean that memetic strings are much more stable than genetic strings as we humans are only  $5.000.000/30 = 130.000$  generations away from our common ancestors with the chimps.

sources (see §2.3). Given all these problems it need not surprise us that there are not many concrete examples of fruitful applications of memology to linguistics. This may have other reasons as well, for instance i) most meme theoreticians were interested in memes in general and forego a discussion of applications, and ii) if they looked for application at all they looked for examples were the evolution of memes *opposed* that of genes as in birth control, dietary customs etc. (Blackmore 1999) or iii) controversial subjects such as the evolution of religion, taboos and political ideas by mind parasites (Dennett 2006, Dawkins 1993) instead of such a ‘humble’ subject as linguistic evolution, iv) further, there already existed a fruitful theoretical body on linguistic evolution with which it would be hard to compete (see §3.25), v) further, linguists were possibly not acquainted with the developments in evolutionary theory and if they were, they vi) probably also knew the unsuccessful earlier application of Darwinian principles to linguistics in the 19<sup>th</sup> century which might lessen their enthusiasm beforehand to apply memology to linguistics, and vii) finally if they had tried to apply memetics, they probably would have found that it is hard to apply, as we will see now when we consider a few concrete examples of the application of memology to linguistics.

Among the few scientists that try to use the concept of memes to describe the evolution of linguistic forms as memetic evolution we find few true linguists. Dawkins who proposed the concept of memes has “catch-phrases” in his original list of meme examples (Dawkins 1976: 192) which comes closest to his example of linguistic memes. His friend Dennett (1995) based his own model of universal Darwinian upon the example of Babel: just as all biological organisms are build out of four bases A, T, C, and G, all extra biological replicators or memes are supposedly build out of a discrete number of bases just as the library of Babel contains all books as it contains all possible (finite) strings of letters); however, despite his use of the library of Babel (as problematic as it is, see §2.3), Dennett doesn’t discuss linguistic evolution as a memetic form of evolution. Oudemans (2008) comes closer to a formulation of linguistic memetic evolution as he proposes that the proto-Indo-European roots are the true memes; these roots can be identified over many generations so that the problem of the error prone transmission for linguistic memes is partly overcome; these roots can further be traced back in many other words in many different languages so that they can be said to have ‘replicated’ instead of just ‘transmitted’. However, the other problems we just signaled for linguistic forms as memes are not overcome in Oudemans’ brief suggestions.

Of the linguists we find some biolinguists and ornithologists who use the meme concept to describe the evolution of bird songs (Baker and Gammon 2008, Burnell 1998, Baker 1996, Lynch and Baker 1993, Lynch et al. 1989). Their research does not differ from that of others working on bird dialects; bird dialect research has shown how some bird songs can be characterized as dialects and how dialects change over time as some bird songs are not completely genetically pre-coded but also partially learnt by imitation of the songs of the previous generation. The concept of memes doesn’t add much value to this research; especially the famous independent element of meme evolution from biological evolution is not shown as it is not shown how bird song memes become adaptively more complex to better manipulate the brains of birds as memes should; the concept of meme is only used to redescribe that bird songs *change*.

One of the few true linguists who use the meme concept is Ritt (2004) in his book *Selfish Sounds*. According to Ritt, the traditional speech laws are instances of sounds spreading memetically through the vocabulary; however again, apart from his

redescription using memology, he doesn't come with new empirical results or insights from what is already known about sound change in diachronic linguistic evolution (see §3.25).

In sum: the once popular memology has found little application in linguistics and the few proposals are either fruitless redescriptions or problematic suggestions; the general problems of memology we outlined make it unlikely that memology will soon find application in linguistics.

**VI) Genetic determinism:** any theory of genetic determinism in the context of cultural evolution means a denial of the autonomous evolution of cultural elements as it holds (supposed) cultural elements to be nothing but genetically coded phenomena; for language genetic determinism would entail the view that not only language skills, but also all linguistic forms and their uses were pre-programmed.<sup>574</sup> For many animal forms of communication and even some human forms of communication this may be true; the vervet alarm call system is to a large extent genetically determined: not only the organs that are used to make the calls, but also the signals themselves, including how to use them and how to react to them (even though some fine-tuning of this system is done by learning) are genetically encoded. The system of human and primate facial gestures used for signaling emotions are also to some extent genetically determined. However, for verbal language the case is of course totally different: it is clear to everyone that the many details and elements of a human verbal language are not genetically hard-coded but culturally transmitted; e.g., the many details of vocabulary and syntax of for instance Tibetan are not hard-coded in the genes of Tibetan people and their linguistic forms are transmitted from parents to children.

However, there are at least five weaker versions of genetic determinism regarding human language use that someone might argue for:

- 1) Language skills and the basic structure of language are innate
- 2) Language skills and the general intellectual skills that are needed for understanding and building a complex language with a hierarchical, recursive structure are innate
- 3) Language skills are innate and determine which linguistic forms are likely to develop
- 4) Language skills are innate and adapted to the cultural forms spoken
- 5) Language skills are innate, but they don't constrain linguistic forms in any (strong) sense and vice versa

These positions share the view that language skills are innate; for at least the physical part (sound perception and sequencing as well as sound production) this is non-controversial, although even these abilities depend upon training and linguistic input and are thus not completely innate and autonomous from (the evolution of) cultural elements.

What the positions differ upon is how much the evolution of cultural forms is shaped and constrained by innate skills, from very strongly (1) to almost not (5). In §3.21 we already discussed language genes and speech anomalies which showed that at least some language skills are innate, but it is hard to assess to which extent. The

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<sup>574</sup> This view can then easily be extended within the gene point of view of evolution. From that perspective the evolution of a language system (which includes broadcasting signals which may confer costs upon the sender, at least in the short term) can only get off-ground if it brings (reciprocal) benefits to the genes of sender and receiver in the long run (Noble 1999). See also Dawkins and Krebs 1978 and Trivers 1971.

first position (1) as defended by Müller (1996), with genes for highly specific grammatical rules, seems clearly wrong (see §3.21c), but position (2) and (3) (which can be compatible) are not unreasonable, even though it would require further specification what skills are innate (and does not an understanding of recursion or hierarchical structures require intensive training?) and how they constrain language skills apart from the trivial phonological constraints and constraints on word and sentence length.

Position (4) is clearly wrong from a *current* perspective as we saw that there are surprisingly not even minor (genetic) adaptations to speaking a *specific* language as one would expect in communities that have long been isolated such as the Tibetans as Tibetan children like any other children acquire any language as easy as all others if they are exposed to it from birth onward (see §3.12).<sup>575</sup> As the latest common ancestor of all modern humans lived 100.000 years ago, this would mean that *adaptations* to cultural forms spoken,<sup>576</sup> if any, are at least older than 100.000 years. However, the fact that languages change so *quickly* makes it unlikely that adaptation to language specific elements have existed back then as well (unless diachronic language evolution was once much slower). At least now we don't find any firm evidence for adaptations to *particular* proto-languages, although that does not exclude the possibility of adaptation to languages in general (like (2) and (3) hold), but it can also be taken as evidence that linguistic forms are not seriously constrained by biology at all (5). We come back to this issue in §4.3e.

**VII) Evolutionary psychology:** the most famous example of a theorist that applies evolutionary psychology to linguistics is Pinker (2000) and (1994). Evolutionary psychology of language origins usually focuses only on the evolution of our brain and not really on the diachronic evolution of linguistic forms, in conformity with our sketch of evolutionary psychology in general which also concentrates on the evolution of human mental abilities and not on the evolution of the products of these abilities. Irony has it, however, that evolutionary psychology postulates such detailed evolved abilities (consisting of many small, domain specific modules) that sometimes seems to give explanations of the evolution of cultural forms proper, i.e. (an extreme case for linguistics): when you postulate a module for every syntactic subrule in the brain (especially when its realization is thought to be seriously dependent on genes and gene expression), then explanations of the evolution of syntactic rules (as realized in the brain through genes) are *a fortiori* explanations of the evolution of (the syntactic aspects or constitution of) linguistic forms. Although this extreme position is to some extent a straw-man, as proponents of evolutionary psychology will admit that some elements of the evolution of linguistic forms (be it only vocabulary, accents etc.) are independent of (gene controlled) brain module evolution, it is nevertheless true that, different from dual-inheritance theory,

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<sup>575</sup> The only example that is sometimes proposed as an adaptation for a *specific* language is the supposed higher percentage of people with absolute hearing in China as an adaptation to their tonal language; this scenario is often invoked in the context of a theory of the origin of language which relates it to signing (Mithen 2005); however, it is generally assumed that Chinese was once a non-tonal language as late as in the days of Confucius (around 400BC) and thus the adaptation would have arisen recently, which seems unlikely. And in fact, it is not true in general that there is a higher percentage of absolute hearing in speakers of tonal languages. Moreover, even if it were found, one would need to demonstrate that absolute hearing is completely genetically determined as the correlation between absolute hearing and tonal languages could be argued to be due to one type of cultural learning which trains both at the same time.

<sup>576</sup> In this case this would mean (highly general) adaptations to linguistic forms before they diverged, thus possibly common to all current languages.

evolutionary psychology implicitly denies the existence of a (semi-)autonomous *adaptive* evolution of linguistic forms: if some linguistic forms are more adaptively useful than others, this is a consequence of the gene controlled brain modules, not of the evolution of linguistic forms themselves.

Although Chomsky himself has explicitly denounced that the explanations of evolutionary psychology are valid explanations about the evolution of language in general (see Chomsky's more recent work in which he *seems* less critical of the Darwinian adaptationist program as applied to language as he once used too, Fitch, Hauser and Chomsky 2005 and footnotes 360 and 601), some of his supporters, or the supporters of competing generative grammar programs, have held that the idea of a Chomskian innate grammar is buttressed well by a mind consisting of many modules who perform different transformational tasks. This picture of the mind as a many module system is of course the picture as offered by evolutionary psychology.

Against evolutionary psychology in language at least four objections can be made that collectively seem fatal to these explanations:

a) genes: same point as above on genetic determinism: evolutionary psychology usually assumes that the brain areas for language are fairly direct under genetic control, which, however, does not seem to be the case (see above).

b) modules: work of evolutionary psychology on language origins assumes that the brain consists of many modules; however, despite our limited knowledge of neurological brain areas involved in language use, it seems that language use is not controlled by many specialized brain modules (§3.21), thus explanations that explain the *evolution or origin of language* in general by *the evolution of modules* are wrong from the start.

c) semantic-based evolution of linguistic forms: as the exemplary object of explanation of work on evolutionary psychology on language is grammar (as a module for individual words or phrases seems absurd), evolutionary psychologists, like many linguists not working on diachronic linguistics, tend to approach language evolution (linguistic forms) from the perspective of grammar, with language change resulting from fundamental, all or nothing, saltationistic grammar rule changes; however, we saw that the evolution of linguistic forms can better be approached from the perspective of semantic evolution, with syntax resulting from semantic combinations that become fixed and more or less complete analogical extensions of these combinations (the syntax 'rules'); this picture of diachronic linguistic evolution presupposes very different cognitive abilities than linguists working in the tradition of evolutionary psychology assume.

d) mentalism: as already remarked in §2.3: although evolutionary psychology is distinct from computationalism, in general and especially in linguistics<sup>577</sup> they often go hand in hand. Linguists who support evolutionary psychology often picture the brain as consisting of many modules whose computations are the input for other modules; computationalism itself presupposes representationalism which in its turn is akin to mentalism. Thus we find Pinker speaking of "language as a window into the mind" (Pinker 2007), thinking that our thought is structured by (the fundamental rules of) our language. However, mentalism had many problems, some of which we touched in §3.12, and defending

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<sup>577</sup> Although in linguistics the main reason why people were attracted to computationalism might not have been evolutionary psychology but their adherence to Chomsky's theory. Historically speaking, Chomsky's theory gave a major push to computationalism in cognitive science, a tradition which itself inspired evolutionary psychology.

evolutionary psychology in linguistics (at least in Pinker's version) would mean swallowing all these problems too.

**VIII) Dual-inheritance theory:** many researchers claim that true theories of the origin of language should be co-evolutionary ones with better linguistic skills and more complex linguistic forms scaffolding each other (Burling 2005, Davidson 2003, Deacon 1997). However, but few researchers actually formulate a theory in which such co-evolutionary processes play a major role. Richerson and Boyd, two of the main proponents of dual-inheritance theory hint that the work of Labov and other sociolinguists clearly fit their model<sup>578</sup> with content bias<sup>579</sup> in this case equal to language internal factors that drive diachronic evolution (factors that can be related to human innate psychology) and with prestige bias in this case equal to dialect prestige.<sup>580</sup> Richerson and Boyd also use the empirical results of the field of

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<sup>578</sup> See most explicitly Richerson and Boyd's summary of their own most recent book: "We have made the case for using Darwinian methods to understand cultural evolution. Culture is stored in populations, so understanding human brains and how population change requires population thinking. Darwinian accounts are one part bookkeeping – a quantitative description of cultural variation and its change through time. In addition, they are one part quantitative budget analysis – a systematic attribution of changes to causal processes. If you are going to study cultural evolution in a serious way, you are going to be driven to Darwinian methods of analysis. You have to be able to describe change and you have to be able to account for change. Several research programs in social sciences have independently converged on the Darwinian methods. The sociolinguists' micro-evolutionary studies of dialect evolution are a particularly sophisticated example (Labov 2001). (Richerson and Boyd 2005: 253)." See also p. 266 "According to sociolinguists, idiolectal variation is the raw material out of which language evolution grows, a quite Darwinian notion (Labov 2001)." What they find particularly useful in the analogy between linguistic variation and Darwinian variation is the gradualness "The case of language illustrates the general principle that the cumulative effect of many small changes can be a powerful source of cultural change. In some cases, only a few differences of phonology, syntax and lexicon separate closely related dialects. Careful dialect descriptions conducted in the United States in the 1930s allow contemporary linguists to describe in some detail the generation-to-generation change in language (Labov 1973). In one generation some dialect changes are rapid enough to be detectable to the trained ear. For example, New Yorkers are gradually tending to pronounce *r* at the end of words like *car* more often. Over time, these small changes accumulate (...) [so that for most of us Middle-English is almost incomprehensible] (id, p. 51)."

<sup>579</sup> See Richerson and Boyd 2005: 71 "Biased transmission doesn't always result from an attempt to evaluate alternative cultural variants according to cultural standards or rules. Biases are often caused by universal characteristics of human cognition or perception. For instance, many linguists believe that some linguistic characteristics are 'marked', meaning that they are harder to produce and perceive than alternative unmarked features. Languages that denote the subject and object of sentences with word order are less marked than languages accomplishing this function by changing the form of the noun. Such unmarked features are simpler, and accordingly appear earlier in first language acquisition. Many linguists also believe that 'internal' language change (as opposed to change that results from contact between languages) typically proceeds from marked to unmarked. Thus, language learners confronted with two slightly different syntactic variants will tend to adopt the less marked of the two, and in this way biased transmission can drive language change (Labov 1994). This hypothesis is somewhat controversial, but if it turns out to be true, it will provide a good example of how biases may arise from the working of human psychology." Richerson and Boyd further apply the idea of biased transmission also to change due to language contact (but note that they are also well aware of cultural tendencies to intentionally complicate language): "Consider the effects of contact-induced language change. The usefulness and intelligibility of new forms is governed by the similarity of the two languages in contact. But why do people want to communicate effectively? Why don't people choose the less- rather than more-intelligible forms? Sometimes they do: think of lawyers, politicians, or sometimes, alas, scientists. (Labov 1994 describes many cases in which language change due to psychological factors decreases communication efficiency.) People may prefer gratuitously complex linguistic forms to signal that they occupy a particular social role or for similar culture-specific reasons. The reason people often do prefer less marked forms must lie in the basic nature of human psychology – people (usually) want to be understood (id., p. 72)."

<sup>580</sup> See Richerson and Boyd 2005: 125 "Studies of dialect evolution also support this hypothesis (that poor people tend to imitate richer ones); locally prestigious women tend to be the most advanced speakers of



diachronic *language* change to argue that both genetic transmission and vertical (generational) cultural transmission cannot account for the evolution of *all* elements in what is traditionally seen as the domain of culture, so that horizontal inner-generational transmission (which is so important to their theory) is the only logical candidate that can then be held responsible for the evolution: “Behavior genetic studies indicate that most of the similarity between the personality traits of parents and children is due to genetic inheritance, not vertical cultural transmission. At the same time, these studies also detect a large amount of ‘environmental’ variation that is *not* shared within families. Children learn a lot from one another, and from adults other than their peers. In some domains – language, for instance – peers are much more important than parents. Immigrant children in the United States usually learn English from their parents, and come to prefer it over their native tongue. When people move from one region to another, their children usually use the local dialect rather than their parents’ (Labov 1994).”<sup>581</sup>

The major advantages of using dual-inheritance theory for language evolution are the same as those of applying memology to language: the linguistic forms are relatively well identified and stable over a number of generations, and the other problems of memology don’t apply or only to a lesser degree to dual-inheritance theory (see §2.3).

In the next section we will try to draw up a more accurate picture that confirms the suggestion of Richerson and Boyd that dual-inheritance theory is the best perspective to look at language evolution. What is almost absent in the sketchy application of their theory to the results of diachronic change (but what is present in other applications of their dual-inheritance model) is how cultural forms themselves tend to become adaptively complex (Richerson and Boyd only discuss, like evolutionary psychologists, how some forms better fit our innate psychological dispositions which drives internal linguistic change); we will emphasize in our more detailed discussion the extent to which the forms themselves become adaptively complex, and in how far this feeds back to the genetic based psychological dispositions themselves.<sup>582</sup>

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evolving dialects. Indeed, the data suggest that popular preteen girls of the working or lower middle class are usually the most important leaders of language evolution in American cities. (Labov 2001).”

<sup>581</sup> See also Richerson and Boyd 2005: 37 “Dialect variation is one example of a cultural system that is strongly influenced by nonfamily environment. Sociolinguists know a lot about the genesis of small-scale variants in dialect. Children almost always learn their native language from their parents at home. However, as youngsters leave the household to interact with peers, they almost always switch their dialect from that of their parents to that of their peers. This is true of language evolution, which is led by younger people, whose dialect is detectably different from that of the older generation. It is also true of people who migrate across linguistic boundaries or gradients. Adults often struggle to conform to the norms of a new region, whereas younger children adopt completely (Labov 1973). In terms of dialect variation, parents have almost no effect on children even if primary language socialization is, as it seems to be, *overwhelmingly* familial. (...) The parents’ large role in socialization disappears from view in this case even if most early language skills are learned from parents. In essence, parents normally transmit basic language traits to children, but the kids in turn acquire from peers the nuances that make up the variation.” Note that Richerson and Boyd base themselves upon older work of Labov; in §3.25 we also discussed Labov’s more recent suggestion that children copy language accurately and conservatively and that linguistic *change* might come from inner-generational processes as grown-ups speak inaccurate and sloppy amongst each other – change which is then fixed as the language passes from one generation to another.

<sup>582</sup> See for instance Richerson and Boyd 2005: 38 “If it happened to turn out (studies are lacking as far as we know) that innate vocal tract anatomy has a modest effect on dialect performance, then a dialect variable would have the same pattern as personality variables. There would be a genetic effect of parents acting through the heritability of anatomical features and a nonfamily environment effect due to dialect learning.” Indeed there is no sign that there are *currently* correlations between certain languages on the one

## §4.3 Key aspects of the problem of the origin of language

*In this section we discuss key aspects of the problem of the evolution of language that constrain Darwinian theories about it. Some of these aspects are ) general, which will be discussed first (§4.31); ) some only apply to i) the evolution of our language ability or ii) the evolution of linguistic forms, which will be discussed after that (§4.32). ) The general aspects of the problem of the origin of language are: a) early versus late evolution, b) gradual versus sudden evolution, c) speech first versus gestures first evolution, d) language treated as mainly innate versus learnt, e) language evolved by natural versus sexual selection and f) language as functional versus non-functional. We will show that it is likely that language is an early, gradual, speech first, learnt (functional) adaptation evolved primarily by natural selection. ) The special aspects of i) the problem of the evolution of our language ability have to do with the level(s) at which the language adaptation is best considered: the genetic, neural, physiological or behavioural level, as well as with the question which elements of the adaptation are exaptations and which evolved 'afresh'. This requires us to delve deeper into what the real or primary function of the language adaptation is, if any. The special aspects of ii) the problem of the evolution of linguistic forms is concerned with 1) the level(s) at which the forms are best considered: sounds (phones, phonemes etc.), words (semantics), syntax, whole dialects/languages etc. and 2) whether their evolution is like neutral drift or truly functional by for instance easing speech, making it more (energy/communicative) efficient, faster, less ambiguous etc. We will show that data are inconclusive regarding genetic and neural exaptations, but that the evolution of speech can as yet best be understood from the level of interactions between individuals. We will further show that although currently the evolution of linguistic forms is neutral (at least net neutral), some changes can be argued to be adaptive, and in the past many more changes have most likely been adaptive.*

In §4.1 we already noted that the phrase 'origin of language' has multiple meanings. That makes general questions about the origin of language ambiguous as long as it is not clear what is meant by the 'origin of language' in a specific context. This becomes highly apparent here, where we focus on the general dimensions of the problem of language use such as whether it arose gradual or sudden or whether it is functional or not, as some aspects of language probably arose sudden (the introduction of new slang) whereas others were gradual (even gradual on very different time-frames, such as the gradual sound changes spreading through the vocabulary, gradual evolution of syntax in existing language, gradual evolution of syntax from scratch, gradual evolution of speech organs), and as some aspects of language are probably functional (language organs, word order) whereas others are not (difficult sounds, some more or less 'useless', intransparent twisters and bells of syntax, especially when syntactic aspects are marked more than once).

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hand and genetic variation related to our biological language system on the other hand: a child from a certain culture raised in any other cultural community will learn to speak the language of the second community without any problem (see below on genetic determinism). However, in the past (before the human population diverged over the earth for the last time around 100.000 years ago) there might certainly have been such a backward causal arrow of linguistic forms upon language organs. See also Dor and Jablonka 2001.

Here, just as in §3.1 we restrict the meaning of ‘human language’ to our complex ordinary, oral language satisfying the 16 design criteria of Hockett. The question of the ‘origin of language *in general*’ refers to the question of the origin and evolution of language from (roughly) our non-verbal primate ancestors to ourselves till this day. The general question has two important sub-questions: a) how did the general human language ability originate and evolve, from our pre-verbal primate ancestors to us modern humans today and b) how did linguistic forms originate and evolve from the simple cries and signs of our pre-verbal primate ancestors to our complex syntactic forms today. Although the general question cannot be answered without clear answers to the sub-questions, it is good to first discuss the evolution of language from a more general level to gain an overall perspective on the problem; after that we turn to the sub-questions. This section has profited a lot from Wildgen (2004) and especially Johansson (2005).

## §4.31 General aspects of the problem of the origin of language

We will discuss the general problem by focusing on 6 aspects, using many of the results of chapter 3.

### a) Early versus late

The basic question is whether our language capacity (and thus language itself) evolved long time ago, in the early stages of our separation from our common ancestor with the chimpanzees, or was the evolution a late development, taking place as the modern *Homo Sapiens* hit upon the stage? ‘Early’ would mean at least several hundred thousands of years ago, and possibly one or two million years ago (Wildgen 2004) with a strong upper limit at 5 million years ago: the time our last common ancestor with the chimpanzees lived who clearly was unable to speak as we do; ‘late’ would mean within the past 100.000 years or so, possibly even during the Neolithic revolution some 40.000 years ago (Li and Hombert 2002). This question is hard to answer as the scientific fields from which an answer to this question could come, don’t constrain the question very strongly. These scientific fields are: 1) paleontology, 2) archeology, 3) animal language, 4) molecular biology/anthropology. Its answering is further complicated by the fact that one can imagine all sorts of scenarios in which rudimentary skills were present for a long time (but possibly undetectable for us with our scientific methods) with more advanced language arriving much later on the scene, either gradually or rather suddenly. The next subsection on gradual- versus suddenness of language evolution addresses questions pertaining to such scenarios; here we first resume the results on the absolute origin in time of language evolution.

1) Paleontology: in §3.22 we discussed the clues from paleontology: the anatomical indicators of language use; this was hard because the true clues were scarce as most bodily changes for language use possibly happened at the neurological instead of the anatomical level and the neurological level does not fossilize. Moreover, the few anatomical clues were often questioned to be true indicators of language use as they could be by-products of other developments or exaptations for other functions. A final complication is that *before* human physiology could adapt to language use, language should already have been used extensively, because otherwise

there would exist no selection pressure on the language organs. If, however, we assume that multiple anatomical clues indicating more or less the same time period are sufficient ground for a first estimate of when at least a *rudimentary* language was spoken, paleontology suggested that language originated at least 500.000 years ago, as ear ossicles and hyoid bones present in both *Homo Sapiens* and *Neanderthals* suggests that language skills were already present in their most recent common ancestor who lived at least 500.000 years ago.

2) Archeology: in §3.22 we discussed the archeological clues which were rather indirect clues as spoken language leaves no direct trace, except in writing (or recording etc.). The first signs of writing date 6 to 10 thousand years ago, a time about which no-one doubts that humans back then spoke a language. The most indirect clues are remnants from activities that were thought to presuppose language use such as hunting, making fire, sailing to other continents etc., but it is doubted whether these activities can really not be performed without language (signs) or whether not a very primitive system of signs (such as chimpanzees already have) would suffice for them. Other indirect clues involved symbol use like ochre use, art etc. One can doubt whether these symbolic uses were really used for the same goals as language use (i.e., language is normally not used for for instance decoration); such doubts make it hard to conclude safely that people in the times from which the clues come, spoke a language, although they do seem to imply some higher cognitive capacities. However, even if these symbolic uses could be linked to language use, it would not help the discussion whether language evolved early or late much further as archeological clues of symbol use date from about 100.000 years ago or earlier, a time we already called late and about which almost everyone is convinced (for other reasons) that people already spoke a language.

3) Animal language: in §3.23 we discussed the results of the study of animal language with one of its focus points the language of primates. The level of language skills of primates, especially chimpanzees, allows us to assess (qualitatively) how big or small the leap from their language skills to ours is and thus whether it could have evolved quickly after our language separates from that of the chimpanzees or not. Depending on how much one attributes to the language abilities of primates, this is still a matter of controversy. All the years of primate research have at least made clear that the language faculties of primates are bigger than anyone could have thought 30 years ago.

4) Molecular biology/anthropology: if we understood what genes are responsible for language skills, it seems we could compare our genome with that of chimpanzees and estimate how long this evolution would have taken. Unfortunately, however, our knowledge of the genes involved in language use is extremely limited (§3.22) and, worse still, even if we knew exactly which genes were related to language use, that would not allow us to calculate when the genes originated as there is no definitive method to assess from an overview of the number of genetic changes between species *when* these changes have happened, since in the case of positive natural selection one cannot assume that the rate of changes is constant (as in neutral, molecular evolution with its more or less constant molecular clock rate).<sup>583</sup> Further, it would still not tell us when language was spoken unless we knew what *set* of genetic changes sufficed for elementary language skills (or more advanced language skills) etc.

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<sup>583</sup> What can be done, though, is by looking at changes in the non-coding environment of coding genes that have been changed during evolution from one species to another to calibrate how long ago these coding genes have been changed. In practice, however, this is hard to do.

So far so good for molecular biology. The only hard constraint that does come from molecular biology is that since the capacity for language today is homogeneously spread across the population (i.e., a child from every subpopulation can learn any language), complex adaptations must have been in place in the last common ancestor of all living people, who most likely lived 100.000 years ago, or at the very least 60.000 years ago at the time different population of modern humans parted ways to different continents (Cavalli-Sforza and Feldman 2003). But again, hardly anyone doubts that language was already spoken back then.

The evidence on whether language arose early or late is thus inconclusive; we nevertheless find the evidence that language was an early adaptation more plausible than the opposite, because it dovetails nicely with the idea that the evolution of language (abilities) was gradual rather than sudden for which we have independent evidence; i.e., when language evolution has to be gradual, it has to start early to end with the complex language we have nowadays. We discuss evidence for the gradualness of language evolution next.

**b) Gradual versus sudden:** the basic question is: did we acquire language in one big shot, without intermediate forms, or did the evolution of language pass through many steps of every time a little bit more advanced language skills and a little more complex proto-language forms? Much depends on one's assessment of how *biologically* complex man's language abilities are. We will discuss these issues further as we discuss the genetically innateness or culturally determinedness of human language. Here we will first briefly review some of the results of §1.22 to prevent misconceptions about what gradual or sudden means, then we turn to a discussion of the literature to conclude that the evolution of language skills has been gradual, and that although diachronic evolution of linguistic forms can be relatively fast, their dependence on language skills means that initial evolution of language will nevertheless have been slow.

We described in §1.22 that whereas 'evolution' theories of Darwin's contemporaries were often non-gradual, Darwin himself, strongly inspired by Lyell, held that evolution was gradual, or more precise *uniformitarian*: regular (in every time period is evolution as fast as in any other) and slow (happening over geological time). At the beginning of the 20<sup>th</sup> century in the circle of Mendelian geneticists, this view was often replaced by saltationism: the idea that single mutations can be responsible for big jumps in the evolutionary record, but this view was again discarded by the modern synthesis as more was known about multiple-loci genetics and genetic variation in the wild (see §1.1). Then in the seventies Elredge and Gould shook the orthodoxy of the modern synthesis by postulating their theory of punctuated equilibrium of long periods in geological time when there is hardly any new evolution and other, shorter periods in which a lot of evolution is going on – which again seemed to imply saltationism (and which has also more than once been represented like that). That was, however, not what Elredge and Gould meant as their short periods of more rapid evolution are still periods within geological time, i.e., periods of millions of years. In more recent years, the discovery of regulatory or master genes such as Hox-genes, controlling large sets of genes during development, seemed to offer a new path-way for saltationism (Schwartz 1999) as 1) mutations in or 2) duplications of Hox-genes could have serious implications for an organism's phenotype: body parts moving around or disappearing, or new copies of old parts sprouting up in new places. However, as far as we know, 2) there is no evidence that any Hox-genes (or other master genes) have been duplicated in the human lineage as

all mammals appear to have the same set, in the same number of copies.<sup>584</sup> 1) Mutations in Hox-genes and other master genes, have occurred though; however, (at least initially) such mutations lead to changes in existing organs and building plans (for instance extra brain growth or encephalisation), but they cannot account for radical new innovations which have to evolve the normal slow way like all other normal, biologically tenable forms of evolution.

For the case of language this means the following: i) if language evolution is seen as determined or constrained by the evolution of language skills (notwithstanding the fact that diachronic evolution of linguistic forms can be much faster as simple or as complex as they are) and ii) if these skills are seen as (relatively) complex, new innovations, then from a biological point of view language can *only* have evolved gradually and slowly, just as most adaptive phenotypical traits have. In our view both i) and ii) are true, and thus language evolution was gradual and slow, and, as a consequence, this evolution started early.

Many linguists, especially in the Chomskian tradition, have, however, thought that language evolved suddenly or saltationistically (Bickerton 1990, Chomsky 1988) often out of the feeling that ‘half a language is no language so it had to arise at once’, although others, less biological naïve linguists in the Chomskian tradition (Jackendoff 2002, Pinker 1994) or outside the Chomskian tradition (Burling 2005, Deacon 1997) have argued contrarily. An argument for saltationism would have some biological plausibility (from the perspective of evolution) if language was seen as the simple byproduct of larger brains, or more specifically of the mathematical ability to do recursion (Fitch, Hauser and Chomsky 2005) as by-products can arise suddenly (although it remains nevertheless biologically naïve to think that language is ‘just a byproduct’). However, the view of some Chomskians (and Chomsky himself in his earlier work, see Chomsky 1986) who think that the the highly complex, homogeneous, monolithic language or grammar module must have arisen all at once, is clearly wrong. Apart from the fact that in our view such a device is unlikely to exist, the defenders of a sudden origin of such a device forget that even if it exists the device would still have evolved by becoming more and more efficiently implemented. The Chomskians think the language organ is so homogeneous that it must have evolved in one big bang. However, in evolution sub-modules often evolve independently in a ‘bricolage’ fashion toward a fully-integrated, efficient system: just as in case of the eye or the different senses have evolved at different times, but nevertheless offer us an integrated, consistent picture of the world. The same would be true of a ‘monolithic’ language device, if it were to exist.

**c) Speech first versus gestures first:** this was one of the dimensions of the problem of language evolution that had a central place in very early discussions of the evolution of language where many authors assumed that speech had been preceded by gesture language, maybe because language evolution was pictured as a one-dimensional process from simple to complex language (with gesture language pictured as simple or ‘primitive’), or because gestures were seen as signs that were more iconic or more natural to understand (which is partly true: although many gestures are arbitrary, some gestures such as face expressions are genetically hard-coded and thus culturally independent and others are easy to understand from the

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<sup>584</sup> Although other non-master genes have been copied during evolution in the hominid lineage such as the genes for red and green sensitivity in our eyes. Li and Hombert’s 2002 suggestion that language evolved saltationistically by a mutation in a Hox-gene is to be discarded as biological naïve.

context as the sign of an action can be like a pseudo-performance of that action etc.). Examples of these older theories are that of Condillac (1746) and Rousseau (1755) and it was also central to Leroi-Courhan's famous account (1965). A problem for the theory that language started as gesture language is that one has to come up with an extra evolutionary process how and why there was a switch from manual language to verbal language. Maybe this is not such a big issue as a large fraction of our language use today is written instead of verbal, without much of a problem and even big apes do not have a lot of problems using the new medium of communication of iconograms. In any case, the traditional answers why gesture language was replaced by verbal language were that with speech the speaker and listener do not need to see each other which is a clear advantage in the dark (Rousseau 1755), as well as that speech grabs the attention much better than manual language (id.). In Leroi-Courhan's scenario bipedalism meant that the front-paws were freed to be used for tool use and over time the hands became so adapted for flexibility that they could be used for signing; signing became then so popular that the hands were too busy (as they were used for both artifact use and signing), so that switching to speech meant freeing the hands again for artifact use.<sup>585</sup> Other, more modern ideas are that speech is more efficient, using less time and energy (Knight 2000). However, one could argue contrarily that gesture language is advantageous in noisy environments or useful to talk more discretely, and that deaf people can go about just fine without much practical problems of not-using artifacts while using language.

Because one needed an extra story about the switching on top of the explanation why and how humans started using language in the first place, the gesture first theories were in general not very popular. However, recently the gesture first theory gained new momentum because of a few new discoveries or developments, which we will discuss briefly. Some of these discoveries show that gesture and verbal language are much more similar than once thought (1 to 6); others that gesture language could have evolved easier than verbal language (7 to 9). One constantly need to remind, however, that current gesture language is different from the gesture language which supposedly preceded verbal language; the former is dependent on verbal language in one form or another, the latter not (see footnote 545).

1) ASL and other sign language as true languages:<sup>586</sup> for ages hearing families with deaf children have employed ad-hoc signs for simple communications; only in modern societies have there been large groups of deaf children or adolescents living together with the opportunity for more complex sign languages to evolve. Famous is the standardized Old French Sign Language (OFSL) developed and used in Paris by the Abbé de l'Épée in his school for the deaf. Standardized deaf languages were always modeled after the natural sign languages already in use by the deaf cultures in their area of origin, often with additions to show aspects of the grammar of the local verbal languages.

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<sup>585</sup> Carstairs-McCarthy's 1996 scenario is still rather similar to this proposal as it postulates that when the hands were not needed for communication, they could be used for carrying things while working. Note that supporters of gesture first scenario's still see bipedalism as an important exaptation for manual gesturing (and tool use for that matter), see Corballis 2002. The attractiveness (and the danger) of Leroi-Courhan's scenario is that it tells a convincing story of man progressing towards the 'homo verbalis' he is nowadays, much in the sense Landau warned us for (Landau 1991, see §3.22). Just like all other scenarios, its episodes need to be carefully checked to change from a convincing scenario into a true one.

<sup>586</sup> For a nice history of sign languages, especially in America, see Padden and Humphries 1988.

OFSL was brought to America by Congregationalist minister and deaf educator Thomas Hopkins Gallaudet who convinced a French teacher of the school of De l'Épée to come with him to teach sign language to American deaf students. There, American sign language was formed by students mixing OFSL with their original 'home' signing language so that up until today OFSL and ASL share about 60% of vocabulary, although British Sign Language (BSL) and ASL have almost nothing in common. For a long time, there was a debate in the USA whether sign language was a true language between the so-called oralists and the manualist; the oralists won and suppressed the use of sign language for a long time. This situation only changed in the sixties as William Stoke, professor of English hired at Gallaudet University proved that sign language *contains phonology, morphology, semantics, syntax and pragmatics just like spoken languages*.<sup>587</sup> Most linguists were convinced by Stoke's case and since then it is common practice to treat sign language on a par with vocal language, which implies that the question on the origin of language could almost just as well be about the origin of sign language. William Stokoe became indeed one of the first serious modern defenders of the gesture first theory (Stokoe 1978), a little later popularized by Auel (1980).<sup>588</sup>

2) Sign language is common: on holiday or with foreigners people start communicating with their hands and feet to get their message across; sign language can, however, also be more institutionalized: native American tribes that did not understand each others' language used a well-established form of sign language to communicate with each other, whereas normal pidgins are usually verbal (Auel 1980); this fact adds weight to the naturalness of sign language.

3) Gestures still accompany normal speech: even in our daily speech we still use gestures to get our verbal message across; this happens to a large extent unconscious: most of the time people are talking, they are making meaningful gestures as well (Goldin-Meadow 1999), which you can notice as you explicitly try to communicate without making gestures which is almost impossible (or think of telephone conversations compared to real conversations, especially when the telephone line is bad and you cannot hear the intonation well). Even congenitally blind people (who can hardly have acquired this habit from observation) gesture while speaking, also when addressing blind listeners (Iverson and Goldin-Meadow 1998).

4) Neurology and aphasia: it is claimed that as one uses sign language the same neurological processes occur or at least similar areas are active as during verbal speech, although there are small differences too (Neville et al. 1998, Hickok et al. 1998b); however, as long as neurological processes in general as well as neurological processes involving language use are not well understood (see §3.21), this fact doesn't have much bearing as it could easily be explained otherwise (by relating the similarities to the task of paying attention etc.). Uncertainty over the basal function of certain neurological areas also leads to vehement debates whether the observation that brain areas normally used for auditory processing are involved in sign processing in congenitally deaf individuals implies that the normal brain areas used for auditory speech processing are more general *modality-independent language modules* (Petitto et al.

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<sup>587</sup> See Stokoe 1960. For more recent work, see Van der Hulst and Mills 1996.

<sup>588</sup> After the first wave of emancipation of manual language as a true language, we now find research on the way deaf language is special or different from oral language (just as from the Sixties onward the differences between oral and written language have been studied extensively from which it has become clear that the mode of language use constrains or shapes the linguistic forms produced). In the case of sign language we find shorter sentences with relatively few relative clauses, fixed word order, but a highly free use of what we would call predicates, adjectives and adverbs.



2001) or not. Similarly, brain lesions studies have been used to claim for the similarity of sign and verbal language by demonstrating that deaf people suffering from ‘sign language aphasia’ have similar neurological damage as people suffering from ‘normal’ speech aphasia (Hickok et al. 2001). Again, these claims are hard to prove given the current status of neurological science.

5) Language acquisition: children of deaf signing parents ‘babble’ during their early development (Petitto and Marentette 1991), and early vocabulary development in sign language seems to be rather similar to that in speech of hearing children of speaking parents (Cheek et al. 2001). And in the case of ‘bilingual’ children acquiring both a signed and a spoken language, the parallels are even stronger, with the same child attaining various milestones simultaneously in sign and speech (Petitto et al. 2001a). This observation further flattens the perceived differences between the two modalities of language use. Further, even in the case of mono-lingual verbal language acquisition do communicative gestures play a major role (see §3.24): not only the general pointing and gazing gestures (which actually precede true speech), but often early ‘word’ combinations are also made of one word and a gesture (id., see also Iverson et al. 1999).

6) Pidgin and creole formation: since sociolinguistics became a major field of studies, the birth of a new language, from a pidgin to a creole (see §3.25) has been witnessed and carefully documented multiple times. Of course, the birth of a language does not mean here the ‘absolute birth’ of language as the creation of a pidgin depends on the existence of a lexifier and a substrate language on the one hand and that of (biological) language abilities on the other hand. Interestingly enough, however, one of the best studied cases of pidgin and creoles formation is not that of a verbal language, but that of a manual language among the community of the deaf in Nicaragua: in the sixties deaf people who had spend their lives among speakers where brought together for the first time and their private home signing languages mixed into a pidgin sign language; as a new generation of deaf children grew up in this community for whom the pidgin was their main language, the pidgin evolved into a creole; this shows that either i) language acquisition is highly cultural determined and only dependent on general intelligence and not specific language skills, or that ii) language skills are ii)a general, not mode-specific (manual, oral) language skills or ii)b mode-specific, but once evolved in the manual mode, now exapted for the verbal mode with the manual mode still available as ‘back-up’.<sup>589</sup> Whatever explanation is valid, any complete explanation of the origin of language need to take the fact of the birth of a complex manual language into account.

7) Mirror neurons: in §3.21 we already discussed the so-called mirror neurons: a neural system activated both by performing an action and observing the same action by another person. There we already remarked that some (such as Rizzolatti & Arbib 1998) speculate that these play a major role in language evolution, imagining that language evolution arose from mimicking actions that are the topic of conversation using iconic manual (and oral-facial) gestures. In §3.21 we already criticized this idea heavily: first of all it is unclear whether humans and primates really have mirror neurons (which were first discovered in monkeys), secondly it is unclear how mirror neurons work and thirdly whether they are truly involved in mimicking as the monkeys in which they were found are not good at aping (see §3.21); further,

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<sup>589</sup> See Senghas et al. 2004 for a defense of the Chomskian perspective which is like ii)a and according to Senghas the only possible explanation for what happened in the case of the deaf in Nigaragua. Polich 2005 strongly criticizes this perspective.

although language *learning or acquisition* certainly involves an element of imitation, it is unclear whether the absolute origin of language can be traced to imitation (or the evolution of better imitation skills). Finally, the argument for the role of mirror neurons in language evolution is based on the assumption that the meaning of gestures is more readily understandable than that of vocal words; an assumption which is only partly true (see §3.21) in as far as only some gestures have an innate component or are related to the actions they are signs of (however, also note that in verbal speech the prosody of vocal sounds can also reveal a lot about their meaning).

8) Ape studies: it is evident that monkeys and apes use facial (and bodily) gestures for social communication; however, their control of their facial gestures is more or less involuntary (Tomasello 2003); on the other hand, they do have voluntary control over their hands and they have been observed to use manual gestures for communication involving reference (Leavens and Hopkins 1998, Leavens, Hopkins and Bard 1996, Savage-Rumbaugh and Lewin 1994, Call and Tomasello 1994), also in the wild (Vea and Sabater-Pi 1998). Apes and monkeys further also use gaze to ascertain the message gets across (Bard 1992), and it is currently a matter of controversy whether apes can use information from the gaze of others (Tomasello et al. 1999 and Call et al. 1998), although monkeys clearly cannot (Anderson et al. 1996). This actual observed behaviour seems a good indication that proto-human language started out as gesture language; however, we saw in §3.23 on animal language that apes and monkeys also use vocalizations for communication, both social (grooming) and referential (alarm calls etc.). Further, apes were able to use iconograms for at least basic forms of communication, which indicates that the modality of language (gestural [manual, facial], verbal [vocal], written [iconic]) might not have been a key constraint during the evolution of language on which more below.

9) Exaptation: finally, one of the perceived advantages of sign language is that it eases the evolutionary transition to language use as readily existing organs in pre-verbal hominoids could be exapted for signing; most notable are the bipedal posture which freed the arms for communication as well as the primate hands which are already under free cortical control (Corballis 2002) while, as we just discussed, they lack more or less voluntary vocal control for speech (as well as voluntary control of their facial gestures for that matter which are important in gesture language). However, the same bipedal position can be invoked as an exaptation for speech as it decoupled breath and stride (see §3.21), freed the face from its basic nose downward to its nose forward position which allows for easier communication, and finally it is sometimes argued that the bipedal position somehow eased brain-growth, the lowering of the larynx or the loosening of the jaw muscles which are all indirectly related to language evolution.

Of course language evolution need not be a black and white scenario of either gestures or speech first: it is perfectly fine to consider a scenario in which gestures and sounds were first used *together* to communicate with speech later taking over (Auel 1980). An advantage would be that in such 'multi-component' signaling the detectability and discriminability of the early, rudimentary signs would be increased significantly. Interestingly, this is what Darwin himself implicitly thought as he defended his mimetic/onomapoetic theory of the origin of language: "I cannot doubt that language owes its origin to the imitation and modification, aided by signs and gestures, of natural sounds, the voices of other animals, and man's own distinctive cries (Darwin 1872: 56)."

Unfortunately, there is inconclusive evidence for either possibility or for the combination of both; though manual and verbal language are much more similar than once thought, the reasons why gesture rather than verbal language would evolve first are not so strong. As gesture first explanations require an *extra* explanation of the transition to verbal language, Occam's razor suggests we opt for a verbal language only explanation, which in fact we will for that very reason. However, the use of Occam's razor is here not so clear-cut as in other cases as the verbal first explanations require the acknowledgment of i) the supporting role of proto-gestures in language acquisition (and evolution) and ii) the fact that manual language is rather similar on multiple aspects to verbal language so that an full evolutionary account of verbal language skills need to include an account of some general ('neurological') language skills that can be exapted for manual language. If applied in such a modest way, Occam's razor can guide us: focus on verbal language, keep your eyes open for any exaptations and possible supporting roles for gestures (which gestures still have today) and only invoke a strong 'gesture first' or 'gesture only' stage during hominoid language evolution if evidence against the possibility of early, gradual vocal language evolution is found (which as yet is clearly not the case as the evidence we have, as little as it is, seems to point in exactly the opposite direction).

#### **d) Innate versus culturally determined:**

One of the biggest discussions in linguistics is whether language use is largely innately or culturally determined. As everybody knows that language is not completely genetically determined (innate), nor the result of straight tabula rasa conditioning (cultural), the real question of the innateness of language is more or less a question of shades of grey: *how much* of language is innate. Further, many people mean different things by innateness (Griffiths 2002 and see footnote 367), hence the question of the shades of grey is also a question *from which grey scale*. In chapter 3 we collected evidence pointing towards the view that the grey shades on most scales are in favour of culturally determination than innateness. In this section we will briefly sum up these results; we don't define innateness ourselves, from our discussion it will be clear what innate means in every instance and what aspect of language is denied or accepted to be innately determined.

Before we review these results one initial warning: humans learn to speak thus they are pre-dispositioned to learn a language. However, this predisposition does not mean that my *ability* to use language is innate: humans pick their nose, and thus they can be said to be predispositioned to pick their nose (the fingers even exactly fit the nose holes); however, the behaviour of nose-picking does not seem to be innate. Hence, to argue for innateness more than a *mere predisposition* or *possibility* is required, otherwise any possible behaviour man can practice can be called innate. On the more concrete arguments for innateness (or a language acquisition device), our review concentrates.

Two principle arguments pro innateness were:

a) poverty of the stimulus: children learn their mother tongue very quick on the basis of low quality input. This can only be explained by an innate language acquisition device that universally constrains, and by constraining helps children to find patterns in the speech data.

b) language universals: languages have certain universal features. These can only be explained by common innate constraints on language (acquisition/use).

However, we saw that a) the stimulus is not poor, that the details of languages are learnt well into the teenage years, that proto-grammar of child-speech

can hardly be characterized using the categories of adult grammar, that children follow many different patterns of acquisition, making also sorts of mistakes regarding syntax and semantics and finally that language acquisition is not so much about learning a structure as about learning to interact socially and pragmatically.<sup>590</sup> We also saw b) that supposed language universals turned out to be either highly general and non-informative or not universal at all, and that any remaining universals (if found) could also be explained by common origin of all languages (from one language) or other constraints, instead of by innate constraints.

We further saw that there are far too few genes to code for language rules, that the brain is highly plastic with many regions involved in speech (with individual differences in the exact regions involved, especially in the case of brain damage early in life when children still learn speech normally), and that apes can be taught the rudiments of language without having used it and thus being adapted to it. All these facts further strengthen the case of language being strongly controlled by cultural evolution.

With the facts from different branches of science on our side, we believe that although man is adapted to learn a language, specifics of language are generally not strongly innate, be they on the phonetic, semantic or syntactic level (see also on the evolution of proto-language below f). In the future, when we know more about how language is coded in our genes and being processed in our brain, the dispute will be finally resolved since then we know in detail what is innate, to what extent and what not.

#### **e) Natural selection or sexual selection**

In §1.5 we distinguished many phenomena that fall under the general header of ‘sexual selection’ and our extensive discussion can now pay off. It was shown that most often when something is called sexual selection we deal with normal natural selection related to sexual organs or sexual reproductive strategies that can be understood as direct adaptation; only in the case of ornamental selection or sexual selection *proper* did we encounter a phenomenon that was not a straight-forward extension of natural selection as it involved the evolution of mate choice on the basis of a handicap which posed a direct disadvantage and only indirectly – as sign of good health – an advantage. If we now discuss whether language evolution was (primarily) due to sexual selection or natural selection, we mean *by sexual selection proper* (ornamental selection) *or by natural selection* (including the other cases of sexual selection). It will turn out that it is unlikely that sexual selection played a major role, although minor elements might have been shaped by natural selection. Note finally that we postpone issues of how natural selection exactly shaped the organs (de nova evolution, exaptation, spandrel etc.) to the next section.

In answering the question whether language evolution might have been a case of sexual selection, we have to look at two issues: i) the more general issue whether there are major changes in the sexual life of humans compared to other primates possibly creating a niche for ornaments being important in the race for the best mate, and ii) the more important issue whether some elements of language use

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<sup>590</sup> See Seidenberg and Macdonald 1999: 574 as quoted by Johansson 2005: 183 “Many of the classic arguments [for innateness such as the poverty of the stimulus] rest on the assumption that the child’s task is grammar identification, and these arguments simply no longer apply if the task is instead acquiring the performance system underlying comprehension and production.”

can be regarded as (primarily) ornamental so that they might have arisen by sexual selection proper.

**i) The sex life of primates and hominoids:** in the last 50 years a lot of man hour have been devoted to the study of the sex life of humans (both historical, sociological and cross-cultural) and that of other primates. Ever since the famous work of Alfred Kinsey who applied the same statistical rigour of his wasp research to the study of the sexual behavior of the human male (1948) and the human female (1953) we know of the big diversity in sexual libido, orientation, preferences and activities amongst humans within a single culture (as true as the critique has been on the representativeness of his huge sample and on the trustworthiness of the questionnaire and the interviews he and his coworkers conducted); ever since Foucault (1984, 1984, 1976) wrote his popular *History of sexuality* (and the more serious work on the history of sexuality of our own and other cultures that has appeared since the second World War), we know that the sexual life of humans in *previous ages* has been as active and diverse as our own despite the often existing moral laws and customs (including the supposed puritan Victorian ages); ever since the work of Margaret Mead (1949, 1935, 1928) we know of the diverse sexual customs including the different sexual freedom and taboos of other tribes and cultures; and ever since research on animal sexuality we know of the non-reproductive sex animals engage in, or the “biological exuberance of the animal world” as Bagemihl (1999) calls it in his extensive review of the topic. It is impossible to do justice to this huge body of research, even if we only focus on the evolution from hominoid sexuality until the Stone Age.

Here we will only briefly indicate the diversity of sexual life-forms amongst primates and the fact that there is no reason why humans are more likely to have sexual ornaments. As said, the mating systems of primates are really different with monogamous gibbons, semi-harems among orangitans, full harem structure in gorilla's, various strategies amongst chimps with females sometimes opting for monogamous relationships with a preferred male, extra-group loose sexual relations or promiscuous mating within the group,<sup>591</sup> and the ‘matriarchal’ bonobos engaging in all sorts of sexual-bisexual relations with sex as a means to resolve conflicts and establish friendship. The longer these primates are studied, the more complex the systems turn out to be: the monogamous gibbons are in reality more semi-monogamous (Brockelman et al. 1998), orangutans males choose between semi-monopolizing groups of females (with or without their consent) and roaming the forest for females the dominant male isn't watching to seduce or rape (Singleton and Van Schaik 2002). That makes the diverse human sexual life not very exceptional amongst primates, so that before we dive in to more detail on human sexual life, it seems unlikely beforehand that hominoids have been subject to strong sexual selection. Only two facets of human sexuality seem really different: 1) although humans form more or less monogamous pairs we keep living in bigger groups, unlike other monogamous primates where couples live solitary (Deacon 1997), 2) humans are sexually active when fertilization is not possible and ovulation is concealed whereas other apes often advertise when they are fertile. However, 1) many species

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<sup>591</sup> It is unfortunate that the little data that is available on which reproductive strategy of one of our closest cousins is the most succesful is contradictory (Vigilant et al. 2001); possibly also due to frequency dependent selection on the success of the strategies? The data would tell us more about the selection forces human reproductive strategies have been subject to.

of monogamous birds live within larger groups and even the monogamous gibbons live within ear-shot of each other and 2) other apes have sex for other reasons than reproduction only and hidden ovulation does not really seem to complicate reproductive strategies.

The standard (quantitative) means to look for sexual selection is the presence of sexual dimorphism with in monogamous species males and females usually very similar and in polygynous species males usually bigger and (more) ornamented than females; Plavclan (2001) reviews the evidence for sexual selection amongst primates and finds a clear correlation between the nature of the sex-life and the dimorphism: huge alpha male gorillas with silver-stripes on their back and small gorilla females are in line with their polygynous system, and the same holds for the quasi-monogamous male and female gibbons which are hard to tell apart. The strength of sexual selection in the hominoid lineage can be estimated if we look at the evolution of dimorphism; humans and chimpanzees have similar levels of dimorphism which can be interpreted as a sign of continuity in mating systems since the last common ancestor; however, *Homo australopithecines* is often portrayed as having a large male/female size ratio, suggesting a polygynous sex system equivalent to gorillas (Ward et al. 2001); however, more recent studies argue contrarily (Reno et al. 2003) re-assessing the size ratios estimates, and from *homo ergaster* onward there is consensus on the level of human dimorphism being only modestly higher or similar to the level today (Plavclan 2001).

All in all, there is little reason why there would be any sign of strong sexual selection amongst humans (apart from the existing *stabilizing* sexual selection).

**ii) language as ornamental:** the claim that language is primarily ornamental can be distinguished in 1) the general claim that language as a whole is ornamental and 2) the more specific claim that a fundamental element of language is ornamental.

The general claim is seldom encountered in the literature with the exception perhaps of Geoffrey Miller's *The mating mind* (2000), in which he relates almost every aspect of human culture (in which language plays a key role) to sexual selection with language primarily used for chit-chatting, joking, telling stories,<sup>592</sup>singing etc. and thus for attracting and keeping a partner by offering amusement. Miller gives a lot of anecdotal evidence of ugly men who because of their humour were able to attract a lot of mates such as Charlie Chaplin who had many children by many different wives. However, as important as humor is in modern human societies, it seems unreasonable to see the primary function of language to be amusing a partner; Miller's idea fitted well within the rediscovery of sexual selection as an important evolutionary mechanism in its own right in the nineties (see §1.5) which made it seem the panacea for all outstanding evolutionary problems, but on closer inspection ornamental selection for joking etc. seems an unlikely candidate explanation for the evolution of language: being humorous requires quickness, wit and social intelligence (things that can be the object of selection), but being humorous seems a character

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<sup>592</sup> McNeil 1996 and Heeschen 2001 invoke story-telling even as the fundamental function which drove selection on language skills. Ironically, however, their scenarios are both based upon natural selection and not sexual selection. Although it cannot be denied that story telling is unique and important to humans, it seems clear that this is not the only or most important function of language and it is hard to imagine a course of evolution in which story-telling was the selective advantage driving language evolution instead of for instance a byproduct of this evolution: it seems that story-telling presupposes language use but not the other way round (simple proto-language does not seem to be a good tool for narrating).

trait that cannot easily be ‘biologized’: what would a humour organ, let alone a humour gene mean?

2) More serious theories of the origin of language akin to sexual selection are theories that relate it to music. Music is certainly important in human mating (and in social group bonding in general), although it is not exclusive to humans: think of bird song, frog’s quacking and chimp’s grunting. In language evolution theories music is envisioned as an intermediate stage easing the transition to language, not as the real function of language. The idea of a role for music in language evolution goes all the way back to Rousseau (1781), Darwin (1872, 1871) and the famous linguist Jespersen (1922), and has recently been re-introduced by Mithen (2005), Vaneechoutte and Skoyles (1998), Verhaegen (1998) and Donald (1991).

Arguments pro are:

x) music still plays an important role in language use: intonation, melody and prosody in general are vital to language use; this is even acknowledged in the folk wisdom that its not the *content* that gets the message across, but the *tone*.

xx) music can be a good intermediate stage of language evolution: chimps can already ‘sing’ so that the transition to more complex human singing seems rather straight-forward; because of singing there could be a selective pressure on the sound production organs towards wider sound and pitch ranges, more sounds and better control of the organs; these advanced sound production organs would then be highly useful for speech.

xxx) music is social: as said music has a group building quality; Dunbar (2003) for instance suggests that vocal chorusing may have played a group-binding role in *homo erectus*; however, in this case it is not music as such that is selected for its sexual attractiveness, but music selected by natural selection in the context of language as an adaptation for social-group-binding, which we will discuss below under f).

Arguments contra are:

x) big leap: although music skills may have played some role in the evolution of our *physical* language organs and our sense for rhythm, it is a far cry from singing to full, complex syntactical sentences (even though music can also have a hierarchical ‘syntactical’ structure).<sup>593</sup>

xx) different neurological mechanisms: although little is known with great certainty about the neurological mechanisms underlying speech, it appears that music has its own neural mechanisms that are quite distinct from those used for language (Mithen 2005); a hypothesis supported by the fact that there further exist cases of people who are completely a-musical but still having normal language skills (Sacks 2007, Balter 2001); in the case of music laying at the roots of language one would

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<sup>593</sup> Okanoya argues for strong analogies between the complex structures of bird song (which are supposedly shaped by sexual selection of females for attractive male singers) and the syntactic structure of human sentences; however, except that both can have a complex hierarchical structure, there seem to be big differences between syntax which function as (almost mathematical) repetitive structures with semi-random variation in bird-song and syntax which is the result of semantical generalizations in human speech. Okanoya seems to reduce syntax even more to mathematics than the Chomskians do. Further, the sexual selection Okanoya thinks of balances between random drift (such as the molecular evolution in Kimura’s vision) and run-away selection a la Fisher which are both not true cases of sexual *ornamental* selection as described it in §1.5 and upon which we concentrate here. Note finally that comparisons of human skills with avian skills are difficult as our latest common ancestor with the birds is even older than the dinosaurs so that any comparison has to presuppose massive parallel, convergent evolution (which seems a priori unlikely to be any good).

expect, contrarily, significant overlap between language and music skills, and their respective neurological mechanisms.

In sum, the evolution of music skills may have been important exaptations for the evolution of our physical speech organs and they may have functioned in the context of social-group building, but theories of the evolution of music do not have much to say on how language *proper* has evolved.

Finally, if we combine i) and ii) we observe another serious blow to theories ascribing a central role to sexual selection in language evolution: sexual selection leads to dimorphism; however, whereas in birds usually only males sing (see §3.23) so that sexual selection is a possible explanation for bird song, male and female language skills are roughly equivalent,<sup>594</sup> a fact that makes theories of sexual selection theories for the evolution of human language a priori improbable. Of course, one can imagine that males evolved language skills and that due to embryological constraints females developed them as well just as males have nipples because females have them, but in such a case female language skills would most likely have been impoverished versions of male skills (just as male nipples don't have milk channels running through them), which is of course not the case.

Conclusion: sexual selection *in the sense of ornamental selection* seems to be unlikely to have played a significant role in the evolution of language. However, this does not exclude the possibility of secondary aspects of our language system being under the control of sexual selection: the descent of the larynx in males after puberty and the subsequent deepening of their voices might have very well been a consequence of female choice, with females sexually selecting males for deep voices (Darwin 1871), possibly because bigger men had (as a result of acoustics) by nature already slightly deeper voices, so that that there is a natural reason why females choose such males and selection for a lowered larynx took off.<sup>595</sup>

#### **f) Functional or non-functional**

In our evaluation of the previous dimensions of the problem of the origin(s) of language we already implicitly assumed that language use is functional. However, some researchers question whether language use has a function at all (in the sense of an evolutionary *adaptation* that leads to differential reproduction as in our definition of function<sub>II</sub>). Of course, it seems at least clear to everyone<sup>596</sup> that when we use language we make promises, give orders etc. which are obviously functional in the

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<sup>594</sup> It is often assumed that women are better than men in language, whereas men are better in math and geometry; it is further often assumed that women talk more than men. However, one can seriously doubt whether these folk wisdoms are true (Dunbar 1996) and whether differences in skills or frequency of language use, if true at all, are not due to social factors instead of biological ones.

<sup>595</sup> Note that selection for the descent of the larynx during puberty could in principle have occurred before (full) language skills had evolved – a possibility that further disentangles this sexually selected trait of our language system from the evolution of its primary functions. Note that a problem with the view of active female choice is that women get better information on a male's dimensions by simply looking at him instead of listening to the depth of his voice.

<sup>596</sup> People who regard language primarily as a complex mathematical system of rules to form well-formed strings such as the Chomskians and other people working on generative grammar seem to do, sometimes deny that language has a function at all. However, when pressed they will admit that obviously language can (as an accidental consequence of the primary mathematical structure) be used for all sorts of tasks, but that they, theoretical linguists, don't wish to entertain themselves with question of pragmatics. Linguists working in a different paradigm such as cognitive grammar put more emphasis on the functional/pragmatic aspects of language; as a result their work can be easier integrated into the Darwinian frame-work.



everyday sense of the word ‘function’, but which need not imply that language use has an evolutionary advantage: i.e., my pointing finger is an excellent tool for nose-picking, but the size of this finger is clearly not an adaptation for nose-picking causing differential reproduction of nose-pickers and non-nose-pickers); we will first discuss i) whether language use has a function (i.e., function<sub>II</sub>) at all, then ii) what function (if any). As in the whole first part of this paragraph we look at the function of language use in general, not that of specific language organs or linguistic forms. We will answer the first question positively in this § and postpone the second question to the next §.

To many people it seems clear that language is at the core of what makes us humans, such as Philip Lieberman: “it is difficult to identify any aspect of human behaviour (...) that would not profit from language (Lieberman 2003).”<sup>597</sup> Others, on the contrary, maintain that although language can be used for many functions, it need not increase survival value as it seems that for instance a deaf person can easily survive and even reproduce in a community of normal speakers as Robin Burling tries to defend.<sup>598</sup>

The general problem whether language use has a function<sub>II</sub> (at all) can be analysed more systematically if we distinguish 4 cases (see §1.23): 1) language evolved as true adaptation (i.e., one which evolved *de novo*), 2) language evolved as an exaptation (i.e., an adaptation that arose out of the functional use of an organ that once evolved for another function and that is re-shaped, re-adapted for its new function), 3) language is a spandrel (i.e., a functional use of an accidental by-product or *non-functional part* of an organism without new selection pressure on it to adapt it for this use, with non-functional in the sense of function<sub>II</sub>) or 4) a vestigial (i.e., an evolutionary remnant that once might have been useful, but which now is no longer used or is used for a different function without selection pressure on this new function).

The general problem is difficult to solve as due to constraints (evolutionary, developmental, physical etc.), a given trait may not appear as a true adaptation, even though it is one (although of course not the fictitious ‘perfect’ adaptation a Panglossian dreams of, see §1.23). Finally, the picture is also complicated by the fact that a trait can not only evolve by natural, but also by sexual selection (in the sense of ornament selection). As shown in §1.5 these traits are often highly a-functional (from the perspective of survival), as they have to be costly to be good indicators of health etc., but as such they serve to attract a mate, i.e. they serve a function (for reproduction), but only by having a strong a-functional aspect (for immediate survival). This means that research on whether language is an adaptation, exaptation, spandrel or vestigial feature can easily go off-track if serious a-functional elements of language use are found: one can then not exclude the possibility of a *de novo adaptation* as the origin of language use may still be due to *de novo sexual selection*. Fortunately, whether a trait evolved by natural or sexual selection is independent of the fact whether the same trait is a true adaptation, exaptation, spandrel or vestigial feature as any sexually selected trait can also be an adaptation, exaptation, spandrel or

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<sup>597</sup> In similar vein, see Lieberman 1998: 5 “[After quoting the initial lines of the Gospel of John: In the beginning was the Word.] Speech is so essential to our concept of intelligence that its possession is virtually equated with being human [Lieberman presupposes that we are intelligent animals]. Animals who talk are human, because what sets us apart from other animals is the ‘gift’ of speech.”

<sup>598</sup> See footnote 564. However, although deaf can easily survive and do beget offspring, it can be doubted whether deaf people really have the same *reproduction chances*.

vestigial feature, so that the question of the nature of the adaptation and whether it evolved by natural or sexual selection can be treated independently as well.<sup>599</sup> Further, in the previous section we indicated that the evolution of language skills by sexual (ornamental selection) alone is unlikely, so that we can now focus exclusively on what *kind of function* we deal with in the case of language.

In the literature we find many diverging opinions on the problem whether language is an adaptation, exaptation, spandrel or vestigial feature. We will first briefly discuss the proposals and then use the tools of §1.23 to argue that *if* it makes sense to speak of the adaptive value of language use *in general*, we best call it an exaptation. The debate would, however, become more subtle if it were more often admitted that language use (or our linguistic abilities) is not a homogeneous whole with one function, but an evolutionary ‘bricolage’ product, some elements of which are better adapted or exapted than others (see §4.32). A complication of the debate is that some linguists know little of evolutionary biology,<sup>600</sup> whereas some biologists have naïve views of linguistics.

A fair number of linguists still reject the view of language as an adaptation. Bickerton (1995) seriously considers the possibility of language being a spandrel, and Chomsky (1988) can be interpreted in this way too.<sup>601</sup> The biologist Gould (1991b), co-inventor of the notion spandrel, also plays with the idea of language being a spandrel and so does Ragir (2001). The nature of the spandrel is different per author: whereas Chomsky sees language use as a spandrel of our ability to understand mathematical recursion, Ragir sees it as a spandrel of encephalisation; however, although both mathematical recursion and encephalisation may have been important for language use, it does not make sense to regard language as a spandrel of either: just as being good in math does not imply mastering a lot of languages or just as adding more memory to a computer doesn’t turn it into a universal translator, so mathematical recursion or bigger brains do not bring language use into existence.

A rather peculiar argument for language being a spandrel comes from the Chomskian David Lightfoot (2000) who also believes that individual rules of grammar are genetically hard-coded. Lightfoot first claims that a true adaptationist (or

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<sup>599</sup> Although it might be the case that *quantitatively* sexually selected traits evolve more often as an exaptation than a *de novo* adaptation, relative to naturally selected traits.

<sup>600</sup> A clear exception is Rudie Botha who is highly critical of Darwinian explanations of language and who dissects many definitional and epistemological shortcomings in the literature on language evolution. He is most critical of Darwinian explanations in general in his 2001a, 2002ab, in which he seeks to demonstrate that there is inconclusive evidence to conclude *either* whether our language organs evolved specifically for language *or* not, which leads him to conclude that we should dismiss Darwinian (adaptive) explanations of language all together. Botha may make a valid point about inconclusive evidence (although our own assessment of the situation using the tools of §1.23 below is different); however, this does not allow us to dismiss Darwinian explanations of language evolution all-together as there seem only to be these 2 possibilities (language being an adaptation or not), unless one leaves the frame-work of Darwinian explanations all together which is, however, the only scientific frame-work we have in biology. For Botha to make his point, he should have shown either a) that *language* is such a special trait that it can principally not be analyzed in terms adaptation or non-adaptation, or b) that when there is inconclusive evidence for option a) or b) neither is the case. Unfortunately, he does not prove a) or b) which is not surprising: a) we give good reasons below why language, like any other trait, can be seen as an adaptation using the tools of §1.23 and b) would be an elementary fault of logic: ‘if I don’t know whether A or not-A is the case, then neither A nor not-A’.

<sup>601</sup> Since 2005, Chomsky calls himself a firm supporter of the ‘adaptationist program’ rejecting any claims that he is biologically naïve (Fitch, Hauser and Chomsky 2005). With adaptationist program he, however, seems to refer to Gould’s version of evolutionary adaptationism which emphasizes spandrels, exaptations, non-functional uses of functional parts etc. In his view, language is the (non-functional) byproduct of our cognitive mastery of the mathematical relation of recursion; as such language itself is not an adaptation.

as he calls it “singularist” (2000: 135) claims that every feature of an organism is an adaptation; then he presents the case of an supposed a-functional subcase of a grammatical rule, thus claiming that there are language traits which are not adaptive, from which he thinks he can conclude by analogy: “of course, precisely the same could be true of Universal Grammar as a whole: UG may have evolved as an accidental side effect of some other adaptive mutation. (...) Natural selection may have played no direct role in the evolution of UG specifically (id., p. 145).” However, apart from the doubtful assumption of the existence of UG, let alone of a genetic ‘rule by rule’ hard-coded version of UG, this analogy is clearly wrong: there exist many non-adaptive side effects (also of language use)<sup>602</sup> created by adaptive selection as any modern evolutionary thinker would agree, but that does not imply that such a large trait as language use in general *must* or is even likely to have evolved as a side-effect of another trait.

Since the people that treat language as a spandrel don’t have a very strong case, we’d better look at the case presented by people who regard language as a *de novo* adaptation or exaptation. In §1.23 we described three methods to determine whether something is a *de novo* adaptation or exaptation: i) by engineering models, ii) by (altering) experiments iii) by comparative models (involving different species and especially evolutionary ancestors), using allometry in particular. We can use these to test whether language use is a *de novo* adaptation or exaptation for the species ‘homo’ (and not for other species). We conclude with an a priori argument why language has to be an exaptation *if* we find that some organs are adapted for speech.

i) Engineering models: we can build models to see whether language is effective for the function for which it is designed and whether there is a tight fit between feature and function; however, as long as it is unclear what function language use itself is used for (i.e., the ultimate question *why* we started speaking in the first place and continue to do so today), these models are rather incomplete. In the next section, we will discuss possible suggestions for the ultimate function of language. For now, however, if we assume that language itself has a function, we can nonetheless easily observe that our phenotype (morphological and behavioural) is clearly adapted for (physical) language use (speaking and hearing): our tongue is highly flexible and as such highly effective for speech, our larynx is descended allowing for a wider range of speech, our mouth cavity is a good sound resonator etc. See the anatomical indicators of language use (speaking and hearing) in §3.21.

The best idea of the engineering perspective comes, however, not from the anatomical indicators, but from the costs of language and the complexity of actual performance. Children spend many daily hours over many years, from birth to early teenage, on language acquisition which is a huge cost; but without these huge cost they would have severely lowered survival and reproduction chances as speechless adults. When these costs are not for sexually selected ornaments as we just argued, they must have some use, otherwise natural selection would long have selected against investing in these costs. Further, actual performance, though slightly different per individual, is amazingly complex, involving complex, accurate rapid sequencing

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<sup>602</sup> Jackendoff 1999 explicitly argues that language use is not perfect, but that it does have the ‘patchwork character’ typical of evolved systems. If it is admitted that language is a patch-work system, the so-called Chomsky paradox (Li 1997) resolves: how can such a highly perfect system of rules evolve by small irregular steps. The answer is: the assumption of the paradox is wrong, language use is not a perfect system but a patch-work system which can evolve in the fashion just described. For some patchwork elements in language use or ‘fossils’ as Markus 2004 calls them, see the ‘twisters and bells’ of syntax mentioned in §3.25.

(for ‘minimal difference pairs’ such as ‘cat’/‘get’, ‘shoot’/‘shot’ etc.), a knowledge of thousands of words and their subtle shades of meaning, the use of thousands of word concatenations, the mastery of complex syntactic structures etc. Such complexity and refinement of the ‘language tool’ must almost imply functionality if sexual selection is excluded.

ii) Altering experiments: these are technically hard to perform in the case of humans, and even if they could be performed, proposed experiments would fortunately not pass any ethical committee. From time to time, however, individuals with genetic defects or neurological damage can reveal us something about how ‘normal’ humans were designed for language use. This evidence was reviewed in §3.21 and 3.22. We found that although little was known with certainty about ‘language genes’ and ‘language in the brain’, the empirical data did not point into the direction of one homogeneous language module in the brain; it seemed that *many* different genes and many different neurological areas were involved in language processing and that some language skills (‘verbal IQ’) might have a genetic basis.

Further, a good measure to demonstrate whether a trait is genetically a spandrel or an adaptation is to look at the distribution of variation in genes: little variation indicates an adaptation, quite some variation a spandrel. The variation of the distribution in the human population of the only gene known to be involved in language use, the famous FOX-P2 gene (see §3.21), shows a pattern indicating natural selection (Enard et al. 2002); however, the FOX-P2 gene is typical of HOX-genes in general, key genes that play a role in many developmental programs and that show little variation anyway, hence the case is possibly not as strong as Enard et al. present it.

iii) Comparative models: we can compare human language skills (ethologically, anatomically, neurologically, genetically) with the language or sign use skills of other animals including other primates as we did in §3.23. The clear differences in ethological skills (§3.23) and anatomy (§3.21) as well as the differences in neurological and genetic skills, though less pronounced mainly due to our limited knowledge of the neurological and genetic basis of language complicating the comparison, make it highly likely that humans have been selected for language use. Ethologically: we spend a lot of time using and learning a language, with adult language use being fast and quite complicated compared to any other animal’s sign use. Anatomically: although speaking takes a lot of learning, our flexible tongue etc. are well suited for speaking (instead of just singing or repetitive vocalising as many other animals do). Neurologically: rapid sequencing of sounds in speaking (a process faster than correctable by “online monitoring” by our brain is but one example of the complex neurological basis of language, which is as far as we know not found in any other animal.<sup>603</sup> Genetically: as just said, as little as we know about genetics of speech, one of the few identified genes that does seem to play a role in speech, the FOX-P2 gene show a distribution in humans (i.e., with little variants) which seems to indicate strong natural selection in the hominoid lineage upon this gene (Enard et al. 2002). Thus from comparative models at all sorts of levels we find evidence that language is an adaptation.

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<sup>603</sup> Unless one finds other rapid sequencing activities by animals and argues there is a common neurological basis underlying these activities; however, the speed of the process makes the rapid sequencing much more likely a domain specific activity. See below our criticism on proposed analogies between sequencing in tool making and alliance formation with the rapid sequencing in language use.

These three tools make it clear that language is an adaptation, although leaving it unclear whether language is a *de novo* adaptation or an exaptation. However, there exists a clear a priori argument why language has to be an exaptation using the backward selection of (the evolution of) cultural elements on language or at least *speech* skills.<sup>604</sup> The argument runs as follows: before there can be selection on *dedicated* speech skills, people already have to ‘speak’, i.e., use simple vocalisations to make themselves understood as there is no need for dedicated language skills when they are not yet used and as the evolution of these skills takes time, language already had to be used long (possibly slower, simpler etc.) before the dedicated skills were ‘fully’ evolved (see below on the evolution of early vocalisations to more complex language, from passively receiving signals to actively sending them in interactional contexts). This means that our existing organs already had to be useful for rather primitive communication before they could be truly adapted for speech; but this means that language is an exaptation as an exaptation involves by definition an adaptation or reshaping of pre-existing organs and the genes coding for them for their current function (Dor and Jablonka 2001).

*In sum: language is an early, gradual, speech first, learnt (functional) adaptation evolved primarily by natural selection.*

## **§4.32 Special aspects of the problem of the origin of language**

*In this section we will first look into more detail for what function language evolved (part I). Although language need not have one unique adaptive function nor one that was the same during all phases of adaptive selection, there must have existed one adaptive advantage (in each phase of the evolution) that triggered its evolution in the first place. We focus on the relation of language use and hunting, tool use, child care and socio-politics. We show that although the first three, as is sometimes doubted, have played a major role in human evolution and that they certainly have benefited from language use, the evolution of language has most likely been triggered by the need for adaptations in the realm of socio-politics. After that (part II) we turn to i) evolutionary explanations of language skills and ii) linguistic forms. Our discussion of the skills will be brief here as most elements of it have already been reviewed in chapter 3; our discussion of evolutionary explanations of linguistic forms will show the strength of the dual-inheritance perspective and will focus on the evolution of phonetics and phonology, semantics, syntax and ‘language as a whole’. This perspective emphasized the pragmatic aspects of language. We will show that dual patterning, the key to the infinite possibilities of human language, most likely evolved after semantic sound-words started to be combined in pragmatic ways. Syntax, though important but we think less than is sometimes claimed, is most likely a late innovation; it also results from pragmatic word combinations that become fixed and generalized over time.*

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<sup>604</sup> In contrast with hearing skills. We saw that, apart from the selection on our hearing canal which probably optimized hearing in the range of typical human vocalizations, our hearing skills are in range or even less of those of other primates.

## I. The function of language

We have shown in the previous § that it seems highly plausible that language use has an adaptive value given the costs involved in developing and maintaining the trait. Here we will focus on the function of language. Of course the language system, like any biological system, is a bricolage system of more or less well-integrated elements that evolved in different periods and possibly for (initially) different functions. However, here we will focus on what triggered the evolution of human language in the first place (in its key phases), what functional advantages did language confer (in its key phases)? As language evolved in the Stone Age, we should look for elements in this hunter-gatherer context that were beneficial to humans – the time since we abandoned the hunter-gatherer life-style is too short for any substantial (biological) evolutionary changes so that explanations that postulate an advantage of language use for agricultural or industrial societies can be discarded easily. The hypotheses put forward are numerous and often complex; what about the suggestion that the initial complexity of language arose out of a quantum-mechanical macro-jump... (Sedlak 1995)? Here we will only discuss a selection of four proposals that are prominent ones in the literature.

The four prominent hypotheses are: language evolved for a) hunting, b) artifact use and production, c) child care and teaching, d) social (extra-kin) relations. Note that these are not always well delineated from each other as for instance in Roebroeks' scenario in which at least a), b) and c) play a role: "The 'quality' education needed to become an expert Pleistocene hunter could not do without a complex form of information, transmittal interaction in which the transcendence of the *here and now*, (...) played a key role (Roebroeks 2001: 451 as quoted by Johansson 2005: 195)." Some may be therefore tempted to summarize these four hypotheses into a more general one, for instance language use evolved for information exchange seeing humans as adapted to a 'cognitive' or 'information-based niche' (Pinker 2003). However, this is like throwing the baby of specificity with the general bath-water away and is apt to be prey to the just discussed fallacy of replacing a real evolutionary adaptation<sub>II</sub> for an adaptation in the sense of common (all the things you can do with language): although information exchange seems advantageous to humans in general (and so are all the processes it is supposed to enable such as proposed by the 4 mechanisms), Darwinian models of reciprocal altruism immediately question whether this is truly the case asking 'why inform each other (instead of misinforming see below d) on social relations) or 'sharing information about what?' (technological, environmental, social knowledge), which brings us immediately back to the four more specific alternatives. We will show of all four that they have played a key role in *human evolution* in the Stone Age (as is sometimes doubted), but that the first three hypotheses: language evolved for a) hunting, b) artifact use and production, c) child care and teaching, are not likely the reason why language evolution was triggered in the first place, though they may have a supportive role in the evolution of language and they too may have benefited from language use. We finally show how d) socio-politics, supported by the other three factors in human evolution, is the most likely trigger behind human evolution.

### a) Hunting behavior

*The theory that language originated for hunting is quite common in the literature; to assess this theory we need to evaluate multiple sub-questions about 1) the importance of hunting for survival, 2) the actual occurrence of language use during the hunt, 3) its use. After that we are in a good position to question whether 4) language evolved for this function or not. One*

*warning beforehand is that this theory can be criticized a priori on the ground that it would make language skills a property that would only have a selective advantage for hunters (i.e., males), whereas we just saw under our discussion of sexual selection that a theory that presupposes different language skills for each gender seems to be headed astray before it goes off ground.*

1) Was hunting an important means of survival for hominoids?

We are really accustomed to the image of the hunter-gatherer male as a hunter that this may seem a strange question. However, in recent years many have questioned the idea that meat and or hunting was one of the main sources of energy for hominoids which puts doubt on the theory that language evolved for hunting as this view at least implies that meat was an important food source for our ancestors. Further, even if meat were an important part of our diet, its possession need not come from hunting but for instance from active or passive scavenging.

Here we will first discuss i) whether meat was important for humans and then ii) whether it was acquired through hunting. After that we discuss the issue of *when* hunting became important and whether the hunt was like a true active hunt, or more like scavenging etc.

i) The principal means of subsistence of many of the remaining hunter-gatherer societies in the world is hunting, these dietary customs may be more the result of forced migration to areas where other food sources are scarce than of preference and thus don't need to reflect historical circumstances. Ragir (2000) indeed thinks it didn't, claiming that hunter gatherers got far more calories from fruit and plants than from meat, and some go so far that meat was only an addition not a requirement at all.<sup>605</sup> This might at least be true for our nearest cousins as chimpanzees only start looking for meat when fruit is abundant (Watts and Mitani 2002). However, in an extensive meta-study Kaplan et al. (2000) contend this, reviewing a lot of work that clearly shows that hunter-gatherers received much more calories from eating meat (by almost a factor of 2:1), also tracing in the literature where the view came from that meat-eating were unimportant to hunter-gatherers (supposedly only one study of a currently living hunter-gatherer tribe that almost exclusively focused on foraging by Lee 1979).

ii) Jane Goodall (1963) first discovered that chimpanzees hunt (contrary to the then current image of chimpanzees as friendly tree dwelling vegetarians), and more recently it was found that orangitans hunt too, although less frequently and less systematically than chimpanzees whose style of hunting (in modest, almost exclusively male groups) gives the impression of being pre-planned and well-coordinated (Mitani and Watts 1999) with a success rate per complete hunt exceeding 50%, and as such comparing favourably to human hunting prowess (Mitani and Watts 2001).<sup>606</sup> However, despite the high success, chimpanzees do not engage

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<sup>605</sup> And interestingly Ragir 2001 adds to this the idea that language did not arise for hunting but for gathering as gathering by current hunter-gatherer societies requires an almost encyclopedic knowledge of thousands of edible and non-edible plants compared to the more limited knowledge required for hunting. The main supporters of the dual-inheritance view also linked the origin of cultural evolution (the transmission of cultural knowledge and thus language) to the transmission of knowledge of food sources in an environment like the savannah of our ancestors were (unlike in the world of other primates) food was distributed in many patches and survival depended upon knowledge of the resourcefulness of these patches. See also footnote 419 where we have discussed this view more extensively.

<sup>606</sup> Domínguez Rodrigo 2002 further claims that chimpanzees that live close to the savannah hunt more than their cousins residing deeper in the rain forest; the savannah is of course also the territory of our hominoid ancestors.

in hunting often. That is why some view the function of chimpanzee hunting not to be food acquisition, but *male bonding* and coalition building (Stanford 1998, Mitani and Watts 2001) with some speculating whether early human hunting had a similar function (O'Connell et al. 2002).<sup>607</sup> If so and if language arose during the hunt, we can question whether it supported the hunting process itself or the supposed primary function: *male bonding*. We discuss this option below.

Although there is currently little controversy that meat was important to our hominoid ancestors, it is still uncertain i) *when* meat became such an important source of subsistence and ii) whether all was obtained by hunting instead of active or passive scavenging (i.e. chasing predators away or being satisfied with the leftovers).<sup>608</sup>

i) The different clues for meat-eating come from the paleontological record: teeth characteristics and isotopic evidence indicating changed dietary habits and the archeological record, primarily bones with cut marks from stone tools, the oldest of which are thought to be 2.5 million years old and which start to be common around 2 million years ago (O'Connell et al. 2002, Domínguez Rodrigo 2000); as this time period does not coincide with the period of the first more or less clear clues of language use this pleads against the hypothesis of language as adaptation for hunting, although it does partly overlap with the early evolution of larger brains (which are thought to be fundamental for language) and with the emergence of the first stone tools in general (see the timeline in §3.22); note that these clues for meat-eating can be both clues for scavenging (O'Connell et al. 2002) and hunting (Domínguez Rodrigo 1997), which brings us to the second issue.

ii) Chimpanzees do occasionally scavenge (Mitani and Watts 1999, Domínguez Rodrigo 2002), although their meat consumption comes mainly from active hunting which pleads for humans hunting too. Further evidence pro hunting against at least passive scavenging is that unlike jackals we get sick if we eat rotting meat as our digestive system is not adapted to breaking down carrion lacking the relevant enzymes and bacteria (Ragir 2001); if scavenging were important, this would almost certainly have been the case. Further, active scavenging might be even more dangerous than hunting (think of killing deer versus chasing a lion a way who has just killed some game), that's why hunting might be more probable too (Domínguez Rodrigo 2002).

2) Is language actually used during the hunt?

Now it is ascertained that our ancestors hunt for meat, a further question is whether they used language during the hunt. As said, chimpanzees hunt in a coordinated way, yet their hunt seems almost silent or to involve only minor signs.<sup>609</sup> Why would the human hunt be any different? Or if it would require rudimentary signing, why would some of the chimpanzee grunts not suffice?

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<sup>607</sup> An even further speculation in similar vein is made by Stanford 1999 who argues that language might have arisen to coordinate meat-sharing in the group rather than as adaptation for hunting proper. As such it is a variant of the general thesis that language arose out of a social need which we discuss under 4) below, and so does Quiatt's idea that language arose in relation to the division of labour between man (hunter) and woman (gatherer) in hunter-gatherer societies as this division supposedly required language to coordinate efforts. The advantage of Quiatt's idea is that it naturally assumes that both males and females are subject to selection on language skills.

<sup>608</sup> For a good overview of the different clues in the fossil and archeological record about hunting (teeth changes, bones with cut marks from stone tools etc.) see chapter 5 of Lewin and Foley's *Principles of human evolution*.

<sup>609</sup> Unless of course chimpanzees use signs we are as yet unaware of.



What further pleads against language use during the hunt is that signs can either be heard (in case of vocal language) or seen (in case of gesture language), thus giving away the location of the hunter to the prey.<sup>610</sup> Only for setting up a trap would language thus be really useful, but than language use would better be explained as adaptation for tool use.

3) For what is language used during the hunt?

A further problem is that even *if* language is used during the hunt, it need not be for the hunt as such (setting up a trap etc.), but it could also be used for social grooming – just as possibly in the case of chimpanzee hunting.

4) Finally, even if humans used language *during* the hunt *for* the hunt, does it really prove that language evolved for the coordination of hunting?

Language could already have evolved before hunting became a major source of subsistence; as language was then commonly used for all sorts of tasks (just as in our time), language would also be used routinely for hunting without of course having evolved for hunting. We saw, however, that language probably evolved much later, so that this is an unlikely option. But even if we suppose that (proto-)language evolved during this period it needn't even have evolved for hunting or even in direct relation to hunting (as the meat-sharing or labour-division hypothesis still presuppose, see 607). Language could for instance have evolved for tool use in general, a scenario to which we turn now.

In sum: between more than 2.5 million years and 2 million years ago major dietary shifts towards meat eating have occurred in the hominoid lineage, most likely due to hunting. This time frame does not coincide with the evolution of language which is usually dated 0.5 million years ago. Thus language cannot directly have evolved for the hunt. Meat eating may, however, be responsible for the growth of our brain, which in turn may be responsible for more intelligent hunting strategies so that a positive feedback loop between hunting and bigger brains was formed; the bigger brains are an important exaptation for language, and thus indirectly and from a backward looking perspective eating meat and hunting may have paved the way for language as important exaptations for language use.

## **b) Tool use**

We can ask a similar set of questions about tool use as about hunting. 1) When did tool use begin and does this overlap with the likely origin of language? 2) What is the supposed (evolutionary) relation between language and tool use?

1) When did tool use start?

This question is hard to answer as many other animals than humans use tools: elephants, for instance, have been observed digging holes to drink water and then ripping bark from a tree, chewing it into the shape of a ball, filling in the hole and covering it with sand to avoid evaporation; the elephants later went back to this spot for a drink (Holdrege 2001). However, how intentional was this instance of tool use and was it a onetime lucky shot, or did elephants dig and cover holes more often?

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<sup>610</sup> Unless voices were used during the hunt to scare off animals into traps, but in that case simple grunts would again suffice and even relatively simple language need not evolve.

Some criteria for real tool use thus seem to be the intentionality<sup>611</sup> and repetition of the tool use. The elephants' use of branches to swat flies or scratch themselves is thus a better example of tool use than their digging water holes. Similar repeated and intentional tool use has been observed in animals closer to us on the evolutionary scale in all the great apes and some monkeys.

More complex tool use also involves tool making, although sometimes the difference is hard to tell and the increase in complexity hard to discern: an Egyptian vulture who carefully searches for a good medium-sized rock to throw it at an egg to open is more complex than a chimpanzee randomly braking off a branch to tease a fellow chimp, although the latter looks more like tool making. Amongst our primate relatives, tool making has only been reported for bonobos (who have even been taught to make tools, see McNeil 1996, Savage-Rumbaugh & Lewin 1994) and chimpanzees (see the overview by Boesch and Boesch 1990). Chimpanzees have even been observed to use tool-composites: multiple tools in conjunction to achieve a certain goal (Sugiyama 1997), or to systematically adapt a tool to a task at hand, for instance shaping rods for ant-dipping (Humble and Matsuzawa 2002) or sharpening sticks into spears (Pruetz and Bertolani 2007).

Human tool use is thus not exceptional amongst primates. However, tools are usually thought to have played a major role in human evolution (Ambrose 2001, Schick and Toth 1993). Human hand anatomy underscores that; whereas chimpanzee hands are a compromise between the demands of quadrupetal walking, tree climbing and object manipulation, human hands are well fit for tool manipulation: the thumb together with the index finger allow a firm grip to pick up tools. It is, however, uncertain whether the hands evolved explicitly for tool manipulation or whether hominoid hand anatomy is simply due "to relaxed selection for tree climbing and knuckle walking (Johansson 2005: 200)."

The tools have usually been thought to be used primarily for hunting and/or as weapons, although other animals usually use tools for other tasks such as gathering, egg or nut cracking, scratching, 'nest-building' etc. (even chimpanzees who have been observed to hunt sometimes using spears, normally use tools for other functions).<sup>612</sup> Support for the hunting hypothesis comes from the fact that the first recognizable stone tools are dated some 2.5 million years ago or more (Semaw 2000, Semaw et al. 1997, 2003), more or less the same time as the hominoid change in diet towards meat eating (although tool use might have been older as non-stone tools such as the wooden sticks chimpanzees use usually are not preserved in the archeological record).<sup>613</sup>

The stone tools themselves are the subject of controversy: whether they remain more or less the same over time or whether they change (progressively); a major distinction is at least between Oldowan tools (simple sharpened stones for all sorts of purposes) used from 2.5 million years ago onward and Acheulean tools (more complex, consistently shaped hand axes) used from 1.5 million years ago onward (Lewin and Foley 2004, chapter 6). The distinction is not an absolute chronological one: the old Oldowan tools were still in use in Europe as late as less than a million

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<sup>611</sup> Compare Marc Hauser's definition of a tool: "An object that has been modified to fit a purpose or an inanimate object that one uses or modifies in some way to cause a change in the environment, thereby facilitating ones achievement of a target goal (Hauser 2001: 83)."

<sup>612</sup> See for instance Backwell and d'Errico 2001 who discuss early stone tools related to 'ant-fishing'.

<sup>613</sup> Some of the evolution in hominoid hand anatomy occurred well before the stone tools appear in the archeological record in the context of the *australopithecines* (Alba et al. 2003), which could indicate non-stone predecessors of later stone tools.

years ago, although elsewhere they had long been replaced by Acheulean tools (Roebroeks 2001). More recent studies, taking into account multiple sites from many different locations tend to favour the view of gradual evolution (Lewin and Foley 2004), both of Oldowan tools (De la Torre et al. 2003 and Kimura 2002) and Acheulean tools (Wynn 2002, Klein 2000). The transition from the older type to the newer type is nevertheless sometimes seen as a major innovation or cognitive breakthrough, even thought to be related to the origin of language (Aiello 1998), but there is no reason to regard it as a breakthrough of its own (if it really is so sudden) rather than as related to language.

## 2) The relation between tool use and language

Few authors have invoked (complex) tool making as the driving force behind the evolution of language (Gibson 1990): tool use, which enhances survival, requires coordination or communication. However, tool use and tool making is a solitary activity which hardly seems to require language: stone tools were made by flint knapping and this is a silent activity too, even among tribes that until recently lived in Stone Age conditions (Dunbar 2003). A more indirect path of the evolution of language as adaptation for tool use has also been proposed: complex tool making requires intelligent planning and this requires language, tools helped humans to survive and thus there existed a selection pressure on language skills.<sup>614</sup> In similar vein (complex) tool is thought to involve multiple, hierarchical steps (first sharpen the point of a wooden rod, create a hole in a piece of stone, add glue to the rod or tie the stone to it, whereby the first to steps can be interchanged with each other, but not with the third etc.). Now combining multiple elements into composite tools is a sequential and combinatorial activity and this is characteristic of language too; hence we find Ambrose (2001), building upon earlier work by Greenfield (1991) and others, arguing that tool making co-evolved with syntactical language as both are based upon the ability to perform sequential and combinatorial activities. Although it is argued that the abilities to build composite tools evolved first, providing the perfect exaptation for language (Wildgen 2004). However, a loose analogy between combinatorial activities seems hardly sufficient to claim any relation between the evolution of language and tool use (with either one in the role of cause or effect): finding one's way back into the forest home is also a combinatorial activity (first left, then right, then straight-ahead and then right again, not in any other combination otherwise you end up in the wrong place), but no one would claim that being able to orientate and finding one's way back has anything to do with speaking a language (if so, insects, birds and many other animals would then be likely to speak a language), or that a language speaker is necessarily able to find its way back for that matter.

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<sup>614</sup> Others only relate the clear *acceleration* of the *pace* of technological change and innovation (which, as we just saw, occurred much later than the first use of tools millions of years ago) to the (sudden) acquisition of language and or the cognitive breakthrough that caused or was caused by language evolution (Mellars 1998, Donald 1997, Diamond 1991). However in this scenario there is no selection on language skills *for* tool use; language is already in place, enabling the exchange of technological expertise which leads to an *acceleration* of the *pace* of technological change and innovation. Thus in this scenario (which as such is unproblematic) tool use is of little importance in understanding the evolution of language.

Note, however, that these scenarios often include the idea that the archeological record shows that technological artifacts suddenly increased in complexity and that the reason for that is the sudden acquisition of language; these assumptions might be wrong: technological changes (from a non-Eurocentric perspective) were much more gradual and so is probably language evolution (see §3.21). Moreover, relating technological changes (in the Neolithic period) to sudden language acquisition probably places language evolution in the wrong time period (id.).

Others envision the relation between tool use and language to be more indirect: for instance, tools were used for hunting which required language, with a possible positive feedback loop of hunting to energy rich diet to bigger brains to better tools and hunting etc. (but then the explanation of the origin of language would be better characterized as falling under a) language as adaption for hunting); still others claim that the transmission of the knowledge involved in tool use required language, which would relate the origin of language more to teaching than to tool use, an option we discuss below; however, what pleads against the idea that language evolved for instructions about tool use is that teaching involving tool use is usually then by demonstrating instead of verbal instruction (Dunbar 2003); it is further a fluid line between explicit demonstrating and overlooking/imitating or doing something because one is placed in the same situation and then 'independently' invents it (see §2.2 on animal culture and its transmission); in such a case of overlooking or accidentally learning to manipulate a tool even less language is required than in the case of explicit demonstration.

In sum: both the moment humans started to use tools and the indirect relation between language and tool use make language an unlikely adaptation for tool use; from the time onward language started to evolve, tool evolution could, however, have sped up technological evolution, through instruction, teaching etc. which we will discuss now.

**c) Child care and teaching:** much of our knowledge comes from teaching and without this knowledge we would not survive; language is the one of the principal means for teaching, thus it seems not unreasonable to propose that teaching is the driving force behind language as King (1996) does. We will first 1) state some facts about child care, child development and when it occurred, 2) then we will review the suggestion of King and others that language evolved for teaching.

#### 1) Child care, child development and learning

Primates are clearly the product of K-selection (see §1.32): primate babies mature slowly and require parental care for an extended period, often several years; during these years primates learn a lot about their environment, food gathering, climbing etc., but most or at least a great deal of the learning concerns living in a social, hierarchical group (Joffe 1997).

In humans the effects of K-selection are even stronger: human babies are even more immature and helpless at birth than other primate babies, requiring intensive parental care for many years which seriously restricts the activities parents can employ; the childhood period is also even further extended with humans taking some 13 years to become physically mature and even more to become socially mature, a period during which children have to learn a lot; we already saw that language learning is not finished until the early teens (§3.24).

It is sometimes claimed that human are born premature, but that view is incorrect if one does not define what premature means, since the gestation time for mammals of our size is normal (compare the multivariate allometric analysis by Sacher and Staffeldt 1974). This gestation time is principally determined by an infant's brain size at birth (either due to constraints on the possible oxygen delivery from the mother to the infant brain or because of the small diameter of the birth canal through the pelvis, which cannot be increased much without causing inefficient bipedal running, Byrne 2000), and the human infant brain at birth looks much like a chimpanzee infant brain. However, unlike the chimpanzee the human infant brain

has a lot of extra-utero growing to do; thus a human baby can only be called ‘born premature’ from a *physical* point of view if one compares its gestation time to its *adult brain weight*.<sup>615</sup> In addition, the infant human brain is possibly also more plastid and less pre-programmed than the infant chimp brain.

In §3.21 we already discussed the evolutionary hypotheses that try to account for human and primate brain growth; it is important to distinguish the overall evolutionary trend towards bigger brains in the primate lineage from the extra brain growth of humans compared to other primates which occurred some 800.000 years ago.<sup>616</sup> The evolutionary hypotheses that postulate an adaptive reason for this growth (hypotheses we considered much more plausible given the huge costs of brain tissue) include knowledge of food types for gathering, increased vision for gathering, tool making and social ‘Machiavellian’ intelligence (the one we deemed most succesful). Some of these would certainly profit from the ability to learn, or more specifically from verbal instruction, but not al of them. Heightened vision is not related to language skills, and tool making (which as we just saw is often taught by demonstration not instruction) not strongly either. The transmission of knowledge used in food gathering and social knowledge on the other hand could certainly profit from language skills. It seems clear that hunter-gatherer tribes are socially much more complex than chimpanzee or other primate societies, and their diet is also broader; the extended juvenile period in humans (as well as that of primates for that matter) seems also largely spend on learning.

It is at least certain that active teaching of children, by parents and others, is ubiquitous among humans across all cultures studied (Tomasello 1999b); for primates on the other hand is there no solid experimental evidence of deliberate (and systematic) teaching (Tomasello 2000c), although there exists lots of anecdotal evidence of it (King 1996, Savage-Rumbaugh 1994). To assess the anecdotal evidence one should always ask whether the cultural transmission could not easily be due to emulation or accidental learning (for instance because of being in the same location), instead of due to explicit teaching, let alone instruction by linguistic means (see our discussion of animal culture in §3.21); if a more simple process could account for the transmission, it is probably the right one.

## 2) Language driving the evolution of teaching

It is thus likely that (explicit) teaching is typical of humans; teaching would certainly profit from language, even though many forms of instruction do not necessarily require language. These facts seem to give the teaching/child care hypothesis of the origin of language some plausibility. A further advantage of the scenario that language evolved for teaching is that whereas the hunting and tool use hypothesis would restrict language use more or less to one gender, the teaching hypothesis doesn’t.

However, scenarios of language that claim language evolved for teaching (King 1996) step all too easily into the teleological pitfall: language cannot have evolved *so that* we could *start* to teach children; instead teaching must have been

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<sup>615</sup> That’s why human babies look morphologically more similar to chimpanzee babies than adult humans compared to adult chimpanzees.

<sup>616</sup> See timeline in §2.32. The date of 800.000 years ago is a matter of huge controversy, not in the least as the fossil findings of infant and child hominoids are both scarce and hard to interpret (especially when fossils have to be compared with existing human variation of brain sizes). The date of 800.000 years ago would mean that *Neanderthals* and *Homo sapiens* had a prolonged growth, but earlier hominoids such as *Homo erectus* not. See for an overview of the issues and fossil findings involved in the controversy, Lewin and Foley 2004, chapter 8.

present before selection on better communication and language skills became important.<sup>617</sup> Thus either language has evolved for some other function than teaching (contra what the theory claims) after which it could well be used for teaching, or teaching was already well under way *before* language was used for it.

We now turn to a different group of scenarios that claim the first thing: language evolved in social groups in close connection to the evolution of social relations, after which it could be used or exapted for many other things, including teaching (social) knowledge.

#### **d) Language and social group relations**

In §3.21 we already discussed that the most likely reason for the evolution of bigger brains was the evolution of social intelligence. Although other primates already live in highly social groups, human social groups are much larger and complex and thus demanding more cognitive capacity to handle than the social life in other primate groups already does. One of the key advantages of the idea that brains evolved for social intelligence is that it can easily explain the trend towards *ever bigger*, and thus more expensive brains once brains started getting bigger: the evolution of social intelligence can set off an inner-group arms race for even more clever politics, for instance in a Machiavellian fashion (Dunbar 1998; 1996) or in such a way that asocial free-riders who try to profit from group life without paying its costs are punished.

In this section we will first briefly review  $\alpha$ ) the basic aspects of the evolution of primate group life and group size, and  $\beta$ ) after that their relation with language use.

#### **$\alpha$ ) the evolution primate group life and group size**

We mention five key processes involved in (primate) group life evolution and their different, often opposing effects on group size (Krause and Ruxton 2002; Zemel and Lubin 1995):

1) external predation: bigger groups can offer defense against predators, although a big group may also attract more predators than a small one would. There are thus conflicting external forces working upon the ideal (primate) group size. The optimal group size further depends on predator characteristics. Unfortunately, there exists conflicting empirical data on the optimal group size of primates, also dependent on what type of predator in what territory a certain primate species faces: some claim that heavy predated primates tend to live in bigger groups (Heesy 2000), while others claim the opposite (Stanford 2002). For hominoids, however, the defense argument is most prominent in the literature, thus favouring *larger groups* (Lewin and Foley 2004; Aiello 1998, Dunbar 1996).

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<sup>617</sup> Another hypothesis that suffers from similar teleological pitfalls is proposed by Foster (1991), who is very negative about evolutionary biology in general; Foster claims that language evolved in the context of play with language not used for communication in the narrow sense of the word, but for mimicry and random repetitive playing. However, Foster does not tell us how this initial stage of mimicry and playing contributed to our survival and thus how it could have evolved in the first place, even if playing would later turn out to be an excellent exaptation for language.

Knight (2000) and Ragir (2001) take this last possibility more seriously; in their view, especially the creative and combinatorial aspects of playing make it fit as an exaptation for language. They also add a storyline of why playing is evolutionary advantageous. However, as we noted earlier as we discussed the combinatorial and sequential aspects of tool making: analogous combinatorial and sequential elements of a task are not sufficient ground for postulating a shared evolutionary process or more general adaptation behind it. It could be the case, though, that mimesis or imitation in child play were important for language evolution, if a plausible function (different from later language use) could be found for it.

2) inter-group competition for resources: in §1.23 we discussed the possibility of multi-level selection, with most controversially selection at the level of the group. Selection at this level means that groups of animals face other groups of animals in the struggle for survival. Although the level of mixing between groups is usually so strong in humans and other primates that true group selection can be ruled out, we do find that competition between groups can be an important factor affecting optimal group size (with a (selective) advantage to *individuals* that happen to life within these groups): humans and chimpanzees (Williams et al. 2004) maintain territories and defend them against groups of conspecifics. Large groups will easily win from smaller groups; however, too large groups would soon exhaust the resources of a certain territory which poses a limit upon the size of the group (unless perhaps as the group as a whole would migrate or travel continuously as some groups of female chimpanzees (Koenig 2002) and of course some human nomadic groups do); smaller groups that inhabit difficult to find niches are then at an advantage. Thus although large groups may have been advantageous and result in more territory per group member (Williams et al. 2004), there is a clear natural limit on group size from the perspective of inter-group competition. For humans, bigger groups seem favoured (think of the rise of large agricultural societies contra small groups of nomadic tribes in resource-poor environments since the Neolithic period).

3) intra- or inner-group competition for resources: within a group there are two opposing forces: a larger number of food seekers/hunters increases the chance of providing food for the whole group over the long run, especially when food occurs in rare but rich patches (such as ripe fruit trees) or when hunting collectively is more successful than hunting individually. In larger groups, however, the competition for food by one's own group members increases strongly (especially as group members are often close to each other during eating opportunities), decreasing food availability for low-status member of a group. Empirical results on the relation of group size and eating opportunities are scarce; Matsumoto-Oda and Hosaka 1998 have shown that chimpanzee group size (which is quite variable in the wild) is to a considerable extent correlated with local food availability. It would be interesting to see whether the same holds true for humans in hunter-gatherer societies.

4) intra-group competition for mating opportunities: since the rise of evolutionary psychology it has become sort of a cliché to say that males and females differ in reproductive strategies, given the generally larger variance in reproductive success for males than for females, which is reflected in the fact of fiercer competition for mates among males than among females. Males profit from living in a group insofar as that brings them closer to the opposite sex, although it also increases chances of conflict with other males; it is therefore, as we saw, not uncommon amongst primates to find big harems with one male at the top of a group of females and with other males living solitary outside of the group either looking for secret mating opportunities or for the time they can be on top of the harem. On the other hand, females may want to join a group to protect themselves (as a group of females) from sexual predation and infanticide (with fewer males being an advantage). It is not directly clear what all these trends would imply for human group size with semi-monomogamous couples within bigger groups in hunter-gatherer societies.

5) intra-group aggression and politics: group life has thus advantages as it protects from predators and can give access to food and mates; however, group life also increases conflicts over food and mates. Conflicts lead to aggression, but aggression is costly, thus groups are in need of a means to downplay aggression and resolve conflicts. The easiest way of doing this would be that everybody in the group

behaved calmly and non-aggressively. However, this would not work since when an aggressive individual nonetheless pops up in such a population, he would exploit all others (and then the aggressive variant would spread and the population would be faced again with the same dilemma of controlling violence). A real solution to this dilemma lies in the evolution of social intelligence, starting with being able to identify individual animals as individuals and remembering past interactions with each individual so that one could differentiate behaviour on the basis of past interactions (opting for tit for tat strategies etc., see §1.23). But in a large group, this possibility is soon limited by the cognitive powers of most animals. For social evolution to evolve, then either i) the group size has to be relatively small, or ii) the social system has to be relatively simple (Dunbar 1996).

Both solutions are found in the primate lineage i) primate groups are relatively small compared to human groups, and ii) there is a slight trend of less intelligent primates living in smaller, stabler groups.

i) the smallest groups are usually kin groups: help your brothers and other close family. Even in human populations kin groups are of course still highly important subgroups. Kin selection (gene based or not) can be invoked to explain the clustering of groups by kin. A general finding is that kin groups are more important in the so-called lesser primates, than in the higher primates: though not totally unknown among for instance baboons (Byrne 2000), non-kin groups are far more common among chimpanzees than among baboons.<sup>618</sup>

ii) kin groups are also stable groups: the identity of your blood brothers does not change. As such permanent groups, they are easy to keep track of, limiting cognitive demands. A little bit more complex system is the strict dominance hierarchy we also find among many primates: you should only know whom you should obey and whom should obey you and then you can apply simple transitivity rules: if A has to obey B and I have to obey A, I should also obey B. The dominance system becomes soon complex when simple transitivity is lost and dominance relations change over time; it then pays to keep track of all the power relations of all those around you and bid for a higher status at the right moment.

What really puts stress on cognitive demands, however, is when one-to-one (static or variable) dominance relationships are abandoned in favour of relations based on status and friendship or even more opportunistic coalitions. In such a case you need not just know your own position in the dominance hierarchy and that of those close to you: you need to monitor who hangs around with whom, who is friend of B, who will protect C if I attack him, who can be my friends. This mechanism can set off an inter-species “cognitive arms race” (Dunbar 1996) about who masters “social interplay and manipulation” best. In §3.21 we already argued that the antagonistic evolution of social intelligence is the most probable explanation for the big brains of humans and other primates: we don’t need big brains to face the external (natural) environment, we only need it to cope with our fellow-beings in the social environment.

A high level of social intelligence and complex group formation processes already evolved in the common ancestors of humans and chimpanzees as chimpanzees already live in highly complex societies with non-stable, not-fully hierarchical non-kin groups (De Waal 2000). Both humans and chimpanzees further spend a lot of their daily time on building and maintaining relations as well as

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<sup>618</sup> This fact was empirically confirmed by DNA analysis of chimpanzee group mates. See Goldberg and Wrangham 1997.



monitoring and judging the behaviour of others.<sup>619</sup> These facts allow us to infer that our common ancestor has been the subject of selection for living in groups, possibly as defense against predators or for territorial gains and better food access, which in turn led to selection for cognition. Humans have even bigger brains and higher social intelligence than chimpanzees. If we extrapolate the trend we might conclude that this evolved due to stronger pressures on living in groups which in turn may be due to stronger selection pressure on safety against predators and the need for stronger territorial claims, c.q. more reliable food access, even though the social intelligence is not used primarily in the context of predator defense etc. but to outwit one's fellows (which ultimately in the struggle for survival is of course nonetheless about access to resources and mates). The idea that there was even stronger selection pressure on predator defense etc. is at least in line with the hominoid change in habitat, from tight forests towards the much more open savannah habitat where predator defense, territorial claims and the 'semi-coordinated' food gathering (food which often is located in patches) as well as hunting even more crucial than in the old habitat of our common ancestor with the chimpanzees (Dunbar 1996).

Combining these 5 trends, it seems likely that there has been selection on living in bigger groups in the hominoid lineage (be it for predator defense, food gathering) and that living in bigger groups required more social intelligence to keep the group together, but will also likely offset an innergroup competition for better social skills. Indeed, it is found that humans on average tend to live in bigger groups than chimpanzees: the largest chimpanzee group ever recorded consisted of less than 120 individuals, including children, whereas average hunter-gatherer societies easily comprise far more.<sup>620</sup> Finally, this idea is also buttressed by the observation that among primates neocortex size is positively correlated with group size and social network size within groups (Kudo and Dunbar 2001).

## **β) Socio-politics and language**

There are multiple theories that relate socio-politics to language skills, some highly general, others much more specific. We discuss them from highly general scenarios to more specific ones.

1) Communication: the most general theory holds that politics is about communication (agreements, formation of alliances, coalition building, trading of favours, outlining a common strategy, making conditional threats etc.). A better communicator would be a better politician, with higher status and thus better reproductive chances, hence leading to selection pressure on better communication skills (Hermann et al. 2007). Communication itself would profit a lot from language as a highly efficient form of communication and thus selection on political skills leads to selection for language skills (Dessalles 2000). One can object to this view by claiming that proto-language must have started with a tiny vocabulary, which is

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<sup>619</sup> Dunbar 1996 went as far to test empirically what people were talking about by secretly overhearing their conversations at rail road station, restaurants etc. According to him, about 66% of all conversations (by both males and females) were about 'gossiping', chats about interpersonal issues (where politics and sports did not even count as gossiping). This number may be eschewed by the places Dunbar chose for observing his experimentees; it remains a remarkable finding nonetheless which supports the idea that language evolved for 'gossiping': social communication about ourselves and others (if we can equate current function with the function for which language evolved).

<sup>620</sup> Of course we sometimes find smaller groups of humans or even solitary living ones, but so do we in case of the chimpanzees.

supposedly insufficient for social manipulation. However, chimpanzees are able to do social manipulation with even less signals, and the fact that chimpanzees *actually use* signs for social manipulation and inner-group politics (De Waal 2000) is an extra argument in favour of the view that language evolved for socio-politics.

2) Common visions: Gärdenfors proposes that a key stage in the evolution of humans and human group life in particular was inspirational leadership: “creating common visions” (Gärdenfors 2003). Although, Gärdenfors acknowledges that social cognition in general can have evolved in a competitive setting, at this key stage social intelligence was put to a cooperative use for the benefit of all. Whereas chimpanzees can act like rational economists (Jensen, Call and Tomasello 2007a), they cannot willingly do something *pro bono*, and they can be vengeful but not spiteful as humans can. Some now regard language as a derivative of these extended cooperative and social skills (Moll and Tomasello 2007, Tomasello et al. 2005). However, although it cannot be denied that humans can act in the way Gärdenfors proposes, one has to be extremely careful in interpreting this phenomenon within an evolutionary framework, let alone use it in a scenario for language evolution. This is not the place to review the enormous body of literature on the evolution of altruism or cooperation (see §1.23); from a biological point of view,<sup>621</sup> somehow the cooperation should be advantageous at one level of *selection* or another, be it the group, the individual or the gene. Gärdenfors should first make clear how creating common visions helps groups (compared to other groups), and how it is robust against corruption from within (with people benefitting from the implementation of the common vision without contributing to it). Others like Tomasello want to relate the origin of language to the evolution of social altruism need to rethink this issue first as well, especially as other socio-political hypotheses about the origin of language are available in which it is much less controversial who profits from the evolution of the costly language skills.

3) Status: in the first scenario language evolved as a communication tool for socio-politics with the better politician also being the best speaker; this politician had more status and hence more offspring. A variant of this theory is that females recognized the better politician by his language skills so that they become the object of true sexual selection, with “talking individuals engaging in a competitive display to advertise their informational abilities (Dessalles 2004: 1).” Just above we discounted sexual selection as the *whole and sole* force behind the evolution of language, but here sexual selection acts as a supporting force driving the evolution of language even further than natural selection for socio-politics already does. As such, it is much more plausible than the stand-alone version of language evolution as a process of ornamental selection; however, it suffers from one similar problem in explaining the evolution of language as the general theory of ornamental selection: it does not explain why both males and females evolved language skills to (more or less) the same extent.

4) Grooming: the most original version of the socio-political hypothesis is the verbal grooming hypothesis (Dunbar 1996). Dunbar observes that to maintain friendships and alliances within a group (and thus the group integrity as a whole) one has to spend time with each other for bonding; primates primarily do this by

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<sup>621</sup> Of course one can also take a non-biological point of view arguing that with ultimately non-selfish cooperation humans have transcended biology and natural evolution; however, such a point of view would then still have the burden how humans could have transcended biology (which often leads to substance dualism as the biological brain is also subject to natural evolution and thus the transcendence should have come from elsewhere) and how this transcendence has been maintained.

grooming each other, a time costly one-to-one activity (as an ape cannot groom more than one ape at the same time). Dunbar now calculates, on the basis of a linear extrapolation<sup>622</sup> of group size and network size within the group, how long humans would have to groom each other to maintain friendships and alliances (and thus indirectly the integrity of the whole group) in their much bigger groups. According to him, humans would have to spend up to 12 hours a day of grooming, thus hardly leaving time for any other activity. However, if this activity would not be performed, the group would fall apart according to Dunbar; but if the group fell apart the individuals would face big survival challenges since we just saw that there existed a strong selection for big groups in the savannah territory. Dunbar now proposes that language is a more time effective way of maintaining friendships and alliances (and thus group integrity); as such language use is thus some sort of ‘verbal grooming’.

Verbal grooming has many advantages compared to normal grooming: i) one can direct words at much more people than one at once, ii) it doesn’t have to be performed one-on-one: a group of people can gossip simultaneously and get bonded in a small fraction of the time it would take them to groom each other in all pairs combinations,<sup>623</sup> and iii) during chatting the individuals that bond almost automatically exchange valuable information about others.

According to Dunbar his theory also fits well with his observation that humans use speech mainly for gossiping (see footnote 619). A serious objection to Dunbar’s view, however, is that “verbal grooming” may also be achieved, as Bickerton (2003) puts it, “by pleasant, but otherwise meaningless sounds (quoted by Johansson 2005: 213).” Dunbar’s theory thus needs to add another layer to its plot to explain why the pleasant sounds exchanged became meaningful sounds.<sup>624</sup>

5) Social scripts: Aiello (1998) tries to extend the socio-political hypothesis in a highly speculative direction as he proposes that the natural order of social actions and events provided the script that lies at the basis of syntactic procedures. We come back to this speculation below; the fact that syntactic relations can have many, many forms (regarding both ‘surface’ and ‘deep structure’) speaks against Aiello, as well as that we believe that syntax evolved rather late in language evolution.

If we now look back at all the theories of the evolutionary function of language, we think that Dessalles’s (2000) and Dunbar’s (1996) views that language evolved for socio-politics is our best shot at cracking the puzzle of the evolutionary function of language as their theories match the existing empirical data of neocortex size and growth in relation to group size parameters; it also fits well within the time-frame of human evolution of §3.21, which the theory of hunting and tool use didn’t.<sup>625</sup>

When one combines the idea of language as evolution for social group politics with existing ideas about the change of habitat from woods to savannahs and the resulting selection pressure on group size as well as the change towards a more

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<sup>622</sup> The time needed for grooming is more or less constant per individual groomed, thus insofar this extrapolation seems fair. It can be doubted, however, whether network size within the group has to grow linearly as well with group size.

<sup>623</sup> However, apes might also use transitive information about who is grooming whom (if my friend also grooms you, you must be a friend too) to form effective alliance without the need to groom each other one-on-one.

<sup>624</sup> Of course pleasant sounds can also have a meaning, but it is a far cry from musical and rhythmical sounds to the sounds we use in speaking etc.

<sup>625</sup> In favour of the socio-political theory of language evolution (in whatever form) pleads further that both animals (§3.24) and little children (§3.25) are much better at learning (human) language when it takes place in a social, interactional setting.

energy rich diet on the basis of hunting (Dunbar 1996), one can have the outlines of a more definitive theory of language evolution in the context of hominoid evolution in general, a theory with multiple feedback loops some of which are stronger than others:

As hominoids started to hunt, their diet became richer which allowed bigger brains to evolve that in turn improved hunting techniques which led to even more meat consumption and bigger brains to evolve (the first phase of brain enlargement). As hominoids started to hunt and brains started to grow tools started to become more advanced, which led to better hunting techniques, again bigger brains and in turn better tools. As hominoids started to hunt, their change of habitat required them to live in bigger groups. To maintain the integrity of these groups socio-political skills were needed which caused bigger brains and language to evolve. The existence of language in turn led to better teaching, better tools, better hunting coordination and set off inner-group battle for better social skills. The better teaching, tools and hunting provided a more energy rich diet, which allowed the brain to grow bigger (second phase of brain enlargement) and the inner-group battle for better social skills consumed much of the brain tissue for better social and language skills etc. Hence language evolution is an exaptation of bigger brains and indirectly of hunting, group size and tool use. Although it evolved primarily for socio-politics there are also all sorts of back arrows of language on hunting, tool use and teaching amongst others, so that one cannot say that there is one unique function for which language evolved.

## II. Specific explanations of language use

Thusfar we have more or less pretended that language use is one homogeneous trait for which one evolutionary, adaptive explanation could be given; we did emphasize, however, that language use is a patchwork trait and this implies that in reality we should be able to give adaptive explanations for the different elements of language use (and their maladaptive aspects). The main distinction we can make is between the evolution of i) our language skills and ii) the linguistic forms, although admitted: the boundary separating them is not always crystal clear (when somebody thinks that genetic rules are genetically hard-coded his view of the boundary is vastly different from someone who thinks that almost all aspects of linguistic forms are the result of accumulative cultural evolution). We will first look at language skills because we can be relatively short about them; after that we turn to linguistic forms.

**i) Language skills:** in the previous section we have already seen that there are many levels at which we can look at language skills and many at which we can observe adaptations. We saw that language skills evolved slowly, a long time ago, by natural selection, and probably for *inner-group* politics. This means that language skills are primarily an adaptation at the level of the individual (the inner-group actor), with strong inner-group selection upon individuals that were best able to use language.<sup>626</sup> Nevertheless, also at other levels we find adaptations: the genetic, neurological, phenotypical (both morphological: the dimensions of our mouth, the flexibility of our tongue etc. and behavioural: shared attention, pointing, listening, peacekeeping

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<sup>626</sup> And possibly partly also as an adaptation at the level of the group: there might have existed pressure on bigger groups as a form of protection against predation and ascertainment of food resources, which would have required a means of controlling violence by quick communication and alliance formation within the group; if so, language also evolved due to selection at the level of the group, with some groups using language more successful than other groups..

by arguing etc.). We just discussed many examples of adaptation at these levels. What we haven't discussed, however, is any (possible) adaptive evolution of linguistic forms themselves – a subject to which we turn now.

**ii) Linguistic forms:** the evolution of linguistic forms, from the evolution of proto-language (proto-linguistic forms) to our current multitude of languages (linguistic forms) is a vast subject we have barely touched as yet. This question can be focused at many different levels: that of phonemes, words, word combinations, syntax, whole languages etc. Here we will focus, like Jackendoff (2002) on a number of key levels: a) phonology, b) semantics, c) syntax, d) 'whole' dialects/languages. This means we won't focus on the evolution of rhyme, specialized jargon, compound word formation etc., unless it turns out that they play a pivotal role in the evolution of one of the key stages. These 'lesser' phenomena could still be included into a more complete frame-work in which the key stages supposedly play a major role. We will see that human ontogeny and first language acquisition provide good clues about how a simpler proto-language might look like and how it could become gradually more complex and that the strength of the dual-inheritance theory becomes truly apparent in the discussion of the evolution of linguistic forms.

**a) Phonology:**<sup>627</sup>

There is a huge difference between our vocalizations and those of apes: because of the duality of patterning and also the infinite combinatorial possibilities that result from it we have a much more diverse and complex phonology as well as a larger lexicon than primates; we can also produce more sounds or at least sounds and sound differences that are out of range for primates (although they can also produce some vocalizations that are out of reach for us). The key stages, from the perspective of the linguistic forms, are 1) the enlargement of the number of vowels, consonants or sounds in general that could be used for speech and *especially* 2) the origin of the duality of patterning. Partly these two key stages also depend on the evolution of new skills (creating more sounds, mastering dual patterning), but here we approach the evolution of the skills from the perspective of the evolution of phonological forms.

**1) The enlargement of the number of vowels, consonants or sounds in general:** we saw that apes can easily understand or decode human speech (as they can be trained on pairs of minimally different verbal cues), but with great effort can they make sounds that vaguely resemble human speech; humans themselves can make many sounds used for communication, but not all are used for speech: crying or cries of pain can mean 'comfort me', but these sounds are not considered speech as one key aspect of speech is dual patterning (§3.11): the composition of meaningful words out of meaningless units: the sounds. This difference between general communicative sounds and speech has also been found at the neurological level: the former are not under neurological control, the others are (Deacon 1997), although – like any biological phenomenon – there are in between cases: interjections are often just sounds but they are normally considered part of (dually patterned) speech and Gilles de la Tourette patients lack neurological control of complete (dually patterned) entities such as swearing words yet we still call their words dually patterned. Dual

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<sup>627</sup> The mechanics of speech perception and speech production were already treated in §3.21 and will be excluded here, although there will be some overlap: the origin of certain speech sounds is a consequence of the evolution of advanced or simply new language skills.

patterning made an enormous amount of words possible, which could never have existed if only a (finite) number of sounds were available *without* the possibility of *combining* them into new linguistic entities.

For dual patterning, two sounds would suffice as with any binary coding all words can be coded. However, we currently use much more sounds than 2. That may be for economic reasons: with only 2 sounds strings would soon become very long,<sup>628</sup> and look rather similar so that they will be hard to recognize, especially in speech streams. Another, also likely reason is because of evolutionary lock-in: the dual patterning can only have arisen at a time there was a clear (evolutionary) need for more ‘words’ than sounds available: if a few basic cries had sufficed for human communication, the complex and costly neurological machinery for speech segmentation and other abilities involved in dual patterning would never have evolved; as long as possible or at least for a long time the need for more ‘words’ would most likely have been solved by adding new sound differences to the existing repertoire of meaningful ‘word-sounds’<sup>629</sup> or cries; only when that was no longer possible, could the need be felt for such a thing as dual patterning which seriously enlarges the number of possible words (we come back to this in a minute). Even though dual patterning could then be successful with only a few basic sounds as building blocks for many new word combinations, the fact that quite an extensive range of sounds was used by the time dual patterning evolved, would most likely mean that they kept being used as some sort of historical lock-in (just like the unhandy QWERTY-keyboard, see §1.23).

The evolution of dual patterning could, however, also mean that the number of sounds man could make or which he actually uses in speech is no longer subject to very strong natural selection, as with dual patterning not that many sounds were needed. Any trait that is no longer strongly selected for tends to weaken, just like the small hands of the once land-dweller, the whale, have shrunken down to useless miniature stumps. This might in principle mean that humans in the past could make sounds they nowadays can’t. We do find this implausible however, because biological skills are rather conservative and although humans can’t produce the subtleness of the sounds of languages which are not their native ones, as babies they can in principle learn the sounds of all languages equally well.

The existence of dual patterning does mean that not all sounds humans can make need to be used in a language. We therefore find that the *number* of sounds used in a language as well as the *actual sounds used* varies a lot. It also means that people who are severely speech handicapped and are somehow unable to pronounce certain sounds can find workarounds to making themselves understood with only a slight problem to their listeners. This again demonstrates that speech is possible with less than full human equipment.

On the origin of human sounds not much theoretical work exists. We can distinguish at least two phases: the origin of i) the basis sounds and ii) the

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<sup>628</sup> Average humans know somewhere between 5,000 and 500,000 words, depending on counting compound words, words in different languages etc. Even if we would estimate the number of words of early humans to be 500 (which compares well with the 25+ sounds and gestures chimpanzees are said to make in the wild and which is still much less than the number of words/lexigrams Kanzi understands), using binary dual patterning would make half the words already 8 sounds long (as  $2^9 = 512$ ), if there were no further constraint on production of sound or syllabic combinations (CVCV etc.), which seems highly unlikely.

<sup>629</sup> We use the word ‘word-sounds’ to describe sounds that function like a word today: as a unity with a meaning in a stage when dual patterning had not yet evolved.

phonological ‘system’ (the basic structure in which sounds are related to other sounds).

i) The origin of basic sounds: there exist two basic problems for how speech got started; 1) we saw that speech has to start before there can be a selection on our language organs so that it is hard to imagine what the first sounds must have looked like and, 2) almost all the sounds we use today require language organs which chimps and early hominoids lack. MacNeilage (1998, 1994) has formulated an interesting hypothesis that addresses both issues. Before we could speak in the modern sense of the word, which requires the accurate neurological control of our tongue, lips etc., the neural control of jaw motions was available as an exaptation, since we need this control to chew and bite and our primate cousins also use it for lip-smacking. From this control of the jaw motions and biting/lip-smacking, it is a small step to babbling which mainly involves a steady airflow from the lungs.

MacNeilage imagines that the first sounds correspond to the simple up-down jaw motions with the tongue fixed in three different places. This gives rise to three ‘syllables’ or ‘CV-clusters’: coronal + front (e.g., te-te-te), labial + central (ba-ba-ba) and dorsal + back (go-go-go).<sup>630</sup> Normally humans produce these syllables by moving the tongue rather than the jaws, but they can just as well be produced in this fashion and when neurological control of the tongue has not yet evolved, it is the only way these can be produced. Note that the first sounds are thus not isolated sounds, but consonant-vowel *clusters* which are easy to make. Only much later would Darwinian selection upon the vocal tract and neural control of speech organs produce crisp ‘individual’ sounds that could freely be combined in multiple CV-clusters or syllables.

MacNeilage further supports his hypothesis by three facts: the (supposed) presence of the CV-clusters in 1) infant babbling, 2) all human languages, 3) Ruhlen’s proto-world vocabulary as recreated from the roots of all known language-families in the world (Ruhlen 1994). However, all three can be questioned. Almost everyone believes that languages change too fast to recreate the words of the ‘Ursprache’ as Ruhlen thinks she can, arguing that her results are based upon a wrong understanding and use of statistical methods such as glottometrics. For the universals of phonology holds the same things as for the supposed universals of semantics and syntax: it is unclear how far they reach and whether they truly exist (Sampson 1999): it is hard to deny that all language have vowels and consonants and that all languages include basic vowels like ‘a’ and ‘o’, but the number of consonants and vowels as well as their relative frequencies and the use of individual vowels in strings of sounds is often vastly different. And finally, in child language acquisition we do find simple, repetitive babbling sequences that can be matched by CVCVCV patterns. However, children can learn all sorts of sounds: think of the tonal qualities of the sounds Chinese children learn early onward, or the many click-sounds of the !Khosas. As such a simple extrapolation from a simplified picture of ontogeny and child language acquisition to hominoid language evolution in general looks a little bit too quick and dirty to be true. The fact that MacNeilage has updated his list of the first sounds multiple times in the past seems to confirm this. Nonetheless, MacNeilage scenario is interesting in as far as it offers a possible story of how proto-humans could make linguistically meaningful sounds *before* their organs were adapted to speech and how a *continuous* transition from these sounds to modern vowels was possible. If fits well the

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<sup>630</sup> The set of primary sounds that MacNeilage proposes has developed and changed over the years. The set of the three CV-clusters + one CVC-combination is the set from MacNeilage and Davis 2000.

dual-inheritance theory with the cultural evolution of sounds and language use putting selection pressure on language organs.

ii) The phonological system: there exist many different ideas on the principles of phonology. Some assume a Chomskian like innate system of phonetic values and rules which requires fine-tuning to the actual language spoken in the community the child grows up in, others believe that phonetic values are almost wholly cultural determined and variable from dialect to dialect and from idiolect to idiolect, with phones or phonemes<sup>631</sup> being almost completely learnt and only slightly limited by the physical constraints of our speech organs. As said, this is not the place to review these issues. We saw that some vowel changes occurred in groups of vowels such as in the famous Great Vowel Shift with vowels ‘pushing’ each other out of range, and that vowels are often depicted as a series from long to short depending, amongst others, on how far the mouth is opened; consonants are also usually grouped (labials, coronals etc.), depending amongst others on the position of the tongue, lips etc., and consonant changes are also sometimes appearing at group level (labials changing in coronals). It doesn’t matter whether such group level changes (as regular as they are) are due to physiological constraints or completely pre-coded systems of rules in the brain, what matters is that there is some phonological *system* in which the individual vowels and consonants occupy a certain position. An important question is how this system evolved. A definitive answer of course depends on a clear answer to the question of the exact nature of the phonological system. However, multiple authors show how the sound system could evolve by self-organization across generations (Demolin and Soquet 1999), an idea worked out with a lot of rigour in computer simulations (De Boer 2001). We saw that although self-evolutionary processes are not Darwinian insofar as the former evolve because of internal principles only and not due to external selection; Darwinian processes can be compatible with and depend upon processes of self-organization (see §2.1 and appendix 1). The evolution of the sound system might be exactly such a process, with natural selection creating some of the constraints (the physiological speech organs and the elementary neurological wiring required for speech) and self-organization of the sounds within the phonological system doing the rest of the work in the evolution of the phonological system.<sup>632</sup> Different self-organized systems may become the subject of cultural evolution and thus again we see the interplay of biological and cultural evolution the dual-inheritance theory proposes.

## **2) The origin of the duality of patterning:**

The key phase in the evolution of phonology is the evolution of dual patterning. Jackendoff (2002) sees it as the major cognitive step in language, enabling an open-ended growth of symbols. The main question is how this system might have evolved when only pseudo-syllables or individual sounds within some sort of a phonological system existed.

In the previous section we already argued that when the number of individual sounds needed becomes too big, it pays out to have a system of dual patterning (a point elaborated upon by Nowak and Krakauer 1999 and Plotkin and Nowak 2000). A hard question, however, is how this system might have evolved. It

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<sup>631</sup> See footnote 335 for a short discussion of the competing traditions in phonology.

<sup>632</sup> Note that this self-organization seems to fit best with a picture of gradual *cultural* evolution of phonemes (phonetic values) with little genetic (and neurological constraints) control over the system. If so, this would be one argument in favour of a largely culturally determined and transmitted set or system of phonemes (constrained only by physiological possibilities).



seems to be a big step from single sounds (or pseudo-syllables) with meanings to a system in which individual sounds have no longer a meaning of their own, but only in combination with other sounds; it seems also quite a discrete jump from isolated sounds with individual meanings to combinatorial phonology. Both the big leap and the few imaginable inbetween steps make this question such a though one. This time it is also hard to use findings from child ontogeny and first language acquisition as children from early onward seem to master (at least passively) dual patterning, so that there is no example of a stage with meaningful use of individual sounds with meanings to a situation in which individual sounds only have meaning within words (see §3.24).<sup>633</sup>

Here we will first look at the evidence of dual patterning in other species and then come back to the big question. We do find that other species combine sounds or sound units into larger strings, notably in the songs of birds and whales, and we saw that there are some hints of primitive compositeness in primate vocalizations as well (Ujhelyi and Buk 2001, §3.23). As such compositeness thus occurs in multiple, hardly related branches of the ‘evolutionary tree of life’ which implies that it can evolve relatively easily. However, humans combine sounds much faster than these animals, so fast that there can be no feedback from the brain on the production (i.e., the famous rapid sequencing). This rapid sequencing is hard to explain: many sequential tasks involving the coordination of many subtasks are invoked as exaptation for this sequencing: from throwing which requires the sequential coordination of many muscles (Calvin 1993), to tool creation or an understanding of social hierarchy (see above), but the rapid sequencing of sounds seems such a domain restricted activity that the proposed exaptations to explain the sequencing seem to be only meager analogies (id.). But there is an even bigger problem for the evolution of dual patterning, as dual patterning is more than just composing sounds: birds, whales and primates may combine sounds, but it is uncertain whether the *composition* of sounds *creates a new meaning* (apart from the meaning of just the two sounds individually). However, the fact that *a combination of old, basic sounds can mean something new* or above the combination of the meaning of the two sounds is the essence of dual patterning, not the fact that sounds are combined.

How might this key feature of dual patterning have evolved? This issue is hardly ever addressed as discussions of dual patterning too often focus on compositionality alone. We can only solve this issue by taking a thorough *use* perspective of language. It cannot be the case that existing sounds gradually became meaningless so that *a little later* they could be combined to great use into many new

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<sup>633</sup> This is the usual picture from first language acquisition which may of course be wrong. It is true that from early child production one cannot or at least not easily discern dual patterning as childrens’ one-syllable productions are just too short, vague and similar to each other to identify dual patterning instead of single sounds (‘syllable’ or ‘babbling-like’ sounds). The only good test for dual patterning we are aware of comes from testing whether young children can discern minimally different vowel or consonant pairs, e.g., difference between ‘cat’ and ‘hat’ in adult speech. From quite an early age they can do this which seems to indicate that they learn dual patterning almost right from the beginning. However, one can of course also interpret the finding that children can understand the difference between for instance ‘cat’ and ‘hat’ differently: children can have a very good hearing as well as a large memory for speech forms so that they can identify each sound-cluster individually, without as yet being able to segment words into sounds. If so, parents recognize the understanding of dual patterning in their children, because *they* use dual patterning in speech and think children understand speech in the same way, whereas in fact they might not. And if so, child language acquisition might tell us something about the transition to dual patterning; however, even then probably not as much as we like as we don’t have a situation in which individual sounds evolve into sounds within a dual patterning system as there is no stage of such a transition in *active use* by the children as they have to learn *only* dual patterned forms.

meaningful combinations: evolution can't look ahead and meaningless sounds would just be selected against. This implies that the linguistic forms used would have to have meaning at *any* time of their evolution, from individual sounds with meaning to words composed of meaningless sounds. But how? Maybe a solution lies in an analogy with the evolution of Egyptian hieroglyphs. Hieroglyphs started as simple pictures of the things they represented; as the number of hieroglyphs grew, they became harder to remember and some might even have been used for multiple meanings; at that stage some hieroglyphs evolved into determinatives, frequently used hieroglyphs (such as water) which not only had their normal meaning ('water') but also functioned to narrow down the meaning of a logogram (water + bird = duck). A little later glyphs evolved, mainly on the basis of earlier determinatives, that are called phonetic glyphs: glyphs for syllables or sounds cluster as well as single-consonant characters that functioned like an alphabet. And finally, the hieroglyphic script including the three types of glyphs evolved into the less formal hieratic and demotic scripts.<sup>634</sup>

Similarly, the evolution of speech could have started with individual sounds with meaning. Soon the number of sounds humans can make and discern was less than the meanings humans needed, so certain sounds became overcoded by having multiple meanings. In that case the true meaning of the sound had to be discerned from the context of its utterance, until even the context became not enough to discern the even bigger number of meanings. An easy solution, that could have started in parallel at the moment sounds become overcoded, was stringing individual sounds with individual meanings into larger units. At first, the combination would mean nothing more than the two individual sound-words after each other. However, some common 'sound-words' could evolve easily into determiners or category markers that are still common in many languages today (i.e., 'water' + 'bird' for 'duck' with 'water' a category marker).<sup>635</sup> Then the determiners could become stuck to the other 'sound-words' because of frequent use of the combination, but still be recognized as individual unit, as some sort of meaningful sound-syllables within a larger word. Even later these syllables could evolve further into meaningless sub-parts as often happens with syllables within normal words that once had an independent meaning, but have lost it in their current use. In such a way individual sounds could have evolved, similarly as logograms evolved into phonetic glyphs.<sup>636</sup>

The key of this approach was *to find a use for each linguistic form*, and the most surprising result possibly that semantics is dominant over phonology so that even semantic strings of sounds come earlier in the evolution of proto-language than meaningless sounds within words: the 'end-state'. This approach is still mere speculation; its details have to be filled in and tested, but its structure follows logically from the (semantics) perspective combined with the nature of Darwinian evolution as a slow, non-forward looking process. We now turn to semantics itself.

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<sup>634</sup> The actual history of Egyptian hieroglyphs is more complex and erratic (Kamrin 2004).

<sup>635</sup> Even the gorilla Koko was speculated to have made this transition, although the story of his combining the signals of water + bird to sign 'duck' has not been replicable. Note that the category markers are fairly common and occur early in child speech, often allowing a huge spurt in vocabulary.

<sup>636</sup> The analogy is of course incomplete: at the time hieroglyphs evolved dual patterning already existed and the phonetic glyphs, which evolved from the logographs and the determinatives, could simply match the already existing dual patterning present in speech as good or bad as possible.

## b) Semantics:

Semantics is a difficult field of studies to define. We have implicitly touched upon findings of this field multiple times before. Semantics is literally the field concerned with signs ('semeion') and what they mean ('semainein'). In philosophy, semantics often indicates the field concerned with the truth conditions of a sentence (possibly because 'meaning' in philosophy is often conflated with the literal meaning, i.e. truth or falsehood of descriptive sentences only), in linguistics semantics usually partly overlaps with pragmatics and is concerned with the study of interpretation of signs or symbols as used by agents or communities within particular circumstances and contexts. Here we will use semantics in the loose sense of the field concerned with *why signs mean what they mean as they are used* and we will focus on some key aspects of human sign use. We will bracket philosophical considerations on how (human) signs can have meanings at all as we simply observe that animal signs (such as those of the vervet monkeys) already have specific meanings. Any discussion of the specifics of human sign use will profit from Hockett's design criteria. Of special importance are 2) directional reception, 4) interchangeability, 6) semanticity<sup>637</sup>, 7) arbitrariness, 10) displacement, 11) productivity, 12) traditional transmission, 14) prevarication, and 15) reflexiveness. In §3.23 we already reviewed how the language use of different animals scored on these design criteria; we saw that all the above aspects could already be found in the language of one species of animals or another, although not to the extent they are found in human language.

Put thus bluntly, the question of the (absolute) evolutionary origin of semantics is a non-question. The Darwinian use perspective of language starts from the assumption that signs have a use and hence a meaning; the question of the origin of semantics then coincides with the question of the origin of signs and sign use at all in the animal realm, a question outside the scope of this study as it would involve the evolution of perception organs and the subsequent reception of environmental cues or natural signs such as colours etc. From passively receiving natural signs it is a small step to actively broadcast signals to manipulate their receivers. (One may want to reduce the scope of semantics to the actively broadcast signals only, but that does not fundamentally change our discussion). Later come actively using new signs or sign combinations (free production) and the collective establishment of meanings of signs such as the apes Sherman and Austin were already able to as they establish the joint-reference of a new logogram. This joint-reference seems to assume a theory of mind and 3<sup>rd</sup> order intentionality: the mutual understanding that you and I know that we want to give a sign a certain meaning. This intentionality is hard to explain within a Darwinian frame-work (see §3.13), yet a discussion of its origin falls again outside the scope of our discussion on the evolutionary origin of the semantics of human signs.

There are, however, a few interesting questions to be asked about the evolution of human signs: what type of signs or words evolved first? There are multiple hierarchical sign-typologies such as famous one of Peirce who distinguished the icon, index and symbol. However, we saw that any absolute distinction between icons and indices is hard to uphold as any icon has an indexical side and any index an iconical one (hence both are in some sense 'symbols' as a symbol has both aspects, see footnote 382). Thus, Peirce's categorization cannot be used in a *historical* discussion as the aspects of signs cannot have evolved after each other as each of the three already assumes the existence of the others (contra Deacon 1997).

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<sup>637</sup> Semanticity in Hockett's sense means 'reference' and is to be distinguished from our broader use of 'semantics' as the field dealing with why signs mean what they mean as they are used.

Better clues on the origin of human signs might come from child language acquisition. What signs come first and what is their use? The first signs will be ones related closely to survival (warning/waning cries) and reproductive means (court signs). There will be hardly any synonymy among the early signs and of course no signs about signs (the word 'word') as you first need a lot of signs before it makes sense to speak about signs or 'function words' as grammar still had to evolve (e.g., articles, conjunctions etc.). But not only were no function words among the early words, they also did not function like current words that are fit within the grammatical structure of nouns and verbs; just like words in child language such as 'knife' can be used for the 'object' knife and the action of 'cutting', so the use of early words cannot be analyzed using normal grammatical categories.

The first uses of words will be highly fixed and contextualized, just as those of the vervet monkeys. Only later will they become usable in multiple contexts. This de-contextualizing will likely occur when more and more signs come into existence, so that the use of each sign becomes more specific, just as in child language acquisition the problem of over-extensions of sign use is solved when the child learns to use more and more signs. The origin of 'function words' can lie in the evolution of category markers as the system of sound-words become overcoded (see previous section on phonology). How grammar could evolve further is the topic of the next section.

### **c) Syntax:**

Describing the evolution of syntax is hard as intermediary language forms have left no traces in fossils nor in writing. The ontogeny of child language, the formation of pidgins and creoles as well as current diachronic evolution with the grammaticalisation of content words can provide us with important clues; however, they can not teach us about the absolute origin of syntax as all these developments depend on the existence of 'full' human syntax, respectively: parents or other adults with full competence teaching a child, adults (such as traders or masters and slaves) speaking their own 'fully' complex language and languages with 'fully' complex structures changing into other 'fully' complex structures in the case of diachronic linguistics. The three processes are strongly gradual, which at least allows us to conclude that in language evolutionary processes can be non-discrete. Here we will first concentrate on some general ideas on the origin of syntax and then concentrate upon three stages of syntactic complexity: 1) (basic) structure, 2) hierarchy and 3) recursiveness.

It is certainly possible for language to be used in a meaningful way without syntax: people suffering from grammatic aphasia or children at the one-word stage clearly demonstrate this. However, language without syntax is seriously limited. Just as Nowak and co-workers argued that after a certain number of sound-words have evolved, dual patterning is the only way to make seriously bigger vocabularies possible, so they argue that after a certain critical mass of words (which is seriously limited by the learning demands put on each generation to learn their parent's lexicon) syntax is highly efficient in enlarging the possibilities of communication (Nowak et al. 2000).<sup>638</sup>

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<sup>638</sup> Carstairs-McCarthy goes one step further in which syntax is even stronger linked to phonology as she imagines that syntax derived or evolved from the dual patterning of combinatorial phonology (with syntax a dual patterning of semantic signs). Given the fact that we believe that dual patterning in phonology is a rather late phenomenon which evolved after semantic elements were combined (in a proto-grammar

Depending on how much ‘grammar’ one supposes to be ‘innate’, views on biological skills required for learning syntax are different; at the one extreme we find of course radical Chomskians who presuppose an innate instruction for almost every grammatical rule (see §3.21), on the other extreme people who claim there are no specific syntactic skills at all, only the highly general capacity to learn and understand relations, sequential structure, hierarchy and or recursion, for which all sorts of other sequential tasks or actions involving (social) relations are claimed to be early exaptations: tool making (Ambrose 2001), social interactions following certain scripts of A acting on B etc. (Aiello 1998),<sup>639</sup> possibly of the kind of reciprocal altruism (Bickerton and Calvin 2000) etc. Although we opted for a view more akin to the second view as we presume that we don’t have many specific innate grammar skills, we were critical of the many proposed exaptations for syntax skills based on loose analogies with sequential tasks. Here we will concentrate on the evolution of syntactic forms proper (against the background of little innate constraints on syntactic forms).

Syntax can be argued to start at the two-word<sup>640</sup> stage when two words are first combined into a string. In child language, we saw that the two-word state can be interpreted as frozen formulas that arose by semantic relationship or as fragments of adult speech. These frozen formulas can be said to just link two words with little extra meaning apart from just the sequence of the two words individually, but as the number of frozen formulas grows, it becomes more likely to interpret the frozen formulas as containing proto-grammatical categories (agent + action instead of subject + verb etc., see §3.14). In proto-grammar the frozen combinations can of course not be interpreted as fragments of adult speech, but the two word combinations of early language evolution can evolve from combinations of symbols with less extra meaning than the concatenation of the individual symbols to a situation in which we find proto-grammatical categories (Jackendoff 2002).<sup>641</sup>

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fashion), Carstairs-McCarthy’s theory which seems to be based upon the traditional hierarchy of linguistics as phonology-semantics-syntax seems chronologically wrong.

<sup>639</sup> Armstrong et al. (1995) even try to derive the primacy of SVO order from social scripts. They believe that language started as gesture language and that the first signs were iconic in nature; then they argue that iconic signs of actions have a certain order that might be interpreted as a “precursor of grammar”. Finally, the natural order of iconic signs is supposedly ‘actor – action – object acted upon’, so that SVO is the natural word order. Take ‘I hit you’ which is supposedly signed by pointing to doing the pseudo-action of hitting you by me. However, even if we admit that language started as signing and that the first signs were iconic and that iconic signs imply a certain order, we can’t conclude that SVO is the right order. Take the action: the lightning struck me, which I sign by shaking (I was struck by the lightning) or take even the action ‘I hit you’: I can sign this action by first pointing to you, then making an iconic sign of hitting followed by pointing towards me or, contrarily, by first pointing to me, then making an iconic sign of hitting followed by pointing to you; in the first case we would have a ‘natural’ OVS-order, in the second case an SVO.

<sup>640</sup> One word utterance can be grammatical: think of imperatives: “walk!” However, this sentence is grammatical because of the paradigm of the verb “to walk” whose forms normally function within bigger sentences. Proto-grammatical one-word forms that lacked any possibility of being combined into larger wholes can, however, not be called grammatical and so can’t the one-word utterances of children. The attempt to see the one-word utterances of children as having full-blown adult grammar and the corresponding deep structure, but with the one-word dropped into the surface-structure, might make sense if one takes the full consequences of the Chomskian paradigm (Wanner and Gleitman 1982), but if one is critical of the Chomskian paradigm, this idea sounds too far-fetched to be true.

<sup>641</sup> Another possibility is that the word combinations learnt are combinations of what we would now call nouns: water + bird = duck. True proto-grammatical combination would then evolve later. However, in child speech this kind of ‘compound word formation’ or word categories (food versus apple and bread) develop later than the proto-grammatical combinations, hence maybe this is also the case for the absolute

But even if proto-grammar would evolve, the step to ‘full-blown’ modern syntax is still a far cry. To understand how this transition might have happened, we need to find a few basic elements of syntax that might be added one by one to proto-grammar, as it is clear that such a complex structure as modern syntax cannot have evolved all at once. Some claim that modern syntax is such a monolithic whole (Chomsky 1995) that it is impossible to isolate elements: ‘half a grammar is no grammar’. If so, it seems hardly likely that it could ever have evolved. But we have seen how language develops step by step in child language acquisition (and in each step providing greater functionality), so it seems very likely that language itself can evolve step by step. A few key steps are the evolution of (Johansson 2005: 233):

- 1) structure: sentences are not just random concatenations of words, but sequences ordered according to certain rules (whatever the rules exactly are)
- 2) hierarchy: the structure in a sentence can have sub-structure
- 3) recursion: the same rules and structures may recur at different levels of the hierarchy, so that a structure may contain a substructure which is another instantiation of the same type of structure etc. (a process that *in theory* could be repeated ad infinitum).

1) Structure: we just argued that syntactic structure arises as concatenations of words are no longer just concatenations of individual symbols, but a little more: a structure in which individual symbols fulfill a certain role. The debate on the first steps toward syntax has two important dimensions: a) what are the first linguistic units used in combinations, and b) how did the structure evolve?

i) Some (Arbib 2003, Wray 2000) claim that concatenations evolved in an ‘analytic’ fashion as Hurford (2000) calls it: by re-analyzing linguistic wholes (i.e. words) into subunits that become independently recombinable elements in the next stage, whereas others (Jackendoff 2002) claim that concatenations evolved in a ‘synthetic’ fashion when linguistic units were combined into larger wholes which somehow became syntactically structured. The latter position will be our view; although we do find that some children learn stock phrases first as linguistic wholes and only later learn the structure of the stock phrase<sup>642</sup> and although we do observe that some words are (by analogy) re-analyzed as containing morphemes whereas ‘in fact’ they don’t, such processes are unlikely for language evolution: children learn the structure of stock phrases because structure is already present in them and some words are analyzed as containing morphemes (although they didn’t) because other words already have morphemes and the former words look so similar to the other words containing real morphemes. However, both cannot be the case in early syntax evolution as morphemes and stock phrases cannot yet have evolved.

ii) Some (Bickerton) claim that words were combined into patterns according to *pre-existent* rules (i.e. the rules or their template evolved before words were put into the template), whereas others hold that syntax evolved *after* word combinations were formed (Gärdenfors 2003). The former position is typical of some Chomskians and other people who think of language as genetically encoded, the latter fits better with a view of the dual-inheritance theory with language evolving as

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origin of language (§3.24). Moreover, proto-grammatical combinations seem much more useful than compound word which also pleads for the former evolving earlier.

<sup>642</sup> A process which might also happen with etymological research (“hello” in Austrian: “Grüssgott” which is “Gegrüsset seiest du bei Gott” or in learning a foreign language: “please” in French: “sivupleh” which any Frenchman immediately recognizes as “si’l vous plaît.”)

a cultural product. Suffice to say we think the latter position has the best cards: it seems unlikely from a Darwinian perspective that rules evolved *before* they were used.

The evolution of structure seems to be the hardest element in the evolution of language. However, we saw that Alex the parrot as well as some apes already understood (proto-grammatical) sequences of symbols so the step is not as big as sometimes portrayed. Moreover, the step need also not be an all or nothing one too: proto-grammatical structure could be introduced step by step. First for instance combinations of basic general-purpose action words with object (i.e. 'look or get + object'), then combinations of an 'agent + action' or an 'action + patient' etc. Compare, the proto-grammatical utterances of children. What makes these proto-grammatical combinations distinct from pure syntax is that they are much more *semantically* informed than current syntax (i.e., 'agent + object' instead of 'subject + verb' and sentences like 'you eat' instead of 'it rains'). Note also the normal diachronic linguistic process of grammaticalisation which can happen in just a few generations or a shorter span of time; in the evolution of language we might just as well have simple content words which almost naturally delexified into function words (Hurford 2003). Guy Deutscher (2005) even treats language evolution as a whole mainly as a normal (cultural) diachronic process, in which language and grammar is gradually "unfolding" by processes of delexification and grammaticalisation.

In any case, with an increase in the number of proto-grammatical combinations, the possibilities of combining proto-grammatical combinations into larger combinations might have come in reach: e.g., combining 'agent + action' and 'action + patient' into 'agent action patient' (which by the way is still a far cry from SVO). This brings us to the next step: hierarchy.

2) Hierarchy: we described the evolution of hierarchy as the evolution of substructure within syntactic structure. This is a little vague description as any three-word sentence already has structure within structure. Now, just as structure did not evolve all at once, so hierarchical structure may have evolved slowly. For instance the application of adverbial constructions or adjectives to nouns could have evolved much earlier than complex rules regarding sub-phrases. With the evolution of substructure came almost instantly within reach the evolution of substructure of substructures. Think of double adjectives such as 'the quick, brown fox' and adverbial constructions such as 'the brown-haired man'. In principle (although often not in practice) these substructures could have new substructures of their own: 'the dark-brown-haired man'. In the theoretical case of infinite chains of substructures within similar structures we speak of recursion.

3) Recursion: ever since Chomsky claimed that the key feature of language is recursion and that recursion is something that cannot be learnt without being innate (Chomsky 1957), recursion is put to the fore-ground. We already discussed it extensively in the context of animal language, where we were highly critical of putting so much emphasis on recursion. Here we will briefly sum up again a few reasons why the emphasis on recursion is undeserved:

- i) some people with an SLI never learn recursion, but are nevertheless perfect language speakers (Bloom 2000)
- ii) children communicate well before they learn to apply recursion (§3.24)
- iii) there are languages (pidgins and creoles) which lack (full) recursion (§3.25)

iv) in some languages like Eskimo recursion (or at least relative clauses) were only introduced as they learnt to write

v) full recursion is not used in practice as already at the fourth level of recursion people tend to get confused (Christiansen and Chater 1999)

vi) recursion is again not an all or nothing phenomenon: stock-phrase recursion such as 'Jimmy said that Mark had said that Sheela had said that ... etc.' may have preceded the evolution of multiple adjectives, in turn may have evolved before the embeddedness of relative clauses etc. Note that in stock-phrase recursion the semantic relation of recursion (the number of people saying something about someone else) is present, whereas in the full syntactic case of embedded relative clauses this is usually absent.

All in all, full recursion, if it exists at all instead of just in the minds of the creators of formal grammars, is probably a late addition to language. As such it is a prime candidate for being a cultural feature of language (Harder 2004), contra Chomsky's innateness hypothesis based upon recursion which he has upheld from the fifties (Chomsky 1957) till now (Fitch, Hauser and Chomsky 2005). Recently more and more people have tried to understand how recursion might have evolved by cultural means using computer simulations of language evolution (Kirby 2002) often involving connectionist network (Christiansen and Chater 1999) which can be trained to understand (and produce) recursive sentences up to the 4<sup>th</sup> level with the same accuracy as humans.

A way to imagine how recursion might have evolved *culturally* is to imagine a grouping of words into headed units and the application of structural rules to headed units as a whole rather than to individual words (Jackendoff 1999). This is already the case in the stock phrase recursion we just mentioned. Note finally that an understanding of recursion is present in many other cognitive tasks that animals perform, such as the transitivity of dominance relations in groups of primates (which would just imply structure and hierarchy), and the understanding of kin groups within a group, in which each individual in turn has its own rank (Bergman et al. 2003). Although we are quite skeptical about applying cognitive features from one domain to the domain of language, here we have a clear case of the understanding of primitive recursion in primates, which at least allows us to conclude that understanding recursion might not have been as difficult as sometimes held.

By breaking syntax down in multiple elements, we have thus been able to outline a scenario, which though not very well tested, envisions a possible route for evolution of syntax as a slow, accumulative process instead of an all or nothing big step. In this process the semantics of word combinations are key.

#### **d) Language as a whole:**

Some people even talk of the evolution of whole languages or dialects. However, we saw that in the strict sense there are no languages: there are only individual people that (partly) share a lexicon, a pronunciation and certain syntactic rules with another so that they can (mutually) understand each other; the overlap in lexicon, pronunciation and syntax and the level of (mutual) understanding can further vary from each two-pair of individual language speakers (Millikan 1984). Languages, or better: dialects are nothing more than clusterings of idiolects with similar lexicons, pronunciation and rules, usually by people that live in the same region and that share a common history. As such clusterings are real, and languages or dialects can be thought of as real entities. Similarly, Darwinism has shown that species in the traditional sense of the word do not exist, but it can nevertheless accept to speak of



species as (local) clustering of individuals that share typological features, as well as common ancestry. Researchers that speak of the evolution of whole dialects or languages are on a par with researchers who speak of evolution at the species level. In chapter 1 we were highly critical of biological evolution at the species level. However, 'species evolution' at the level of whole languages may be less controversial as there exist organisms (i.e. humans) that *recognize* languages as languages, so that languages can be the object of selection (§1.3).

There exist two adaptive evolutionary hypotheses about the evolution of languages. These hypotheses start from the fact that it is strange that there are so many languages (many thousands). Why would one language not suffice for the whole human population? The standard answer is probably: because languages change so fast that ultimately the boundaries of a human population simply can no longer understand each other. This would mean in biological terms: speciation by neutral drift. However, there are two hypotheses that try to provide an adaptive reason why there are so many languages: 1) to hinder understanding between tribes so that information can be concealed to outsiders (Baker 2003), 2) to create a group feeling among the speakers of a dialect (against speakers of other dialects) (Dunbar 1996). Both hypotheses are about *group formation*, the first about limiting the group size, the second about creating a tighter bound group. To evaluate them as *adaptive* explanations we have to do two things: a) test whether the idea works in practice and b) test whether there existed active selection pressure on either limiting a group or creating a stronger group feeling in the hominoid lineage.

a) We can use the standard tools of sociobiological research on group formation to evaluate these scenarios as they are far from trivial: a1) smaller groups may for instance damage chances of access to territory or food resources (although smaller groups may also limit conflicts), and a2) more tightly bound groups may prevent diffusion of technological information and unwillingness to surrender to other groups in war time when this would be the only means of survival. b) We can look at actual hominoid evolution to see whether we find evidence that supports one scenario or the other.

a1) In the previous paragraph we found an overall tendency towards bigger groups in the hominoid lineage which speaks against Baker's idea of dialects as group size limiting adaptations; however, the optimal group size is the product of many contrary evolutionary forces which are hard to disentangle. Even a bigger group may need a limiting force present to guarantee its survival; in theory this could be done by adaptive selection on dialects. Nevertheless, this would need to be a very delicate issue and subtle shaping force; in practice we find dialects groups of all sorts of sizes, from a couple of people or a village to a whole region or nation (the American English dialect with its many varieties) with thus *no natural dialect group size*. This is a strong argument against Baker's idea.

a2) Dunbar's ideas are harder to test since what counts as a good measure of being a tightly bound group? One suggestion might be to look for genetic homogeneity, with tightly bound groups being more homogeneous than less tightly bound groups. Dunbar connects an interesting speculation to his suggestion: the genetic Eve is much younger than the genetic Adam, which means that for the last tens of thousands of years women (and their genes) have crossed the globe much more and faster than those of men (possibly because of trading, out-marrying and being taken hostage as war booty). This means women should be able to survive in much more different groups than men, and that's the real evolutionary reason behind the well-known sociolinguistic fact that women take up a new dialect much easier and

with much more willingness than men, so that (middle-class) women are the main drivers of language change (see §3.25). With suggestions like the above Dunbar's work comes into the realm of sociolinguistics. We saw that sociolinguistic work is well-integratable into the Darwinian frame-work of the dual-inheritance theory, especially when the developments happen unconscious or (relatively) unintentional, such as processes driven by prestige bias as well as language internal factors (see §4.2). Darwinians should not redo that work, but integrate the findings of sociolinguistics into their own work and the other way round.

Until now, suggestions about adaptive reasons behind the evolution of many languages seem rather weak; the existence of them can thus better be regarded as an accidental byproduct of language being a cultural product which evolves so quickly that language speciation takes place.

In §3.25 we already discussed multiple other aspects of diachronic language evolution with a Darwinian parallel. Main issue was whether current diachronic evolution is adaptive or more like drift (or a balance of stabilizing selection with corruption and new adaptive generalizations by rule formation balancing each other). The evolution described in this section is, however, clearly adaptive as the evolutionary steps described in phonology, semantics and syntax have led to a huge increase in the functionality of language.

## §4.4 Evaluations on the basis of the ideal of §2.3

*In this § we assess the current state of Darwinian explanations of language on the basis of the ideal as formulated in §2.3. We start with some general remarks and then analyze each of the conditions of the ideal explanation. We conclude that although not all elements of the evolution of language can as yet be explained and although further empirical work on the proximate mechanisms involved is required to fill in the details of the evolutionary explanation, the dual inheritance theory comes close to the ideal in explaining the origin and evolution of language.*

In general, we can conclude that the dual-inheritance frame-work is the only frame-work that can get a good grip on the problem of the origin of language. The normal theory of natural or sexual selection does not really suffice as in the problem of the origin of language the *evolution of cultural forms* (i.e., linguistic units), which are not hard-coded in the genes, caused selection pressure on the biological language skills right from the start of the evolution of language skills. And the other models of cultural evolution do not suffice either as they either analyze the cultural evolution as a far too autonomous process (as the ontogenetic models, ideational selection and memology do) or only analyze the evolution of cultural forms from the side of biological skills and not the other way round (such as cultural ecology, sociobiology and evolutionary psychology do). As we have analyzed Darwinian explanations from the dual-inheritance perspective and as the definitions of the fundamental concepts of the ideal explanation for cultural evolution could only be formulated parallel to the definitions for Darwinian evolution in biology within the dual-inheritance theory, our work thus far almost automatically satisfies the definitions of the ideal based upon the dual-inheritance theory as formulated in §2.3. This is especially true of the

mechanisms identified in sociolinguistics as its discussion of diachronic language evolution is fully compatible with the dual-inheritance theory.

Also in line with the ideal is our discussion of the 6 general aspects of language evolution as it concluded that language is an early, gradual, speech first, learnt (functional) adaptation evolved primarily by natural (and thus not sexual) selection: i.e., the ideal assumes that the evolution of complex traits is slow and thus has to start early, that complex traits are adaptations and that natural selection is far more common than sexual selection (in the sense of ornamental selection).

Further: the use perspective of language, reintroduced into philosophy by Wittgenstein, is the *conditio sine qua non* for any Darwinian explanation of language as in any good Darwinian explanation every stage of the evolution of a trait needs to be net functional (or neutral). The strict adherence to this use perspective allowed us for instance to conclude that in the evolution of linguistic forms, the combination of sound-words occurred before individual phones became meaningless sounds or 'phonemes' (see footnote 335).

Finally, not all aspects of language use could be explained within the Darwinian frame-work. We saw that for instance intentionality is very important in actual language use and that for learning a language a 'theory of mind' is very useful too; both intentionality and having a 'theory of mind', however, can as yet not easily be explained within a Darwinian frame-work. Nonetheless, although language use is often intentional, language evolution (both of linguistic forms and linguistic skills) proceeds more or less unconsciously and can thus well be explained within the frame-work of the dual-inheritance theory. Of course, the more proximate mechanisms behind this evolution we can identify the better.

After these general remarks we turn to the criteria of the ideal of §2.3.

A specific Darwinian explanation in cultural evolution is ideally:

- i) a non-tautologous (and the more informative, the more proximate mechanisms it describes);
- ii) historical narrative (that can sometimes be extended to the future);
- iii) that uses the four key concepts – transmission, variation, selection and fitness – in the following way, as defined in the section on dual-inheritance theory just above
- iv) to explain (the origins of) cultural design *and* its less than optimal maladaptive sides (which includes a cost-benefit analysis of the cultural variant);
- v) in a way that is on the whole better than its competitors (i.e., other specific cultural evolutionary explanations) according to the following factors: accuracy, consistency, scope, simplicity and fruitfulness;
- vi) whereby its success and acceptance is further dependent on sociological factors and discoveries and developments in other branches of science, especially regarding consilience.

If we now turn to our dual inheritance explanation of the origin of language as adaptation triggered for socio-politics but also adapted for other functions, we see that:

- i) the explanation is non-tautologous: many proximate mechanisms could be identified and many surprising results were found such as those on the origin of dual patterning; of course more proximate mechanisms still need to be identified to buttress the adaptive explanation of language, especially mechanisms concerning the neural and genetic processes involved
- ii) the explanation is an historical narrative that can triangulate, using multiple facts from different sciences, that human language most likely started to

evolve half a million years ago or a little bit earlier; it can also tell a likely story about the relation of language evolution to that of other key human features such as hunting, tool use and teaching; the explanation can even be used to make future predictions as it can predict that evolution of linguistic forms will never stop as the balance between corruption and new streamlining will never be restored; in addition it can predict that many languages will die and that increased globalization will lead to slightly pidginized, streamlined 'world' languages

iii) the explanation is formulated within the dual-inheritance frame-work and satisfies the definitions of the ideal almost automatically (which would not have been the case had it been formulated within one of the other frame-works of cultural evolution); extra care has been taken to assure that there are no teleological traces in the adaptive explanation (such as the suggestion that language evolved *for* teaching) and that the proposed evolutionary steps are small

iv) the explanation points explain the functional use of the complex trait language with its huge learning and maintenance (brain tissue) costs; the explanation takes secondary functions of language use other than socio-politics into account and is well aware of the maladaptive aspects of language evolution, be it due to historical lock-in (the first vowels and consonants being dependent on the nature of our organs which were not fit for making phones), drift (languages speciating because the cultural forms change so quickly) and for instance design compromises (the costs of the possibility of suffocating due to food in the air pipe that voluntary breath control introduced)

v) the explanation is better than its alternatives; we explicitly contrasted 4 different adaptive explanations and found that the socio-political one is the most promising one; it is *consistent* with historical facts on when language evolution started; our explanation as a whole seems complex as it involves many proximate mechanisms, however, all these mechanisms fit within the *one simple* frame-work of the dual-inheritance theory to explain almost the whole problem of the origin of language (big *scope*) and as such the explanation is more *accurate* than other ones; our explanation was also fruitful as it uncovered some surprising results (phonology evolving after sound-words were combined, grammar nothing but solidified semantics) etc.

vi) the explanation would finally not have been possible without developments in other sciences, such as of course neurology or genetics, but also in the philosophy of language: if mentalism was still the dominant or only theory of meaning, it would be hard to explain how language could evolve (see our discussion of mentalistic interpretation of the signs of vervet monkeys); pragmatism thus paved the way for a Darwinian explanation of language.

Thus, although not all elements of the evolution of language can as yet be explained and although further empirical work on the proximate mechanisms involved is required to fill in the details of the evolutionary explanation, the dual-inheritance theory comes close to the ideal in explaining the origin and evolution of language.



# Summary

*Every chapter and sub§ already has a summary of its own, written in italics at its beginning; the summary here provides the red thread that runs through the four chapters.*

What counts as a Darwinian explanation in biology has varied from time to time and from biologist to biologist (§1.1). On the basis of modern research we have formulated the ideal of a Darwinian explanation in biology (§1.4) that can be used to currently assess the adequacy of a particular example of such an explanation. Central to this ideal are more precise definitions of the fundamental concepts of replication, variation, selection and fitness on the basis of current status of biological research (§1.2). We stress the importance of a good understanding of the proximate mechanisms involved in evolution if we want that our historical, ultimate explanations of adaptations are non-tautologous (§1.3). We conclude chapter 1 with a discussion of sexual selection and demonstrate that all forms of sexual selection except ornamental selection can be seen as straightforward sub-cases of natural selection. Ornamental selection on the other hand can only be treated as an indirect case of natural selection as it always include a 2-stage development, with a handicap used as indicator of a functional attribute (§1.5). As such, explanations involving ornamental selection will always be harder to test than regular cases of natural selection.

Evolutionary explanations outside of biology in the realm of culture come in many forms. We define culture as *all non-genetically determined behaviors, ways of life and beliefs as well as the artifacts and institutions of a population that are passed down from generation to generation*. We stress that human culture differs from animal culture in being cumulative, possibly due to different learning and transmission mechanisms in our species (§2.1). Darwinian explanations of biology should acknowledge at least a) the idea of cumulative evolution, but b) at the same time the abandonment of progress or direction in evolution, c) the idea of multiple cultural phenomena in competition with each other for survival and d) the idea of culture as being shaped by selection. We discuss 8 Darwinian or evolutionary models of culture that satisfy these criteria, 2 ontogenetic and 6 phylogenetic ones. We demonstrate that the dual-inheritance model is by far the superior one as it best disentangles the evolution of human skills to create or use cultural artefacts from the evolution of cultural artifacts themselves (§2.2). We then formulate the ideal Darwinian explanation of a cultural phenomenon in parallel to the ideal Darwinian explanation in biology.

In chapter 3 we turn to our discussion of the evolution of language. We define human language using Hockett's design criteria for (human) language (§3.1). One of the biggest obstacles to such a theory is the dominant formal-grammar paradigm of Chomsky (§3.12). Research from other areas in linguistics such as biolinguistics, animal language research, first language acquisition studies and diachronic linguistics including socio-linguistics show, however, that this paradigm is fundamentally flawed. The mechanisms identified in these areas can be used as the proximate mechanisms that constrain possible Darwinian ultimate explanations of the evolution of language (§3.2). Other constraints on Darwinian explanations come from the philosophy of language (§3.13). We show that mentalism in the philosophy of language is hard to combine with Darwinism which takes a fundamental use

perspective of language; we further show that language cannot be a prerequisite for thought, but some elements of thought (such as memory) are necessary and others (such as a theory of mind and self-awareness) are very helpful in acquiring, understanding and using language. This disentanglement of language and thought allows us to treat the evolution of language to a certain extent independent from the evolution of thought.

In chapter 4 we define the problem of the origin of language as about the biological evolution of language skills and the evolution of proto-linguistic forms into our current languages. We show that the problem of the origin of language has to be treated from the perspective of the dual-inheritance theory as only this theory can accommodate the fact that in the evolution of language the cultural evolution of linguistic forms caused selection on biological language skills (§4.2). We show examples of Darwinian explanations in linguistics that implicitly or explicitly assume one of the other 7 Darwinian models of cultural evolution we distinguished in chapter 2 and we show why they fail (§4.2). We then focus on a number of general aspects of language evolution, using the material from chapter 1 and 3, to show that language is most likely an early, gradual, speech first, learnt (functional) adaptation evolved primarily by natural (and thus not sexual) selection (§4.31). We show that although language use most likely has not one unique functional use, its evolution was most likely triggered in the context of socio-politics (§4.32). We indicate the possible causal interrelations between a change of habitat, group size, brain growth, hunting, tool use, teaching, socio-politics and language use. We then take the perspective of the dual-inheritance theory to look at the evolution of proto-linguistic forms: their phonology, semantics, syntax and their grouped occurrence within dialects. Most surprisingly, not syntax but phonology (especially the dual patterning of linguistic forms) is seen as the key break-through in the evolution of linguistic forms as it made the infinite growth of semantic elements possible. From a thorough use perspective we show how meaningless individual phonemes evolved *after* meaningful sound (or word-sound) combinations had evolved. We further show that whereas current diachronic evolution can be either adaptive or more like drift (or a balance of stabilizing selection with corruption and new adaptive generalizations by rule formation balancing each other), the evolution of proto-syntax out of fixed semantic combinations is clearly adaptive as they have led to a huge increase in the functionality of language. We then (§4.4) evaluate our discussion of the evolution of language by assessing our work in the light of the ideal of a Darwinian explanation in culture as formulated in §2.3. We conclude that although not all elements of the evolution of language can as yet be explained and although further empirical work on the proximate mechanisms involved is required to fill in the details of the evolutionary explanation, the dual inheritance theory comes close to the ideal in explaining the origin and evolution of language.

# Appendices





## Appendix 1: A note on self-organization in biochemistry

In chapter 1 we discussed chemical replicators and the selection forces on their stability, fecundity, copying fidelity and their role in auto-catalytic processes. We discuss these examples here afresh to understand the difference between *internal* self-organization and Darwinian design due to *external* selection. That is important because many pseudo-Darwinian explanation outside of biology are actually examples of self-organization.

Auto-catalytic reactions are chemical reactions in which at least one of the products is also a reactant. The rate equations for autocatalytic reactions are fundamentally nonlinear and interestingly this nonlinearity can lead to the spontaneous generation of order. The speculations about the RNA-world hypercycle are a dramatic example of how the order of life might have been created bottom-up. On the basis of such examples of emergent order, some researchers hold that auto-catalytic reactions also constitute a basic element in the origin of more modern life and even in the maintainment of life architecture today (Kaufman 2002, Ulanowics 1997).<sup>643</sup> That might be true, however, chemical auto-catalysis and this physical type of self-organization *alone* cannot account for adaptive design of living beings. To understand how design arises one needs the Darwinian principle of selection too. Auto-catalytic processes and other forms of self-organization are dependent on system *internal* principles<sup>644</sup> (whose workings may however depend on system external factors such as enough energy in the surrounding environment), natural selection is a system *external* principle. Only selection can bring about *functional* designs as adaptation to *external* environmental forces (be they organic or anorganic).

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<sup>643</sup> An example of an autocatalytic reaction in our body is the binding of oxygen by hemoglobin.

<sup>644</sup> Internal principles are physical and chemical properties such as mass, electrical charge, magnetic properties, chemical affinities etc. Masses attract each other, electrical charges either attract or repel each other etc. This happens 'spontaneously'. No *external* forces are needed for these processes to take place (although external forces can prevent them from happening). That's why these properties of a system of particles are called system *internal* principles.

Other examples of self-organizing processes (Ball 1999) are a) in physics: structural (order-disorder) phase transitions and spontaneous symmetry breaking such as spontaneous magnetization in the classical domain and superconductivity and Bose-Einstein condensation in the quantum domain (but with macroscopic manifestations), b) in chemistry: (liquid) crystal formation and self-assembling mono-layers and c) in biochemistry: the spontaneous folding of proteins and other biomacromolecules, micelles, and the formation of lipid bilayer membranes. On higher ontological levels people also speak of self-organization such as the homeostatic 'self-organization' of an organism as a whole or the self-organization of a capitalist economy (Krugman 1996) or the self-organization of groups of people as in herd behaviour or group thinking (Estep 2006, 2003). Equilibrium considerations do often play a major role in these fields. However in these higher ontological fields the word self-organization is used in a looser sense than in the case of physics and bio(chemistry) where it means 'a process of attraction and repulsion in which the internal organization of a system, normally an open system, increases in complexity without being guided or managed by an outside source.'

Theories of self-organization in these higher ontological areas compete with Darwinian explanations outside of biology in these areas (and of course other forms of explanations). Self-organization alone was unable to account for the origin of life and adaptive design as the Darwinian principle of external selection was needed to explain this. I expect that theories of self-organization in these higher ontological areas need to be supplemented by an external principle such as selection as well.

Auto-catalytic processes (and other ‘self-assembling’ processes) are thus *not an alternative to Darwinism*<sup>645</sup>, although they may play a secondary role as proximate mechanisms in the evolutionary origin, embryological development and current constitution of living beings as they either constrain or support certain developments. How big a role they play, however, is an *empirical* question. They do form a lively research object as chemists would love to exploit them since then one can let reactants do the hard work of *scaling up* the synthesis of chemicals instead of doing this oneself.

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<sup>645</sup> The program to understand the origin and constitution of life out of auto-catalysis and other self-organizing processes is primarily taken up by physicists (Bak 1996) who prefer their own physical principles above the “vague” biological notion of selection to account for the origin of life.

However, without the Darwinian principle of selection, we cannot give a good account of this. See also our discussion of proximate and ultimate mechanisms in §1.1.

## Appendix 2: A note on biosemiotics

Another discipline that tries to combine biology and linguistics is biosemiotics, a subdiscipline of semiotics with strong philosophical affiliations. Semiotics is the methodology, theory or science of signs (its practitioners don't agree what to call it<sup>646</sup>). Its two founders are De Saussure and the little older Peirce, who famously said that the "universe is perfused with signs if not composed of them exclusively" and who also was the first to make the man-sign analogy that is so important to biosemioticians "for every symbol is a living thing, in a very strict sense that is no mere figure of speech."<sup>647</sup> Together with that of Jacob von Uxeküll<sup>648</sup> the work of Peirce is the biggest inspiration for biosemiotics.

The term 'biosemiotics' was coined by F.S. Rothschild in 1962, but the *linguist* (hence not biologist) Thomas Sebeok (1920-2001) has done much to popularize the field.<sup>649</sup> All biosemioticians share the basic idea of semiotics that a sign is something (A) that stands for something else (B) in a certain regard (C) for someone D. A is a material thing, B is a referential object, C is the sense of the referent B, called interpretant by Peirce, and D is the interpreter (normally a human, but in the context of biosemiotics this can be anything alive). The relation between A, B, and D is called by semioticians the *triadic* relation.

Biosemiotics can perhaps best be described as a field that studies the production, action and interpretation of signs in the physical and biologic realms in an attempt to integrate the findings of scientific biology and semiotics to form a new view of life and meaning, as immanent features of the natural world, to thus, according to one of its proponents, bridge the gap between 'mind and body'.<sup>650</sup> Its practitioners come from all sorts of disciplines, but are mainly philosophers and

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<sup>646</sup> See for instance Sebeok 2001: 8, which reviews the literature on how semioticians such as Peirce, Saussure, Morris, Eco, Deely and many others have regarded their discipline.

<sup>647</sup> Charles Sander Peirce, *Collected papers of Charles Sander Peirce* edited by C. Hartshorne, P. Weiss and A.W. Burks in 8 volumes, Harvard UP 1935-1966. Quotes from respectively CP 5.448n and 2.222. See also Marcel Danese's preface to Sebeok: "If I had to sum up in a phrase what Thomas A. Sebeok has taught a whole generation of semioticians, I would choose the following: he has shown us that life *is* semiosis." To say that signs are somehow living beings is different from saying that the essence of living beings are semiotic processes, but biosemioticians conflate the two. Peirce already conflated the two. He not only said that signs are living things, but he also held that "the problem of how genuine triadic relationships [i.e. signs] first arose in the world is (...) [an improved formulation] of how life first came about (CP 6.322)."

<sup>648</sup> Von Uxeküll was interested in how living beings *subjectively* perceive their environment. Compare, for example, a rose as seen through the compound eyes of a fly, continually flying through the air, and then as seen in black and white by a dog (with its highly efficient sense of smell), and then again from the point of view of a human. Von Uxeküll called these subjective worldviews *Umwelten*. His writings have a certain poetic flavour. They are consistent with Darwinism, although they are also a bit superfluous. Everything that can be known about the perception of (other) organisms can be studied objectively, describing what the world is like for a bat or a dog is not much different from the question how the world of a colour-blind human is different from the world of a normal human or how your perception of red is different from that of my own. Much can be said about that by observing behaviour; about the pure internal subjective side, however, nothing much can be said.

<sup>649</sup> For instance, amongst many other documents, by editing the *The semiotic web* series (Mouton de Gruyter) from 1986-1994.

<sup>650</sup> Sebeok 1994: iii. See also 1994: iv "So, from the dawn of civilization to the present age, it has always been recognized in Western culture – at least implicitly – that there is an intrinsic connection between the body, the mind, and culture, and that the process that interlinks these three dimensions of human existence is semiosis, the production and interpretation of signs."

people working on the theoretical foundations of their discipline. This group is far from homogeneous, containing naturalists but also quite a few people who are attracted to the Gaya hypothesis repopularised by James Lovelock in the nineties, or even to pan-psychism (as Peirce himself sometimes was). Biosemioticians share a conviction that traditional biology is too reductionistic and mechanistic by taking physics as its paradigm, whereas they hold that biology is actually more of a science dealing with signs and their meaning, the intentions behind them etc. Biosemiotics itself is insofar a typical 'philosophical science' in the bad sense of the word as the papers in this discipline often start by outlining what (bio)semiotics is according to the author, after which he or she reformulates the results of other sciences in his or her own terminology. Biosemiotics has, as far as we know, not created its own empirical results.

The integrating role biosemiotics ascribes to itself can best be understood from Peirce's metaphysical idea of signs as intermediaries between nature and mind. This intermediary position of signs is conceived to be two-sided: from nature to mind and from mind to nature. From nature to mind: the mind can only know things about reality because of the *signs* of things: smoke as a sign of fire or bones as the signs of dinosaurs etc.<sup>651</sup> From mind to nature: man's theories (collections of signs) 'construct' an image of nature and this image is what we are really dealing with when we speak of nature.<sup>652</sup> Some biosemioticians distinguish the former as *biosemiotics proper* from the latter as *system-observer semiotics* (for instance Claus Emmeche, in Sebeok 1991: 78). Biosemiotics proper deals with sign processes in nature in all dimensions including the emergence of semiosis in nature and its evolution.<sup>653</sup> Biosemioticians often think that the emergence of semiosis coincides with the emergence of cells in which the chain of signs called DNA codes for the phenotype, although some locate this emergence earlier. Some see all processes in anorganic nature already as signs, smoke or heat as a sign of fire for instance, because they can *potentially* be taken as signs by a living organism.<sup>654</sup> Biosemioticians see the

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<sup>651</sup> In the two examples we infer the existence of a second thing from the existence of another thing, the primary thing which seems to be known in and of itself. We know the existence of fire from smoke, but the existence of smoke in and of itself. However, many semioticians make another step. Either in the direction of postmodernism (all signs refer to other signs, smoke is only smoke in contrast to clouds, dust etc. and thus is smoke only a sign), or – as most do like Peirce – in the direction of phenomenology, claiming we don't know smoke in and of itself (smoke an sich), but only the signs of smoke (its white colour, its moveability etc). Semiotics is thus well at home in the tradition of Kant (whose work Peirce studied intimately) for holding that *we cannot know the real essences of things in themselves, but only their nominal essences, i.e. a collection of marks or signs by which we recognize the objects as they appear to us.*

<sup>652</sup> Phrases such as 'man constructs his own reality' are notoriously vague, but unfortunately semioticians often use these words: "The *raison d'être* of semiotics is, arguably, to investigate whether or not reality can exist independently of the signifying codes that human beings create to represent and think about it. Is the physical universe a great machine operating according to natural laws that may be discovered by human reason? Or, on the contrary, is everything 'out there' no more than a construction of the human mind projecting itself onto the world of sensations and perceptions? Although an answer to this fundamental question will clearly never be possible, one of the important offshoots of the search for an answer has been a systematic form of inquiry into how the mind's products and the body's natural processes are interrelated [through signs]. (Sebeok 1994: iv)."

<sup>653</sup> Semiosis, according to Peirce, is the process of interpreting signs as referring to objects.

<sup>654</sup> See for instance John Dooly (translator of the Neo-Aristotelian *Tractatus de Signis* (1612) by Poinot) in Sebeok 1991: 63f. Dooly calls the potentiality of anorganic elements to become signs for living beings physiosemiosis. This view is of course much more problematic than the simpler view that with living cells semiotic processes emerged, because physiosemiosis entails that everything around us is potentially a sign, although it never need to become an actual sign since it can be destroyed before every being taken as a sign (and does it make sense to speak of a potentiality which is never actualized?). Some semioticians combine the view of physiosemiosis and biosemiotics in general with the speculative idea that the

emergence of multi-cellular organisms and the relations between these organisms primarily as processes of coordination and signal exchange. They treat the evolution of human language as only a special version of these processes: as sign processes between human brains or as biosemioticians prefer to say: between human minds (see Csányi, Hoffmeyer and in resp. Sebeok 1991: 34f, 111f). Signal exchange and human language in particular presuppose, according to biosemioticians subjectivity or a self which is understood as the inner interpretative centre or *Innenwelt* of an outer milieu or *Umwelt*. Thus we are back at biophenomenology and the work of Jakob von Uexküll in whose writings the idea of a semiotic self is already present.<sup>655</sup>

In sum, the work of biosemioticians seems to be compatible with Darwinism and some of it might be thought to be useful for this study. However, most of the intuitions in this field are poorly worked out and rather vague. Biosemioticians write a theory of everything in just about 10 pages. Moreover, although biosemiotics wishes to overcome the duality of ‘the realms mind and matter’, it rests on the age-old metaphysical idea of the two-fold nature of signs as material carriers of an immaterial, spiritual idea that exactly presupposes this duality. Just saying that signs participate in both realms is not enough to overcome dualism, otherwise the Cartesian pineal gland which miraculously connected the *res cogitans* with the *res extensa* would have sufficed to overcome dualism as well. In some branches of ethology signs are no longer primarily regarded as conveyers of information, but as manipulation devices or instructors. These manipulation devices don’t have the triadic structure of signs in the (bio)semiotic sense and can much better be integrated into a naturalistic picture of the world than the triadically structured signs. Thus, although biosemiotics seems not opposed to Darwinism, some of the work of its followers needs serious emendation and especially conceptual clarification to be of any use to Darwinism. Even with these emendations and clarifications one could wonder whether supporters of biosemiotics would ever give better clarifications than biologists of DNA transcription and the working of enzyme signals or (bio)linguists of the evolution of languages or whether it will just remain a sterile branch of semiotics.

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evolution of signs is teleological towards the goal of man’s understanding. Then we get close to Hegel’s panpsychism of which Peirce abstained, although he sometimes allured to it.

<sup>655</sup> See for instance Jakob’s grandson Thure Von Uexküll summarising his grandfather’s view on the self: “The semiotic self is the hidden interpreter in the radical sense that even the single cell has its semiotic self or its ‘Ego-ton’ (J. v. Uexküll 1940) as hidden interpreter of the impressions on its receptors. The private character of signs and their hidden interpreters (‘semiotic selves’) (Sebeok 1991: 457)”. On the ‘semiotic self’, see also Thomas Sebeok 1991: 333-344, 1978, and Hoffmeyer 102ff in Sebeok 1991. The boundary between the *Innenwelt* and the *Umwelt* is called the endo-exosemiotic boundary by semioticians. This boundary is also the boundary between what is (publicly) perceptible of a sign (i.e. its material side) and the private *understanding* of the referent of the sign (the signified content or sense), a referent which itself is again publicly (intersubjectively) perceptible.

## Appendix 3: Chomsky's theory

Chomsky has written numerous publications on his ideas on language (see literature list), from *Syntactic Structures* (1957) in which Chomsky first postulated his famous distinction between surface and deep structure to *The minimalist program* (1995) and more recent work within the outlines of this program which forms a serious revision of his earlier work, amongst which are other classics such as *Aspects of the Theory of Syntax* (1965), *Cartesian Linguistics* (1966), *Language and Mind* (1968), *The Logical Structure of Linguistic Structure* (1975), *Reflections on Language* (1975), *Rules and Representations* (1980), *Lectures on Government and Binding* (1981), *Knowledge of Language: its Nature, Origin and Use* (1986) and *Language and Thought* (1994). These books overlap partly, but there is also important development in them: the syntactic rules become more complicated, issues of binding (words that are syntactically connected) become more evolved, there is more attention to methodological issues such as making models as small as possible and introducing as few as possible layers between observed speech and the deep structure. The core of Chomsky ideas remains nevertheless the same and the changes in Chomsky's program can best be considered on the basis of the original program. That's why we explain Chomsky here using his most famous book of 1965 *Aspects of a theory of Syntax*,<sup>656</sup> which is the crowning work of the standard theory (1957-1965) covering not just *aspects*, but the *whole* of generative grammar Chomskian style. In the footnotes, we will indicate what has changed in the Chomskian program over the years and at the end we briefly list the major changes in the different phases of this program, from the standard theory (1957-1965), to the revised standard theory (1965-1973), the revised extended standard theory (1973-1981), the Government and Binding/Principles and Parameters theory (1981-1990) and finally the Minimalist Program (from 1990 till present).

Chomsky became famous by his attack on behaviourism regarding language use and language learning in psychology. He wrote a highly influential article (1959) against Skinner (1957), but his critique was also directed at Quine and Wittgenstein [51].<sup>657</sup> In this article he claims that hardcore black-box *empiricism* cannot account for man's ability to learn a language by "general mental learning faculties" such as "induction", "association", "generalisation" etc. [56]. Instead, he proposes more *specific* "innate" language faculties which he collectively called the "language acquisition device" [10]. In practice, these faculties are not concerned with the actual first language acquisition of children, but with a priori "mentalist knowledge" or "understanding" of an "innate grammar"<sup>658</sup> called "linguistic competence" [4]. In

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<sup>656</sup> Numbers in []-brackets in this § refer to this book.

<sup>657</sup> See also Chomsky 1965: 51 "[My] view contrasts strongly with the empiricist notion (the prevailing modern view) that language is essentially an adventitious construct, taught by "conditioning" (as would be maintained, for example by Skinner or Quine) or by drill and explicit explanation (as was claimed by Wittgenstein), or built up by "elementary "data-processing" procedures (as modern linguistics typically maintains), but, in any event, relatively independent in its structure of any innate mental faculties."

<sup>658</sup> From 1986 onward Chomsky uses the word 'I-language' instead of 'innate grammar': "in the hope of overcoming persistent misunderstanding of the technical notion 'grammar'. Since the origins of work in generative grammar in the 1950s, it has been pointed out that the term 'grammar' is being used with systematic ambiguity: to refer to the internal states of Jones's faculty of language, and to the linguist's theory of that state. But that usage proved confusing. I therefore suggested that we restrict the term 'grammar' ('particular' or 'universal grammar') to the theories constructed by the linguist, and refer to the

many places Chomsky explicitly places his postulate of an innate language acquisition device in the philosophical tradition of rationalism with its ideas about ‘speculative grammar’ and ‘innate ideas’, such as present in the work of Descartes (think of his 1966 book *Cartesian linguistics*), the Port-Royal logicians, Leibniz and Von Humboldt [47f]. Chomsky believes that the method to reveal man’s innate ideas of his innate grammar is to use introspection just as Plato did in the *Meno* regarding innate ideas of math [24]: people already know unconsciously what the innate grammar is like, but they need a linguist who points this out to them by asking them about the grammaticalness of certain constructions (but to consciously know the rules of one’s grammar or to become a fully-fledged linguist more is required, see footnote 658). Unlike Plato, however, Chomskian *linguists* don’t have an extra mental intuitive organ that allows them to see the nature of innate grammar; they have to rely on *empirical* “introspective reports” of native speakers [18] on the grammaticalness of certain constructions to test their models.<sup>659</sup> The art of linguistic science is to set up experiments in which a response to the grammaticalness of (minimally different) constructions brings about which competitive model of innate grammar is correct. Thus the ‘performance’ of native speakers reveals something about their original innate ‘competence’ of speaking a language, as Chomsky would put it [11].

With his theory Chomsky primarily wishes to explain the *creative* [6] aspect of human language which he sees as one of the key characteristics of human language: how we can understand, i.e. *interpret* and *form* “indefinitely many sentences using a finite number of lexical items” [15]. For him, this infinite number of possible sentences is guaranteed by the fact that, in principle, sentences can become as long as you like because one can always add an extra (embedded) element to them. The newly constructed sentences will remain ‘grammatical’, although (ultimately) ‘unacceptable’ for normal discourse [11]: ‘the man at the station wore a hat’ can be transformed into ‘the man at the station whose roof was renewed by my brother wore a hat’, ‘the man at the station whose roof was renewed by my brother who is married to John’s sister wore a hat’ etc.

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internal state that grammars seek to describe as I-languages (‘I’ to suggest internal, individual, intensional) (Chomsky 2003: 270 in *Chomsky and his critics* ed. by Antony and Hornstein).”

<sup>659</sup> The basic tool for testing details about innate grammar is to present subjects (native speakers) a list of sentences (often a list of minimally different sentences) and to ask them whether a sentence is grammatical or not. These sentences are isolated and contextless, almost ‘monadic’ entities that have to evoke, all by themselves, an image of a possible state in the world (a description). That really constrains what is seen as grammatical. In real speech with its topic-focus-articulation much less well-defined sentences occur that are perfectly well understood in the context of the previous words or sentences spoken. Moreover, native speakers, even linguists, often differ whether a certain sentence is grammatical or not; that makes grammaticalness a *statistic*, instead of an absolute, logical property of a well-formed or not string of symbols.



Because (*only*)<sup>660</sup> ‘embeddedness’ of sentences allows sentences to become indefinitely long, the grammatical property of ‘embeddedness’ is central to Chomsky’s theory of human syntax. But also the other way round, dissolving ambiguities to ‘understand’ the true meaning of sentences is another key motivation for Chomsky’s project. These ambiguities between the clearly visible ‘surface’ structure and the real ‘deep structure’ are often due to embedded elements in sentences and thus elucidating embeddedness can help to better understand human language [5].<sup>661</sup>

Chomsky himself speaks in the context of ‘embeddedness’ of the ‘iterative’ [137] character of grammatical rules, or the ‘recursive generation of sentence structures’ [141]. ‘Iteration’ and ‘recursion’ are fundamental properties of computer languages, and indeed Chomsky admits that he models natural languages after computer languages, that are, however, ‘simpler’ because the distinction between ‘deep structure’ and ‘surface structure’ does not apply to them [136] (which indeed was true for the programming languages of the Sixties – MH).

Just before Chomsky the project of logicism to reinterpret natural language as *logical* strings of symbols collapsed,<sup>662</sup> because natural language and its semantics were too complex and context sensitive to be cast into context-less logical well-formed strings of symbols; now Chomsky tried to formulate a richer artificial language of (universal) grammar, that was again ‘context independent’ [33], to

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<sup>660</sup> Without embeddedness it is hard to imagine how sentences can become indefinitely long: since a sentence has a subject, verb, an direct and indirect object, some temporal and locative elements what more can you add to a sentence except conjunctions? On the basis of embeddedness Chomsky also claims that human grammars should be modeled as type-0 or unrestricted grammars within the (now so-called) Chomsky hierarchy, a containment hierarchy of formal grammars (Chomsky 1959, 1956). The type-0 grammars generate exactly all languages that can be recognized by a Turing machine and which are also known as the recursively enumerable languages.

Note that Chomsky’s approach to language creativity (indefinite creativity) is typically that of a syntactician and not a semantician as he pinpoints language’s creativity in its capacity of making indefinite *long* sentences with a fixed number of lexical items; in practice, however, the length of a sentence is more or less limited, but the possibilities for a language to create new semantic concepts and new uses of old concepts, metaphorically or not, aren’t. This semantic aspect of creativity is just as important as the possibility of creating novel (grammatical) combinations with existing words (a number of combinations that is indeed limited if the lexicon and the sentence length is fixed).

<sup>661</sup> Embedded elements are not just relative clauses as in the example of ‘the man at the station’. other embedded elements can be ‘genitive constructions’: ‘the people’s man at the station’, ‘adverbs or adverbial constructions’: the ‘very tall man at the station’ etc. These embedded elements give rise to ambiguity, take for instance ‘flying planes can be dangerous’. The ambiguity lies in the word ‘flying’ which can be verbal or nominal, i.e. compare ‘flying planes are dangerous’ versus ‘flying planes is dangerous’ [84]. The task of generative grammar is to resolve these ambiguities.

<sup>662</sup> See for instance P.M. Pietroski article *Small verbs, complex events* (in *Chomsky and his critics* (2003) ed. Louis M. Anthony and Norbert Hornstein), that clearly links the prospects of the “traditional analytic project in philosophy – that of investigating thought by investigating knowledge” with Chomskian framework for the relation “of logical form to linguistic structure (209).” In his article, he gives the example of the inference of ‘Peter boiled the water’ to ‘the water boiled’, which is valid according to him, but not according to Fodor and Lepore. The latter because according to them the logical form of ‘to boil’ as a transitive verb is different from ‘to boil’ as an intransitive verb, whereas according to Pietroski by stipulating a deeper logical structure of the verb ‘to boil’ there is but one true analysis of ‘to boil’ which can account for both cases and the inference. See also Pietroski 2002.

In our analysis, Pietroski’s article only proves that just as the project of logicism was unattainable, so is that of Chomskian inspired analytic philosophy. The semantics of natural languages is too complex to be cast into the contextless ‘enriched logic’ of universal grammar; semantics has always precedence on syntax, as has been demonstrated in §3.25. And what about the validity of the inference? We would say the inference is valid in many contexts, but in some others not as for instance in the context ‘he boiled the water [but he failed because the gas run out].’

reinterpret the sentences of natural language as ‘computable strings’ of grammatical symbols (and lexical items), where ‘computable’ means ‘well-formed’ according to the (iterative) rules of grammar.<sup>663</sup> This computational view of language processing inspired almost a whole later generation of cognitive psychologists such as Ray Jackendoff and Steven Pinker as well as philosophers such as Jerry Fodor and Hilary Putnam who set forth functionalism in the theory of mind.

With this background information we can summarize Chomsky’s position on language learning. According to him man has an innate language acquisition device. This consists of a contextless universal grammar and the rules to apply it to the imperfect data a child is exposed to as it learns a language. These rules ‘delimitate’ the many ‘hypotheses’ [30] a child may have about the regularities of language use and the exposure to language use trigger the device to construct out of the universal grammar the ‘generative grammar’ [51] of the particular language the child is exposed to. ‘Generative’ in generative grammar has nothing to do with a grammar in development, ‘generative’ means generating in the sense of the German ‘(wieder)erzeugen’ as used by Von Humboldt and means the rules to *generate* correct well-formed strings [51]. On these strings ‘transformational rules’ (making a question out of a normal sentence, embedding a relative clause etc.) can work, which are also part of the grammar [141]. These rules are often language dependent. Basic to all strings, however, is the division in ‘Noun phrase’ and ‘Verb Phrase’ [65], each of which can be further ‘categorised’ in other ‘categorical subcomponents’ [142]. These divisions give rise to the famous Chomskian trees.

An example of such a tree is given below for the sentence ‘he happily smoked his cigar’.<sup>664</sup>

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<sup>663</sup> The comparison with computers becomes even stronger as Chomsky speaks for instance about ‘memory limitations’ of the human mind [15] and the ‘computation’ of linguistic ‘formula’. In work a little later he couches his theory expressively in terms of the Computational Representational Theory of Thought (CRTT): “I know of no other account that even attempts to deal with innumerable facts, and I know of no proposed explanation for the fact that our judgments and behaviour accord with certain rule systems other than the assumption that computation involving such rules and the representations they provide takes place in the mind – exactly the assumption one would make without hesitation in the study of perception and other topics. (1980: 129f) (...) Linguistics is the abstract study of certain mechanisms, their growth and maturation. We may impute existence to postulated structures at the initial, intermediate, and steady states in just the same sense as we impute existence to a program that we believe to be somehow represented in a computer or that we postulate to account for the mental representation of a three-dimensional object in the visual field (id. 188).” See also Chomsky 1995: 225-235; 1994: 40, 52; 1994: 153-4; 1986: 239. Elsewhere, however, he seems to renounce the comparison of the computations of a mind with the computations done by a computer, pointing to the difference between natural things and artifacts, but unfortunately he does not make clear in what regard natural computations differ from artificial/software computations: “It has been common to try to relieve uneasiness about computational approaches by invoking computer models to show that we have robust, hard-headed instances of this kind: psychology then studies software problems. That is a dubious move. Artifacts pose all kinds of questions that do not arise in the case of natural objects. Whether some object is a key or a table or a computer depends upon designer’s intent, standard use, mode of interpretation, and so on. The same considerations arise when we ask whether the device is malfunctioning, following a rule, etc. There is no natural kind of normal case. (...) Such questions do not arise in the study of organic molecules, nematodes, the language faculty, or other natural objects, viewed (...) as what they are, not in a highly intricate and shifting space of human interests and concerns (1994: 43-44).”

<sup>664</sup> The terminology used to name the grammatical elements, especially regarding the categorization of the lexical items, varies from author to author. Here is just an example what a tree might look like. The Government and Binding theory of the 80s first allowed for transverse connections between the branches of the tree, thus breaking down the strict vertical, hierarchical structure of the tree. The minimalist program found itself to have broken with the hierarchical sentence model. However, the *introduction* of linguistic phases in the models of the minimalist program can be argued to be hierarchical still.

1)	‘He’					‘happily smoked his cigar’
	Noun-Phrase					Verb-Phrase
						/ \
2)	‘He’					‘happily’ ‘smoked his cigar’
	subject, human			adverb	verb-object	
						/ \
3)	‘He’	‘happily’	‘smoked’			‘his cigar’
	idem	adverb	verb	direct object-phrase		
						/ \
4)	‘He’	‘happily’	‘smoked’	‘his’	‘cigar’	
	idem	idem	idem	poss. pronoun	count noun, concr.	

For many sentences more than one tree is possible, due to semantic ambiguities.<sup>665</sup> These ambiguities may arise, however, only because one forces sentences unto the Procrustes’ bed of universal grammar.

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<sup>665</sup> The above sentence could already be read as ambiguous, namely as ‘Happily, he smoked his cigar’ i.e. ‘I am relieved that he smokes his cigar [so he didn’t start the ugly habit of chewing on tabacco],’ more commonly expressed as ‘Fortunately, he smoked his cigar’.

In general, the tree model can easily be criticised from the perspective of semantics and pragmatics. Although there is certainly hierarchical structure in a sentence (which can be expressed in grammatical rules), the smallest elements of a sentence can give a completely different meaning to a sentence as a whole, and elements of for instance a noun phrase can not only determine the meaning of the noun phrase as such (as in the hierarchical model) but the verb phrase as well and vice versa, thus breaking down the hierarchical model (think of the participles). In fact, any word in a sentence can actually qualify the meaning of any other word in it. Compare ‘Mother and Little John went to the doctor’, with ‘Jack and Jill went to the doctor’ and ‘Jill and Jack went to the doctor’: the people mentioned in the subject qualify ‘doctor’ (in the first sentence more likely a child doctor, in the second a general practitioner and the third a gynaecologist) and ‘went’ (the nature of the gathering: in the first sentence maybe a protective, caring form of accompaniment, in the second a supportive and in the third a happy, amorous being together) etc.

Another semantic reason for breakdown of the hierarchical model is the study of *topic-focus-articulation*. This is a pragmatic field of studies about the informative content of elements of a sentence, which ironically was partly founded by Chomsky and Halle with their influential *On accent and juncture in English* (Chomsky et al.1956) and *The Sound Pattern of English* (idem, 1968). Chomsky thought that one element in every sentence, determined by the (universal) Nuclear Stress Rule, was stressed more than all else (an element that could be labeled as the focus of the sentence). Currently, the topic is defined as that part of the sentence that is being talked about. Once stated, the topic is therefore ‘old news’, i.e. the things already mentioned and supposed to be understood. The focus on the other hand determines which part of

In the ‘categorical subcomponents’ lexical items are fit. These lexical items are fit into a fixed number of categories: nouns for instance in groups as ‘count nouns/mass nouns’, ‘animate/inanimate’, ‘human/non-human’, ‘abstract/concrete’ etc. [108] and verbs in ‘abstract/concrete’, ‘transitive, intransitive’ etc. [114f]. Ideally, one should make a hierarchical tree of all these categories, so that it could be used for an a priori “system of all possible concepts” [160].<sup>666</sup>

The categorisation of lexical items can be seen as part of grammar or of semantics; Chomsky is unsure what position to take [150], although he opts for the latter. Take the sentences: a) ‘man the kicked ball the’, b) ‘the man kicked the ball’, and c) ‘the house kicked the idea’. Sentence a) is clearly a-grammatical, but for the sentences b) and c) the case is different. One can either say i) that both sentences b) and c) are grammatical, but that only one of them makes sense or one can say ii) that the verb ‘to kick’ (a lexical item) requires as input (i, verb, ii) an ‘animate’ noun (i) an a ‘non-abstract noun’ (ii). Only when this input-rule is satisfied, is the sentence grammatical. This last position is Chomsky’s position. Strict rules for the input of verbs are, however, hard to formulate due to meaningful ‘metaphorical interpretations’, which make Chomsky utter that “any boundary [between syntax and semantics] is quite tentative” [163].<sup>667</sup>

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the sentence contributes the most important (new) information. Topic and focus can be marked by many means (word order, using passive voice to transform an object into a subject, emphasizing the topic using clefting, using periphrastic constructions or topic markers as in Japanese and Korean), but also by stress and ‘dislocations’ – ‘dislocations’ from the perspective of isolated sentences, but perfectly well located for pragmatic means. Since sentences are read from *left-to-right* (or the other way round as in Hebrew) one often needs word order (what comes earlier and what later) to get the message across. Chomskian trees on the other hand presuppose a top-down model instead of the natural left-to-write flow of information, as such they are projecting an artificial model upon language claiming that the natural left-to-right-order is but a ‘transformation’ applied to natural ‘innate grammatical’ structure of the sentence.

<sup>666</sup> Chomsky knows that “the subcategorization [of lexical items in categories] is typically not strictly hierarchic, but involves rather cross classification. Thus, for example, Nouns in English are either Proper [John, Egypt] or Common (boy, book) and either Human (John, boy) or non-Human (Egypt, book). Some rules (for example, some involving determiners) apply to the Proper/Common distinction; other (for example, rules involving choice of relative Pronoun) to the Human/non-Human distinction. [79].” Despite this knowledge, Chomsky sticks (at least in 1965) to the formation of (unique) hierarchical trees of lexical items.

Further, the categorization of the lexicon Chomsky presents seems universal, but is actually highly typical of the language he speaks; but rarely does he add that the categorization is the categorization “in English” [79]. If he knew some more exotic languages he would give up a unique categorization, at least a unique innate and universal, fixed categorization. We saw in §3.25 that languages can have many determiners for for instance long objects, small objects, things to eat etc. Can anyone believe these are universal and innate?

The best one can do to find categorization of the lexicon is just by empirically observing what categories are present in a language, which might or might not be placed in a hierarchy (being human taking precedence over being long in a system of grammatical rules). However this hierarchy will probably have many exceptions (natural gender versus grammatical gender etc.), especially from a diachronic perspective. Any claim of an a priori system of all possible concepts is regarded with scepticism by post-transcendental, post-Kantian philosophy.

<sup>667</sup> The most famous example Chomsky gave of a grammatically correct sentence that was nevertheless meaningless was: “colourless green ideas sleep furiously”. Writers have nevertheless attempted to provide the sentence meaning through context. Even an informal literary competition was held at Stanford University in 1985, in which the contestants were invited to make Chomsky’s sentence meaningful using not more than 100 words of prose or 14 lines of verse (see <http://www.linguistlist.org/issues/2/2-457.html#2> retrieved at April, 2<sup>nd</sup> 2008). An example entry from the competition, from C.M. Street, is: “It can only be the thought of verdure to come, which prompts us in the autumn to buy these dormant white lumps of vegetable matter covered by a brown papery skin, and lovingly to plant them and care for them. It is a marvel to me that under this cover they are labouring unseen at such a rate within to give us the sudden awesome beauty of spring flowering bulbs. While winter reigns the earth reposes but these

## Changes since the standard program

This initial sketch of the standard program (1957-1965) may suffice. The extended standard program (1965-1973) added more syntactic constraints and introduced X-bar theory, first proposed by Chomsky (1970). X-bar theory does not anything substantial, it consists in a general rule for structures containing X (an arbitrary lexical category such as a noun, a verb, an adjective or a preposition) and i) an optional specifier, ii) an optional adjunct, iii) or any number of optional complements; the bar derives from the notion of X + one of i, ii or iii as X with an overbar (X̄). They were introduced to account for linguistic substructure in a sentence that ordinary grammatical analysis did not see, but that could be made visible using the *substitution test*.<sup>668</sup> The introduction of X-bar phrases made the Chomskian trees even longer and notoriously complex.

The revised extended theory (1973-1980) contained restrictions upon X-bar theory as developed by Jackendoff (1977), assumptions on the position of the Complementizer (i.e. a word such as *that* on their position and or omission in sentences: *He hopes you go ahead with the speech* versus *He hopes that you go ahead with the speech*), and the controversial rule 'move'. The term refers to the relation between an *indexed constituent* and its *trace*, an empty (phonologically null) category that occupies a position in the syntactic structure marking the initial position of a linguistic expression that is moved in the course of a transformation.<sup>669</sup> The rule marks a shift of attention in generative grammar in around 1980, away from focusing on specific rules (the only 'rule' is 'move') to 'Principles and Parameters' constraining them. The complexity of the previous rules is now, however, reflected in the complexity of the principles and parameters of the Government and Binding theory (1980-1990).

The shift to Government and Binding theory<sup>670</sup> is a bigger shift than the previous ones. It is the first theory to use principles and parameters-theory (Chomsky 1981) which is now dominant in formal grammar research. The central idea of this theory is that a person's syntactic knowledge can be modelled with two formal mechanisms: 1) a finite set of fundamental *principles* that are common to all languages; e.g., that a sentence must always have a subject, even if it is not overtly pronounced,

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colourless green ideas sleep furiously." It thus seems hard to rule out *a priori* that a grammatical sound sentence is meaningless, even though some sentences are unlikely to be formed.

<sup>668</sup> For instance, to the sentence, 'He studies linguistics at the university,' someone could reply, 'Oh, she does, too'. The word 'does,' here, stands for the entire phrase, 'studies linguistics at the university' which is called a Verb-bar or V-bar phrase and is seen as a complete unit of the whole sentence. In other words: if the V-bar phrase above were not defined as such, the sentence would have three separate phrases directly underneath sentence: the Noun phrase (He), the Verb phrase (studies linguistics), and the Prepositional phrase (at the university). To substitute for two of them, together, as shown, implies, however, that these two, together, make up one V-bar phrase within the sentence.

<sup>669</sup> An example: the statement 'I want to go' can be contracted to 'I wanna go' with 'want to' contracted to 'wanna'. 'Want to' can, however, not always be contracted to wanna. Take the statement 'I want John to go', which is transformed into an 'echo'-question: You want whom to go? and which we normally use as Who do you want to go? This last question can, however, not be contracted to \*Who do you wanna go? Move  $\alpha$  is used to explain this: if 'who' is moved to the beginning of the sentence, it will leave a (phonologically null) trace. The existence of the trace will block the contracting of 'want' and 'to'. Thus in generative grammar using Move  $\alpha$  the question is written (in first instance): 'Who [index] do you want to [trace] go?' Suffice to say that this complex explanation (using metaphorical and unclear notions such as blocking, moving, echo questions, phonological null traces etc.) is criticized firmly by people that don't support generative grammar. On a similar contraction of 'going to' to 'gonna', see Deutscher 2005.

<sup>670</sup> The name refers to two central subprinciples of the theory: *government*, which is an abstract syntactic relation, and *binding*, which deals with the referents of pronouns, anaphors, etc. It made the tree-rules more complex and elements in the tree more interdependent.

2) a finite set of (binary) *parameters* that determine syntactic variability amongst languages; e.g., a binary parameter that determines whether or not the subject of a sentence must be overtly pronounced (this example is sometimes referred to as the Pro-drop parameter) (Baker 2001, Haegeman 1994).

Within this framework, the goal of linguistics is to identify all of the principles and parameters that are universal to human language. As such, any attempt to explain the syntax of a particular language using a principle or parameter is cross-examined with the evidence available in other languages. The research program to identify the particular parameters that transformed universal grammar into the grammar of a specific language, lead to phrases such as *The language lottery* (Lightfoot 1982). As the principles and the possible parameter settings are innate, language acquisition is seen as a process of exposure that merely triggers the parameters to adopt the correct setting of the language of the community in which the child is raised.<sup>671</sup>

If you add enough parameters and make the universal grammar as general as possible, however, any mathematician or natural scientist can tell you beforehand from experience that this research program will always succeed and is thus empirically void: you can model any data pattern, given enough parameters.<sup>672</sup> Chomsky c.s. seemed to have realized that, as the latest paradigm in ‘generative grammar Chomskian style’ is called the Minimalist Program (Lasnik 2002, Chomsky 1995) which is basically Government and Binding theory + *economy of derivation* and *economy of representation*. Economy of representation means that grammatical structures must exist for a purpose, i.e. the structure of a sentence should be no larger or more complex than required to satisfy constraints on grammaticality (as in X-bar theory with its many unary branches); economy of derivation means that transformations only occur to transform *uninterpretable features* into *interpretable features* (i.e. the word ‘dogs’ is interpretable as ‘more than one dog’ because of the ‘s’ behind ‘dog’). Both notions are somewhat vague, and the precise formulation of these principles is a major area of controversy in current research (Lappin et al 2001). That is not surprising as there is no general theory of simplicity and complexity (§1.32); their formulation is nonetheless a laudable step towards Occamized theories. An additional

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<sup>671</sup> Note that this automatic learning process by small cues of the Minimalist Program is a big change from Chomsky’s original presentation of learning as a process in which a child has a mental overview of a (delimited) set of hypotheses about possible grammars out of which upon being exposed to linguistic phrases it chooses the correct grammar of the language of the community in which it is raised [1965:30]. Both conceptions of the process of language learning seem to have been present in the work of 80’s already. See Chomsky 1980 : 136 “Learning seems pretty much like what Peirce called ‘abduction’, a process by which the mind forms hypotheses according to some rule and selects among them with reference to evidence and, presumably, other factors. It is convenient sometimes to think of language acquisition in these terms, as if a mind equipped with universal grammar generates alternative grammars that are tested against the data of experience with the most highly valued one selected; I have occasionally used this metaphor, but I don’t think it should be taken seriously. If we take it partially seriously, then under this concept of learning as ‘abduction’ or ‘self-design’, the question whether language is learned or grows will depend upon whether the mind equipped with universal grammar presents a set of grammars as hypotheses to be selected on the basis of data and an evaluation metric, or whether the steady state grammar arises in another way – for example, by virtue of a hierarchy of accessibility (...) and a process of selection of the most accessible grammar compatible with given data.” In later work the latter position is identified with the minimalist program.

<sup>672</sup> See also Tomasello 2003 and Sampson 1999 who indicate that when complex sets of data are studied and modeled, spurious structures are often found whereas in reality there are none whatsoever. They question the reality of any grammatical universals (except those arising from cognitive considerations as they should as cognitive linguists, see §3.24) and argue that the proposed examples are based on a too-narrow sample of languages, or on forcing ‘odd’ languages into the prescribed form.

aspect of minimalist thought is the idea that the derivation of syntactic structures should be *uniform*; that is, rules should not be stipulated as applying at arbitrary points in a derivation, but instead apply throughout derivations. This is a laudable step as well as it will remove ad-hoc speculation from generative grammar. It has resulted in Bare-Phrase Structure (or BPS), which tries to overcome the problems of X-bar theory and does no longer distinguish between deep structure and surface structure (a distinction which started the Chomskian program in 1957). Notable differences are 1) that BPS structure is derivational (built from the bottom up) whereas X-Bar Theory is representational (a structure for a given construction is built in one fell swoop, then the lexical items are inserted into the structure), 2) that BPS does not have a *preconceived structure*, while in X-Bar Theory, every phrase has a specifier and a complement and 3) that BPS has only binary ('minimal') branching while X-Bar Theory permits both binary and unary branching.

Especially, the first difference is important as it seems to introduce a true theory of performance (instead of competence); however, that is not true: at the background of BPS is still the universal grammar with its representational structure in which the derivational rules of BPS are fit; moreover, the BPS structure of sentences was soon changed back by Chomsky into a non-derivational phrase structure with sentences a priori divided into phases, whereby the movement of a constituent out of a phase is (in the general case) only permitted if the constituent has first moved to the left edge of the phase (the so-called *Phase Impenetrability Condition*).

Thus although there are major changes to Chomsky's ideas, the basic ideas have remained the same and against the Minimalist program much of the same critique can be addressed as against the earlier versions of the Chomskian program.

# **Korte samenvatting van het proefschrift “Darwiniaanse verklaringen van de oorsprong van de taal”**

## **(short summary in Dutch)**

In hoofdstuk 1 wordt op basis van de huidige stand van zaken in de biologie en de wetenschapsfilosofie een ideale Darwiniaanse verklaring voor een biologisch fenomeen opgesteld; centraal daarin staan nieuwe, scherpe definities van de begrippen ‘replicatie’, ‘variatie’, ‘selectie’ en ‘fitness’. Er wordt tevens getoond dat de meeste vormen van seksuele selectie tot eenvoudige vormen van natuurlijke selectie kunnen worden teruggebracht, behalve ‘ornament selectie’.

In hoofdstuk 2 wordt het begrip ‘cultuur’ gedefinieerd als alles wat niet genetisch overgeërfd wordt van generatie op generatie, en wordt erop gewezen dat menselijke cultuur verschilt van diercultuur voorzover mensencultuur veel sterker een cumulatief karakter heeft waarbij de ene innovatie op de ander voortbouwt. Er worden 8 modellen voor culturele evolutie geformuleerd, 2 ontogenetische en 6 phylogenetische. De ‘dual-inheritance’ theorie blijkt de beste kaarten te hebben omdat deze theorie het best recht doet aan de interactie tussen natuurlijke en culturele evolutie. Tot slot wordt de ideale Darwiniaanse verklaring voor biologische fenomenen aangepast om culturele fenomenen te kunnen omvatten.

In hoofdstuk 3 wordt het begrip ‘taal’ gedefinieerd met behulp van Hockett’s design criteria. Daarna wordt getoond waarom Chomsky’s theorie van de generatieve grammatica een van de grootste obstakels is geweest voor verklaringen van de oorsprong van taal. Inzichten vanuit 6 verschillende wetenschapsgebieden: biolinguïstiek, paleontologie, archeologie, onderzoek naar diertaal (ethologie) en taalverwerving, en sociolinguïstiek tonen dat Chomsky’s paradigma onhoudbaar is; deze wetenschapsgebieden leveren ook de te identificeren ‘proximate mechanisms’ die elke ‘ultimate explanation’ van een evolutionaire adaptatie zoals taalgebruik nodig heeft om toetsbaar te zijn.

In hoofdstuk 4 wordt de oorsprong van taal vanuit het perspectief van de ‘dual-inheritance’ theorie beschouwd, waarbij expliciet wordt getoond dat voorbeeldverklaringen voor de oorsprong van de taal vanuit een van de andere evolutionaire modellen tekort schieten. Er wordt getoond dat taal waarschijnlijk een vroege adaptatie is die langzaam is geëvolueerd door natuurlijke, (niet seksuele) selectie, waarschijnlijk van het begin af aan als gesproken in plaats van gebarentaal en sterk cultureel bepaald is. Er wordt getoond dat taal primair ontstaan is in de context van socio-politiek, maar dat het ook van nut kan zijn geweest voor o.a. jagen, het maken van artefacten en onderwijs. We bespreken ten slotte de evolutie van linguïstische proto-vormen vanuit het perspectief van de ‘dual-inheritance’ theorie.

De slotconclusie is dat hoewel niet alle aspecten van de evolutie van taal reeds verklaard kunnen worden en hoewel meer empirisch werk nodig is om de details van de onderliggende mechanismen die een rol spelen in taalevolutie te onderzoeken, onze verklaring niettemin dicht in de buurt komt van het in hoofdstuk 2 geformuleerde ideaal.





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## About the author

Michel Heijdra studied physics from 1997 to 2002 in Leiden, where he graduated cum laude with a master thesis on the history and foundations of the concepts of space and time at the Institute for the History and Foundations of Science in Utrecht. Part of his studies of physics was a 1-year minor in Science Based Business for which he wrote a second thesis on the technological and economic chances of the lab-on-a-chip technology at the Mesa+-institute for nanotechnology and the faculty of technology studies in Twente. In parallel he studied philosophy in Leiden which he finished summa cum laude in 2003 with a master thesis on the concept of life in Hegel and Darwin. From 2002 till 2003 he spent 8 months in Berlin studying mainly German idealism. He also received his master degree (summa cum laude) in the philosophy of science from Leiden University in 2003.

He started working on his PhD in 2003, and was a visiting scholar in Princeton from the end of summer 2009 till the beginning of 2010. For 3 years he taught bachelor-2 students how to read philosophical texts using texts from Hegel and Wittgenstein, he was a guest lecturer at multiple master seminars on respectively Heidegger, globalization theory and the evolution language, and he was responsible for the literature assignment of multiple master students. He translated, together with German scholar Hans Verboven from the University of Antwerp, the work *Der Arbeiter* from Ernst Jünger and will soon publish, together with philosopher Suzanne Metselaar, a volume with interviews of prominent Dutch philosophers on their method of philosophizing. Apart from these 2 volumes, he wrote more than 15 publications, from articles in international journals, to chapters in books, papers in conference proceedings, lemmas in dictionaries and popularizing essays in popular media.

From 2006 till 2007 he was president of the national PhD network (PNN Promovendi Netwerk Nederland) where he interacted with multiple stakeholders (Ministry of Education, VSNU, rectorenoverleg, labour unions, local PhD councils, journalists and others) to improve the working conditions and position of PhD candidates in the Netherlands. After his full-time work as PhD, he started working at consultancy firm McKinsey & company where he worked in the financial and banking sector and worked on sales and organization strategy and risk assessment. He recently moved to the Generale Thesaurie of the Ministry of Finance in The Hague, where as a senior policy maker he will focus on financial stability related issues.

Michel Heijdra lives together with Agnes Penaat in The Hague and they are expecting their first child in Autumn 2009.