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Defusing eliminative materialism: Reference and revision

Maurice K. D. Schouten^a; Huib Looren de Jong

^a Department of Psychology, Vrije Universiteit, Amsterdam, The Netherlands

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Defusing eliminative materialism: reference and revision

MAURICE K. D. SCHOUTEN & HUIB LOOREN DE JONG

ABSTRACT *The doctrine of eliminative materialism holds that belief-desire psychology is massively referentially disconnected. We claim, however, that it is not at all obvious what it means to be referentially (dis)connected. The two major accounts of reference both lead to serious difficulties for eliminativism: it seems that elimination is either impossible or omnipresent. We explore the idea that reference fixation is a much more local, partial, and context-dependent process than was supposed by the classical accounts. This pragmatic view suggests that elimination is not the prime model for understanding the complex relations between the mind and brain sciences, and that we have little ground for concluding that in general psychological kinds do not exist. We suggest that reference changes are better seen as continuous rather than completely eliminative.*

1. A brief historical tour of reduction and elimination

1.1. Introduction

Eliminative materialism (henceforth EM) has been a hot topic in the philosophy of mind since Paul Churchland put the issue on the agenda in 1981 (Churchland, 1989a), after earlier attempts by Rorty (1970) and Feyerabend (1963). EM was originally proposed as an alternative to classical reductionism, because the latter position was deemed unable to account for theory *revision*, be it moderate or revolutionary, in science. The eliminativists' view in contrast was that revision abounds in science, and often is so massive that the things spoken of in the overthrown theory can no longer be thought to exist. Thus, the possibility of a massive revision of the way theoretical terms refer to entities is an important background assumption of eliminativism. "Phlogiston", for instance, was eliminated as a scientific term because Lavoisier's oxygen theory, suggesting a radically different and much more adequate conception of combustion, made clear that "phlogiston" must have been referentially empty. It has been suggested that this assumption of massive theory revision, however, leads to thorny questions about what the reference relation involved amounts to. In this paper, we will present a critical analysis of eliminativism; to that aim, we explore the underlying notion of reference of theoreti-

M.K.D. Schouten, Department of Psychology, Vrije Universiteit, De Boelelaan 1111, 1081 HV Amsterdam, The Netherlands. Email: MKD.Schouten@psy.vu.nl

cal terms. A review will be offered of the main theories of reference change. We will then suggest a modest and ecumenical proposal for reference revision underwriting a form of explanatory pluralism (McCauley, 1996). Our final conclusion will be that the pop-version of eliminativism—the “nothing-buttery” view—is a simplification which from a more sophisticated view of reference and theory change can be effectively defused. We start with a brief historical tour of the main issues.

1.2. *Reduction and the identity theory*

Within the framework of logical-positivist philosophy of science, a view on mental terms was formulated which held that mental and physical events can be empirically identified (more or less like heat and average molecular kinetic energy). This so-called *identity theory* held that there are lawful psychophysical correlations between mental kinds and physical kinds (Place, 1956; Feigl, 1958; Smart, 1959). Because these mental and physical kinds are nomically coextensive, it may be possible to *reduce* psychology to neuroscience.

The received view of reduction-as-derivation, on which the identity theory was based and which in one way or another still continues to exercise its influence, was articulated in its canonical form by Ernest Nagel (1961). A smooth theory reduction required the satisfaction of two necessary and jointly sufficient conditions for a reduction to take place: (i) *connectability*, and (ii) *deducibility*.

According to Nagel, theory θ_2 is reduced to theory θ_1 when, under a set of boundary conditions, the former is a logical consequence of the latter. For this to be possible we need first to *connect* the vocabularies of θ_1 and θ_2 . This is done by means of so-called bridge laws which involve term by term connections between θ_2 and θ_1 [1]. Bridge laws are usually taken to be biconditionals (“iff” statements), or nomologically coreferential statements, hypothetically expressing referential identities (Schaffner, 1967) [2]. A paradigm case of a bridge law is the identification of “temperature” and “mean molecular kinetic energy”.

The identity theory suggests that each kind predicate of psychology may be shown empirically to be coreferential with some kind predicate of a more basic theory, presumably those of a full-grown neuroscience. On this view, mental kinds are identical with neuroscientific kinds. In the classic example, “pain is the firing of C-fibers”, “pain” is claimed to refer to C-fiber stimulation. In addition to the possibility of nomic correlation, psychology is *entailed* by neuroscience. Thus, neuroscience makes psychology redundant in a sense. In another sense, however, psychological kinds are legitimated ontologically by identifying them with neuroscientific kinds.

1.3. *A problem with reduction*

Classical reductionism has been challenged by a number of authors who have emphasized that theory changes are not as neat and tidy as was formerly supposed. Usually, the reduced theory is to some extent corrected or revised in the process of identification and reduction (Schaffner, 1967; cf. Nickles, 1973; Hooker, 1981).

Although theory changes may not be as massive as, for instance, Paul Feyerabend (and Thomas Kuhn) suspected, to some extent they do occur, usually under pressure of new theoretical and empirical developments. Kenneth Schaffner's prime example is that the concept of *gene* of classical genetics (θ_2) has changed under pressure of developments within molecular genetics (θ_1). Therefore, it must be taken into account that the reducing theory θ_1 may change the would-be reduced theory θ_2 . Apparently, there is not always, indeed there may hardly ever be, a smooth reduction from θ_2 to θ_1 ; reduction is much more "bumpy" than that (Churchland, 1979). Only in ideal cases can one theory, θ_2 , be classically reduced to a new and more general theory, θ_1 . In all other cases, θ_2 must be revised, so that a new theory θ_2^* closely resembling θ_2 is created. It is the revised theory that is entailed by θ_1 . In sum, by and large it is only legitimate to talk of *modified* or *revised* theories θ_2^* being reduced to theories θ_1 . Theories θ_2^* are like theories θ_2 but they do not coincide. In these cases then, reduction takes place, not strictly, but only approximately. Reductions as envisaged by the classical reductionist are not likely to happen in science. Rather, reductions are almost always approximative or revisionary. As we will see, it is this feature of theory succession that EM needs.

2. Eliminativism and its discontents

2.1. Elimination

As mentioned, classical reduction in principle leaves ontology untouched: the reduced theory is preserved; bridge laws guarantee that the taxonomy of the reduced theory is *vindicated* by the taxonomy of the reducing theory. However, some authors have suggested that there may be more to theory change than mere ontological absorption. For instance, Paul Feyerabend's point was more drastic than Schaffner's (Section 1.3) since he argued that in science there is always revision and often such revision is so massive that we cannot but speak of replacement, rather than derivation (Feyerabend, 1981, p. 80). There is no theory θ_2^* closely resembling theory θ_2 . It is often the case that these competing theories are *dissimilar* to such an extent that these theories are not even about the same objects and processes. In such cases, identity statements cannot be formulated.

Whereas the identity theory derived much of its inspiration from Nagel style reductionism, EM plays a different card. Cleaving to the idiom of post-positivist authors like Kuhn and Feyerabend, eliminativists usually claim that belief-desire psychology and computational neuroscience are radically incommensurable, so that the former will be replaced by the latter, as it proves much more empirically and conceptually successful. In recent years, EM has been flamboyantly defended by the Churchlands. In the Churchlands' view, market-place psychological truths are likely to end up as real *idola fori*, as long-standing illusions that do not refer to anything. Developments within cognitive neuroscience show that psychology as we know it today will go the way of a long and colorful procession of equally unfortunate theories which have been replaced in centuries past. One parade case of elimination is the repudiation of phlogiston in chemistry (e.g. Kuhn, 1970b; Hooker, 1981;

Churchland, 1979, 1989b). The Churchlands have claimed that belief-desire psychology will be similarly wiped out as its kind terms are proven to be otiose.

The possibility of replacement shows that the classical account of reduction is inadequate. In addition, it suggests that there is much more to be said about theory change: what does it mean to reduce, replace or revise a theory? A plausible view (e.g. Churchland & Churchland, 1994) seems to be that there is a continuum from classical reduction (with complete invariance of meaning and reference) to elimination (with radical change of meaning and reference). This indicates that the underlying dimension of theory change may have something to do with reference. The classical view of reduction has no way to relate non-referring and referring theories. We will see that this failure is compensated by the introduction of a so-called *analog relation* between theories.

2.2. From reconstruction to deconstruction

Throughout their work, the Churchlands have been looking for a more sophisticated account of theory change. In their view, we should construct an *analog* or a (roughly) “equipotent image” (Churchland, 1979, 1989b) of θ_2 , i.e. θ_2^* , within θ_1 , in order to derive it from θ_1 . On their account, deduction is, not an intertheoretic, but an intratheoretic dimension to reduction (e.g. Churchland, 1989b, pp. 47–52): it is not something that occurs *between* theories, but rather *within* theories. This is to account for the fact that often reduced theories have an ontology that is non-existent. Often, according to both Churchlands, theory successions show that the world as catalogued by the earlier theory does not exist, and a non-referring theory θ_2 cannot be connected to a referring theory θ_1 via bridge laws. In such cases there cannot be coreference, let alone *nomological* coreference.

Paul Churchland (1979) proposed a new conception of Nagel’s connectability requirement that is less stringent: intertheoretic connections usually involve nothing more than mere ordered pairs of expressions, such as <“temperature”, “mean molecular kinetic energy”>, <“lightning”, “discharge of static electricity”> or <“cloud”, “mass of tiny particles in suspension”>. According to Churchland, we only need “the minimal assumption that the second element of each pair truly applies where and only whenever the first element of each is normally *thought* to apply” (1979, p. 83). In this way, Churchland surgically removes the core ingredient of referential stability from the classical picture of reduction: it is not required that reference is invariant for reduction to occur. Thus, he eagerly moves to an account that primarily holds that theory change proceeds by way of *transplantation* of vocabularies. This view opens up the possibility of “replacement-reductions” (Sklar, 1967), i.e. large-scale theory changes in which the older theory contains terms that are non-referring.

According to the Churchlands, the connection between theories is much looser than the one dictated by Nagel’s connectability condition: we need only construct ordered pairs of theoretical terms, rather than material-mode identity statements. Whether the *successor relation* between theories is to be characterized as a reduction or a replacement-reduction depends on the *degree of similarity*, or closeness of the old

theory θ_2 and its image θ_2^* within the new theory θ_1 . At one extreme, these alternatives are commensurable; at the other, they are *incommensurable*. In the former case both theories are continuous and stand in a smooth reduction relation, in the latter case both theories are *discontinuous* and the one theory is discredited by the other. The commensurability or incommensurability depends on the amount of *revision* or *reconstruction* that was required to map θ_2 via θ_2^* on to θ_1 . In the case of a reduction, the revised image is a perfect mirror image of θ_2 , thus permitting (post-hoc) identity statements and a verdict of classical reduction. What happens to the earlier ontology, then, is dependent upon what happens to the earlier theory (is it reduced or eliminated?), and not the other way round (Churchland, 1989b, p. 50). Whenever there is commensurability, there is ontological conservation; whenever there is incommensurability, there is ontological elimination. In between, there is a continuum of less radical to more radical revisions.

All in all, smooth reductions between fitting theories are rare. Most of the time there is considerable reconstruction; even the succession of thermodynamics by the kinetic theory is not an example of pure classical reduction since even here elimination rears its head. Hooker (1981, pp. 48–49) says: “thermodynamics is simply conceptually and empirically wrong and must be replaced”. Although the situation may not be as serious as in, for instance, the phlogiston case, even in this classic example we witness considerable reconstruction. Theory changes almost invariably lead to considerable revision, so that we are led to a verdict of replacement. This is strongly reminiscent of Feyerabend’s doctrine of semantic variance and incommensurability that was intended to silence reductionism once and for all: it is hardly ever possible to deduce one theory from the next (Feyerabend, 1965b, p. 227, n. 19; Scheffler, 1967; Devitt, 1979). Apparently, it is not steady cumulation and continuity that plays a major role in theory succession, but rather incommensurability and elimination.

3. Reference, revolution and revision

3.1. *Introduction: a puzzle about reference*

Sometimes theory changes are so profound that we are forced to conclude that the two theories are separated by deep ontological discontinuity. Remember the claim put forward by EM, namely that science will ultimately dispose of psychological concepts and entities. This claim has been stated in a slightly more precise way that involves the notion of reference. Paul Churchland states that certain theories are replaced and eliminated, because those theories suffer from “massive referential disconnection” (Churchland, 1989b, p. 284). When the conceptual body of a theory is shown to lack firm referential contact with the world, then this theory will be an obvious candidate for elimination.

In the Churchlands’ view, neuroscience is well on its way to showing that belief–desire psychology is thus massively disconnected; to them it is crystal clear that psychology’s concepts do not map on to parts of nature. In other words, belief–desire psychology is one of those theories that is massively referentially

disconnected. Before eliminativists are in a position to make such a bold claim, however, they should make clear what they mean by the term “referential (dis)connection”. What makes it the case that a theoretical term is or is not connected to natural reality?

The answer to this question is not as straightforward as it is often taken to be. Stephen Stich, searching as he says for a deconstruction of the deconstruction of mind, has pointed to the fact that reference raises a painful dilemma for the eliminativist (Ramsey *et al.*, 1990, pp. 502–503, 521, n. 6; Stich, 1991, 1994, 1996). Following a suggestion by William Lycan (1988), he notes that there are two options for the eliminativist to give flesh to the doctrine of referential disconnection. Either you choose the *descriptive theory* of reference, which is a standard move for eliminativists, or the *causal-historical theory* of reference. However, both moves appear to be damaging to the eliminativist position, be it in different respects.

The descriptive theory was defended by Rudolf Carnap, Gilbert Ryle and David Lewis among others who have all, in one way or another, trod the Fregean path. The theory claims that reference is fixed by meaning. Theories function as clusters of identifying descriptions, or as a “sortal physiognomy” (Bell, 1979), by which we can pick out the objects and processes these theories refer to. The causal-historical view on the other hand claims that reference is fixed in a baptismal event after which this reference is passed along a chain of communication. For instance, the reference of the word “gold” was fixed when someone pointed to a sample of gold and said “That is gold”. Its reference was determined in this initial act of name-giving and from then passed on.

Both views burden the eliminativist position with problems. As Stich succinctly formulates it; “[o]n the description theory, eliminativism is trivially true; on the causal-historical theory, eliminativism is trivially false” (Stich, 1994, p. 185). So the question of reference brings the eliminativist either to the Scylla of falsity or to the Charybdis of triviality, and it is not as yet obvious whether there is a way to sail past the two of them, without giving up on EM. Let us now first take a closer look at the answers to the question of reference.

3.2. *Theories of reference*

3.2.1. *The descriptive theory of reference.* The descriptive view holds that the reference of a term is determined by the cluster of descriptions in which the term appears. This view was embraced by both Kuhn and Feyerabend. It says that alternative theories are, at least now and then, so distinct that they are characterized by thorough semantic incommensurability (Devitt, 1979; Boyd, 1991), the scientist being “trapped in the web of his own meanings” (Scheffler, 1967, p. 46). This meant that, in the words of Kuhn, theories (paradigms) “cut up the world in different ways” (Kuhn, 1970a, p. 268), that is, the lines that separate referents are redrawn when a new theory establishes itself (Kuhn, 1970a, p. 269, n. 3; 1970b, pp. 101–102; cf. Scheffler, 1967; Fine, 1975). Different theories are claimed to postulate different worlds (Kuhn, 1970b, p. 111).

Paul Churchland’s *network theory of meaning* is presented as a particular brand

of meaning holism (1979, p. 61; 1988, p. 56). It remains relatively close to earlier descriptive views defended by authors such as Ryle (1956, p. 90), Hanson (1958, pp. 7, 61–62) and Feyerabend (1965b, p. 180), and criticized by Peter Achinstein (1964), Hilary Putnam (1965), Dudley Shapere (1966, 1969) and Isaac Scheffler (1967) among others. Churchland's network theory of meaning holds that the meaning of a term or statement is a function of the whole of all coherently connected, semantically important statements of the theory in which it dwells (1979, p. 61; 1988, p. 56; 1989b, pp. 271ff.; cf. Churchland, 1986, p. 344). Like his philosophy of science, the network theory of meaning has now found a "natural home" in connectionism (Churchland, 1993, p. 667; for discussion, see Haselager, 1997, chapter 5).

Paul Churchland's view of reference is, as he says, "roughly Fregean": meaning is determined by a relevant system of sentences or by activations in vector space (plus context) which fix reference (Churchland, 1989b, p. 287). Because conceptual and neural networks are subject to endless configuration and reconfiguration, we must conclude that "our taxonomies form, dissolve, and reform, even as we watch" (p. 285). The plasticity of these networks makes "our access to natural kinds fluid, uncertain, and problematic" (p. 287). Given the Stich dilemma, this Fregeanism leads to severe difficulties.

3.2.2. Problems with the descriptive view. In a recent paper, Patricia Churchland stated that "the possibility of nontrivial revision and even replacement (. . .) is the crux of what makes eliminative materialism eliminative" (Churchland, 1996, p. 286). But how are we to establish which revisions are sufficiently non-trivial to count as true theory shifts? And, furthermore, which of the non-trivial changes are of the reductive and which are of the eliminative variety? The question of reduction vs. replacement is all about the amount of similarity between θ_2 and θ_2^* . Only when there is "strong analogy" (Schaffner, 1967, p. 144) or "close similarity" (p. 144; Churchland, 1979, pp. 83–84), we can properly speak of reduction. However, these notions have usually been left intuitive: what is strong, close, similar, etc.? [3] A sharp-edged criterion of sufficient non-triviality (or, dissimilarity) does not seem to be forthcoming.

Similar questions plagued, in particular, Feyerabend (1965a,c) over 30 years ago. Which theory revisions are to be considered non-trivial (Achinstein, 1964; Putnam, 1965; Shapere, 1966, 1969)? Without a robust boundary of sufficient non-triviality even very small changes in the theoretical network would bring about changes in the references of the terms. When is a theory change sufficiently non-trivial to effect a conservative reduction, and when is it sufficiently "massive" to allow for a true replacement-reduction? The boundary between reduction and replacement, between ontological retention and ontological elimination, appears to be a blurry one (cf. Hull, 1974, pp. 42–44). No hard and fast criterion seems to be forthcoming for distinguishing eliminative or revolutionary (i.e. "bumpy") theory changes from "smooth" theory changes.

Hence, even very minor changes in the way that one conceives of, say, mental states imply that belief–desire psychology and cognitive neuroscience refer to distinct

entities. It appears that such a consequence is undesirable for the true eliminativist because it renders EM, albeit logically correct, extremely *trivial*. This view of reference, then, yields a form of *paneliminativism* (Stich, 1994, p. 184), which makes elimination cheap and uninteresting (any new discovery or insight counts as eliminative). When we allow that both belief—desire psychology and cognitive neuroscience are talking in different ways about mental states, we are left with radically different ontologies, just as the eliminativist would have it, or so it seems. But the same goes for relatively minor, and intuitively rather continuous, conceptual changes within cognitive neuroscience itself: new developments imply a redescription, and, therefore, an elimination of the earlier ontology (recall that ontology follows theory). If the Churchlands really favor a semantics that is—as they claim—broadly Fregean in outline, then the question that arises is whether EM runs the risk of being reduced to insignificance.

3.2.3. The causal theory of reference. Saul Kripke and Hilary Putnam have vigorously opposed the Fregean view of reference (Kripke, 1972; Putnam, 1975). Their proposal gave substance to the notion of *theory-transcendent* (Shapere, 1966) or *trans-theoretical* (Putnam, 1973) terms. These are terms that function in different theories and, in point of fact, preserve reference across these theories. On the descriptive view, we were forced to say that gold as conceived by Archimedes (“gold consists of particles of gold”), Pliny (“gold is the only thing that loses no substance by the action of fire”), or Kant (“gold is a yellow metal”) is eliminated from our ontology. In contrast, the alternative would say that we now know gold under a different description, namely “gold is a metal with atomic number 79”, and that “gold” now refers to the same entity as “gold” when uttered by, say, Archimedes. According to these theorists, then, the descriptive theory of reference must be wrong. The alternative is a theory of reference that submits that the reference of theoretical terms is grounded by certain causal connections with the objects these terms refer to. A term’s reference is not determined by a cluster of descriptions or a conceptual network, as in the descriptive view of reference, but rather by a causal chain of communication, reaching back to an initial baptismal event.

Obviously, however, some revision takes place in science: the way “gold” is defined has changed over the centuries. Where Archimedes, Lucretius, Pliny and Kant used a *contingent* description, contemporary metallurgists use a *necessary* description to identify gold. *Having atomic number 79* is a necessary or essential property of gold, where *being a yellow metal* or *losing no substance by the action of fire* or *consisting of particles of gold* are contingent properties. However its meaning may vary, the reference of “gold” has remained the same, according to the causal view.

Echoing Skinner (1971/1980, p. 203), we may say that no theory changes what it is a theory about. People were not talking about different things when saying “gold” before and after the discovery that gold is a metal with atomic number 79. And roughly the same may be true of psychological terms like “belief” and “desire”. In this view, belief—desire psychology and neuroscience use different criteria for the identification and individuation of beliefs and desires. Nevertheless, they are talking about the same entities, namely, beliefs and desires. Now the second horn of Stich’s

dilemma comes into view for the eliminativist. Psychology and cognitive neuroscience are talking about the same entities in radically diverging ways. Thus, eliminativism turns out to be simply *false* on the causal view. When psychology and cognitive neuroscience are essentially talking about the same things, it cannot be the case that psychology was not talking about anything. It cannot be that psychological terms like “belief” and “desire” are referentially disconnected. The same referents hide between our changing descriptions of them.

3.2.4. Problems with the causal view. What does the causal view say about theory change? The causal view emphasizes that the reference of theoretical terms is conserved across even the most extreme of theory changes. Although Putnam may say that “we don’t carry it so far as to say that *phlogiston* referred” (Putnam, 1976, p. 184), this is certainly not what his causal-historical theory ordains. Theory change is made ontologically conservative to the limit (Fine, 1975; Enç, 1976). The account seems in the last resort committed to the counterintuitive claim that reference *never* changes. We may call this consequence *panconservatism*: reference is extremely steady.

Further, in what sense can there be progress? It seems that the causal theorist has no way to decide whether what we have is a trivial or a non-trivial theory change. Redescriptions that occur across theory changes may turn out to be arbitrary as far as the causal theory can tell. This is a direct consequence of the fact that the causal view is driven by essentialism. According to this view there is an independent world out there to which our theoretical terms are thought to refer. A new scientific discovery leads to a necessary a posteriori description: it brings out an essence (Kripke, 1972). But, one may ask, when do we blow the whistle and determine that we have actually grasped an essence? Whether *having atomic number 79* is an essential property of gold depends upon our intuitions and new discoveries. There may come along a new physics that induces us to reconceive our current notion of gold. In sum, there appears to be no way for the causal theorist to decide whether scientific descriptions are adequate descriptions of reality; the labels we paste on to the world may be largely arbitrary for all the causal theory can say. So the question remains: how to distinguish between trivial and non-trivial theory changes and between bumpy and smooth theory transitions?

3.3. The dilemma revisited

The obvious advantage of the causal-historical theory of reference over and above the descriptive, Fregean view that the Churchlands embrace is its ability to account for referential stability; the causal-historical theory thereby avoids *paneliminativism*. However, it suffers from at least one serious drawback as well: it collapses into *panconservatism*, that is, reference never changes. Hence, which one of these theories of reference is chosen, it seems to lead to either of two evils for the eliminativist: triviality or falsity, paneliminativism or panconservatism.

Intuition suggests that we should look for some intermediate position, somewhere between continuity and discontinuity. Reference changes may not be as

massive and ubiquitous as was supposed by Kuhn, Feyerabend and the Churchlands. Causal-historical theories, on the other hand, may have made reference too stable. The history of science suggests that theory changes are at times accompanied by reference changes. Therefore, to realistically account for theory change, we need to search for a more suitable account of reference. Such an account must be able to account for both stability of reference (all-out *ontological conservation*, at one extreme), as well as for reference changes (all-out *ontological elimination*, at the other extreme).

4. The pragmatics of reference

4.1. Churchland on pragmatism and realism

As said earlier, Paul Churchland opposes the causal-historical view of reference. Theoretical terms and, consequently, statements owe their reference to the global conceptual scheme in which they function; there is no “theory-neutral or intension-independent relation that connects words to unique natural sections of the world” (Churchland, 1989b, p. 281). Churchland goes on to submit that *natural kinds* are law-bound, that is, the only kinds worthy of the predicate “natural” are just those kinds mentioned or implicated by laws of nature (p. 288). This suggests a comparatively cavalier attitude towards other kinds, which are mere *practical kinds*, i.e. kinds that do not figure in real laws. Gold, for instance, is not a natural kind, but only a practical kind, *pace* Putnam and Kripke. And the same must be true of kind terms like “water”, “belief”, “desire”, “pain”, and so forth. These are kind terms that do not figure in some basic law of physics and, therefore, they refer to mere practical kinds. Churchland concludes that “[g]enuine natural kinds form a very small, aristocratic elite among kinds in general, being found only in the most basic laws of an all-embracing physics” (p. 295). Natural kinds are catalogued only by our most basic partitionings.

However, Churchland’s view of natural kinds appears to backfire. It appears to withhold natural-kind status from *all* scientific terms, except perhaps a very few of our most basic physical terms, like “mass”, “length”, “energy” and “momentum”. On Churchland’s view, it is now only fundamental physics whose predicates truly refer to natural sections of the world [4]. This means that well entrenched theoretical terms like “gold” and “temperature” only have a practical status. And apparently the same goes for the *neurocomputational* terms that Churchland puts forward as viable alternatives to psychological terms. These terms do not have natural-kind status, but at most only practical-kind status. They are as provisional as the psychological terms they are supposed to replace. There is no principled reason why these terms would not, eventually, also be eliminated.

On Paul Churchland’s view, as we just saw, there is hardly anything that can serve as a privileged or special description of the world. Most terms do not strictly refer, natural kinds are scant. Churchland (1992) even professes that “there is no real prospect either of there being, or of our ever finding, a uniquely correct Final Theory. (...) There is no Final Science” (pp. 422–423; cf. 1989b, pp.

150, 194, 231, 253, 293–294). There is a real possibility that we may not be able to get out of some deep but local error minimum. A fortiori, it is unlikely that there are many Natural-kinds-with-a-capital-N.

Churchland (1992) presents his view as a form of *pragmatic realism*. In fact, as early as 1982, Churchland suggested that we should head for *pragmatism* (Churchland, 1989b, pp. 149–150), that is, “the proper course to pursue in epistemology lies in the direction of a highly naturalistic and pluralistic form of pragmatism” (1989b, p. 194). Prima facie, this line of argument seems at odds with his earlier view of the reference of natural kind terms, when he claimed that *only* terms mentioned in the Final Laws of a Final Physics would *really* refer to sections of the world. Whether there are natural (Final) laws and kinds was, however, left open as an empirical question (pp. 293–294). Churchland even doubts the integrity of *truth* (pp. 139–151) and *reference* (pp. 220, 276–277, 294; 1992). These orthodox epistemic criteria belong to the traditional picture of knowledge with its emphasis on linguistic structures. But if the traditional, sententialist paradigm has to go, so must its concomitant epistemic virtues. These notions should be “reconceived” (p. 220), and this is “not even very difficult” (1992, p. 422). In Churchland’s philosophy of science, it is not truth, but global and pragmatic (“superempirical”) virtues like coherence, simplicity and explanatory power that determine a theory’s epistemic value (1989b, pp. 139–151). However, thus far Churchland has provided no indication whatsoever of what reference as reconceived in connectionist terms should look like. How can we do without semantic notions like reference? To eliminate these is just to change the subject (Fodor & LePore, 1992, p. 205). Indeed, it is not at all clear how to formulate EM (or scientific realism, for that matter) without a notion of reference.

What Churchland’s pragmatic realism does show is that a firm distinction between natural and practical kinds cannot be made. We do not have absolute, a priori criteria to delineate natural and practical kinds. In short, it is not possible to step out of our conceptual schemes, our theoretical or neural networks, and award some terms natural-kind status. We are, therefore, led to the conclusion that if *some* kinds are merely practical, in the sense of provisional rather than natural, then this must go for *all* concepts. This contention ties in with his plasticity thesis which states that the number of alternative conceptual schemes is enormous. What this means is not that “anything goes”, or that any odd (re)conceptualization is as good as the next one and might as well be eliminated, but only that there cannot be a special way of dividing up the world. The pragmatic version of realism boils down to the view that science should aim at being more and more finely tuned to natural (and social) reality. We are increasingly better able to conceptually exploit the natural information (about distal features of the environment) contained in our sensory states. Some causal transactions between our conceptual networks and the world result in more parsimonious, or more illuminating, or more excellent activations than others. As Churchland suggests, “global excellence of theory is the fundamental measure of truth and ontology” (1989b, p. 139).

Just as pragmatic considerations enter into theory evaluation, they figure in questions of reduction and elimination. The analog relation A_R into which both the

would-be reduced theory's equipotent image θ_2^* and reducing theory θ_2 enter, is "exceedingly complex" (Hooker, 1981, p. 49). It supposedly involves metatheoretical or normative commitments of scientists and the state of technological development and practice (p. 50). Whether there is a sufficient degree of similarity to herald a reduction or a sufficient degree of dissimilarity to even license an elimination is largely determined, not by formal criteria, but by mundane pragmatic factors, some social, some political, others more personal (Churchland, 1986, pp. 283–284).

4.2. *The pragmatics of elimination and the indeterminacy of reference*

Stich's account of ontological inference brings into sharp relief the Churchlands' point that the question whether theory change counts as moderate revision or revolutionary revision is decided pragmatically. Stich too has advertised a form of pragmatism (Stich, 1990, 1996), the ontological decision being nothing more than a "judgment call" (Ramsey *et al.*, 1990, p. 503).

In an early response to Lycan's (1988) challenge, Stich suggested that the best road the obdurate eliminativist could take was to look for some intermediate position between the descriptive and the causal-historical extremes (Ramsey *et al.*, 1990, p. 521, n. 6). At present, he considers this reply wrong, because there is "no correct account of reference" (Stich, 1996, p. 6) for theories that are seriously defective. In these cases, reference is indeterminate. There can be no uniquely correct (Final) word-world mapping. Joining hands with defenders of the causal-historical view, Stich now claims that a term is subserved by a *multitude of groundings*. That one of these heterogeneous, possibly infinite causal chains is picked out and legitimized by our intuitions as "the" reference relation, is described as "quirky", "idiosyncratic", "little more than a historical accident", and "rather arbitrary" (Stich, 1996, pp. 6, 48–49). Intuitions are indeterminate as to what is to count as a case of ontological retention and what as a case of ontological elimination. From this indeterminacy, it is argued that there is no fact of the matter whether the posits of psychology, or other defective theories, refer to anything at all, leaving EM "profoundly uninteresting" (Stich, 1991, p. 241). Decisions concerning intertheoretic similarity-fit and ontology are made largely through processes of social negotiation, in which political, personal and social factors rise to the surface (Stich, 1996, p. 8).

4.3. *Conclusion: where do we stand*

The preceding discussion leaves us with two conundrums. Firstly, we have reviewed Churchland's pragmatic realism which recognizes no special way to carve up the world (without claiming that all conceptualizations are equally valuable). As Stich points out there is no principled way to pick out more and less correct groundings of reference. Abandoning essentialism for a more pragmatic account removes the threat of panconservatism that bothered the causal-historical view of reference. However, the move towards pragmatism is itself seriously threatened by indeterminacy. The road taken by Paul Churchland leads to the view—explicitly adopted by

Stich—that there is no principled way to constrain reference changes at all. Thus, the first problem is, how to stop pragmatism from sliding down the slippery slope towards indeterminacy. This problem will be addressed in Section 5.3. Secondly, the distinction between elimination and reduction is dependent on a theory of reference, so that the reference change involved in elimination is trivial on one theory of reference, and impossible on the other. Thus, it is unclear what constitutes a non-trivial theory change, reduction or elimination. The second problem, then, is how to steer a middle course between referential inertia and referential disconnection. This problem will be the subject of Sections 5.1 and 5.2. In our view, both issues can be construed as pointing to a defusion of (simple) eliminativism, and to explanatory pluralism—which is not at variance with the best of Churchland's writings.

5. The context-sensitivity of reference

5.1. Refining reference: the dynamics of reconnecting world links

Anticipating Stich's (and, possibly, the Churchlands') view, in 1975 Arthur Fine suggested that radical reference changes may be indeterminate, that is, there may be no fact of the matter to decide questions of coreference: "*whenever a case can be made for sameness of reference, an equally good case can be made for difference of reference*" (Fine, 1975, p. 27). As suggested above, the indeterminacy Fine signaled may well go deeper and may be due to the fact that there can be no all-embracing ("Final") theory of reference.

To meet such an objection, philosophy, has attempted to amend the causal-historical theory in such a way as to be able to account for reference change. Kim Sterelny, for instance, conceived of theories as being *multiply grounded* (Sterelny, 1983; cf. Devitt, 1979; Stich, 1996). A specific theoretical term is subserved by a causal network. It is connected to reality, not by *one* link instantiated in a baptismal event, but rather by means of *a number* of links. Sterelny's example here is "platinum" which is connected to the kind directly (via causal links) as well as indirectly (via links established in conversation). Thus, reference is thought to be multiple. Our terms do not map on to reality in one privileged way, but in a multitude of ways. The idea is that in theory change some, or perhaps all, of the causal networks underlying the uses of the theoretical terms the original theory contains are *regrounded*. That is, some connections are lost, some may be reconnected, whereas other links are maintained. Although this move certainly improves the causal-historical theory, it may be that it does not go far enough.

In a recent paper, Burian *et al.* (1996) investigated the notion of "gene" and what it might refer to in terms of natural kinds. They argue that biology proceeds from local theories, and that its concepts should be understood in terms of their local roots. Because "gene" can be defined in various ways (e.g. functionally or structurally), different reconstructions of the history of the concept can be given. Among other things, this depends on the theory of reference one prefers, the descriptive one with its discontinuist bend, or the causal-historical one which allows

for continuity. The point Burian *et al.* make is that there is no uniquely correct reconstruction of the meaning and reference of the concept of gene, and that, therefore, general philosophical theories of meaning and reference are misguided.

So, reference may be dependent on local context. This feature has been described by Philip Kitcher in his account of the *heterogeneity of reference potentials* (Kitcher, 1978, 1993). A scientific term does not have a single, homogeneous reference, but is instead associated with a reference potential, which is defined as “the compendium of modes of reference for a term” (1993, p. 78), a mode of reference being the way in which a concept or a term singles out an object or a process. The ways in which a term points to a referent are numerous and context-sensitive, sometimes picking out objects by description, sometimes by ostension (*refixing*), and sometimes the picking-out is more conservative and goes back via a Kripkean causal chain to some primordial act of demonstrative ostension by a first user.

Consider Kitcher’s (1993) “phlogiston” case, which happens to be one of the Churchlands’ favorite examples (Churchland, 1988, p. 44, 1989b; Churchland & Churchland, 1994, p. 46; cf. Hooker, 1981). They use the example to drive home the point that psychological terms like “belief” and “desire” are like “phlogiston” in that they fail to refer and that they are, therefore, candidates for elimination. Kitcher argues, however, that the story is not as straightforward as the Churchlands think it is. The theory of phlogiston was introduced and defended by Becker, Stahl, Priestley and Cavendish to explain phenomena of combustion. It was thought that a substance, i.e. phlogiston, was released the moment an object was set on fire. The crucial point here is that something was thought to be *emitted* in combustion, whereas later theories (e.g. Lavoisier’s theory) have stressed the fact that something, namely oxygen, is *added* to the burning substance.

When we determine the reference of “phlogiston” (or “dephlogisticated air” or “inflammable air”) by the phlogistonians’ core thesis that *phlogiston is emitted in combustion*, then we must conclude that in fact its reference is empty and that, therefore, most of Priestley’s statements must be deemed false. With Kitcher, however, we contend that this is too simple. Such a diagnosis ignores the fact that Priestley discovered some very important insights and truths about chemical processes, although these insights were mostly wrapped in a flawed language. Sometimes Priestley’s use of the term “dephlogisticated air” (as when he observed that dephlogisticated air facilitates breathing) makes clear that it *refers* to oxygen (Kitcher, 1993, p. 100).

What Kitcher’s analysis hopes to establish is that different modes of reference are used in different contexts. The reference potential of a term like “dephlogisticated air” is heterogeneous: its reference is fixed differently in different contexts. To reconstruct the referent of a term we must always take into account the particular (historical) context in which the token was uttered [5]. Utterances of the token “phlogiston” sometimes fail to refer, sometimes they do not. It is too simple to say that it *always* fails to refer, and that, therefore, a theory like phlogiston theory (or belief–desire psychology) is massively disconnected.

The picture of reference emerging from Kitcher’s analysis is more dynamical

than the classical options. Theory change amounts to the redirecting of reference potential (Kitcher, 1993, p. 103), and thereby it allows for “continued reapplication and redefinition” (Kitcher, 1978, p. 539) or “endless construction and reconstruction” (Churchland, 1989b, p. 150), while maintaining a degree of stability and continuity usually encountered in the practice of science.

5.2. *A modest proposal: a reference continuum*

We will suggest that the notions of regrounding references (Sterelny, 1983) or shifting reference potentials (Kitcher, 1978, 1993) may be deployed to underwrite the notion of a *continuum of intertheoretical relations* ranging from pure reduction to pure elimination (cf. Churchland & Churchland, 1994). Presumably, in some cases of theory change, lots of revision or reconfiguration takes place, whereas in other cases there is relative continuity. In the former case, most links are severed and replaced; in the latter, only some links are changed, whereas most others are maintained. Whenever elimination happens, it is mostly not as massive as the Churchlands (at least in the more simplistic reading of their eliminativism) seem to suppose.

Writing about the “impasse” (*mutatis mutandis*, our “eliminativist dilemma”) in which the philosophy of science found itself, Arthur Fine concluded that both the descriptive and the causal–historical view are “totalitarian” or “inflexible” views (1975, p. 26). We suggest that the required flexibility can be achieved by the introduction of a *continuum of reference*, ranging from referential connection, or better, coreference, to referential disconnection. Coreference only obtains in the case wherein reference is established by ostension, referential disconnection happens when references are fully fixed by description. The traditional views of reference, the descriptive and the causal–historical, are ideal types, marking the poles of the continuum of reference. Between these extremes, there are all kinds of referential hues and shades. How we fix the reference of a term depends on contextual factors. Sometimes these factors involve more descriptive elements, in other cases causal elements dominate. The continuum of reference underpins a more sophisticated view of reduction and elimination.

5.3. *Reference continuum and indeterminacy*

The question then arises, what distinguishes the continuum-of-reference view from the view exemplified by Stich, that reference is indeterminate; put differently, what distinguishes pragmatism from relativism (“anything goes”) with respect to reference change? The answer to this question should also provide some clues toward a resolution of the problems with the causal and descriptive views sketched in Sections 3.2.2 and 3.2.4: is there a non-trivial distinction between elimination and reduction, between revolution and smooth transition? At the deepest level, these are metaphysical questions about realism and relativism and as such beyond the scope of this paper. Nevertheless, two lines of reasoning can be mentioned that bear upon this

question and support the continuity view. The first appeals to the notion of tradition, the second to referential and pragmatic realism.

As to the first, Laudan (1996, chapter 8) argues that *the rational weight of the scientific past* precludes the possibility that science could transform itself overnight beyond recognition. Although there is no recognizable "essence" in science, and in principle any change in practices and standards is possible, in practice changes in standards are remarkably continuous. The reason is, according to Laudan, that scientists tend to cling to proven practices, and possible new standards are vetted by the existing successful practices. The assessment of the success of practices is a pragmatic affair: Laudan argues that scientists' assessments of a scientific practice are based on a prephilosophical notion of success, i.e. one that precedes, and does not depend on methodological views of explanation, justification, etc. The tradition provides a yardstick for including achievements in the scientific canon.

We suggest that Laudan's account can be applied, *mutatis mutandis*, to reference change. The tradition constrains reference change, to the extent that proven word-world relations limit the redeployment of concepts. Like changing scientific standards, changing reference is continuous rather than arbitrary; pragmatic reasons get in the way of wild new labeling. What Stich (1996, pp. 6, 48–49) misleadingly calls "quirky" and "idiosyncratic" may be rightly considered contingent dependent as it is on historical and social factors, and underdetermined by the facts (cf. Barnes *et al.*, 1996), it nevertheless carries a "rational weight" of past successful application that guarantees a bias for continuity in changing concepts.

The second way to constrain the arbitrariness or indeterminacy in reference change emphasizes success in manipulating things and experiments as a foundation for a realism about entities (if not about theories). Hacking (1983, 1993) has defended such a realism about entities, although not about theories. He notes that scientists are unabashedly realist about the entities they work with: their being able to manipulate an entity, using it to produce further experimental data, is what supports the "*experimental argument for realism*" (Hacking, 1983, p. 265). The possibility of manipulating an entity such as an electron, and to use it as a tool to investigate something else, is what makes the hypothetical real. That is, experimental practice is the test of reality. How these entities are described is an entirely different matter. In Hacking's (1993) view this means that things (individuals) do not change, but the descriptions under which we know them do.

In our view, Hacking's argument for a realism about entities provides one anchor point for the continuity of reference. It may be construed to support a kind of *referential realism*. Radder (1988, 1996) proposed a referential realism congenial to Hacking's experimental realism. Radder's proposal is to look at the material realization of science, that is, at the way experiments are executed, as described in the language in which an experimenter and a layperson can communicate with each other about fundamental aspects of an experiment; a theoretical term refers to an entity in a human-independent reality if the material realization of an experiment can be described in that common language. Thus, theoretical terms directly pick out entities in reality; we may not know what they are and how to describe and represent them, but we know that they exist and are referred to by our terms. This should of

course not be confused with representational realism (Radder, 1988, p. 103). In the view of the referential realist, theory changes (for example, new ideas about electrons) do not in general pose deep problems with respect to reference: the old and the new concept of electron refer to the same entity. In general, terms can be said to corefer to the same human-independent entity if both terms stand in a relation of formal correspondence and if the material realization can be reproduced (Radder, 1996, p. 76).

As to conceptual discontinuity, the experiment that realized the first reference-fixing may have been wrongly interpreted, and the referents of its terms may have changed, but that does not however compromise the original reference. "Phlogiston" refers to an experimental phenomenon in a certain domain (e.g. combustion), and the original experiment allows interpretation in terms of the oxygen theory—hence, "oxygen" and "dephlogisticated air" corefer: "the term 'phlogiston' refers in a certain domain to the same element to which in that domain the term 'oxygen' (or 'hydrogen') also refers" (Radder, 1988, p. 113). Reference thus is relative to a domain of a human-independent reality. Insofar as the domains do not coincide, the coreference can be partial: e.g. oxygen may cover a wider range of phenomena than phlogiston. Hence, reference change, radical as it may be, seems to be constrained by the inertia of previous reference fixations. This then seems to support a rejection of all-out indeterminacy of reference redeployment. As Pickering (1995, pp. 116, 140) puts it, at least some decisions are "non-trivial", forced moves dictated by the rules of a discipline.

In conclusion, in this section we presented some arguments to the effect that reference is anchored in material interactions with the world, and in the rules of a discipline, a tradition of successful interactions and solutions. This limits the freedom of choice in reference change: it discourages discontinuity—and rightly so. The past embodies successful practices, and its terms somehow refer to the real world albeit in changing ways, as the domains to which they apply may shrink or expand. Explanatory pluralism, which recognizes locality and context dependency of reference, and which envisages change within continuity of reference, seems to best capture the practice of scientific inquiry. Thus, it seems plausible to consider reference change as non-arbitrary, and as not indeterminate, but constrained by and hence continuous with previous reference-fixings. As in the case of phlogiston, new terms are about the same mind-independent reality as the old ones, and redeployment is constrained by that reality.

6. Conclusion

According to EM, belief–desire psychology is referentially disconnected and, therefore, it is facing elimination. But, we have asked, how should we fill in the required notion of reference? Both classical views of the issue, the descriptive theory and the causal–historical one, suffer from serious drawbacks, because they lead either to panconservatism by making reference too rigid or they lead to paneliminativism by making reference too erratic. Drawing the strands of the previous review and discussion on reference change together, the following picture seems to emerge:

- *Reference is multiple*, that is, science involves *numerous* word–world mappings. There is no such thing as “the” reference of a term. Rather, reference is fixed pluralistically. There exist many different, but overlapping perspectives on reality.
- *Reference fixing is local, partial and context-dependent*. The way a term is pinned on to the world is crucially dependent on a context of inquiry or a domain of reality. The way in which a word–world connection is established depends (at least to some extent) on a particular point of view. All of the viewpoints offer *partial* perspectives on nature. It is not, in our view, the case that referents are fixed from one special perspective, or, again, that there is something as “the” referent of a term.
- *Reference is provisional*, Reference is not as rigid as the causal–historical theorists thought. On the other hand, it is not as fluid as the descriptivists suggested. It must be conceded though that reference changes do occur. These take place by regrounding or redirecting reference potential.
- *Reference is contingent, but not indeterminate*. Although we acknowledge that changing reference is a contingent affair in the sense that it involves an irreducible pragmatic component, we have brought to the fore some considerations suggesting that such changes need not be indeterminate or arbitrary. Reference appears to be rooted in material interactions with the world and in the practices of a scientific tradition. Both factors appear to constrain the plasticity of reference changes.

In short, our discussion of the eliminativist dilemma indicates that reference is more pragmatic than the classical views of reference would have it. The way in which a reference is singled out is dependent on the particular context involved. We submit that it is unlikely that there is a highly general, context-insensitive theory of reference. This should be taken to mean that, to some extent at least, our decisions regarding the connected questions of theory reduction vs. elimination and of ontological retention vs. elimination are not dictated by universal formal rules. The pragmatic nature of reference—a claim which to us seems inescapable and with which the Churchlands at times seem to agree—suggests that theoretical concepts represent provisional, context-dependent and partial perspectives. Our discussion of the issues suggests that there is no reason to conclude that in general psychological terms fail to refer, that is, that psychological kinds do not exist. It is more likely that our psychological terms, like most or perhaps all theoretical terms, have a reference that is only provisional, partial and local. The referents of psychological terms differ across varying contexts. Sometimes these referents are fixed by description, sometimes by ostension (or material realization). In some contexts psychological kinds may ultimately be found to fail to refer, whereas in other contexts they do have referents.

Looking back now on the issues of reference and reduction, we conclude that there is a lot more to these issues than the “nothing-but” slogan suggests. What we have said indicates that the simplistic view of elimination as formulated by Paul Churchland (1989a and subsequent publications) can be effectively defused. Whole-

sale elimination is just one extreme of a continuum of reconceptualizations. Elimination, then, cannot be the sole model for intertheoretical relations between psychology and neuroscience.

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Notes

- [1] Caveat: in the following we prefer to steer clear of difficult issues surrounding *meaning* or *intension* as much as possible. Our emphasis in these pages is on *reference* and the difficulties it poses for EM. Feyerabend's principle of meaning variance can be plausibly interpreted as a principle of referential variance, as he mistrusts meaning: "even the most detailed conversations about meanings belong in the gossip columns", meaning has "no place in the theory of knowledge" (1965c, p. 230).
- [2] As Sklar (1967) has noted, this was not originally part of Nagel's orthodox account of reduction. Nagel was only interested in "correlatory laws". But this proved too weak: it accounts only for the reduction of θ_2 plus the correlatory theory to θ_1 , and not as intended, for the reduction of θ_2 to θ_1 (p. 119).
- [3] Paul Churchland attempt to account for "semantic similarity" in the connectionist idiom of state spaces was severely criticized by Fodor and Lepore (1992).
- [4] Fodor and Lepore (1992) hold forth that Paul Churchland, like Kuhn, Feyerabend and others, is committed to *referential holism*, the view that "two languages can share any of their ontology only if they share quite a lot of it" (p. 11). For Churchland this means that "only the final, literally true physics will be able to refer to *anything at all*" (p. 211, n. 10). Reference holism often follows from meaning holism, as Fodor and Lepore rightly observe (p. 211, n. 8).
- [5] Field (1973) and Devitt (1979) have claimed that there is *partial* reference: "phlogiston" refers *partially* to oxygen, perhaps partially to hydrogen, and partially to nothing.

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