FORUM ON CULTURE AND EXPLANATION IN HISTORICAL INQUIRY

4.

SOME AFTERTHOUGHTS ON CULTURE AND EXPLANATION
IN HISTORICAL INQUIRY

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ABSTRACT

I argue here that the articles in this forum contain basic agreements. All three reject naturalism, reductionism, and monism while retaining causality as an explanatory category, and all three emphasize the role of time and argue for a view in which culture is regarded as both structured and contingent.

The differences among the explanatory proposals of Hall, Biernacki, and Kane are as important as the similarities: while Hall favors a Weberian approach, Biernacki argues for a primarily pragmatic explanation of culture, and Kane for a primarily semiotic explanation. I argue that all three positions face immanent problems in elucidating the exact nature of cultural explanation. While Hall leaves the problem of "extrinsic" ideal-typical explanation unsolved, Biernacki simply presupposes the superiority of pragmatic over other types of cultural explanation, and Kane does the same for semiotic explanation. Hints at cultural explanation in the form of narrative remain underargued and are built on old ideas of an opposition between "analysis" and "narrative." This is also the case with the latest plea for "analytic narratives." I conclude that a renewed reflection on this opposition is called for in order to come to grips with cultural explanation and to get beyond the old stereotypes regarding the relationship between historical and social-scientific approaches to the past.

When I volunteered to comment on this forum, my main motive was curiosity. Having been trained as a sociologist some time ago and having turned myself into a philosopher of history in the meantime, I was curious about how three prominent American historical sociologists tackled one of the central problems of philosophy of history and of the social sciences, that is, the problem of historical explanation. This will be the focus of my comments, which I have subdivided into three parts. First, I shall chart some basic agreements between the methodological positions of the three panelists. Second, I will identify some important methodological differences. And third, I will formulate some methodological problems raised by the papers that are unresolved as yet and that might stimulate further reflection.

I. BASIC AGREEMENTS

I first want to chart the common ground in the philosophy of history occupied by Hall, Biernacki, and Kane.

They all *argue against all forms of naturalism, reductionism, and monism*. Hall rejects the monistic explanatory claims of positivism explicitly (346-347), and Biernacki does the same for the monistic explanatory claims of Geertzian culturalism. Kane is less explicit on this subject, although in an earlier version of her paper she too stated explicitly that “it has been effectively demonstrated” that reductionism, cultural or otherwise, is invalid.¹

The second common characteristic of the papers is that, although they reject naturalism, they *do not reject causality as a major explanatory category* in the human sciences, as is often the case with anti-naturalists in history. Instead of the rigid and positivistic covering-law view of causal explanation, they all subscribe to *comparison* as the only way to validate singular causal explanations—and in this respect they all subscribe to Weber’s methodological position.²

The third common characteristic is that they all *emphasize the role of the time dimension* in explanation. However, exactly what this role is is less obvious to me and is in need of further clarification, especially because the difference between the historical and the social-scientific approaches is often located in the time dimension.

The fourth and last common characteristic of the papers is that they all explicitly *argue for explanatory pluralism*, that is, for multi-factor explanatory approaches. They all argue against mono-causal explanatory strategies, as most sensible persons nowadays do. Nevertheless, they all argue that *one* essential and irreducible explanatory factor is the cultural factor. All three authors hold that explanation in historical inquiry cannot proceed without taking culture, that is, meaning, into account. And all subscribe to the view that those cultural meanings are neither somehow determined, nor wholly accidental: they hold that culture is, in a fundamental sense, both structured and contingent.

These four common characteristics highlight the commonalities with Max Weber’s position—that of good old *sinnverstehende* and *comparative sociology*. Therefore it is no accident that Weber is very present in Hall’s and—to a lesser extent—in Kane’s and in Biernacki’s papers. Hall seems even to be advocating a *return* to Weber’s ideal-typical methodology, and Kane’s remarks on the “causal significance” of culture also have a clear Weberian ring. Her explicit and repeated emphasis on the indispensability of “meaning construction” and “symbolic structures” is close to Weber’s focus on “worldviews” (*Weltbilder*). Since Weber has been the patron saint of modern historical sociology for some time now, this may hardly be surprising. However this may be, it provides a focal point for me to analyze and compare the papers in relation to Weber. Before that I will first try to identify some interesting differences among the three papers.

1. Anne Kane, *Ft. Worth—Social Science History-paper* (original version), 1. All parenthetical page references in my text are to the versions published in this issue of *History and Theory*.

2. For the problems of causal explanation in history see my book: *Konstruktion der Vergangenheit: Eine Einführung in die Theorie der Geschichte* (Cologne, Weimar, and Vienna, 1997), esp. 189-323, and my article: “Can Histories Be True? Narrativism, Positivism and the ‘Metaphorical Turn,’” *History and Theory* 37 (1998), 317-320.

II. IMPORTANT DIFFERENCES

One of the most important differences among the papers is their conception of culture. Most of the time Biernacki criticizes “essentializing” views on culture, which he attributes to the godfathers of the “cultural turn” in general and Clifford Geertz in particular. But while Biernacki emphasizes the analytical and nominal status of culture in historical explanations, both Hall and Kane subscribe to a less nominal, more realist view of culture.

Hall’s more realist view of culture can be derived from his characterization of his central topic, collective religious suicide. Collective religious suicide, he says, is “not a singular social phenomenon,” but consists of “two alternative cultural structures” with alternative “cultural logics” (345-346). Culture, according to Hall, is definitely *not* merely an analytical construct of the sociologist, but a process in historical reality *itself*. Kane defends a similar view of culture, stating that “culture’s autonomy rests on the *metaphoric* nature of symbols and the patterned relationship of symbols within a structure” (314).

This is not the only interesting difference among the papers. Next to the difference with regard to the “essence” of culture—real according to Hall and Kane, or nominal according to Biernacki—is an interesting difference between Biernacki and Kane with regard to the question whether culture and meaning can fruitfully be analyzed by historians as a semiotic system. While Biernacki invests a considerable amount of intelligence and energy in deflating the Saussurean view for historical use, Kane flatly states the opposite view: “Meaning construction must be analyzed *in the first and last instance* in reference to the internal, or the semiotic, structure of symbolic systems.” Consequently she locates “the *locus of meaning*” in “*symbolic structures*” (314, italics mine). Her analysis of the Irish Land Movement in terms of symbolic structures, later identified as metaphorically structured, identity-forming, and legitimacy-creating narratives, is explicitly legitimated by her semiotic view of culture.

Biernacki, in contrast to Kane, proposes that the semiotic model of cultural meaning must be supplemented by a *pragmatic and contextual model of meaning construction*. At the same time he opposes the reduction of meaning construction to *intended* meaning and its *unintended* consequences, as Weber and Hall would have it. New, and interesting, is his proposal—based on analyses of Benedict Anderson, Karl Marx, and his own research—to regard the *structure of practice* itself as an *unintended* and *unconscious* locus of meaning-construction for the actors. His message in this context seems to be that there is always more meaning-construction going on than meets the eye of the actors. This view brings Biernacki surprisingly close to one of the godfathers of the “cultural turn,” Michel Foucault, and his concept of discourse.

So the concepts of culture and meaning in the three papers are not the same. While Hall adheres to a Weberian concept of culture, in which meaning is located in the subjective interpretations and the intentions of actors and their world-views, Kane adheres to a semiotic concept of culture in which meaning derives

from the mutual relations of signs in a system and *not* from actorial intentions and interpretations. Biernacki's pragmatic concept of culture takes a kind of middle position between these two, emphasizing *both* the semiotic meaning *and* the pragmatic use of systems of signs (302).

This difference among the concepts of culture in the three papers must and does have consequences for the concept of explanation in each paper. Now it's time to take a closer look to see what is going on behind the curtains of the cozy "cultural consensus" on explanatory fundamentals. Let's start with Hall.

Hall on explanation in sociohistorical inquiry

a. "Intrinsic" and "extrinsic" history

Hall introduces a distinction between the "intrinsic" (that is, actor-oriented) approach and the "extrinsic" (that is, observer-oriented) approach in historical inquiry. While he attaches the "intrinsic" approach to "specific history," he attaches the "extrinsic" approach to "configurational history":

The practice of "specific history" can be defined as investigation of an object's "intrinsic meaning" whereas "configurational history" is directed toward extrinsic, theoretically constructed objects. That is, specific history is based on the *verstehende* project of identifying sociohistorical objects in terms of their meaning to historically located individuals who participated in the events being emplotted. The boundary between specific and configurational history can be located by examining the point at which it becomes difficult to argue that an object of inquiry is constituted in relation to intrinsic meanings. (333)

"Configurational history" is thus not limited to the horizon of the historical actors, while specific history supposedly is. Therefore, according to Hall, the relations between events in specific history are relations of meaning that can be framed in the temporal order of narrative, while configurational history "must employ some other structural device of plot, series, or analysis to arrange a discussion of cultural objects and practices (for example, cultural histories of privacy in different historical epochs, sexuality over centuries, dieting in heterogeneous circumstances)" (334). Now Hall's examples make abundantly clear that, in the case of configurational history, he is thinking of *long-term* phenomena, which are identified *ex post* by the historian and which could *not* be experienced as such by the actors in the past nor explained by their intentions because the configurational phenomena simply transcend the actors' experiential horizons. Therefore Hall feels entitled, as many sociologists before him, to contrast *meaning*, *temporal order*, and *narrative* on the one hand with *structure*, *configurational order*, and *analysis* on the other. And he feels entitled to link the first trio of concepts to specific history, and the second trio of concepts to configurational history.

Although these dichotomies may be fine in theory, Hall is sufficiently acquainted with historical studies to distrust them in practice. On the basis of Lynn Hunt's *Politics, Culture, and Class in the French Revolution* (1984), he immediately starts relativizing his theoretical dichotomies. In Hunt he detects "a series of hermeneutic and structural analyses of specific materials" (334). And while hermeneutics has been attributed to specific history, and structural analysis to configurational history, Hunt's study, which *prima facie* looks like an indis-

putable specimen of specific history, is relabeled as “a transitional case” (334). Hall also offers a remarkable explanation for its “transitional” status:

Hunt’s study thus is a demonstration that detailed event history can employ a metastructure of extrinsic analysis even though the phenomena described might reasonably be asserted to have intrinsic linkages. Perhaps with richer data, Hunt would have pursued specific history more concerned with intrinsic linkages. In any case, her study employs a procedure that addresses a recurrent problem of specific history. Often, the historical record is too thin for inquiry to tell “what happened” through narrative, even though an intrinsically ordered plot could be constructed if adequate historical evidence were available. (334)

Hall’s comments on Hunt are revealing for several reasons. First, the theoretical notion that specific history is just a “plain” and chronologically structured narrative of subjective meaning immediately collapses as soon as confronted with historical practice.³ All historical accounts of intentional actions are embedded in accounts of their “structural” contexts—institutional, ideological, mental, material, and so forth—and these latter accounts don’t have a temporal structure themselves, as Hall himself acknowledges.⁴ This “*dual structure*” of narrative history, here exemplified by Hunt, was already observed by the historical sociologist Philip Abrams back in 1982 in his attempt to pinpoint the characteristics of historical and of sociological inquiry: “What typically passes as narrative history is contained by and itself contains analysis. . . .”⁵ So, if Abrams is right, as I think he is, *all* historical narrative is “transitional.”

Hall could also have found the same type of argument in the theoretical analyses of narrative provided by Aristotle, Johann-Gustav Droysen, William Walsh, Louis Mink, Frank Ankersmit, and Hayden White.⁶ All these theorists of narrative emphasized that narrative as a form of explanation does *not* have a temporal structure; instead, narratives are structured by plots (Aristotle), “central ideas” (Droysen), “colligatory concepts” (Walsh), “configurational comprehension” (Mink), metaphors (Ankersmit), or tropes (White).⁷ Most theorists of narrative have emphasized that narrative, contrary to the popular conception, does *not* follow the time arrow, but constructs a synchronic *Zusammenhang* connecting tem-

3. Cf. John R. Hall, *Cultures of Inquiry. From Epistemology to Discourse in Sociohistorical Research* (Cambridge, Eng., 1999). In this book Hall distinguishes four basic discourses in the field of sociohistorical inquiry: 1. The discourse on values; 2. Narrative discourse; 3. Discourses of social theory; and 4. Discourse of explanation and interpretation. He emphasizes that in the practice of inquiry these discourses usually form mixes and *bricolages*.

4. Cf. Hall, *Cultures of Inquiry*, 67: “At the extreme, the intrinsic narrative is a story told in the first person, from one or another subjective viewpoint in the course of unfolding events.” Hall, however, acknowledges on page 93 that even in intrinsic narrative “at least some narrative connections are extra-intrinsic products of inquiry, not derivatives of causal or meaningful sequences in unfolding history.” This, however, does not lead him to a reconsideration of the distinction between intrinsic and extrinsic narratives.

5. P. Abrams, *Historical Sociology* (Ithaca, N.Y., 1982), 305.

6. Hall in *Cultures of Inquiry* draws heavily on White’s and Ricoeur’s theories of narrative, although he does not deal with this aspect of their theories.

7. For an overview of the positions in narrative theory, see Lorenz, *Konstruktion der Vergangenheit*, 127–189, and *History and Theory: Contemporary Readings*, ed. B. Fay et al. (Oxford, 1998).

porally very disparate events and thus creates its *own* temporality. That Mink characterizes the historical mode of understanding as “*configurational* understanding”—and thus uses the very concept of configuration for history *tout court* and not just for a “social-scientific” type of history, as Hall does—is as telling as can be.⁸ So synchronicity rather than diachronicity is typical for narrative explanation, as Mink emphasizes explicitly. Therefore, I find Hall’s contrast of narrative versus analysis—and its concomitant contrast of specific versus configurational history—questionable as a basis for genre distinctions.

The second “revelatory” aspect of Hall’s comment on Hunt concerns his explanation of Hunt’s use of narrative strategies. Hall’s idea is that Hunt took “refuge” in configurational history just because “the historical record was too thin to tell ‘what really happened’ through narrative” (334); this betrays three important questionable presuppositions.

The first problematic presupposition is that historians, ideally, produce a “pure” and “intrinsic” narrative history that consists of the history as it was experienced by the historical actors, a history that is temporally structured and does not contain any “configurational impurities.” This implies that history is basically the “*untold story*” of the actors, and that it is the historian’s task to (re) tell this story. Moreover, since “intrinsic” history is ideal history, “extrinsic” history is only second best. In my view, however, there is not *one* ideal model of history; neither is there *one* ideal relationship between “intrinsic” and “extrinsic” perspectives in narratives. A demographic or economic history is as legitimate as a biography, and each contains both intrinsic and extrinsic elements.

The second problematic presupposition is that there is a “natural” or ideal relationship between research and writing in history: a “thick” historical record, in Hall’s view, corresponds with a normal narrative form. This presupposition conflicts with the well-established “relative autonomy” of the composition phase (*Darstellung*) vis à vis the research phase (*Forschung*) in history. Therefore, the form of representation in history is always *underdetermined* by historical research. Research only determines the *limits* of historical representation.⁹

The idea of a “determinative” relation between research and composition in history has been discredited by all the abovementioned theorists of narrative, who have recognized that narrative is basically a retrospective construction and selection of the historian. Although Hall recognizes, with Schutz, the fact that “for the acting subject, even in relation to ‘the same’ events, meaning is unstable, contextual, and dependent on temporal perspective” (337), he does not draw conclusions from the fact—although stated by him elsewhere¹⁰—that the same holds

8. L. Mink, “History and Fiction as Modes of Comprehension,” in *History and Theory: Contemporary Readings*, ed. Fay et al., 121-137.

9. See *Probing the Limits of Representation: Nazism and the “Final Solution,”* ed. S. Friedländer (Cambridge, Mass., 1992); and C. Lorenz, “You Tell Your Story, I Tell Mine: Some Reflections on Truth and Objectivity in History,” *Österreichische Zeitschrift für Geschichtswissenschaften* 10 (1999), 563-585. Interestingly, Hall could have referred to Droysen, who held that a narrative representation of history is dependent on the possibility of reconstructing a *continuous* development, and that continuity presupposes a continuous record. See J.-G. Droysen, *Historik*, ed. P. Leyh (Stuttgart/Bad Canstatt, 1977), 234.

10. Cf. Hall, *Cultures of Inquiry*, 91: “Narration, both in lifeworldly conduct and in inquiry, is a narration of narration, the interpretation of preinterpretations.”

true for the *historians* who construct narratives. If he had realized that it is the *historian* who determines the relationship between the “extrinsic” (retrospective or anachronistic) perspectives and the “intrinsic” perspectives of the historical actors, he would not have tried to connect the narrative perspectives directly to “the state of the record.”

Hall’s third problematic presupposition is his idea that the “thickness” of the historical record is a simple “given,” and thus not a notion *relative* to the questions asked and the representational goal. This presupposition is not correct: a biography, for instance, presupposes a different kind of record than a world history, and the same record that may be regarded as “thick” for a world history may be very “thin” for a biography. The quality of the record is thus wholly question- and frame-dependent, and the idea of a “natural” relation between the state of the record and narrative representation a chimera.

b. *Configurational history and explanation*

Now it’s time to take a closer look at Hall’s configurational history. This type of history is characterized by Hall as analytical, theoretical, and as working with theoretically constructed “sociohistorical models” of an ideal-typical character (336). It is later exemplified in his two models of collective religious suicide. According to Hall configurational history aims at the construction of models or ideal types that *transcend* specific histories and the “intrinsic meaning” horizons of the actors. They bring out the “extrinsic” general characteristics of “intrinsic” specific histories. Framed in Jon Elster’s terminology, Hall holds that configurational history deals with the *sub*-intentional and the *supra*-intentional aspects of social action, while specific history deals with its intentional aspects¹¹: “Whereas the balance of inquiry in specific history is tipped toward using narrative to analyze unfolding situations as they are meaningful to the actors involved, configurational history tilts toward analyzing the structural interplay of diverse events and phenomena and their unintended consequences” (335). Configurational history is thus presented as the core business of “analytic” social scientists, just as special history appears as the core business of “narrative” historians.¹²

This Weberian view of the interdisciplinary division of labor has a long pedigree, as does the view that the sociological models somehow fulfill an explanatory function in relationship to specific histories. This view, however, contains several unresolved problems that are inherited by all who adopt the Weberian position. Let me specify two of these problems that occur in Hall’s paper and that are central to the topic of culture and explanation.

The first problem is in what sense models and ideal types can be regarded as explanatory. In Hall’s case this problem boils down to whether his ideal types or models of collective religious suicide fulfill *explanatory* functions vis à vis the cases. Hall’s statements regarding this fundamental question remain ambiguo-

11. Jon Elster, *Explaining Technical Change: A Case Study in the Philosophy of Science* (Cambridge, Eng., 1983), 84ff.

12. In *Cultures of Inquiry* Hall connects configurational history firmly to social theory. See, for example, the scheme on 206.

ous.¹³ On page 346 he characterizes ideal types, just as Guenther Roth did, as “benchmarks . . . useful for comparative analysis;” on page 345 he refers to two of his own ideal types as “interpretive models rather than causal explanations;” and on page 342 he states: “This model [of collective religious suicide] comes close to a generic description of what happened in Jonestown, . . . Waco, and . . . Japan.” Now the important question is, whether—and if so, in what sense—“interpretive models” and “generic descriptions” can be regarded as explanatory. If the models are derived from the cases under scrutiny, this seems an unlikely option, unless we presume that historical cases explain themselves.¹⁴ Or is the relevant point for Hall *just* that his models show “the potential for theorizing even highly contingent sociohistorical phenomena”? My objection to that move would be the question whether *all* theorizing in the sociohistorical domain is not based on the presupposition of contingency. And if the *point* of theorizing is *not* explanation, what is it then? Since Hall is addressing the logic(s) of inquiry and whether narrative is explanatory, my question is on his table; I am not sure the paper provides an answer.¹⁵

Reading Hall, I almost felt tempted to play the positivist devil’s advocate and ask whether it is not the business of social science to formulate *theoretical mechanisms* which *explain* what is otherwise only *described* by historians.¹⁶ Or is the distinction between description and explanation just a superseded figment of the positivist imagination? If so, is the hermeneutic view victorious, according to which description is a form of interpretation and interpretation is a form of explanation, *not* in need of any theoretical supplement? Is Geertz’s “thick description” really all there is in social science? Or is Philip Abrams’s suggestion perhaps right “that Max Weber’s celebrated divorce of history and sociology was perhaps premature, indeed, possibly not a necessary separation at all.”¹⁷

Hall surely blurs the borderline between explanation and interpretation almost completely.¹⁸ This “ecumenical” move, however, has its price, one not recog-

13. See for instance Hall, *Cultures of Inquiry*, 217: “The identification of the sociohistorical object and the component changes theorized as its basis yields an analytic framework for the causal or interpretive investigation of configurational change by throwing into relief the issues that need to be pursued.”

14. Hall’s remark about “avoiding generalizing from single instances or cases that have mutually influenced one another” (340) indicates that he is aware of the problem.

15. In *Cultures of Inquiry*, 98-103, Hall deals with several authors who claim that narrative *per se* is explanatory, but refrains from presenting his own position, as far as I can see.

16. For a recent overview of the debate and for a defense of the mechanisms view, see Axel van den Berg, “Is Sociological Theory Too Grand for Social Mechanism?,” in *Social Mechanisms: An Analytical Approach to Social Theory*, ed. P. Hedström and R. Swedberg (Cambridge, Eng., 1997), 204-237. See also E. Kiser and M. Hechter, “The Role of General Theory in Comparative-Historical Sociology,” *American Journal of Sociology* 97 (1991), 1-30.

17. Abrams, *Historical Sociology*, 188.

18. Cf. Hall, *Cultures of Inquiry*, 150-151, for his view on explanation and interpretation. Remarkable, given the traditional positions and debates, is his almost complete *identification* of both intellectual operations. The only distinction he makes is with regard to their criteria of evaluation: “the difference between interpretation and explanation does not derive from logic. Rather, the two projects operate under different conditions. Under a regimen of interpretation, multiple criteria for adjudicating among accounts coexist, leaving the validity of any single account open to external challenge, even when it is sustained internally. By contrast, the regimen of explanation is based on the claim that there

nized by Hall: as a consequence of this move, the borderline between specific and configurational history vanishes almost completely too. Basically, there is just *one* type of history—that is: interpretative = explanatory history. And as most narrative theorists never tire of pointing out, narrativization *is* essentially interpretation, so we end up with the thesis that *all* history is narrative history. This conclusion will, of course, be welcomed by all narrative theorists, but can *not* be welcomed by Hall, because he has presented narrative discourse as fundamentally distinct from explanatory and interpretive discourse, not to mention his distinction between the discourse of narrative and the discourse of social theory. Or does social theory basically consist of generalized narratives? This would surely be a new and original view, but I doubt whether Hall would subscribe to it. Or is “social theory” just a vacuous label, covering a whole range of *other* “discourses,” as Axel van den Berg recently concluded?¹⁹

The second related unresolved problem Hall inherits from Weber is the question whether, and if so, in what sense, the *comparison* of ideal types with empirical cases yields *explanations*.²⁰ As is well known, one of the functions Weber attributed to ideal types is that of a “yardstick” to “measure” their “distance” from reality. This comparison of “impure” empirical reality with “pure” ideal types may sharpen our eye for the complexities of empirical reality, but it remains unclear *why* such comparisons would be regarded as “explanations” of any kind. The problem is this: ideal types offer some kind of *definition* of the phenomena they describe, but definitions usually are not regarded as *explanations* (unless one defends the type of narrative theory of Frank Ankersmit, which is not the case with Hall²¹). One gets the impression that Hall does identify both—as Weber did occasionally—so in any case he is in good company.²²

is only one appropriate set of criteria for evaluating the adequacy of alternative accounts about a given phenomenon. Despite these different contexts, however, as I am construing them, both explanation and interpretation share a core enterprise that tends to blur the line between them.” I can’t go into Hall’s arguments here; however, see my *Konstruktion der Vergangenheit*, 83-189, for the numerous traditional arguments *against* the identification of explanation and interpretation. See further, Hall, *Cultures of Inquiry*, 98-103 (“The Explanatory and Interpretative Potential of Narrative Accounts”).

19. See Van den Berg, “Social Mechanisms,” 205: “More and more, it seems, what passes for ‘sociological theory,’ or, more ominously, ‘general sociological theory,’ deals with matters of epistemology, ontology, and philosophy of science, at the expense of the more mundane business of theorizing about the social world. ‘Sociological theory’ nowadays seems to be theorizing about theorizing, not attempting to formulate coherent accounts of things happening ‘out there.’”

20. For a recent overview of the varieties of comparative history, see *Geschichte und Vergleich: Ansätze und Ergebnisse internationaler vergleichender Geschichtsschreibung*, ed. H.-G. Haupt and J. Kocka (Frankfurt am Main and New York, 1996).

21. For Ankersmit’s position, see my article “Narrativism, Positivism and the ‘Metaphorical Turn,’” 317-324.

22. Jonathan Sperber brought up this problem in a review of recent studies of the German bourgeoisie. In some of these studies, an ideal type was used that specified a democratic political orientation as one of the characteristically bourgeois values. However, empirical research brought out that for much of the nineteenth century the major part of the German bourgeoisie had other political preferences than the ideal type specified. He, justifiably, asked what explanatory uses such comparisons between an ideal type and empirical phenomena have. The problem is that “as was often the case with Max Weber himself, conclusions are built into the definitions” and “we are left with arbitrarily assigned ascriptive characteristics.” See J. Sperber, “*Bürger, Bürgertum, Bürgerlichkeit, Bürgerliche Gesellschaft*: Studies of the German (Upper) Middle Class and its Sociocultural World,” *Journal of Modern History* 69 (1997), 271-297, and esp. 285-286.

In the end my problem with Hall's analysis of history and cultural explanation is that his own position remains unclear: he rejects both the (neo-) positivist and the historicist view—this much is clear (see 346), yet by rejecting the idea of a “transhistorical generality” for his own mode of analysis (346-347), his “middle position” has a relativist ring and remains without clear contours. His remarks that the discourse of narrative may “shade off” into the discourse of social theory or into the discourse of explanation and interpretation may be true, but are not very helpful in this respect.

I find far more insightful his idea that the generalizing approach may yield knowledge of the “patterned logic” of (cultural) developments, showing “path dependency” (341). I also find his case studies convincing illustrations of this phenomenon. Although he is not explicit about it, here we could locate the relationship between generalizing and individualizing approaches in sociohistorical inquiry. Generalizing approaches aim to uncover a (general) developmental or evolutionary logic, while particularizing approaches aim at the reconstruction of a particular chronology. Philip Abrams's analysis of this relationship, based on the example of the “deviant career,” is still as convincing as ever:

the process [of becoming a deviant] has both an *analytical logic*—the sequence of stages that in principle must be passed through—and an *empirical chronology*—[the actual biographies of those who become deviant]. The process can be specified both as a formally ordered progression, such as the corridor of deviation, and as an actual history: the movement through the corridor of individual deviants. Both types of sequence must be examined if adequate explanation is to be achieved. But the vitally important point is that while processes of social becoming have this doubly historical character—and so can be described either logically or chronologically—the *relationship between the logic and the chronology is not fixed*.²³

From this angle, which can also be backed by Wolfgang Schluchter's developmental interpretation of Max Weber,²⁴ it becomes possible to specify the different relationships of generalizing and particularizing approaches to time. This specification is surely needed given the frequent and not very precise references to “the indispensability of the time factor” in reflections on sociohistorical inquiry.

Biernacki on culture and explanation

What about culture and explanation in Biernacki's paper? The first thing to note is that, unlike Hall, Biernacki does *not* make a distinction between how historians and social scientists usually go about studying culture. He draws no distinction between “individualizing” and “generalizing” approaches, and no clear distinction between explanation and interpretation either. His stance on this issue is conciliatory, as is Hall's, but also cloudy; as he put the matter in another essay: “The riddle of what constitutes an adequate explication and of how to distinguish

23. Abrams, *Historical Sociology*, 276.

24. W. Schluchter, *The Rise of Western Rationalism: Max Weber's Developmental History* (Berkeley, 1981). Cf. J. Habermas, “Geschichte und Evolution,” in Habermas, *Zur Rekonstruktion des historischen Materialismus* (Frankfurt am Main, 1976), 200-260.

causal claims from interpretative ones has vexed the best minds in philosophy for more than a century,” and leaves the question at that.²⁵

The second thing to note—in comparison to Hall—is Biernacki’s critical drive. As I observed earlier on, Biernacki develops a powerful argument against reductionist explanations that presuppose culture as the *ultimate* ground and constituent of human reality. He effectively dispenses with semiotic essentialism and reductionism, and rightly argues that showing the indispensability of culture as an explanatory factor is never sufficient to establish the priority of culture over other explanatory factors. In this respect his paralleling of the reductive drive of the old social and the new cultural history is very enlightening.²⁶

However, criticizing the weaknesses and illegitimate claims in culturalist positions, and developing a more solid position of one’s own, are two different undertakings.²⁷ And I must admit that I am more convinced by Biernacki as a critic than by Biernacki as a new theoretician of cultural “practice.” The main reason for this is that, although his argument for the pragmatic *dimensions* of meaning-production is interesting and important, this argument only establishes the thesis that the pragmatic aspects of signification cannot be *reduced* to the semiotic aspects; given the state of the debate, this is pretty important. But it does no more than this. Biernacki’s argument, in other words, does *not* yield the conclusion that the pragmatics of sign use is *more important* than its semiotics. But this is the conclusion he suggests by his “illustrative” use of the three “pragmatic” case studies of Karl Marx, Benedict Anderson, and himself, in which semiotic analyses are lacking.

My problem here is that Biernacki does not follow the methodological recipe he is handing over to the “culturalist” authors he is criticizing. Against culturalists he claims that it makes no sense to *postulate* the fundamental explanatory role of cultural factors because the explanatory role of culture can only be assessed in comparison with the role of *non*-cultural factors. I find myself in complete agreement with him on this issue. However, he suddenly “forgets” his own comparative recipe when he assesses the pragmatics of culture in his three favorite examples (in Benedict Anderson’s *Imagined Communities*, in Karl Marx’s analysis of the fetish-character of commodity-exchange in the first vol-

25. See R. Biernacki, “Method and Metaphor after the New Cultural History,” in *Beyond the Cultural Turn: New Directions in the Study of Society and Culture*, ed. V. Bonnell and L. Hunt (Berkeley, 1999), 73.

26. See Biernacki, “Method and Metaphor,” 62-68.

27. This does not mean that I am convinced that all of his critique aimed at “culturalists” is to the point. Labeling Carlo Ginzburg as a “reductionist” on the basis of one sentence in one article, for instance, is not my cup of hermeneutics. Although in the older studies by Ginzburg, such as *The Benandanti* (Turin, 1972), one can easily detect the traces of Marxist materialism, such as his attempt to link cultural phenomena like “witchcraft” to an amorphous materialist “low culture,” this is no longer the case in his later books. In *The Cheese and the Worms* (New York, 1982) the link between Menocchio’s ideas and material culture is very thin indeed, and in *Ecstasies: Deciphering the Witches’ Sabbath* (New York, 1991) the link between culture and “material circumstances” has all but disappeared. In the last study a semiotic analysis of myths is presented as the major explanation for a whole range of symbolic similarities in space and over time; only “in the last instance” does Ginzburg hypothesize an ancient shamanistic culture as the “underlying” explanatory factor. Significantly, culture is now explained by culture and no longer by any materialistic substratum. So the “reductionist drive” of culturalism is less serious in Ginzburg than Biernacki suggests.

ume of *Das Kapital*, and in his own study of the fabrication of labor). Instead of treating these three analyses of cultural practices as *theories*, and their explanatory claims as claims that can only be assessed in *comparison* with the explanatory claims of *other* theories on the same subject (there is no lack of competing explanations of, say, nationalism or the market economy), Biernacki presents his favorite theories *directly* as *adequate* descriptions and explanations of what is going on in “practice” (that is, in reality).²⁸

Alas, the concept of practice is as problematic as the concept of “experience” in this context. Both are frequently used as illicit substitutes for “reality” and must be confronted with the same critical question, namely the question how a representation relates to *alternative* representations of the same phenomena. Since naive realism is damaged beyond repair, there is no way around “critical” and plural versions of realism (such as “internal realism”²⁹). So if my reading of Biernacki is correct, one could accuse him on this point of doing exactly what he criticizes the Geertzian culturalists for: favoring one type of cultural explanation (pragmatic instead of semiotic) over all others without specifying the arguments for this preference in the cases under review.

Kane on culture and explanation

What about Kane’s view of culture and explanation? Like Biernacki, and unlike Hall, she makes no distinction between an individualizing, historical approach, and a generalizing, social-scientific approach to culture. She simply assumes that all scholars studying culture are haunted by the same set of questions, and that historians will be interested in general sociological questions, theories, and approaches (311-312).³⁰ Because this point is neither evident nor argued for, it is worth noting. Nevertheless, Kane—like Hall—is confronted with the problem of how to connect the generalizing and the particularizing approaches in her interesting study of the Irish Land Movement. And—like Biernacki—she is confronted with the problem of how to connect the “objective” semiotic analysis of meaning to the “agentic” analysis of meaning (that is, the pragmatic, “subjective” side of meaning-construction). Her problem is, in her terminology, how to connect “cultural structures” and “cultural models” to “action” and to “contingent events.” How the contingent events are explained by the cultural structures, however, is not easy to grasp.

28. In “Method and Metaphor,” 81, however, Biernacki is far more to the point than in his SSHA-paper, in which the comparative argument is completely absent: “If we do not legitimate our concepts and theories by attaching them to an ultimate foundation in the objects under study, we can do so by adopting the procedures of inquiry and contest best suited for laying bare our self-made construal of those objects.” The research design, therefore, should contain “an attempt to comparatively assess opposing explanations.”

29. See my “Historical Knowledge and Historical Reality: A Plea for ‘Internal Realism,’” in *History and Theory: Contemporary Readings*, ed. Fay *et al.*, 342-377.

30. Kane is even prescriptive vis à vis historians when she states that “cultural explanation of historical processes and transformations . . . *must* uncover the construction and transformation of meaning, and that in this historians *must* begin with the investigation of the structures of culture” (313, italics mine).

Actually, Kane faces *two* explanatory problems: the first one—and by far the most important—is the explanation of the different Irish “discourses,” the “Discourse of Retribution” and the “Discourse of Conciliation,” followed by their later “fusion” in the “Constitutional discourse around the dominant symbol ‘Land’.” The second explanatory problem is the history of the Irish Land Movement and war itself, in which these discourses functioned.

Her discourse on “discourses” and “semiotic relations” notwithstanding, her explanation of the genesis of these discourses looks pretty much like a traditional historical narrative. It is mainly a developmental history, going from one illustrative event to another (without any concern for the representative value of the illustrated cases, however). It is a history of “how certain events are given narrative form” and of how “competing groups often refer to the same event but narrativize it differently to promote their own discursive position” (318). “The crucial theoretical point,” Kane emphasizes, “is that the narrative itself is contested territory” (318). This part of Kane’s explanation is in fact a very short history of Irish ideology, that is, a history of—developing and changing—ideas.

How these ideas are linked to the actions and events of the Land Movement is the other explanatory problem, but Kane barely touches on it. Given her explicit focus on the explanation of cultural “structures of meaning,” this limitation is, of course, legitimate and was to be expected.³¹ Nevertheless, I have a problem with her proposals for the linkage of “symbolic structures” to agency, because she seems to be defending *two* positions instead of one.

In her introductory paragraph, she adopts the agency/structure position of social theorists like Anthony Giddens and Pierre Bourdieu,³² emphasizing the “explanatory focus on the recursivity of meaning, agency, and structure—and more specifically, the mutual transformation of social structure, social action, and cultural systems—in historical transformation” (311). On page 312 she asserts, accordingly, that “meaning structure and meaning construction together form the basis for cultural explanation in historical processes.” Structure and action here seem to be equally important. However, in her further analysis she silently moves to a more “structuralist” semiotic position. She posits that historians of culture “must begin with the investigation of the structures of culture” (312)—and thus, by implication, not with its *construction*. And although she recognizes that these “cultural models” are (potentially differently) interpreted by groups and individuals when applied to their experience, she holds that “these structures should be the *initial* theoretical and analytic focus in studying meaning construction” (314, *italics added*).

The ambivalence of Kane’s position can be detected in the following passage: Meaning construction must be analyzed in the first and last instance in reference to the internal, or the semiotic, structure of symbolic systems. I do not claim a causal priority of symbolic systems over human agency, contingent events, or other structural conditions.

31. Nevertheless, she announces on page 311 that she will present an “empirical analysis of political alliance during the Irish Land War, 1879–1882.”

32. See Van den Berg, “Social Mechanisms,” for a fundamental critique of Giddens and Bourdieu and the other modern structure/agency theorists.

However, people do refer first to cultural models as they try to make sense of situations, and shape their strategies for action. My assertion, then, is that the locus of meaning, and therefore the condition for meaning construction, is symbolic structures. (314)

In this passage Kane asserts, first, that although symbolic structures have *no* “causal *priority*” over other factors in historical explanation, they are the “*locus*” and a “*condition*” for meaning-construction and action. These assertions only lead to the conclusion that cultural structures are *indispensable* in historical explanation, no more, and no less; they don’t establish any *hierarchy* of explanatory factors. However, this is precisely the conclusion to which Kane is silently heading in two moves. In the first move, she jumps from indispensability to primacy: symbolic structures are needed “in the first and the last instance” when we explain (“analyze”) “meaning construction.” In the second move, she jumps from the primacy of symbolic structures to the primacy of semiotic analysis. Both moves are unwarranted, and are reminiscent of similar moves in Marxism with regard to the “determination of the economic structure in the last instance.”

Kane’s dilemma is unsolvable as long as she wants to argue that semiotic explanation is more important than other types of explanation, without arguing openly that symbolic structures are more important than other factors. Basically, she wants to establish the *primacy* of semiotic explanation without openly claiming a causal *priority* of symbolic systems over others (314). This, however, is impossible, because both claims are interlinked; one cannot cling to one while rejecting the other. This is, remarkably, exactly what Kane does. The reason for *not* claiming causal priority for culture is, of course, not difficult to imagine. This type of claim would lead her directly to the “culturalist” position that was so effectively demolished in Biernacki’s paper. As in Hall’s and in Biernacki’s papers, in Kane’s paper too the issue of explanation and interpretation thus remains thorny.

III. A RETURN TO NARRATIVE?

One of the most remarkable aspects of both Kane’s paper and of Hall’s book is the complete revaluation of “the narrative approach.” Sentences like Kane’s “Historical studies can be enriched by a narrative approach to the construction of meanings” (329), and “narratives are the consolidating component in a theoretical model of meaning construction and historical process. They also provide the method by which to investigate the recursive relationship of action, structure, and culture” (329) are quite unexpected. In the ears of a philosopher of history such advice given by a sociologist even sounds like an outright paradox. From the nineteenth century onwards, after all, mainstream sociology has often legitimized its very birthright, disciplinary domain, and explanatory potential by *negating* and *opposing* “the narrative approaches” of history and common sense. Have we come full circle at the end of the twentieth century also in this respect? It often looks this way, but some caution and analytic precision would be welcome in this respect, because the meanings of “narrative” and of “the narrative approach” are often highly imprecise. Because the condemnation of “narrative” in the past was often

based on impressionistic judgements, we should be careful not to make the same mistake now that we are exhorted to embrace “the narrative approach” again.

I find several features striking in the recent reevaluation of narrative by social scientists. First, as far as narrative philosophy of history is noticed at all, this usually happens in an uncritical way. Hayden White’s theory of historical narrative, for instance, is often taken at face value, and social scientists rarely take notice of the widespread criticism of White’s original position.³³

Second, it strikes me that as far as social scientists present explicit definitions of “narrative,” these definitions usually are lookalikes of definitions already developed in the philosophy of history. Consider the recent attempts to define narrative in terms of event-structures and to interpret narrative explanation in terms of causal linkages between those event structures, such as is proposed by sociologists like Larry Griffin and Roberto Franzosi.³⁴ Arthur Danto already developed a similar proposal in his *Analytical Philosophy of History* in 1965, and this has been discussed for some time since.³⁵ This does *not* imply that their proposals would not be interesting or unworthy of discussion—far from it—but only that the earlier discussions of Danto’s narrative philosophy are the logical starting point for further debates of similar positions. Kane’s remark that historians might profit from the “narrative approach” suffers from similar flaws, because historians and philosophers of history have actually been discussing narrative for quite some time.

This leads me to my third and final remark on the recent enthusiasm of social scientists about narrative. The latest fruit hanging from the narrative tree carries the promising name of *analytic narrative*.³⁶ Whatever this neologism means, it surely is a clear indication that the disciplinary maps of the social sciences and history—and thus of social science history—have been reshuffled completely since the 1960s. Analysis and narrative are not only no longer regarded as competing approaches to the past, but are now openly connected to each other.

When, however, one looks behind the veils of this newlywed couple, one discovers some pretty worn-out clauses in their marriage contract: “Our approach is narrative; it pays close attention to stories, accounts, and context. It is analytic in that it extracts explicit and formal lines of reasoning, which facilitate both exposition and explanation,” the authors state in their introduction.³⁷ “Where possible, we use formal arguments. We . . . do not provide explanations by subsuming cases under ‘covering laws,’ in the sense of Hempel.³⁸ Rather we seek to account

33. For this debate see *History and Theory: Contemporary Readings*, ed. Fay *et al.*, and my article “Can Histories Be True?”

34. See L. Griffin, “Narrative, Event-Structure Analysis and Causal Interpretation in Historical Sociology,” *American Journal of Sociology* 98 (1993), 1094–1133; R. Franzosi, “A Sociologist Meets History: Critical Reflections upon Practice,” *Journal of Historical Sociology* 9 (1996), 354–392; *New Methods for Social History*, ed. L. Griffin and M. van der Linden (Cambridge, Eng., 1999).

35. A. C. Danto, *Analytical Philosophy of History* (Cambridge, Eng., 1965).

36. R. Bates, A. Greif, M. Levi, J.-L. Rosenthal, and B. Weingast, *Analytic Narratives* (Princeton, 1998), reviewed by Brian M. Downing, *History and Theory* 39 (2000), 88–97.

37. Bates *et al.*, *Analytic Narratives*, 10.

38. Rejecting the Hempelian covering-law theory of explanation has turned into something like a ritual beating of a dead horse. See W. Salmon, *Four Decades of Scientific Explanation* (Minneapolis, 1990).

for outcomes by identifying and exploring the mechanisms that generate them. We seek to cut deeply into the specifics of a time and place, and to locate and trace the processes that generate the outcome of interest.”³⁹

So, analytic narratives are mainly characterized by opposing them to the covering-law model of explanation, by their interest in formal modeling (especially rational-choice modeling), and by their interest in time and place. We are thus, in effect, presented with the application of “analytic” rational-choice theory to “narrative” history. So in the end, the old opposition of “analysis” to “narrative” pops up again, and the same goes for their unquestioned traditional linkage to “social science” and “history.” In other words, behind the flashy new formula of “analytic narratives” the old conceptual oppositions are still in place. Instead of jumping on this shiny new narrative train, it would be better in my view to reflect a bit more on the conceptual rails on which these trains have been moving for the last decades. Only then will we be able to find out how we ended up in the remarkable new predicament in which sociologists are advising historians to follow “narrative approaches” when they are explaining culture.

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39. Bates *et al.*, *Analytic Narratives*, 11-12.