How Narratives Explain*

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Do NARRATIVES explain? The view defended here is that they do, and do so as narratives and not, for example, by paraphrase into some more formal model which does the actual work of explanation. The sort of cases I take as instances of narrative explanations include histories, ethnographies, and psychoanalytic case studies. Narratives explain even though such explanations, on my account, lack those specific characteristics, namely, well-defined formal and semantic features, often regarded as desiderata in an analysis of explanation. The absence of clear semantic and syntactic characteristics engenders skepticism with regard to narrative as a form of explanation. Defending an affirmative answer to the opening question requires explicating a logic of narratives *qua* explanations which accounts for, in the absence of the expected or typical logical features, how narratives explain.

In my first part, I examine certain presuppositions about what an explanation must be—what I call the explanation-as-argument model. I explore the reasons why narrative explanations cannot be made to fit such a model. My second part surveys some attempts, notably those by Arthur Danto and Hayden White, to account for how narratives, as narratives, explain. None of these accounts, I argue, are satisfactory. Either they are not sufficiently general to include plausible cases of narrative explanation, or they leave mysterious the central question of the relevant logical features by which narratives explain.

The third part contains an analysis of an exemplary case of narrative explanation: Clifford Geertz's classic paper, "Deep

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Play: Notes on a Balinese Cockfight."[1] Geertz, in this essay, tells a now-famous story which portrays a specific event—a cockfight—as a reenactment of the social structure among Balinese males. The story of how a cockfight is organized becomes a story of a structure of Balinese society writ small.

Geertz's mode of explanation invokes no laws or even probabilistic generalizations. What it does do is to construct a particular story line, that is, a way of reading the event of the cockfight as a tale about Balinese society. This is the cockfight as narrative. But his narrative presents each event narrated as a token of an event type. Geertz's various narrations recapitulate the general tale he tells; everyday life in Bali instantiates general facts about its social structure. Yet Geertz's telling invokes no intrinsic difference between ethnographic tales and fictive constructions. My analysis attempts to clarify just how his telling is also an explanation. In the context of my analysis of Geertz's essay I examine, as well, certain criticisms of his work. These criticisms, I argue, mistake how Geertz's particular narrative, and how narratives generally, explain.

The Explanation as Argument

How do narratives explain? Here is one example which, though a mundane tale, is celebrated in the philosophic literature. It goes like this:

> Let the event to be explained consist in the cracking of an automobile radiator during a cold night. The [factual] sentences . . . may state the following initial and boundary conditions: The car was left in the street all night. Its radiator, which consists of iron, was completely filled with water, and the lid was screwed on tightly. The temperature during the night dropped from 39 degrees F. in the evening to 25 degrees F. in the morning; the

[1] Clifford Geertz, "Deep Play: Notes on a Balinese Cockfight," *Daedalus* no. 101 (Winter 1972). Citations are to P. Rabinow and W. Sullivan, eds., *Interpretive Social Science: A Reader* (Berkeley: University of California Press, 1979).

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> air pressure was normal. The bursting pressure of the radiator material is so and so much. [Other sentences] . . . would contain empirical laws such as the following: Below 32 degrees F., under normal atmospheric pressure, water freezes. Below 39.2 degrees F., the pressure of a mass of water increases with decreasing temperature, if the volume remains constant or decreases; . . . [etc.].
>
> From statements of these two kinds, the conclusion that the radiator cracked during the night can be deduced by logical reasoning; an explanation of the considered event has been established.[2]

Hempel tells this simple philosophical story to remind historians that if they wish to be scientists, their narratives must be reformulated into this form, or some analogue of it. For Hempel, it is not the narrative which explains; narrative form is an accidental feature of this explanation.

A more traditional narrative is from G.M. Trevelyan's *England Under the Stuarts*. The passage which concerns me is one analyzed by Ernest Nagel in *The Structure of Science*.[3]

> They took ship secretly, galloped in disguise across France, and presented themselves in the astonished streets of Madrid. Charles, though he was not permitted by Spanish ideas of decorum to speak to the poor Princess, imagined that he had fallen in love at first sight. Without a thought for the public welfare, he offered to make every concession to English Catholicism, to repeal the Penal Laws, and to allow the education of his children in their mother's faith. The Spaniards, however, still lacked the guarantee that these promises would really be fulfilled, and still refused to evacuate the Palatinate. . . . Meanwhile a personal quarrel arose between Buckingham and the Spanish nation. The favorite [i.e., Buckingham] . . . observed neither Spanish etiquette nor common decency. The lordly hidalgos could not endure the liberties he took. . . . The English gentlemen, who soon came out to join their runaway rulers [i.e., Buckingham and Charles], laughed at the barren lands, the beggarly populations and the bad inns through which they

[2] Carl Hempel, "The Function of General Laws in History" (1942), in *Aspects of Scientific Explanation* (New York: Free Press, 1965), p. 232.

[3] Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace, 1961), pp. 564–565.

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> passed, and boasted of their England. They were not made welcome to Madrid. . . . They began to hate the Spaniards and to dread the match. Buckingham was sensitive to the emotions of those immediately around him, and he soon imparted the change of his own feelings about Spain to the silent and sullen boy, whom he could always carry with him on every flood of short-lived passion.[4]

My concern is with what Nagel says about this explanation. On Nagel's reading, Trevelyan's narrative is just a type of genetic explanation, that is, an explanation in which some chain of events makes probable the occurrence of some other event.

> A genetic explanation of a particular event is in general analyzable into a sequence of probabilistic explanations whose instantial premises refer to events that happen at different times rather than concurrently, and that are at best only some of the necessary conditions rather than a full complement of sufficient ones for the occurrences which those premises help to explain.[5]

In this account, as well, the narrative form is inessential to the explanation given. Indeed, the narrative is, if anything, a hindrance to explicating what the explanation is. Explanation is analyzed in terms of a fixed form (it need not, of course, be a D-N style model) into which any candidate explanation must be reparsed if it is to be adjudged acceptable.

Accounts of the Hempel and Nagel sort I term "explanation-as-argument" models. If one adopts an explanation-as-argument model, narratives are not explanations, at least insofar as they are narratives. The form of a narrative is not, on these accounts, part of the form of explanation.

As I have argued elsewhere, there is no good reason to accept such Procrustean theories of explanation, that is, the view that some one set of necessary conditions adequately explicates whatever is to count as a proper explanation.[6]

[4] *Ibid.*

[5] *Ibid.*, p. 568.

[6] See my essay "Narrative Explanation: The Case of History," *History and Theory* 27 (1988): 1–13.

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Rather, the philosophical problem is to square disciplinary practices in, for example, history, anthropology, and psychoanalysis with an analysis of explanation which clarifies and accommodates them. The issue is not the possibility of narrative explanations; we have, as the old joke goes, seen it done. Nor is there a question of the legitimacy of such explanations. Absent a positivist belief in the unity of method, the term "narrative explanation" no longer need be classed with the likes of "military music" or "business ethics." Yet, without a theory of narrative explanations, there exists only an unanalyzed practice, a habit tolerated but not at all understood. Recent commentators, indeed, either simply despair of explicating narrative explanations[7] or content themselves with defending narrative explanations as *sui generis,* which is to say, as unanalyzable.[8]

Narrative theorists commonly separate within their classificatory schemes nonfiction narratives—those constrained by facts—from the fictional.[9] But this constraint does not function in any essential way to limit the range of storytelling conventions for purveyors of factual narratives. Hayden White, for one, convincingly shows that history may be emplotted in accord with the conventions of differing genres.[10] Following White's lead, psychoanalytic theorist Roy Schafer argues that case histories may be variously emplotted. The form provided individual lives, like that given histories, is more a matter of creation than of discovery.[11] Histories, of individuals and of epochs, are underdetermined; facts consti-

[7] See, e.g., R. F. Atkinson, *Knowledge and Explanation in History* (Ithaca: Cornell University Press, 1978), pp. 130–139.

[8] W. H. Dray, "Narrative versus Analysis in History," in J. Margolis et al., eds., *Rationality, Relativism and the Human Sciences* (Amsterdam: Martinus Nijhoff, 1986).

[9] Northrop Frye, *The Anatomy of Criticism* (Princeton: Princeton University Press, 1957), p. 75; R. Scholes and R. Kellogg, *The Nature of Narrative* (New York: Oxford University Press, 1966), p. 13.

[10] Hayden White, *Metahistory* (Baltimore: Johns Hopkins University Press, 1973).

[11] Roy Schafer, *Narrative Actions in Psychoanalysis* (Worcester, Mass.: Clark University Press, 1981).

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tute no essential constraint on scientific theorizing or on the historian's art.

More troubling, however, is realizing that the truth value of a history will not be some simple function of the truth value of its individual statements of fact. The semantics of a nonfiction narrative, the claim is, are not those of an extended conjunction. The conjunction model is both too broad and too narrow. It is too broad inasmuch as it cannot distinguish between narrative and chronicle; the conjunction model rules true lists which are not explanations.[12] To see why the model is too narrow, consider any case where a historian makes certain errors of fact which, in the context of the history written, are not critical to the argument. A conjunction model, however, would require declaring that the history is false. The reasons for asserting that a narrative is true is no straightforward function of the truth of the facts cited.

As even more basic problem with the semantics of history arises on the assumption that the elements of some such conjunction are themselves unproblematic entities. What events there are are a function of the questions one asks, of the story one wishes to tell. Lawrence Stone, commenting on the historiography of the *Annales* school, writes:

> The French historians . . . developed a standard hierarchical arrangement: first, both in place and in order of importance, came the economic and demographic facts; then the social structure; and lastly intellectual, religious, cultural and political development. . . .
>
> . . . Since only the first tier really mattered, and since the subject matter was the material conditions of the masses, not the culture of the elite, it became possible to talk about the history of continental Europe from the fourteenth to the eighteenth centuries as '*l'histoire immobile.*' Professor Le Roy Ladurie argued that nothing, absolutely nothing, changed over those five centuries, since the society remained obstinately imprisoned in

[12] Louis O. Mink, "Narrative Form as a Cognitive Instrument," In R. Canary and H. Kozicki, eds., *The Writing of History* (Madison: University of Wisconsin Press, 1978), p. 143.

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> its traditional and unaltered *'écodémographie.'* In this new model of history, such movements as the Renaissance, the Reformation, the Enlightenment and the rise of the modern state simply disappeared. Ignored were the massive transformations of culture, art, . . . etc. which took place among the higher echelons of society in those five centuries. This curious blindness was the result of a firm belief that these matters were all parts of the third tier, a mere superficial superstructure.[13]

Insofar as certain, very familiar, perhaps significant, events are not part of the account which Ladurie deems relevant, they do not enter into a history of the period in question. This suggests, in a dramatic way, that the conjunction model has things *radically* wrong. A narrative is not determined by sequencing some prior set of events. Rather, what comes first is some more general view of what counts; the particular events—the elements relevant to one's narrative—emerge from this.

But why should the relativity of the constituent elements to the tale told be an obstacle to determining a semantics for narratives? Here a full appreciation of the distinction between narrative and chronicle is critical. The claim is this: an historical narrative comprises, in a sense yet to be made specific, not a conjunction of propositions but, rather, something like a single proposition. But is not a conjunction, however many conjuncts it contains, also a single proposition? To put matters that way is to miss the point. A logical conjunction is a single statement, grammatically, but the truth value of that statement is determined by the assignment of truth values to its parts. The whole-part relation is precisely what is problematic, however, in the case of a narrative explanation.[14]

Narratives make problematic precisely this conventional

[13] Lawrence Stone, "The Revival of Narrative: Reflections on a New Old History," in *The Past and the Present* (London: Routledge & Kegan Paul, 1981), pp. 78–79.

[14] Mandelbaum, in Sidney Hook, ed., *Philosophy and History* (New York: New York University Press, 1963), p. 47; Mink, "Narrative Form," p. 147.

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logical relation since, the claim goes, in a narrative evaluation of the parts is a function of the whole. This claim is an item in the "budget of paradoxes" Louis Mink formulates when specifying the distinctive features of narratives as a form of human judgment.[15] More precisely, the paradox is this: as a history, a narrative is a sequence of events, but, as a narrative, it is a single unit, the semantic equivalent of an atomic statement.

> It is an unsolved task of literary theory to classify the ordering relations of narrative form; but whatever the classification, it should be clear that a historical narrative claims truth not merely for each of its individual statements taken distributively, but for the complex form of the narrative itself. . . . The cognitive function of narrative form, then, is not just to relate a succession of events but to body forth an ensemble of interrelationships of many different kinds as a single whole.[16]

What is the argument for attributing to a narrative this particular semantic integrity—that the truth of a narrative is not necessarily determinable from the truth of its parts? Note that the suggestion is not that narratives are indifferent to facts. The claim, rather, identifies as significant the relations a narrative portrays, and that this relation is what requires that a narrative be judged primarily as a whole.

The question of semantic evaluation—of the conditions of truth and falsity—is what is critical here. Novels and symphonies have properties which their words and notes, taken piecemeal, lack. This is no obstacle to evaluation, however, because aesthetic worth is not a simple function of a part-whole relationship; however, truth, on the usual semantic analysis, is. The problem here is one of deciding, in the absence of a determinative part-whole semantic relation, what it could mean to say that a narrative is true.

These reflections on the part-whole relation, it might be

[15] Mink, "Narrative Form," p. 145.
[16] *Ibid.*, p. 144.

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thought, reveal the complexity of narratives, not their immunity to comparative evaluation. Mink insists, however, that there is no basis for adjudicating between conflicting narratives.

> So we have a second dilemma about historical narrative: as historical it claims to represent, through its form, part of the real complexity of the past, but as narrative, it is a product of imaginative construction, which cannot defend its claim to truth by any accepted procedure of argument or authentication.[17]

The special role of narrative in our cognitive scheme, on this account, is that what a nonfiction narrative represents is not some matter of fact. Put another way, the claim that narratives explain embodies, as part of the logic of explanation, the assertion that a narrative is true. But the inability to specify how the truth of the whole is a function of its parts renders otiose, Mink argues, the basis for this claim.

Mink couches his argument here at a very general level. The failure of the conjunction model, on the one hand, and the absence of any determinative rules of narrative construction, on the other, are his reasons for concluding that a narrative is fundamentally an imaginative representation of the past. Its truth is a function neither of its constitutive facts nor of its correspondence to some independent reality.

An example here helps buttress and, I suggest, extend the Minkian argument just rehearsed. The examples show how issues of truth and significance become detached from the particulars of a narrative and rest on prior judgments. There is no *general* standard by which to assess the semantics of narrative explanations.

Morton White, in *defending* the thesis that there is no standard, apart from the preferences of an historian, which establishes what is of importance, remarks:

[17] *Ibid.*, p. 145.

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> Most historians may believe that it is more important for the world to know about Marx's political ideas than it is to know about his carbuncles, and that it is more important for the world to know about changes in the forms of political power in America than it is for the world to know about changes in women's clothing, but these are value-judgments even if they are generally shared.[18]

White's mild relativism here (circa 1965) provoked an irate response from historian Lee Benson.

> Granted that nothing in the nature of the universe entitles us to say that someone who provides a true history of political power in the United States has done work *intrinsically* superior to someone who provides a true history of women's clothing in the United States. Must we end our analysis where Mr. White ends his and thus use rigorous logic to arrive at the absurd conclusion that no objective basis can be found to rank true histories? I do not think so.[19]

> Granted that we are not entitled to rank a true history of Soviet Russia written by Carr above a true history of American's women clothing by a historicist—except on the admittedly arbitrary ground that the first is more likely than the second to deepen men's understanding of how best to go about shaping their societies and their lives.[20]

My point here is not, or not just, that people have found it important to write histories of women's clothing. My claim is that we now understand that such a history at least might illuminate issues of political power, though not in the sense of "political" which either White or Benson had in mind. More generally, the irony of the contrast of priorities both men employ reveals how uncertain any ordering of priorities—any prejudgments of what work is worth doing, of what might be revealing—is, in fact, going to be. Both Le Roy Ladurie and Allison Lurie have tales to tell regarding what is significant. Their tales differ not just in their particulars but at the very

[18] Morton White, *The Foundations of Historical Knowledge* (New York: Harper & Row, 1966), pp. 260–261.
[19] Lee Benson, in Hook, *Philosophy and History*, p. 38.
[20] *Ibid.*, p. 40.

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core, in the sort of factors judged historically important. With respect to specifying what is of significance, the narratives are inconsistent. Yet *this* inconsistency does not force the conclusion that at least one of the narratives is false.

Narratives are not maps, at least not if maps are taken to be articulations of some prior shape which history or culture or society has and which the relevant disciplinary specialist charts. One defense of this claim has primarily been sociological. It is simply the case that, when doing history, there is more latitude regarding identification of significant factors than there is, say, in physics or in chemistry.

A more general epistemological point has been urged, however, a point which underlies the sociological observation of plurality. Two criteria appear available for semantic evaluation of narratives: one based on a part-whole model, the other on a correspondence theoretic view of narrative truth. (The disjuncts are not exclusive.) The part-whole model was considered and rejected. However, found wanting also is the only other apparent candidate. Narratives represent, my examples suggest, the interests of narrators; these interests can generate inconsistent accounts which can be neither ruled out nor reconciled.

Narrative Conventions

Rather than attempting to look beyond conventions of narrative, perhaps the proper strategy is to insist that it is these very conventions which do the work of explanation. In this respect, narrative conventions are constitutive of historical practice and determinative of historical explanation. The suggestion I am entertaining is that the conventions define the possibilities of explanation as well.

This position is most closely associated with, and best developed by, Hayden White, and it is to his explication of it

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that I turn. White's official account of narrative explanation is that of "explanation by emplotment." The view is this: A historian has at his or her disposal a number of culturally received plot types. In one respect, the historian has no choice but to employ these forms; in another respect, however, these forms are what there is to explanation.

> A historical interpretation, like a poetic fiction, can be said to appeal to its readers as a plausible representation of the world by virtue of its implicit appeal to those "pre-generic plot-structures" or archetypal story-forms that define the modalities of a given culture's literary endowment. Historians, no less than poets, can be said to gain an "explanatory affect"—over and above whatever formal explanations they may offer of specific historical events—by building into their narratives patterns of meaning similar to those more explicitly provided by the literary art of the cultures to which they belong.[21]

Important here is understanding what White intends by designating a certain style a "mode of explanation." He means by this only to gesture to certain very general types of exemplars. These, he maintains, exhaust the ways of constituting connections among events in a narrative.[22] As becomes clear in my own analysis, I find something very right in his suggestion that a valid explanation is linked to an exemplary case—a paradigm—of a certain kind. However, White's interest in explanation by exemplification extends no further than a concern for a classification of types. Once White compiles a list of categories sufficiently general for the purposes of his typology of connective strategies, he is done. His analysis of explanation, in this respect, parallels his discussion of tropes, and to the same end. White's concern is not logical but typological.

White's analysis of narrative explanation envisions explanation as a process of "reduction to the familiar." Indeed, White

[21] Hayden White, "Interpretation in History," in *Tropics of Discourse* (Baltimore: Johns Hopkins University Press, 1978), p. 58; see also p. 61.

[22] *Ibid.*, p. 63.

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himself, when addressing the issue of "the explanatory effect of historical narrative,"[23] underscores just this point:

> Historians seek to refamiliarize us with events that have been forgotten. . . . In looking at the ways in which such structures took shape or evolved, historians refamiliarize them . . . by showing how their developments conformed to one or another of the story types that we conventionally invoke to make sense of our own life histories.[24]

But there are at least three problems with any attempt to employ White's typology as an analysis of how narratives explain. First, note that White's is a reductionist strategy; the atoms of this reduction are the categories of figurative discourse and a typology of connective strategies. A narrative explanation is a type of emergent property, built from a felicitous conjunction of such atomic forms. Given a particular work of history, White can only argue that, properly unpacked, it fits into his typological scheme. White has title to being called, perhaps, the Linneaus of narrative explanation. However, his typology reveals nothing with regard to explanatory logic; about what makes a story type an explanation White has nothing to say. If explanation is an emergent property, just how it emerges remains unexplicated by White's reduction. In addition, nowhere does White offer an argument justifying his claim that forms of explanation are limited to the forms of figurative discourse he identifies.

Second, White's analysis relies on an implausible psychology of reading. What explains, for White, is the familiar. The familiar, in this case, are the story types available to a historian.

> The effect of such emplotment may be regarded as an explanation, but it would have to be recognized that the generalizations that serve the function of universal laws . . . are

[23] Hayden White, "The Historical Text as Literary Artifact," in Canary and Kozicki, *Writing of History*, p. 51.
[24] *Ibid.*, p. 61.

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> the *topoi* of literary plots, rather than the causal laws of science.[25]

This is problematic inasmuch as one could name a number of authors—Erik Erikson, Norman O. Brown, Nancy Chodorow, and Christopher Lasch, for example—whose explanatory strategies invoke the less familiar to explain what is commonplace and better accepted. Indeed, White's typologies work against themselves, at least in some cases. For if emplotment provides narratives with their "explanatory affect," and modes of explanation provide the particular ordering, how does White account for explanatory strategies—for example, Marxist or Freudian—whose basic premise is precisely that the stories we usually tell ourselves are mystifications? The familiar may serve to obscure deeper, less readily known or understood reasons which better explain.

Finally, White's view, insofar as it substitutes a psychology of reading for a logic of explanation, is an empirical hypothesis about readers which a typology of plots, at best, only partially justifies.

> In my view, we experience the "fictionalization" of history as an "explanation" for the same reason that we experience great fiction as an illumination of a world that we inhabit along with the author. In both we recognize the forms by which consciousness both constitutes and colonizes the world it seeks to inhabit comfortably.[26]

As a "reader response" theory of explanation, White's proposal falters since he nowhere gives evidence that audiences *in fact* respond to the typological features he identifies. That is, it is one thing to claim that all narratives fit, as a matter of fact, into a typology which reveals them to be more or less degenerate forms of classic *mythoi*. It is quite another, however, to maintain, as White does, that it is in virtue of this feature that

[25] Hayden White, "The Question of Narrative in Contemporary Historical Theory," in *Content of the Form* (Baltimore: Johns Hopkins University Press, 1987), p. 44.
[26] White, "Historical Text," p. 61.

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a reading public responds to or comprehends these narratives as explanations. What may be adequate as a classificatory schema for modes of explanation is not, by virtue of this feature, an analysis of explanation.

There is a another feature which, because common to discussions of narrative explanations, merits examination prior to considering my account. The positions which concern me here maintain that an explanation requires, as part of its structure, inclusion of some type of generalization. Hempel, for example, originally insisted on generalizations that had the standing of scientific laws. This requirement, applied to the human sciences, posed special difficulties since these fields have no laws to offer. Other accounts weaken this requirement in various ways. Motivating this demand is a concern to specify how what is to be explained relates to prior beliefs. Generalizations, of course, do this.

Hempelian models are already extensively and well criticized in the literature, and I shall not rehearse those points here. I concern myself with a broader but related claim, namely, that the generalizations of some sort, for example, the truisms of common sense, are the reason why narratives explain. One's everyday knowledge suffices, on this view, for purposes of comprehending narrative explanation. So, for example, John Passmore writes:

> For the most part, then, there is nothing much to say about historical explanation; nothing that cannot be said about explanation in everyday life. Scientific explanation is the peculiar thing—the odd man out—in the general use of explanation: peculiar in its overriding concern with what is only, from the historian's as from the everyday point of view, one type of explanation; . . . Occasionally, something he [the historian] says will be puzzling, or he thinks it might puzzle us, and so he suggests an explanation which will have that sort of adequacy and intelligibility we expect in everyday life.[27]

[27] John Passmore, "Explanation in Everyday Life, in Science, and in History," in G. Nadel, ed., *Studies in the Philosophy of History* (New York: Harper & Row, 1965), pp.

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The view that our unrefined expectations suffice for an appreciation of how narratives explain recalls Hayden White's claim that explanation is reduction to the familiar. The examples brought to bear against White's analysis are relevant here as well. Whatever one thinks of, for example, *Young Man Luther* or *The Great War and Modern Memory*, it will not do to describe the sort of explanations found in these works as processes of creating order to appeal to the familiar.

Indeed, in the case of Fussell's haunting investigation of the impact of the experience of the First World War on literary language, the emphasis is on how writers found this language inadequate to their experience. This war was something too new. Fussell recreates for us something of their struggle to give voice to the horrors peculiar to modern war. His explanation, rather, seeks to recreate or reconstruct a certain problem and the processes by which writers overcame it; the problem and its resolution, however, lie rather far from the experience of most readers.

Still to be considered, however, is Danto's more sophisticated approach. Danto intends to explicate and defend, by his analysis, narrative as a form of explanation.[28] It is just that he believes that narrative explanations require generalizations of a certain type.[29] Danto outlines a simple-seeming structure for narrative explanations: (1) x is F at $t-1$; (2) H happens to x at $t-2$; (3) x is G at $t-3$.[30]

This has the beginning-middle-end structure of a narrative. Danto argues that his is an analysis of narrative explanations because, in virtue of the structure and relation of steps (1)–(3)

34–35. For a related view, see W. B. Gallie, *Philosophy and the Historical Understanding*, 2nd ed. (New York: Schocken, 1968), pp. 112–115; M. Scriven, "Truisms as the Grounds in Historical Explanations," in P. Gardner, ed., *Theories of History* (New York: Free Press, 1959), esp. pp. 264–271.

[28] Arthur Danto, *Narration and Knowledge* (New York: Columbia University Press, 1985), p. 201.

[29] *Ibid.*, pp. 238, 239.

[30] *Ibid.*, p. 236.

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there is a *narrative;* and in virtue of the content of step (2)—the relation *H* bears to *F-G*—there is an explanation.

The demand for generalization enters at step (2). *H,* he claims, links the state of *x* at $t-1$ to the state of *x* at $t-3$ because, characteristically, *H* is a state associated with such a transition. For example, at $t-1$ I am driving along untroubled; at $t-3$ I have swerved and hit the curb. When I add that I coughed at $t-2$, this links my untroubled driving and the subsequent mishap. A historian's task, as Danto conceives of it, is to discover what the circumstances surrounding a change were, and so what condition *x* possesses at $t-2$ that explains the transition.

Put another way, Danto imagines that a historian knows, in a general way, what might cause certain sorts of changes. What is not known, prior to specific investigation, is what sort of case confronts us, and so which law applies. Once the description is in hand, however, the relevant generalization is readily specifiable. Danto's uncontroversial claim is this: given some pivotal state—Danto's predicate *H*—one can in all likelihood find a generalization which identifies *H* as a state regularly linking *F* and *G.* Danto's central claim, however, is much stronger; it asserts that *it is in virtue of the generalization that a narrative explains.* "For the only point I am seeking to make is that the construction of a narrative requires, as does the acceptance of a narrative as *explanatory,* the use of general laws."[31]

Danto has no direct argument for this stronger claim, remarking that he deems it "beyond argument" that *H* must play the role he suggests.[32] The belief here is that any causal connection expresses a type of regularity, and *H,* since it connects *F* to *G,* must recall such a regularity.

Danto's account is suspect with respect both to its internal logic and to its scope. To begin, Danto does not make that case,

[31] *Ibid.,* p. 239.
[32] *Ibid.,* p. 238.

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even for his chosen examples, that generalizations play the necessary role he assigns them. Consider the quote Danto cites from C.V. Wedgwood's *The Thirty Years War*. "[King James's] son and his favourite Buckingham, indignant at their reception in Spain whither they had gone to hasten the negotiations, returned to England and declared themselves unwilling to participate further in the unholy alliance."[33] Danto takes this to be a perfectly legitimate example of a narrative explanation. He states, with such an example in view, that "once, however, we have the explanation, it is not difficult to find the required general description and the law."[34] Yet what is the correct general description here?

> But the sort of thing which might make a man like the Duke of Buckingham change his mind about the marriage of a prince are not so easy to enumerate in advance. Once we know what turned the trick, we can bring it under a general principle readily enough. But at the same time, that very general principle admits of so many, and so various a set of instances, that we see no reason why this rather than that should have caused the Duke to change his mind . . . .[35]

The problem, in other words, is that the generalization imagined to underlie Wedgwood's explanation—say, that vain people are likely to make rash decisions if their pride is wounded—even when conjoined with the appropriate sort of description of Buckingham, does not permit an understanding of why precisely this event, and not some other, triggered Buckingham's reaction. So while there is a characteristic—Buckingham's becoming indignant—which fits Danto's predicate *H*—"that which happens to *x* and which causes *x* to change"[36]—it is simply false that citing *H* explains *F-G*, *if*, by "explanation," one means, as Danto does, that *H* explains only because it connotes a general law that links events. For what

[33] *Ibid.*, p. 240.
[34] *Ibid.*
[35] *Ibid.*, p. 244.
[36] *Ibid.*, p. 238.

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work is the law doing in the case just described? It does *not* serve to connect the *particular* events, as even Danto concedes.

Consider, with regard to the alleged necessity of generalizations, an explanation of John Hinckley's *obsession* with Jody Foster. Such an account would doubtless involve any number of generally accepted views about people who manifest certain behaviors. But the explanation of why *Jody Foster* is another matter altogether, one which calls not for generalizations but for particulars about Hinckley.[37] If the question is: why is Hinckley dysfunctional, one sort of story is told; if the question is: why is he obsessed with this particular person, a very different story might emerge. In the latter case, it is the details of Hinckley's biography which explain if anything does. Given the diversity of problems for which one may seek explanation, and the concomitant variety of ways of explaining relative to the question asked, the claim that a generalization is necessary ceases to be plausible.

Geertz's Paradigm

In its simplest form, I borrow my leading insight from Thomas Kuhn's notion of a paradigm as it is famously and notoriously expounded in his classic *The Structure of Scientific Revolutions*. A core sense of that vexed term refers to problem-solving models definitive for an area of study. A paradigm, in this respect, is identified with a concrete achievement.[38] An achievement becomes paradigmatic insofar as theoretical speculation and disciplinary practice coalesce around it. Kuhn's examples of revolutionary paradigms are well known and much discussed; they include Copernicus's

[37] For an explication of this type of analysis, see Alan Garfinkel, *Forms of Explanation* (New Haven: Yale University Press, 1981).

[38] See, e.g., Thomas Kuhn, *The Essential Tension* (Chicago: University of Chicago Press, 1977), pp. 284, 306–307, 318; see also his *The Structure of Scientific Revolutions*, 2nd ed. (Chicago: University of Chicago Press, 1975), pp. 175, 187–191.

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repicturing of the solar system, Darwin reimagining the process of speciation, and Einstein's reconceptualization of physics.[39] In terms of intellectual life, each case solved problems outstanding in its immediate area of concern and, more interestingly perhaps, provided models and metaphors for inquiries far removed from the initial field of application.

The concrete examples which function as paradigms need not, of course, be on this revolutionary a scale. Explanations do not "win out"—become paradigmatic—for reasons related only to scientific standards. Social factors are relevant. However, I believe that there is a logic internal to the forms of explanation with which I am concerned, and it is just to the details of that logic that I address my analysis.

It is the acceptance of an explanation of a recognized phenomenon which constitutes a paradigm. A received paradigm need not, and usually is not, the only candidate; nor is the triumph of a particular account a function of its "goodness" as determined by some neutral standard of what constitutes goodness of explanation. Rather, it is when an explanation becomes a model explanation for the related science that there is a paradigm in the relevant sense.[40]

Paradigms are solutions to problems posed by concrete phenomena; they are answers to puzzles. As solutions, they become paradigmatic insofar as they are sufficiently flexible to allow of extension to related phenomena, or phenomena perceived as related. Kuhn speaks of how paradigms characteristically induce gestalt shifts; having become convinced of a certain paradigm, researchers find that pattern in areas not previously seen to possess it. Paradigms have as important role as they do, in Kuhn's account, because Kuhn's way of reconstructing the history of science denies that scientific practice can be characterized by any fixed set of rules or

[39] See, e.g., Kuhn, *Essential Tension,* pp. 226–227.

[40] See the classic analysis by Ludwig Fleck, *Genesis and Development of a Scientific Fact* (Chicago: University of Chicago Press, 1979).

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procedures. The notion of a distinctively scientific method is a *post hoc* myth; the rules are promulgated after the fact to rationalize practices already in place and adopted for other reasons. Paradigms fill the lacuna left by the lack of determinate rules for scientific practice. Because tied to a concrete practice and distinct form, they say, in effect, how solutions should look. Problem-solving accounts, when reduced to formulas, are, Kuhn argues, nowhere near as effective; formalisms do not always communicate what those to be initiated into a field need to know.

My claim is that explanations are paradigms; acceptance of a particular type of solution as paradigmatic is what it is to have an explanation. There is no analysis of explanation, only of accepted solutions, including, perhaps, how these models became paradigmatic. What makes a solution into an explanatory paradigm involves, on this account, an understanding of the audience, the historical context, *and* the logic of the adopted model.

As paradigms become elaborated and extended, an explanation becomes more fully articulated. The processes go in tandem, I suggest; as goes a paradigm, so goes our understanding of explanation. In this respect, there is no separating the analysis of explanation from attention to examples, to cases which are taken to be exemplary instances of problem solving. Narratives explain, on this account, by providing stories as solutions to problems.

I take as an instance of a paradigm of narrative explanation Clifford Geertz's "Deep Play: Notes on the Balinese Cockfight." The selection of Geertz's essay should, for those familiar with his work, come as no surprise. Few writers in any discipline possess as much stylistic grace and methodological self-awareness. Indeed, the "Cockfight" essay is, as Geertz makes explicit in his methodological *coda,* intended as a showcase piece. Geertz proposes viewing "society as a text"; the essay itself is an exercise in the art of "reading" a culturally significant event. Geertz contends, in defense of his interpre-

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tation, that the Balinese cockfight is a self-conscious work of social art, simultaneously an exemplification of and a commentary on the society of which it is a part. "Its function, if you want to call it that, is interpretative: it is a Balinese reading of Balinese experience, a story they tell themselves about themselves."[41] Yet, for all his eloquence as an advocate of his interpretive position, Geertz's essay embodies, rather than states, a method by which interpretation might proceed. It is how Geertz manifests the strategy he never otherwise manages to make explicit that I examine.[42]

The core of the essay, for our purposes, is in how Geertz stages his problem and, so, makes compelling his solution. Early on in his discussion, he indicates to his readers what his interpretive conclusion will be: "For it is only apparently cocks that are fighting there. Actually, it is men."[43] Much of Geertz's preliminary discussion is for purposes of convincing the reader of the cultural importance of the cockfight to the Balinese. This is done in a variety of ways—by providing evidence that the Balinese currently consider it important, by showing how cockfight stories and images figure in both high art and popular culture, by the extent of participation in cockfights, etc. The cockfight is presented as a socially significant artifact, a happening whose importance in its context is not to be doubted.[44]

[41] Geertz, "Deep Play," p. 218.

[42] In her article "Theory in Anthropology Since the Sixties," *Comparative Studies in Society and History* 26 (1984), Sherry Ortner provides a helpful analysis locating Geertz's work in historical context and suggesting reasons why it had the impact it did. See esp. pp. 128–130.

[43] Geertz, "Deep Play," p. 186.

[44] Geertz's method for problemizing the cockfight is more subtle and sophisticated than I can fully credit in the analysis which follows. Part of what Geertz does is, quite deliberately I believe, indicate important aspects of the cockfight which his own explanation does *not* address. His explanation, though paradigmatic, points as well to its own anomalies. Specifically, Geertz notes, but does not attempt to explain, the facts that, in an otherwise sexually nondiscriminatory culture, the cockfight is notable by virtue of its exclusion of women and that the cockfight is sexually charged insofar as the same tired double entendres connoted by "cock" in English are present in Balinese as well. Geertz's explanation points beyond and away from itself, then, toward, e.g.,

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But what *problem* does the cockfight present? Identifying it as an important activity yields, as yet, no clue with regard to how to study it. What is needed is not only evidence of widespread social participation but some feature of that activity which is puzzling. The structurally important feature, then, is not the identification of the cockfight as socially important but how Geertz manages to find something about it which is particularly problematic. For, I claim, it is in identifying a puzzle to be solved, and in the solution offered, that the essay offers itself up as a paradigm case of how narratives explain.

In this instance Geertz identifies the requisite puzzle in the betting structure of the cockfight. The puzzle, however, is complex. One level of complexity, a complexity for which Geertz has a straightforward solution, identifies disparities in betting practices. Specifically, Geertz notes, all bets between the owners of the cocks which are fighting—"center bets," in Geertz's terminology—are even-money bets. However, all other betting activity, which is extensive, is never even money, but always at fixed odds. "And most curiously, and as we shall see, most revealingly, *where the first* [i.e., the center bet] *is always, without exception, even money, the second, equally without exception, is never such*. What is fair coin in the center is biased one on the side."[45] For *this* puzzle about the betting, however, Geertz discovers a direct answer.

He argues that although the center bet is always even money, this fact does not reflect what those making the center bet believe the odds to be. What is critical is not that the bet is even, but the size of the bet. The greater the bet, the more evenly matched, as a matter of fact, the fighting cocks happen to be. Geertz compiles statistics to show that, although one cock is always rated the favorite, nonetheless, in high-bet matches, the ratio of times the favorites won to the times they lost is 1:1;

Freudian or some other very different account of the event. This, I would claim, is all to Geertz's credit. I defend this sort of explanatory pluralism in *Meaning and Method in the Social Sciences* (Ithaca: Cornell University Press, 1987).

[45] Geertz, "Deep Play," p. 195.

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for the smaller matches, favorites won at a ratio of almost 2:1. The size of the center bet determines how those on the side come to set their odds.[46]

What makes the betting of interest is not, then, the *apparent* oddity of the betting pattern. Rather, it is the size of the bets for the important matches, the matches where designation as a favorite is statistically meaningless. For, in an area where a day's pay is about three ringgats, the center bets may range beyond 500 ringgats a match. Yet, Geertz notes, actual changes of social status based on the outcome of these fights is extremely rare.[47] The size of the bet is not attributable, finally, to compulsive gambling. While such behavior exists, it is not the norm; indeed, compulsive gamblers are despised within the society. However, addiction to cock fights is a norm, on the order of football playing in Texas. You are culpable if you bet beyond your means, but betting on or sponsoring a cock is socially sanctioned and, in fact, expected.

It is the function of the size of the center bets which Geertz makes into a puzzle for us. Because the outcome for the larger fights is a coin toss, the size of the bets represents an irrational economic risk. Economic logic is strongly against the very existence of what is, in fact, a socially pervasive custom. It is in this feature of the betting scheme—its failure to hold any apparent advantage to the Balinese—that Geertz finds his interpretive entry. "The questions of why such matches are interesting—indeed, for the Balinese, exquisitely absorbing—takes us out of the realm of formal concerns into more broadly sociological and social-psychological ones, and to less purely economic ideas of what 'depth' in gaming amounts to."[48]

"Deep play," as the term originates, is deep for economic reasons. Bentham coined the term, and by it "he means play in which the stakes are so high that it is, from his utilitarian

[46] *Ibid.*, pp. 194–201.
[47] *Ibid.*, p. 212.
[48] *Ibid.*, p. 201.

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standpoint, irrational for men to engage in it at all."[49] In Geertz's analysis, of course, the depth is revealed by opposing the play to economic reason. Geertz's solution of the prior puzzle regarding the betting scheme sets the stage for the solution he develops here.

> It is, in any case, this formal asymmetry between balanced center bets and unbalanced side ones that poses the critical analytical problem for a theory which sees cockfight wagering as the link connecting the fight to the wider world of Balinese culture. It also suggests the way to go about solving it and demonstrating that link.[50]

A link between betting scheme and wider culture was suggested by noting how the *size* of the center bet accurately reflected the odds the owners attached to it, despite the fact that the center bet is always even money.

Geertz's narrative of the cockfight sets, by narrational design, first, the importance of the event; second, what is problematic about this event; third, why other rational explanations do not wash (economic, status-change, compulsive behavior); and, finally, how Geertz's own story solves the problem he has set. "It is in large part *because* the marginal disutility of loss is so great at the higher levels of betting that to engage in such betting is to lay one's public self, allusively and metaphorically, through the medium of one's cock, on the line."[51] Geertz, having set not only the problem but also, recall, the conditions for answering it—by finding a "link connecting the fight to the wider world of Balinese culture"—finds in the betting structure of the cockfight the social structure of Bali writ small. *This* is the narrative to which I alluded at the outset. Geertz's reading of the cockfight makes it into a story of Balinese society, a story which, but for Geertz's essay, would not be understood as a story about Bali at all. The

[49] *Ibid.*, p. 202.
[50] *Ibid.*, p. 198.
[51] *Ibid.*, p. 209.

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essay is, without a doubt, an interpretative tour de force. What may easily be missed, however, is how Geertz motivates his interpretation, and how it is that he has explained.

The undoubted elegance of Geertz's writing, and Geertz's self-conscious attention to it, may suggest that it is only the rhetorical flourishes which serve to lure readers to this essay. Vincent Crapanzano, for one, advances this thesis.[52] With a view to Geertz's paper, as one of three cases he considers, Crapanzano asserts that "the very figures the authors use to convince their reader—and themselves—of their descriptions in fact render them suspect, and in all three cases this failure to convince is covered by an institutionally legitimated concern for 'meaning.'"[53] The burden of explanation, in other words, is borne by stylistic conventions—and none too successfully, Crapanzano believes.

Where does Geertz go wrong? The answer is that he fails to distinguish between his perspective and that of the natives: "at a descriptive level, he [Geertz] blurs his own subjectivity—his experience of himself in those early Balinese days—with the subjectivity and the intentionality of the villagers."[54] A remarkable mistake to be sure, especially since Geertz is at pains to state in his essay just how difficult it is to understand the Balinese.[55] How does this confusion manifest itself? The damning evidence is Geertz's use of pronouns.

> Thus, in Geertz's essay, and in most ethnography, the "I/you" of the ethnographer's interlocutors in the field are converted asymmetrically into an anaphorically free "I" and an anaphoric—a cumulative "they." Indeed, in most ethnographic texts, including Geertz's, the "I" itself disappears . . . and becomes simply a stylistically borne "invisible voice."[56]

[52] Vincent Crapanzano, "Hermes' Dilemma: The Masking of Subversion in Ethnographic Description," in J. Clifford and G. E. Marcus, ed., *Writing Culture* (Berkeley: University of California Press, 1986).

[53] *Ibid.*, p. 53.

[54] *Ibid.*, p. 70.

[55] *Ibid.*, pp. 185–186.

[56] *Ibid.*, p. 71.

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What this means is that Geertz's voice slyly goes proxy for the natives'; he speaks for them, they do not speak for themselves. "*They* remain cardboard figures."[57]

But Geertz, as we saw, carefully describes and makes problematic the cockfight. This is how he motivates his solution. What, in other words, of the problems Geertz so deliberately poses? Crapanzano pays all that no heed; these factors receive no mention in his essay. What he does claim is that Geertz "reads" the cockfight without regard for what a Balinese perspective might be.

> Toward the end of his essay, as though pulling a rabbit out of a hat, Geertz suddenly declares the cockfight to be an art form, which he understands in a very Western way. . . . We must ask: for whom does the cockfight articulate everyday experience—the experience of status hierarchy—and render it more perceptible?[58]

Geertz, the charge goes, substitutes his perspective for that of the natives, and then plunges into a reading of the cockfight as if he (Geertz) confuses the perspective of the Balinese participants with that of a Harvard Ph.D. who studied with Talcott Parsons, taught at the University of Chicago, and works out of the Institute for Advanced Study. A serious confusion, indeed.

> Piling image on image . . . may assuage Geertz's own theoretical anxiety, but it hardly gets rid of the problem. . . . Cockfights are surely cockfights for the Balinese—and not images, fictions, models, and metaphors. They are not marked as such, though they may be read as such by a foreigner for whom "image, fictions, models, and metaphors" have interpretive value.[59]

Geertz's authority rests on his style, but his style, properly appreciated, subverts this authority.

[57] *Ibid.*
[58] *Ibid.*, p. 72.
[59] *Ibid.*, p. 73.

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> Geertz's colorless, abstract metaphors subvert both his description and his interpretation. Indeed, they subvert his authority. His message is simply not convincing.
>
> Despite his phenomenological-hermeneutical pretensions, there is in fact in "Deep Play" no understanding of the native from the native's point of view. There is only the constructed understanding of the constructed native's constructed point of view. Geertz offers no specifiable evidence for his attribution of intention, his assertion of subjectivity, his declarations of experience. His constructions of constructions of constructions appear to be little more than projections, or at least blurrings, of his point of view, his subjectivity, with that of the native, or, more accurately, of the constructed native.[60]
>
> For Geertz, the cockfight itself becomes a grand metaphor for Balinese social organization, and, as such, closes in on itself. Despite Geertz's ostensible concern for the understanding of the native's point of view, his essay is less a disquisition on Balinese cockfighting, subjectively or objectively understood, than on interpreting—reading—cultural data.[61]

Any reader both of Geertz's paper and of Crapanzano's description of its method might well wonder just who is guilty of constructing a cardboard figure out of whom. For the record, note that Geertz does claim that the Balinese endorse his description.[62] He might be wrong, but he is not, or not obviously, confused in some simple-minded way with regard to marking perspective.

Crapanzano could be dismissed—ought to be dismissed—were it not for what he fails altogether to mention. (Disturbing also is the implicit endorsement, by people who really ought to know better, of Crapanzano's criticism.[63] Especially surprising is John Van Maanen's recent assessment of Crapanzano's critique, for he repeats without contradicting Crapanzano's

[60] *Ibid.*, p. 74.

[61] *Ibid.*, p. 75.

[62] Geertz, "Deep Play," p. 210.

[63] See, in this regard, Paul Rabinow, "Representations Are Social Facts: Modernity and Post-Modernity in Anthropology," in Clifford and Marcus, *Writing Culture*, p. 244. Instructive misunderstandings are also in John Van Maanen, *Tales of the Field: On Writing Ethnography* (Chicago: University of Chicago Press, 1988), pp. 107, 122 n. 5.

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assessment of Geertz's analysis.[64]) No wrong done, of course, in challenging Geertz on the question of whether his interpretation of the cockfight is plausible; however, the issue of whether Geertz has it wrong or right is an *empirical* one. That is, to complain, as Crapanzano does, that Geertz pulls his interpretation "out of a hat" is not to dispute the method of the paper; rather, the remark betrays the fact that the salient feature of the explanation is just missed.

Apparently, the studied style of the "Cockfight" essay overshadows, at least for some readers, its equally deliberate efforts to define and solve a problem. The solution does what one wants of an explanation insofar as it relates the phenomenon to be explained to prior knowledge—in this case, of the larger Balinese culture. In the detail of its structure it points to a strategy of narrative explanation; one must make problematic an activity before interpreting it. The structure is not a semantics of narrative in the sense of providing conditions for judging how the account is truth-functionally related to its parts. But such semantics, I have argued above, are not to be expected from narrative structures. Geertz's paper, then, constitutes a paradigm of interpretation in the Kuhnian sense. This is not to claim, moreover, that there are not other paradigms within which Geertz's account is embedded. The popular success of Geertz's paper is doubtlessly a function of Geertz's style. My primary concern is to argue that attention to style in Crapanzano's fashion misses how, in fact, Geertz's narrative explains.

An inescapable feature of the material I consider is that it self-consciously involves writers who are trained in the precepts of one or another discipline, that is, taught to see and read in certain ways. Moreover, Geertz, while telling someone

[64] "Geertz is taken to task for ducking out of the essay soon after telling the tale and falling back too easily on realist conventions. Crapanzano's main point is that Geertz's claim to have 'cracked the code' governing the role the cockfight plays in Bali is pretentious because it assumes a final interpretation is possible": Van Maanen, *Tales of the Field,* p. 122 n. 5.

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else's story, is presenting it as that person's story. Indeed, a characteristic of what I am calling narrative explanations is that one person's voice goes proxy for all others. There are at least two very different and fundamentally misguided reactions to concern with narrative voice that one regularly encounters. On the one hand, some writers simply ignore, by virtue of ignorance or indifference, the political implications of authorial position. In this case, the sort of worry expressed by Crapanzano does have a point. On the other hand, one might imagine that an alternative is possible, that, somehow, history, anthropology, and psychoanalysis might throw off the yoke of theory and simply let facts wander freely forth. This presupposes, I maintain, a romantic belief in authenticity and essences. This view is also deeply mistaken, carrying with it its own metaphysical and political freight. My claim, which I have defended at length elsewhere, is that there is no principled resolution, no alternative, to the problem of speaking for others. There is no getting it right about who or what another is; there is no essence defining what "right" is. In this regard, narrative explanations are a doing, not a passive recording. Explanations, in this sense, are loci of moral responsibility. Explaining is a doing, not just a saying. As Clifford Geertz has put it, we must sign our interpretations.

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