

Validation in Inquiry-Guided Research: The Role of Exemplars in Narrative Studies

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In this article Elliot Mishler reformulates validation as a process through which a community of researchers evaluates the "trustworthiness" of a particular study as the basis for their own work. Rather than relying for their assessments on an investigator's adherence to formal rules or standardized procedures, skilled researchers, Mishler argues, depend on their tacit understanding of actual, situated practices in a field of inquiry. Validity claims are tested through the ongoing discourse among researchers and, in this sense, scientific knowledge is socially constructed. Within this perspective, Mishler proposes an approach to the problem of validation in inquiry-guided studies that relies on Kuhn's concept of exemplars — concrete models of research practice. He then examines three studies of narrative, suggesting them as candidate exemplars for this area of research since they provide reasonable grounds for evaluating their trustworthiness.

The reason why only the right predicates happen so luckily to have become well entrenched is just that the well entrenched predicates thereby become the right ones. (p. 98) . . . The line between valid and invalid predictions (or inductions or projections) is drawn upon how the world is and has been described and anticipated in words. (Goodman, 1979/1983, p. 121)

. . . rules are only rules by virtue of social conventions: they are social conventions. . . . That is the sociological resolution of the problem of inductive inference. . . . It is not the regularity of the world that imposes itself on our senses but the regularity of our institutionalized beliefs that imposes itself on the world. (Collins, 1985, pp. 145-148)

Acceptance or rejection of a practice or theory comes about because a community is persuaded. Even research specialists do not judge a conclusion as it stands alone; they judge its compatibility with a network of prevailing beliefs. (Cronbach, 1988, p. 6)

The individual scientist tends to assume that data replicated by certain of his colleagues are more likely to prove reliable and representative than those of other col-

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leagues. Although there is no logical basis for such decisions, they represent accumulated, practical scientific experience. (p. 108) . . . The fact is that there are no rules of experimental design. (Sidman, 1960, p. 214)

When I speak of knowledge embedded in shared exemplars, I am not referring to a mode of knowing that is less systematic or less analyzable than knowledge embedded in rules, laws, or criteria of identification. Instead I have in mind a manner of knowing which is misconstrued if reconstructed in rules that are first abstracted from exemplars and thereafter function in their stead. (Kuhn, 1962/1970, p. 192)

Validation: A Reformulation

Those of us in the social sciences who do one or another type of inquiry-guided research have long been aware that the standard approach to validity assessment is largely irrelevant to our concerns and problems.¹ This is not surprising, since the prevailing conception of and procedures for validation are based on an experimental model whereas our studies are designed explicitly as an alternative to that model, with features that differ markedly and in detail from those characteristic of experiments.

These differences in the design of experimental and inquiry-guided studies have not prevented the mis-application of experiment-based criteria and methods of validation to other types of studies, resulting in their being evaluated as lacking scientific rigor. With failure built in from the start, they are systematically denied legitimacy, and the dominance of the experimental model is assured. A new approach to validation is required that takes into account the distinctive features and problems of inquiry-guided studies and, at the same time, provides alternative, applicable methods for researchers. This article is directed to that task.²

Like the fabled Gordian Knot, validation is a mess of entangled concepts and methods with an abundance of loose threads. Sophisticated, technical procedures pulling out and straightening each thread, one at a time, seem to leave the knot very much as it was. The apparent increase in rigor and precision of successive advances in methods have brought us no closer to resolving the special problems

¹ I use the term "inquiry-guided" research for a family of approaches that explicitly acknowledge and rely on the dialectic interplay of theory, methods, and findings over the course of a study. This includes many variants of "qualitative" and interpretive research—ethnographies, case studies, ethno-methodological and grounded-theory inquiries, and analyses of texts and discourses—that share an emphasis on the continuous process through which observations and interpretations shape and re-shape each other. This feature marks their departure from the dominant model of hypothesis-testing experimentation.

² Frustrated by the misunderstanding and devaluation of their work associated with the standard approach, many nonexperimental researchers either dismiss or ignore issues of validation. Kvale (1989b), for example, notes that discussion of the validity of results in qualitative research is "an exception rather than the rule" (p. 73). Nonetheless, my attempt to deal with the special features of such studies is only one of a number of such efforts, which include Cherryholmes, 1988; Katz, 1983; Kvale, 1989a; Lather, 1986; Lincoln & Guba, 1985; Reason & Rowan, 1981. There are parallels among our approaches, particularly in our respective critiques of the experiment-based model of validation, as well as differences in our proposals. Detailed comparisons of the epistemological and ontological assumptions of the positivist tradition underlying experimental models and alternative "post-positivist" perspectives are provided by several of these authors (see also Carini, 1975; Mishler, 1979; Polkinghorne, 1988) and will not be repeated here.

faced by inquiry-guided researchers. Alexander the Great's decisive cut through the intractable Knot—a move that dissolved the problem by doing away with it—suggests that we might do better to begin at the beginning with a radical, conceptual recasting of the problem.

In sketching out a new perspective, I will begin by reformulating validation as the social construction of knowledge. With this reformulation, the key issue becomes whether the relevant community of scientists evaluates reported findings as sufficiently trustworthy to rely on them for their own work. I ground this perspective in recent historical and sociological studies of scientific practice. Further, I suggest that this reformulation is compatible with a growing recognition among mainstream validity theorists of the centrality of interpretation in validation, which poses intractable problems for the standard model. Using Kuhn's analysis of the role of exemplars in science, I then examine several instances of how validity claims are made and may be assessed in inquiry-guided, interpretive studies.

Recent studies in the history, philosophy, and sociology of science have seriously damaged the "storybook image of science" (Mitroff, 1974)—an image that has served to legitimate the dominant conception of validation. These new studies, which focus on actual practices of scientists rather than on textbook idealizations, reveal science as a human endeavor marked by uncertainty, controversy, and ad hoc pragmatic procedures—a far cry from an abstract and severe "logic" of scientific discovery. Validation has come to be recognized as problematic in a deep theoretical sense, rather than as a technical problem to be solved by more rigorous rules and procedures. An extended review of these developments is beyond the limits of this paper, but the quotations with which I began may evoke the tenor and thrust of the argument.³

Further encouragement for an alternative approach may be found in recent views of some of the principal architects of our current governing conception. A new understanding of validity has evolved gradually over the last 35–40 years, from the first codification of standards by the American Psychological Association (APA, 1954) and the influential paper by Cronbach and Meehl (1955). One of the central features of both statements was the partitioning of validity into four types: content, predictive, concurrent, and construct.⁴ This was followed by successive efforts to revise the model, without altering the assumption of different specifiable types, by proposing other typologies.⁵

³ Among the instructive studies and analyses of scientific practice that bear on issues of validation are: Collins, 1985; Gilbert & Mulkay, 1981, 1984; Goodman, 1978, 1979/1983, Kuhn, 1962/1970, 1970/1974a,b; Latour, 1988, 1990, Latour & Woolgar, 1979, Lynch, 1985, Mitroff, 1974a,b; Ravetz, 1971. Useful collections of sociological studies of science are: Barnes & Edge, 1982; Barnes & Shapin, 1979; Knorr, Krohn, & Whitley, 1981; and Simons, 1989.

⁴ Both "predictive" and "concurrent" validities are "criterion-oriented": the first refers to the relation between a test score and a criterion measure obtained "some time after the test is given," the second to a criterion measure "determined at essentially the same time" (Cronbach & Meehl, 1955, pp. 281–282). Content validity, "ordinarily to be established deductively," involves a systematic sampling of test items from a universal of interest (p. 282). "Construct validation is involved whenever a test is to be interpreted as a measure of some attribute or quality which is not operationally defined." . . . Construct validity is not to be identified solely by particular investigative procedures, but by the orientation of the investigator" (p. 282).

⁵ Among these revisions are: Campbell and Stanley's (1963) external-internal contrast pair, updated by Cook and Campbell (1979) to statistical conclusion, internal, construct, and external; Katz's (1983) reliability, representativeness, reactivity, and replicability; Lather's (1986) triangulation, face,

Each new proposal underscored the fundamentally flawed nature of this model. It became clear that validation, the touchstone of scientific inquiry, could not be achieved by applying a formal algorithm to assess each type of validity. Campbell and Stanley's (1963; see also Cook & Campbell, 1979) elegant and influential analysis of different quasi-experimental designs and their respective threats to one or another validity has turned out, in retrospect, to be a death-blow to the typology approach. There are two reasons for this unanticipated consequence, both reflecting Campbell and Stanley's clear understanding that validity assessments are not assured by following procedures but depend on investigators' judgments of the relative importance of different "threats." First, no general, abstract rules can be provided for assessing overall levels of validity in particular studies or domains of inquiry. Second, no formal or standard procedure can be determined either for assigning weights to different threats to any one type of validity, or for comparing different types of validity. These assessments are matters of judgment and interpretation. And these evaluations depend, irremediably, on the whole range of linguistic practices, social norms and contexts, assumptions and traditions that the rules had been designed to eliminate. To Sidman's (1960) statement that there are "no rules of experimental design," we may now add that there are "no rules" for assessing validity. Investigators, of course, follow accepted procedures in their domains of inquiry. However, as will become clear, these "rules" for proper research are not universally applicable, are modified by pragmatic considerations, and do not bypass or substitute for their nonrule-governed interpretation of their data.

Recognition of these unresolvable problems has led to a new perspective in which validity is viewed as a unitary concept with construct validation as the fundamental problem.⁶ This, of course, makes issues of meaning and interpretation central. Thus, Cronbach (1984) states that the "end goal of validation is explanation and understanding. Therefore, the profession is coming around to the view that all validation is construct validation" (p. 126). Messick (1989), reviewing the history of changing conceptions, argues that validation is essentially a type of "scientific inquiry," and that a validity judgment is an "inductive summary" of all available information, with issues of meaning and interpretation central to the

construct, and catalytic; Levy's (1981) communicability, plausibility, generalizability, and interpretability, Lincoln and Guba's (1985) credibility, transferability, dependability, and confirmability. Rather than partitioning validity, some investigators parse the research process into different steps, each requiring its own validity assessment; for example, Brinberg and McGrath's (1982, 1985) "network of validity concepts," Huberman and Miles's (1983) rules for data display and reduction, Lincoln and Guba's (1985) "audit," and Tagg's (1985) "facets" analysis.

⁶ This view had early proponents. For example, Cronbach and Meehl (1955) viewed construct validity as the fundamental issue, and Loewinger (1957) asserted that "since predictive, concurrent, and content validities are all ad hoc, construct validity is the whole of validity from a scientific point of view" (p. 636). However, as Angoff (1988) points out, this view did not become generally accepted until the late 1970s. Consensus on this position is, nonetheless, hardly universal. For example, Messick's (1989) proposal of construct validation as a "unifying theme" is harshly criticized by another prominent methodologist who finds his approach "questionable" and his solution unsuccessful since "there is no agreed upon method for determining construct validity" (Green, 1990, p. 850).

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process. He also expands the validation framework to include social values and social consequences of findings as contexts for validity assessments.⁷

This emergent consensus is good news. It acknowledges, albeit implicitly, that the traditional approach has failed and offers an opportunity for exploring alternatives. The new emphasis on interpretation, and on social contexts and values, resonates closely with the detailed findings of historians and sociologists of science. Both developments encourage us to view all types of research as "forms of life" (Wittgenstein, 1953; see also Brenner, 1981) rather than technical exercises governed by an abstract logic of methodological rules. With this understanding, we may be able to move towards a conception of validation that is more relevant not only to inquiry-guided studies but to experimental modes of research as well.

Trustworthiness: Grounds for Belief and Action

As a first step, I propose to redefine validation as the process(es) through which we make claims for and evaluate the "trustworthiness" of reported observations, interpretations, and generalizations.⁸ The essential criterion for such judgments is the degree to which we can rely on the concepts, methods, and inferences of a study, or tradition of inquiry, as the basis for our own theorizing and empirical research. If our overall assessment of a study's trustworthiness is high enough for us to act on it, we are granting the findings a sufficient degree of validity to invest our own time and energy, and to put at risk our reputations as competent investigators. As more and more investigators act on this assumption and find that it "works," the findings take on the aura of objective fact; they become "well-entrenched" (Goodman, 1983).

This definition and criterion depart in critical ways from standard doctrine. First, by making validation rather than validity the key term (see Messick, 1989), they focus on the range of ongoing activities through which claims are made and appraised rather than on the static properties of instruments and scores. Second, by adopting a functional criterion—whether findings are relied upon for further work—rather than abstract rules, validation is understood as embedded within the general flow of scientific research rather than being treated as a separate and different type of assessment.⁹ In this way, this definition and criterion emphasizes the role played in validation by scientists' working knowledge and experience, aligning the process more closely with what scientists actually do (Collins, 1985;

⁷ Cherryholmes (1988), in a parallel expansion, locates validation within larger systems of socio-political discourse: "Construct validation is a pragmatic and socially critical activity because clear-cut distinctions among social research, social theory, and social practice cannot be sustained" (p. 421); "Decisions about construct validity cannot be disentangled from ethico-political decisions" (p. 440).

⁸ Trustworthiness is the key term in Lincoln and Guba's (1985) analysis of validation in "naturalistic inquires." They pose the basic issue as: "How can an inquirer persuade his or her audiences (including self) that the findings of an inquiry are worth paying attention to, worth taking account of?" (p. 290). We share that view of the researcher's task, but I place more emphasis on other researchers' willingness to act on the basis of, as well as pay attention to, a study, and on the continuing social process through which claims are contested, assessed, and warranted.

⁹ This gives primacy to the "pragmatic" conception of truth in contrast to "correspondence" or "coherence" conceptions, although the latter also enter into our assessments (Ernstvedt, 1989).

Latour, 1990; Latour & Woolgar, 1979; Lynch, 1985; Ravetz, 1971; Sidman, 1960) than with what they are assumed to be and supposed to do.

Further, focusing on trustworthiness rather than truth displaces validation from its traditional location in a presumably objective, nonreactive, and neutral reality, and moves it to the social world—a world constructed in and through our discourse and actions, through praxis. Since social worlds are endlessly being remade as norms and practices change, it is clear that judgments of trustworthiness may change with time, even when addressed to the “same” findings. Finally, truth claims and their warrants are not assessed in isolation, but enter a more general discourse of validation that includes not only other scientists but many parties in the larger community with different and often conflicting views. (See Latour’s 1988 account of shifting conflicts and alliances in the “validation” of Pasteur’s microbial theory of infection; also Richards, 1979, on the reception of non-Euclidean geometry in nineteenth-century England.)

Reformulating validation as the social discourse through which trustworthiness is established elides such familiar shibboleths as reliability, falsifiability, and objectivity. These criteria are neither trivial nor irrelevant, but they must be understood as particular ways of warranting validity claims rather than as universal, abstract guarantors of truth. They are rhetorical strategies (Simons, 1989) that fit only one model of science—experimental, hypothesis-testing, and so forth. Used as proof criteria, they serve a deviance-sanctioning function, marking off “good” from “bad” scientific practice. (See Gieryn, 1983, and Prelli, 1989, for case studies of the rhetoric of exclusion.)

Bazerman (1989), reviewing Collins’s (1985) studies of replication and induction in science, observes that: “Experimentation is so embedded in forms of life that compelling experimental results are compelling only to those who have already entered in the form of life which generates the result” (p. 115). These warrants have less “rightness of fit” (Goodman, 1978) for interpretive and inquiry-guided forms of research which, in turn, may only be compelling to those who have entered that form of life.¹⁰

Conflict and controversy are as much a part of “normal science” (Kuhn, 1970) as the shared concepts, procedures, and findings dutifully inscribed in textbooks. All scientific reports—from spare accounts of methods and findings to philosophical analyses—are partisan forays into contested terrain. Nonetheless, the “truths” of normal science are embedded in complex networks of concepts, linguistic and technical practices, and an established framework of norms and values (Collins, 1985; see also Campbell, 1979, on the “tribal model” of scientific knowledge), and it is not surprising that they are markedly resistant to change. New approaches or new discoveries cannot easily be absorbed, nor can their potential threat to the whole system be defused by tinkering with minor details.

¹⁰ Only a strong faith in experiments could account for their compellingness, since they are so difficult and time-consuming, and so often fail. Collins (1985) points out that, “Experiments hardly ever work the first time; indeed, they hardly ever work at all” (p. 41). Even the apparently rapid spread of a new experimental procedure or piece of equipment requires trial-and-error and modification to meet local conditions and problems. For example, examination of widespread “replications” of studies of vacuums after Boyle’s invention of the air pump shows “that no two pumps are the same and that each transportation through Europe means a transformation of the pump” (Latour, 1990, p. 154; see Shapin & Schaffer, 1985). See also Ravetz (1971) on the many “pitfalls” involved in any experiment.

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For these reasons, I would not expect easy assent to this new formulation of validation. However, by showing that experimentalists are in the same boat as inquiry-guided researchers in that we all rely for the validation of our work on contextually grounded linguistic and interpretive practices, I hope to gain a hearing and perhaps enlist "allies" (Latour, 1988). As Collins (1985) points out, the possibility of changing current practices depends on putting forward "an interpretation of data which has the potential to create some contradictions and reverberate through the social and conceptual web . . . [but] must not appear to be completely unreasonable" (p. 151).

Exemplars: Resources for Inquiry

If validity claims cannot be settled by appeal to abstract, standard rules or algorithms, what would be a useful alternative approach? The indeterminateness of such claims is not a matter of the imprecision of technical methods. Rather, definitions of evidence and rules and criteria for their assessment are embedded in networks of assumptions and accepted practices that constitute a tradition. Recommending new rules for inquiry-guided studies would confront us with the same uncertainties that, as we have seen, undermine the canonical approach. The utility of alternative rules would be limited—as are the standard ones—to their pragmatic function as accounting practices that help researchers monitor, arrange, and order their data in some methodic way.¹¹ Rather than proposing yet another list of rules and criteria, I will rely on Kuhn's (1970) analysis of "exemplars" to suggest an approach to the problem of how claims for trustworthiness may be made and evaluated.¹²

Kuhn's (1962/1970) concept of paradigms and the role they play in "normal science" has had considerable influence in studies of the history and sociology of science. Responding to criticism about ambiguities in the referents of this term, he replaced it with "disciplinary matrix" for the full set of assumptions, theories, and practices shared within a community of specialists. A critical element of this matrix is the "exemplar":

By it I mean, initially, the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts. . . . [and] at least some of the technical problem-solutions found in the periodical literature that scientists encounter during their post-educational research careers and that also show them by example how their job is to be done. More than other sorts of components of the disciplinary matrix, differences between sets of exemplars provide the community fine-structure of science. (Kuhn, 1970, p. 187)

¹¹ Other critics of the standard model are more sanguine about the value of substitute rules tailored to the specific features of inquiry-guided research. For example, Huberman and Miles (1983) provide detailed procedures for data reduction and display, and Lincoln and Guba (1985) offer an elaborate set of axioms, characteristics, and guidelines for "naturalistic inquiries," parallel to those used in experimental studies. Salner (1989) avoids rules but lists nine "qualities and abilities [that] the human researcher needs" (pp. 65–68).

¹² The value of exemplars for clarifying and comparing alternative research models has been recognized by, among others, Bredo and Feinberg (1982) for educational research; Dervin, Grossberg, O'Keefe, and Wartella (1989) for communication studies; and Morgan (1983) for organizational research.

Kuhn views "knowledge embedded in shared exemplars" as a "mode of knowing" no less systematic or susceptible to analysis than that of "rules, laws, or criteria" (p. 192), and also recognizes that these "modes" of doing and acting are not acquired simply by "encounters" with textual descriptions. Skilled research is a craft (Ravetz, 1971; see also Polanyi, 1966, on "tacit knowing"), and, like any craft, it is learned by apprenticeship to competent researchers, by hands-on experience, and by continual practice. It seems remarkable, if we stop to think about it, that research competence is assumed to be gained by learning abstract rules of scientific procedure. Why should such "working knowledge" (Harper, 1987; Mishler, 1989) be learned any more easily, or through other ways, than the competence required for playing the violin or blowing glass or throwing pots?

Technical descriptions of methods in themselves, however detailed and precise, are insufficient for replication, the prescribed route to validation. Sidman (1960) observes that it is "common practice in biological science" for researchers to make personal visits to the laboratories of competent users of an experimental procedure to "learn the required skills firsthand" (p. 109). Replication is a routinely uncertain endeavor and, as Collins (1985, pp. 29-78) argues, the usual notion is misleading and does not correspond to how scientists use other studies as springboards for their own work rather than "replicating" them.

Collins documents the "capricious nature" of the transfer of knowledge and concludes that such knowledge "travels best (or only) through accomplished practitioners," that "experimental ability is invisible in its passage," and that the only evidence of the "proper" conduct of an experiment is the "proper" experimental outcome—not the precision with which the work was done. Finally, he observes that although successive failures to replicate might lead scientists to temporarily suspend their belief that following "algorithm-like instructions" make carrying out an experiment a "formality," this belief "re-crystallizes catastrophically upon the successful completion of an experiment" (p. 76). Thus, by concealing their skills and artfulness from themselves—their own craft and tacit knowledge—scientists reaffirm the "objectivity" of their findings and reproduce the assumptive framework of "normal science."

In sum, knowledge is validated within a community of scientists as they come to share nonproblematic and useful ways of thinking about and solving problems. Representing the "community-fine structure of science" (Kuhn, 1970, p. 187), exemplars contain within themselves the criteria and procedures for evaluating the "trustworthiness" of studies and serve as testaments to the internal history of validation within particular domains of inquiry.¹³ Developing new exemplars is a

¹³ The social production of knowledge is more visible in the histories of initially marginal lines of inquiry that managed, though their methods deviated from established tenets and prescriptions, to carve out niches in the ecological space of science. Prime examples are psychoanalysis, cognitive stage theory, experimental behaviorism, and ethnomethodology—associated respectively with the names of their originators: Freud, Piaget, Skinner, and Garfinkel. Each made problematic a previously taken-for-granted or ignored phenomenon, respectively, dreams and slips of the tongue, the orderly development of cognitive structures, the dependence of stable behavior on the frequency and timing of contingent reinforcements, and the relationship between social norms and actions as practical accomplishments of actors' routine practices. Further each provided an alternative methodology for its study: free association, process observation and interview, schedules of reinforcement and baselines, norm-violation procedures and conversation analysis.

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complex social process, over which individual investigators have only modest control. To move towards this goal, those of us engaged in inquiry-guided and interpretive forms of research have the task of articulating and clarifying the features and methods of our studies, of showing how the work is done and what problems become accessible to study. Although they cannot serve as “standard” rules, a context-based explication is required of how observations are transformed into data and findings, and of how interpretations are grounded.

In the remainder of this paper, I will focus on studies of narrative, one branch of interpretive research, and propose three different approaches as candidate exemplars. My immediate aim is to demonstrate alternative ways to do such studies that may be useful to other investigators. My broader aim is to promote a dialogue about ways of doing inquiry-guided research so that together we can develop a community with shared exemplars through which we confirm and validate our collective work.

Candidate Exemplars for Interpretive Research

There may be several exemplars, each with its own variants, that achieve legitimacy within a community of specialists sharing a perspective and methodology—“search cells” or “language communities” in Koch’s (1976) terms. Together they constitute normal practice—the ordinary, taken-for-granted and trustworthy concepts and methods for solving puzzles and problems within a particular area of work. Legitimacy cannot be legislated in advance. Neither abstract rules nor appeal to an idealized version of *the* scientific method will suffice. Rather, the defining features of exemplars are inferred from the actual practices of working scientists. Like the inductive categories of “natural” objects studied by cognitive psychologists, experiments and types of inquiry-guided studies are both “fuzzy categories” (Mervis & Rosch, 1981; Rosch, 1973, 1978; Rosch & Mervis, 1975). Each includes prototypes—for example, the model experiment, and a range of variants, such as “quasi-experiments.”

As a context for discussing the approaches that I am nominating, tentatively, as candidate exemplars, I will first briefly outline some features of the dominant research exemplar as it has been applied to the study of narratives. All of the studies I will examine, though differing in content and theoretical orientation, share certain characteristics that make for useful comparison: each 1) focuses on a piece of “interpretive discourse,” 2) takes this “text” as its basic datum, 3) reconceptualizes it as an instance of a more abstract and general “type,” 4) provides a method for characterizing and “coding” textual units and 5) specifies the “structure” of rela-

Experimental designs, quantitative scales, and tests of significance are notably absent. Learning these new approaches required apprenticeship through, for example, psychoanalytic training, or at the Geneva Institute, in the Pigeon Lab, or in intensive workshops and seminars. With their paths blocked to establishment journals, proponents of these schools of thought founded their own or circulated unpublished documents through their networks, as was the case, for example, with Harvey Sacks’s lecture notes on conversation analysis, many of which were published posthumously (Jefferson, 1989). Facing resistance and rejection in their home disciplines, they found allies in others: in literature and history, among teachers and educators, and in the ranks of anthropologists and linguists.

tionships among them, and 6) interprets the "meaning" of this structure within a theoretical framework. Interpretive discourse (White, 1989) refers to researchers' understandings of the texts as representing efforts by speakers/authors themselves to describe and interpret their experiences.

As will be seen, the three proposed alternatives share features distinguishing them from the standard approach. Each "displays" the full texts to which the analytic procedures are applied, in contrast to the typical presentation of decontextualized fragments illustrating a coding manual. Further, rather than defining coding "dimensions" that are independent of and isolated from each other, these studies focus on analytic "structures" of relationships among textual features, which then become the basis for theoretical interpretation.

Normal Science and Narrative Research

Many critics of the positivist-based experimental model argue that its assumptions—about, for example, causality and objectivity—are inappropriate for the study of language and meaning (see footnote 2). Their argument would apply to research on "narrative modes of knowing" (Bruner, 1986). Investigators, however, are not governed in their practices by philosophical analyses of their epistemological and ontological assumptions. Skilled researchers working within the standard framework can find ways of adapting and applying their methods to any phenomenon that catches their interest, and narratives have not escaped their net.

Two recent studies (McAdams, 1985; Stewart, Franz, & Layton, 1988) illustrate how this is done. Both use life history narratives to examine issues of personal identity. I will focus on their research practices—on some of the ways they make the dominant exemplar "work" on apparently unsuitable material. Although they warrant their validity claims by an explicit reliance on "standard" methods, it turns out that their success in carrying out their analyses depends fundamentally on their pragmatic modifications of these methods. This is their "practical accomplishment" (Garfinkel, 1967) as researchers. Although I emphasize their research practices in this section, the inappropriateness of their conceptual models for narrative research is an equally important problem that will be addressed at various points. The aim of this brief review of their work is to set the stage for discussion of more appropriate approaches.

These investigators face a difficult task. They must convert voluminous, multi-dimensional, and variable language samples into the types of objects that allow them to apply standard procedures—sampling, measuring, counting, and hypothesis-testing through statistical analysis. To make the problem reasonably tractable, they begin deductively, relying on general theories to specify a few dimensions—power and intimacy motives for McAdams, based on McClelland's model; themes of identity, intimacy, and generativity for Stewart et al. from Erikson's (1950/1959) model of ego development. These concepts—motives and themes—are converted into coding categories that are applied to the original texts: responses to interviews from samples of respondents in McAdams' case and from letters, diaries, and autobiographical memoirs of one person in Stewart et al.'s study. The resultant "scores" are the data for successive stages of description, analysis, and interpretation.

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Their competence as researchers is displayed by their success in accomplishing this transformation—from the messy and diffuse narrative texts with which they begin to the quantitative measures that now represent and stand for those texts. The reduction and transformation of source data—that is, initial observations and descriptions—is a necessary feature of all research. However, different rules and strategies of reduction lead to different re-presentations of the phenomena. These new “objects,” constructed by researchers, include and emphasize only some features of the originals and exclude others as irrelevant to their interests. Interpretive researchers view the transformations achieved by the standard model as deeply flawed distortions in that they exclude precisely those features of the phenomena that are their essential, defining characteristics. Thus, with reference to narratives, representing them as scores for separate motives or themes, as is done in these two studies, excludes both their structural and sequential features, which are specifically what makes them “narratives” rather than some other type of text.

A principal claim of researchers who follow an experiment-based model is that their use of standard methods and procedures allows others to replicate their studies. Thus, Stewart et al. assert the generalizability of their codes: “The coding definitions were designed for use in coding any verbal text for preoccupation with self-definitional issues” (p. 49). Studies in the history and sociology of science, reviewed earlier, make it clear that “standardization” is not easily achieved and that replication is a function of local, situated practices. The problem may be seen in the ways that “standard” methods are modified in these two studies so that they can be applied to the particular and contingent features of their data.

For example, Stewart et al.'s coding units are “meaningful phrases” defined by the presence of a “codable image” (p. 57), which can include any length of text. Adequate understanding and use of this code depends on this particular study's coders' subculture (Mishler, 1984, p. 37) and, in a strong sense, the coding procedure could not be transferred directly to another research context. McAdams found it necessary to alter coding definitions of power and intimacy for individuals' accounts of their “earliest memories,” since these were “rather banal and lacking in feeling tone.” Categories were “broadened to include events and actions similar, though perhaps not identical, to the original characterizations” (pp. 173–174). Broadening or narrowing coding categories is, of course, an option open to other researchers, and the question of whether or not they had “replicated” the procedure would then be unanswerable.

Sometimes inconsistencies or contradictions, appearing at one or another stage in an analysis, require a mid-stream change in methods. Looking at summary scores for the “same” themes in different types of documents referring to the same time period, Stewart et al. found themselves “faced with the dilemma that we had not only different accounts of the period, but accounts in which the scores were in fact uncorrelated” (p. 59). Rather than taking this finding as a test of their hypotheses, they decided to “treat these media as alternative expressions” and “took the higher score for a given month, for all subcategories of that stage, regardless of which medium produced it” (p. 59). McAdams found that “the four main themes for power and intimacy did not appear relevant for the coding” of “negative nuclear episodes.” He “settled inductively on four new themes for each of the con-

tent categories of power and intimacy. In some cases, the new themes bear some resemblance, typically as an opposite, to the original themes used in the analysis of positive nuclear episodes. In other cases, any similarity is lacking" (p. 158).

Similar observations might be made about the situated practices through which any investigator assures the success of his or her work. The main point is that standard methods are poorly standardized, allowing great latitude to researchers in how they specify them, and specification is contextually grounded in the idiosyncracies and exigencies of particular studies. All investigators have to adapt, convert, and translate "standard" methods to solve their practical problems.

McAdam's and Stewart et al.'s on-line, pragmatic decisions are as much a part of normal scientific practice as their use of a coding manual and statistical tests. However, they highlight the problematic nature of their validity claims. Standard procedures—for sampling, coding, and quantifying—are weak and insufficient warrants because when they are actually applied they turn out to be context-bound, nonspecifiable in terms of "rules," and not generalizable. Close examination of the procedures used in any study would reveal a similar gap between the assumption of standardization and actual practices. Other investigators would be unable to determine whether their own versions, or adaptations, of their procedures represented a reasonable equivalent of them. Replication, rather than being assured by these procedures, would be essentially indeterminate.

*Alternative Models for Narrative Research*¹⁴

The three studies I will review below depart in significant ways from normal practice. They do not escape the thorny and unavoidable problems of validation. Nonetheless, I hope to show that they provide reasonable grounds for and ways of assessing their claims for trustworthiness, and, also, that they are more adequate and appropriate models for the study of narratives as a type of interpretive discourse.

—Life History Narratives and Identity Formation

A life history interview with one artist-furniture maker provides the narrative text that I analyze in my study of adult identity formation (Mishler, in press). Reviewing my work in the context of the preceding discussion of standard studies that also focused on issues of identity will help to clarify differences in our respective research strategies and methods.

Informing and guiding my study is the question of how craftspersons sustain their commitments to and motivations for nonalienating forms of work in an inhospitable sociocultural and economic environment. Drawing on William Morris's (1883/1966) concept of the "craftsman ideal," which assigns a high value to craft work as creative, varied, and useful, I try to understand how craftspersons balance

¹⁴ The study of narratives has emerged in recent years as a large and diverse area of inquiry. The three models suggested here as candidate exemplars do not and are not intended to represent the variety of approaches. There are many others that might serve as well and that merit attention. For an appreciation of the range of work, see Bruner, 1986, 1990; Labov, 1972; Labov & Waletzky, 1967; Langellier, 1989; McAdams & Ochberg, 1988; Mishler, 1986a,b, in press; Paget, 1983; Polanyi, 1985; Polkinghorne, 1988; Riessman, 1990; Roscnwald & Ochberg, in press; Sarbin, 1986a,b; White, 1987; Young, 1984, 1987; and two issues of *Critical Inquiry*: Vol. 7, 1980, and Vol. 7, 1981

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that "mode of being" with economic, social, and family demands. I define identity formation as the process by which these problems are resolved over the life course.

The concepts of alienated and nonalienated work are not used to derive testable hypotheses but as issues to explore with respondents to learn whether and how they might be relevant to them in their work. My inductive approach contrasts with McAdams' and Stewart's deductive one, and leads to different methods for collecting, describing, analyzing, and interpreting the interviews. For example, my research interviews are relatively unstructured, with respondents controlling the introduction, content, and flow of topics. Informing them of my interest in how craftspersons live and work, I ask them to talk about how they came to be doing the work they're doing and "what's involved in the kind of life you lead that's related to being in the crafts." Within this frame of a research interview, we have a shared task and purpose: to understand how they came to do and how they view their current work. The personal narrative that emerges is a solution to this task, representing the individual's general solution to the task of making sense of his or her life.

I take it for granted that the account produced during the interview is a reconstruction of the past, shaped by the particular context of its telling. A respondent's re-interpretation of his or her work history is the basic "text" for analysis and interpretation. The problem of "distortion" that troubles Stewart et al.—that is, whether the account corresponds to the "real" past—does not arise since I do not rely on a correspondence model of truth, where the earlier "objective" reality serves as a validity criterion for what is being told now. This is not a weakness, but rather a hallmark of interpretive research in which the key problem is understanding how individuals interpret events and experiences, rather than assessing whether or not their interpretations correspond to or mirror the researchers' interpretive construct of "objective" reality. A concern with distortion places the burden of validity claims on the wrong shoulders—it is the investigator's problem, not the respondent's. Instead of assuming a past reality as a criterion, a potential warrant for the validity of my interpretation is whether it makes sense to the respondent.

My text-sampling procedure does not follow a statistical model, but reflects successive steps of the inquiry: interviews with a small, varied group of artist-craftspersons, repeated listenings to taped interviews and readings of transcripts, discovery of parallel trajectories in their work histories, development and refinement of a model of work history narratives, selection of this respondent as a representative case, and specification of the episodes and structure of his narrative for detailed analysis and interpretation. Thus, the text samples were not drawn randomly but inductively, and chosen as representative of patterns I was finding in the full data set.

Clearly, this form of inquiry-guided or "grounded theory" research (Glaser & Strauss, 1967; Strauss, 1987) involves a continual dialectic between data, analysis, and theory. Its steps are no more mysterious or less attentive to the data than statistical procedures. The latter, as we saw in McAdams' and Stewart et al.'s studies, also require on-line adjustments. This process-dependence of research decisions, though usually viewed as a methodological weakness and a source of contamination and error, is a necessary part of any study.

I view the "personal narratives" that emerge during the interviews as retrospective accounts whose function is to provide a sense of coherence and continuity

through life transitions (Cohler, 1982), that is, as representing the formation of a craft identity. My analytic model focuses on respondents' reports of their shifts between types of work, of the reasons for these changes, and of how they achieved their current work identity. It distinguishes between and then links together the two essential dimensions of any narrative—the “non-chronological” or structural one, and the “chronological” or temporal one (Ricoeur, 1981). The structural component locates work identities within social and cultural contexts that define alternatives and limit choices among culturally available types of work for artist-craftspersons: Art, Craft, Type of Craft, and specific Mode of Craft work. Each succeeding choice is constrained by the previous ones. The second component focuses on the temporal ordering of respondents' actual choices within this structure of general categories, which serve as a “code” to classify the narrative episodes, or “units” of the interview.

The structure of hierarchically ordered categories was empirically rather than theoretically derived. Using it as a framework to locate the “identity relevance” of particular choices led to the discovery that the achievement of a current work identity was neither linear nor progressive. In shifting from one job to another, individuals sometimes made moves within the same category and sometimes moved back to a prior one before going on to succeeding ones. I refer to these shifts as “detours,” as off the straight path to their achieved identity. Further, they are recognized by the respondents themselves, from their current vantage point and achieved identity, as functioning in this way. This is one criterion for assessing the trustworthiness of the model and my interpretation of the identity relevance of job changes.

For example, my analytic distinction between Art and Craft derives from and can be tracked directly back to respondents' ways of talking about their different types of work. For example, the furniture-maker refers to the distinction as present in “this endless discussion that goes on and on and on in schools and between professionals and all that.” As to himself: “I don't consider myself just a craftsperson. I consider myself a designer committed to craftsmanship.” For him, “a craftsperson and an artist are synonymous if you're looking at those that you respect as good craftspeople. Not people who are just churning out objects, but people who are doing personal work, and doing progressive work.” And further, “It has to do probably with their input into creating the object, rather than being given a design or being given something to copy and produce and just giving with their manual skills as—as opposed to their intellect and creativity.”

Further, the episodes that are the plot of his work history narrative, which I constructed from the full interview, include all of the different jobs and transitions that he describes. Thus, he specifies a sequence of changes from entering college as a “chemical engineer,” switching to train as an “architect,” when he first “became involved in the design world” (an Art choice), and changing again to become a “landscape architect.” His first post-college job was as an architect (Art), but then he began to work with a “third-generation craftsman” and “really started to do woodworking” (a Craft choice). After two years, feeling that he was “being locked into Milltown, Indiana, for the rest of my life” and was “wasting” his training in landscape architecture, he moved and “started working as a landscape architect” (a detour back to Art).

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He stayed at this for five-and-a-half years, and then, realizing that "it just wasn't what I wanted to do for the rest of my life," he "did a search and, uh, decided to go" to graduate school for training in furniture making: "totally investing myself in—in, ah, the furniture world as a craftsman" (a switch back to Crafts). It is his own evaluation of his work as a landscape architect as off the path to his current work identity that grounds my interpretation of it as a detour. He received a degree in "crafts, treating furniture as an art form," and then began "teaching" furniture making, and setting up a "shop" and "doing some shows and commission work" (his move to his current Mode of Work and his achieved identity as an artist-furniture maker). Note that his transitions between types of work are explicitly marked by such locutions as: "I decided I wanted to do something else," "so at that time . . . I started working as," "so I did a search and, uh, decided to go," "I ended up, um . . . opting to go."

The view of validation that I have advanced suggests that the questions to be asked about my study, and of any study within any research tradition, are: What are the warrants for my claims? Could other investigators make a reasonable judgment of their adequacy? Would they be able to determine how my findings and interpretations were "produced" and, on that basis, decide whether they were trustworthy enough to be relied upon for their own work? I believe these questions have affirmative answers. The primary reason is the visibility of the work: of the data in the form of the texts used in the analysis, with full transcripts and tapes that can be made available to other researchers; of the methods that transformed the texts into findings; and of the direct linkages shown between data, findings, and interpretation.

I am not arguing that my methods and procedures "validate" my findings and interpretations. That would be counter to my basic thesis that validation is the social construction of a discourse through which the results of a study come to be viewed as sufficiently trustworthy for other investigators to rely upon in their own work. Nor does my study escape the difficult problems of "knowledge transmission," of how others might learn how to do this type of work and of what criteria they could use to determine the degree of equivalence between our respective studies. I am arguing, however, that they would be able to make a reasoned and informed assessment about whether or not my validity claims are well warranted.

I used my own study to contrast one type of narrative research with examples of standard practice. Parallels between the studies, particularly their shared focus on identity and their analysis of texts, allowed me to highlight and clarify differences between them in methods for collecting, displaying, analyzing, and interpreting data. The next two candidate exemplars differ from my own in aims, methods, texts, and models of narrative analysis.

—Narrativization in the Oral Style

A seven-year-old Black child tells a story about her puppy during "sharing time" in her second grade class (Michaels, 1981). It does not match her teacher's expectations, lacking the standard story structure of sequentially connected episodes. (Michaels refers to it as "topic-associating" rather than "topic-centered.") Finding it difficult to understand and missing the point, the teacher treats it as a sign of the child's inadequate language skills. (See Riessman, 1987, on an interviewer's

similar difficulties with a respondent's nonstandard narrative.)

Gee (1985, also 1986) reexamines the story as an instance of an "oral" rather than a "literate" style (Heath, 1982, 1983). Starting with the assumption that, "One of the primary ways—perhaps *the* primary way—human beings make sense of their experience is by casting it in a narrative form" (p. 11), Gee tries to explicate how this child does that. His stylistic analysis reveals that her narrative "shares many features with narratives found throughout the world in oral cultures" (p. 9), with its structure achieved through such "technical devices" as "repetition, parallelism, sound play, juxtaposition, foregrounding, delaying, and showing rather than telling [that] are hallmarks of spoken language in its most oral mode, reaching its peak in the poetry, narratives, and epics of oral cultures" (p. 26).

His route to a description and understanding of the "structures behind her narrative performance" begins with his observation/hearing of a "characteristic prosodic pattern." Her extended stretch of speech consists of "a series of relatively short sequences of words, each sequence having a continuous intonational contour" (p. 12). A fall in pitch does not come until after several such sequences. This contrasts with literate speech, where falling contours tend to mark ends of sentences. Gee suggests that her falling contours have discourse-level rather than syntactic-level functions, and serve to mark the ends of episodes rather than sentences.

Displaying the text in terms of the "lines" that L is aiming at, the "idea units" that she expresses as short clauses, "it becomes apparent that L groups her lines together into series of lines—often four lines long—that have parallel structure and match each other in content or topic" (p. 14). Gee calls these groups of lines "stanzas." Using the stanza as the basic structural unit in his analysis, he finds that the sequence of stanzas in her narrative, each representing an episode, are grouped together: there are three main parts to her story, each of which has two sub-parts. The following excerpts illustrate Gee's structural analysis (1985, pp. 34–35).

Part 1. INTRODUCTION

Part 1A: Setting

1. Last yesterday in the morning
2. there was a hook on the top of the stairway
3. an' my father was pickin me up
4. an I got stuck on the hook up there
5. an' I hadn't had breakfast
6. he wouldn't take me down =
7. until I finished all my breakfast =
8. cause I didn't like oatmeal either //

Part 1B

9. an' then my puppy came
10. he was asleep
11. he tried to get up
12. an he ripped my pants
13. an' he dropped the oatmeal all over

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Part 3: RESOLUTION

Part 3A: Concluding Episodes

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36. an' last yesterday, an' now they put him asleep

37. an' he's still in the hospital

38. (an' the doctor said . . .) he got a shot because

39. he was nervous about my home that I had

Part 3B: Coda

. . .

41. an' he could still stay but

42. he thought he wasn't gonna be able to let him go //

The first part of her story takes place in the child's home, the “setting” described first in two four-line stanzas followed by another two four-line stanzas that introduce her puppy and father. The second part involves going to school and being followed by her puppy, “complicating actions” consisting again of two four-line stanzas, with a brief non-narrative “evaluation” section. The last part takes place in a hospital, the “resolution” of the story in two four-line stanzas and a concluding two-line “coda.” By using terms for the story components—“setting,” “complicating actions,” and so forth—from a model for standard, temporally ordered narratives (Labov, 1972; Labov & Waletzky, 1967), Gee is arguing that this story has a structure that serves the usual functions of narratives despite its different surface appearance.

Gee's close analysis of this structure and features of the child's speech uncovers an underlying theme: her sense of being “counterpoised between the world of the puppy and the adult world,” where “she must deny her own longings and those of the puppy in turn, so he will not disrupt the discipline of that world” (p. 20). Although her story was not “well-received by her teacher” who found it “inconsistent, disconnected, and rambling,” Gee refers to it as a “tour de force” (p. 24). In a “quite sophisticated way” she makes sense of her world through narrativization that both states her problem and its resolution: “why she doesn't have her puppy, why he didn't work out, and ultimately why she must belong to the world of home and school” (p. 24).

Gee's elegant analysis is an important contribution to narrative studies. Further, it provides what we need to assess its trustworthiness—the full text is displayed, as are its “re-presentations” in terms of stanzas and narrative functions: the technical devices that make it work are clearly defined and visible; the underlying structure is specified; and his interpretation is tied directly to the data. These are essentially the same grounds I proposed earlier in describing my study of a life-history narrative as strong warrants for the validity claims that may be made in alternative types of narrative study.

—Proust's Narrative Strategy

White (1989) explicates the “narrative strategy” used by Marcel Proust in his *À la recherche du temps perdu*, by a close textual analysis of one paragraph from this multivolume novel. He “frames” this brief extract by observing that it appears, on

first inspection, as a "descriptive pause" or "interlude" in the action (p. 4). The paragraph relates four successive "characterizations" of a fountain by the narrator, Marcel, as he walks towards it in a garden of the Guermantes' palace where he has been attending a soiree. The text is presented in French because, White argues, translations other than his own blur distinctions that are important for his analysis.

A novel differs in many respects from the narrative texts usually studied by social scientists, such as the life history interviews and stories of personal experience in, respectively, my own and Gee's studies. (However, see Bruner's [1986] argument for studying great works of fiction.) Nonetheless, although White's terms may be unfamiliar, his analysis is generally applicable to other types of texts since he follows a sequence of steps that closely parallel those of more typical "empirical" studies: theoretical formulation of interpretative discourse, selection of a sample text, definition and application of coding categories, redescription of the text in terms of the categories, finding a sequential order of categories, analytic restatement of this finding as a structural model of narration, interpretation of the function of the text in the larger narrative, generalization of the interpretation into a theory of narrative strategy.

White begins by distinguishing "interpretive discourse" from both explanation and description. He refers to interpretation as a "preliminary stage" in efforts to understand an object or event when we are uncertain as to how to "properly" describe or explain it. It is an "effort of deciding, not only *how* to describe and explain such an object, but *whether* it can be adequately described or explained at all" (p. 1). This is White's theoretical category in which he locates Marcel's sequence of descriptions—the passage that is the object for his analysis. He then proposes that the characteristic "modality of discursive articulation" in interpretative discourse is "more *tropical* than logical." That is, it is organized in terms of the meanings and functions of the different tropes and their relation to each other rather than by a series of propositions that are logically or causally connected. It departs from literal or technical language and from relations of "strict deducibility," "giving itself over to techniques of figuration" (p. 2). His analysis focuses on Proust's use of four such "techniques"—familiar tropes of literary criticism: metaphor, metonymy, synecdoche, and irony.¹⁵

The analytic function of these tropes is the same as the "codes" for self-preoccupations, motives, work spheres, and poetic devices found in the studies described earlier. Although they may be unfamiliar to social scientists, they are drawn from a comprehensive category system, refined through a long tradition of literary criticism and textual analysis. As is true of any coding system, an adequate comprehension of what the tropes "mean" requires more than their definition. We must also understand the conceptual framework within which they are located; that is,

¹⁵ These tropes are "fuzzy categories." Burke (1945) refers to them as the "master tropes," and observes that they "shade into one another. Give a man but one of them, tell him to exploit its possibilities, and if he is thorough in doing so, he will come upon the other three" (p. 503). Briefly, a metaphor involves describing or characterizing something in terms of something else, a metonymy describes a whole by one of its parts or aspects, a synecdoche represents the relationship between the parts and the whole, and irony brings together all the terms or "sub-perspectives" so that they interact with and influence one another in a "total form" (Burke, p. 512).

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we must understand them as linguistic practices within a type of discourse. For White, their significance lies in their relationship to each other as they are deployed in an orderly sequence. Thus, he presents a structural model for the analysis of this passage as a narrative, much as Gee and I did in our respective studies.

These tropes are omnipresent in both fictional and nonfictional narrative accounts. Pointing them out, or counting them, would not tell us very much about Proust's "narrative strategy," which is White's primary concern. To this end, he focuses on their specific sequential placement relative to each other and on the overall function of this "tropical" order. (Note the resemblance between this approach and Gee's emphasis on the discourse—rather than the syntactic-level functions of narrative devices.) White summarizes the "model" of narration, displayed in this passage, as a successive movement of the narrator through the four tropes, as alternative descriptions of the fountain: from an initial "metaphoric apprehension" of it, through a "metonymic" characterization as a "dispersion of its attributes," to a "synecdochic comprehension of its possible 'nature,'" to, finally, "an ironic distancing of the process of narration itself" (p. 6).

This "passage" through the four tropes parallels the actual movement of the narrator towards the fountain, with each stage marked explicitly in the text. From afar, the narrator's impression of the fountain is captured in a metaphor as a "pale and quivering plume." Closer, the fountain is "revealed to be 'in reality as often interrupted as the scattering of the fall,'" with new jets of water producing the effect of the "single flow," a metonymic description. At the third stage, the "form" and "content" of the spray are "'grasped together' as a whole indistinguishable from the parts that constitute it," "in the manner of a synecdoche." The last characterization is:

by turns lyrical-elegiac and playful in tone . . . at once ironical in its structure and radically revisionary with respect to all three of the preceding descriptions. . . . It both radically alters the semantic domain from which its figures of speech are drawn and abruptly, almost violently, undercuts the very impulse to metaphorize by its reminder that the fountain is, after all, *only* a fountain (pp. 7-11)

The passage ends in this ironic mode.

White observes that the fourth description is not the "most precise, correct, comprehensive, or appropriate" one. The other three cannot be "adjudged in some way inferior." Rather, it gives us, as we near the end of the passage, the "crucial bit of information that allows us suddenly to grasp 'the point of it all.' . . . to discern something like the kind of 'plot' that permits a retrospective correlation of the events of this 'story' as a story of a particular kind—a specifically 'ironic' story" (pp. 11-12).

He then proposes that the trope-sequence structure of this passage, "considered as a *narrational unit* . . . is related to the three scenes of interpretation that precede it by the four figurative modes which constitute the substance of its own form," and, further, that "as a *model of interpretation* itself, the fountain scene provides a paradigm for how to read the three more extensive scenes of interpretation that precede it" (p. 20). That is, each of the preceding scenes and the relationship among them and the key paragraph reveals the same *structure* of successive tropes—metaphor, metonymy, synecdoche, and irony—with the fountain scene functionally related to each of the others through the same forms of figuration.

Finally, bringing his argument back to the distinction between interpretation and either explanation or description, he states that there is no “logical connection” between the scenes. The relation is “only tropical, which is to say that it is unpredictable, unnecessary, undeductible, arbitrary and so on but, at the same time, functionally effective and retrodictable as a narrative unit *once its tropical relationship to what comes before (and what comes after) it is discerned*” (p. 13). This is his answer to the question of how narration and interpretation

can be endowed with a coherence quite other than the kinds of coherence it may possess at the level of the sentence (grammatical coherence) and the level of demonstration or explicit argument (logical coherence). Obviously, my answer to this question is “figurative coherence,” the coherence of the activity of (linguistic) figuration itself. (p. 19)

Can we make a reasonable assessment about the trustworthiness of White’s analysis? I think we can, and for the same reasons I gave for the preceding two studies, namely, the visibility of his analysis. That is, he presents the full text of the passage, explicitly defines and links the coding categories to specific words and phrases, and shows us the location and sequential ordering of the different tropes, that is, the structure of the paragraph.

One advantage of choosing a relatively unfamiliar “literary” approach as a candidate exemplar for narrative analysis is that it highlights the problematic nature of validation. Although White has shown us what he did, the “rules” that inform his analysis cannot be applied mechanically. We must have some level of specialized knowledge and skills to assess its adequacy and potential range of application. Minimally, of course, it would be useful to have more than high school mastery of French as well as an understanding of tropes. However, that would only scratch the surface of what we have to know to understand White’s research practice as a form of life and, from that understanding, be able to decide whether it would be a productive direction to pursue in our own work. The same requirement applies, of course, to our efforts to assess the validity claims of any study. Since White displays the evidence for his claims, this problem is not his but ours.

I have focused only on the first level of White’s analysis—his description of the structure of the paragraph as a sequence of tropes and his interpretation of this structure as a narrative strategy. He expands his interpretation to the larger narrative context of the core paragraph, the three preceding scenes in this chapter, and then to the novel as a whole. How far we would wish to pursue our assessment of his work depends on the aims and scope of our own studies. Different criteria might come into play, depending on our theoretical interests and the range of inferences that we intend. We would, however, have a place to begin these extended explorations.

Conclusion

In this article, I have proposed an approach to the critical assessment of inquiry-guided research that is more appropriate to the features of such studies—ethnographies, case studies, textual analyses—than the standard experiment-based model. These studies, comprising a significant sector of the theoretical and empirical enterprise in psychology and the social sciences, are not designed as experiments,

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and do not "test" hypotheses, "measure" variation on quantitative dimensions, or "test" the significance of findings with statistical procedures. Criteria and procedures based on the dominant experimental/quantitative prototype are irrelevant to these studies in the literal sense that there is nothing to which to apply them.¹⁶ When the standard model is misapplied, as it often is, inquiry-guided studies fail the test and are denied scientific legitimacy.

Recognizing this problem, other investigators engaged in these studies have proposed alternative validity criteria and procedures that parallel the standard ones, but take into account the special features of inquiry-guided research. Although these efforts have been useful, particularly in their critique of the standard model, I believe that they do not go far enough. By retaining the dominant model as the implicit ground against which alternative approaches are evaluated, the latter continue to be viewed as inadequate, temporary expedients—useful, perhaps, but only until the time that "real" scientific methods are found.

My proposal moves in a different direction. As a point of departure, I argued that the dominant research model is an abstract idealization that does not correspond to how the work of science gets done. I suggested replacing the "storybook image of science" with an empirically based description of scientific practices, of the ways that working scientists produce, test, and validate their findings. When closely observed, as in studies by historians and sociologists of science, research scientists turn out to resemble craftspersons more than logicians. Competence depends on apprenticeship training, continued practice, and experience-based, contextual knowledge of the specific methods applicable to a phenomenon of interest rather than on an abstract "logic of discovery" and application of formal "rules."

Further, the knowledge base for scientific research is largely tacit and unexplicated, learned through a process of socialization into a particular "form of life." The discovery, testing, and validation of findings is embedded in cultural and linguistic practices. Transmission of the necessary knowledge for replicating other work is an uncertain process, depending primarily on personal contact with researchers and observation of their practices. Even this does not guarantee comparability, as one of Collin's (1985) respondents indicates:

It's very difficult to make a carbon copy. . . . But if it turns out that what's critical is the way he glued his transducers, and he forgets to tell you that the technician

¹⁶ My conjoint term "experimental/quantitative prototype" reflects the prevailing view of an intimate and inherent linkage between statistics and experimentation, a position I have not challenged in this paper. However, the relationship is problematic, and it is worth noting that there is a viable, critical perspective that sees these two "methods" as antithetical to each other. It is expressed forcefully by Lewin and Skinner, who are poles apart on most other issues, but share a negative view of the assumed equivalence between experimental and statistical "controls." Thus, Lewin (1931/1935), observing the "commanding significance of statistics in contemporary psychology," argues that reliance on frequencies of occurrences cannot lead to theoretical "laws," which depend instead on the study of the individual case in all its "concreteness." And Skinner (1961), commenting on "The Flight from the Laboratory," attributes it to a deficiency in graduate school training: "They have taught statistics in lieu of scientific method. Unfortunately, the statistical pattern is incompatible with some major features of laboratory research" (p. 247). He goes on to point out various "destructive" effects of the emphasis on statistics, such as their leaving the psychologist with "at best an indirect acquaintance with the 'facts' he discovers" and the "inimical" effect on laboratory practice of statisticians' recommendations. A recent, related critique of the tendency in sociological research to assume that statistical controls can be substituted for experimental controls in causal analyses may be found in Lieberson (1985).

always puts a copy of the *Physical Review* on top of them for weight, well, it would make all the difference. (p. 86)

Within this perspective on science as practice, I proposed a reformulation of validation as the social construction of scientific knowledge. It is evident that the model to which inquiry-guided researchers have been held accountable has little if any reality. Experimental scientists proceed in pragmatic ways, learning from their errors and failures, adapting procedures to their local contexts, making decisions on the basis of their accumulated experiences.

This resemblance between experimental and inquiry-guided studies becomes clear when we shift our attention from single studies to research programs. The typical way of doing experimental work is to conduct a series of successive studies, each building on preceding ones, and this progression is clearly inquiry-guided. The analogue in complex nonexperimental studies is the sequence of different stages—from initial observations, through preliminary coding, through further observations, revisions of coding, and so on—which may be viewed as sub-studies, building progressively on each other. (This is, of course, an insight we owe to “grounded theory”; see Glaser & Strauss, 1967; Strauss, 1987.)

This discovery—of the contextually grounded, experience-based, socially constructed nature of scientific knowledge—should be cause for celebration rather than despair. It does not dispense with methods for systematic study but locates them in the world of practice rather than in the abstract spaces of Venn diagrams or Latin Squares. Assessments of the validity of any single study are provisional. Following the rules of experimental design, quantification and statistical analysis are not truth tests but methodic accounting procedures, and a researcher's documentation of their use is part of the rhetoric of a particular form of scientific life. This perspective does not lead to an empty relativism or to Feyerabend's (1978) anarchic program of an “anything goes” science. Methods are still assessed for their consistency and utility in producing trustworthy findings, and trustworthiness is tested repeatedly and gains in strength through our reliance on these findings as the basis for further work.

The recent convergence among some prominent validity theorists on the primacy of construct validity adds support to the argument I advanced based on studies of scientific practice. Their emphasis on the fundamental importance of theory and interpretation in validation puts the problem beyond the reach of “technical” solutions. Again, this shift away from formal rules and procedures does not mean a retreat from systematic and methodic ways of inquiry. But it does mean that more is involved in these ways (that is, these practices) than was captured by explicit and elaborate lists of types of and threats to validity.

If standard rules will not serve for experiments, neither will they serve for inquiry-guided studies. As an alternative approach, I adopted Kuhn's (1962/1970) concept of exemplars, the “concrete problem-solutions” that show researchers “by example how their job is to be done” (p. 187). In experimental sciences, laboratory exercises do this job. Learning from them depends on more than following a series of outlined steps: heat “x” to 80°C and add “y.” Ravetz (1971) remarks that “one of the things that every schoolboy knows about science is a general property of scientific equipment, which has been given the name of the ‘fourth law of thermodynamics’: no experiment goes properly the first time” (p. 76). Making an experi-

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ment work requires attention to various idiosyncratic features of the laboratory, of instrument errors and artifacts, of the ambient temperature and humidity, and many other factors too numerous and cumbersome to list but easily recognized in practice. Thus, learning from exemplars is a process of contextually grounded practice, which brings us full circle to what we have come to understand as scientific research.

An important task for the less well established areas of scientific inquiry is to develop a collection of relevant exemplars.¹⁷ I proposed three studies as candidate exemplars for narrative research, recognizing that they are only a few of the many potential ones. They vary in types of texts, concepts, aims, and methods and were chosen to suggest a range of alternative approaches. However, they are similar in several important respects that I believe make them strong candidates, and, at the same time, differ from the standard model in ways that make them more appropriate for studies of narratives. These are: the display of the primary texts; the specification of analytic categories and the distinctions in terms of discernable features of the texts; and, theoretical interpretations focused on structures, that is, on relations among different categories, rather than on variables.

In each study, the text is available so that other researchers can inspect it and assess the adequacy with which the methods and interpretations represent the data. Further, the availability of the primary data allows for a reasonable judgment, albeit a preliminary one, of whether and how representative it might be of other texts. That is, the question may be addressed, in an empirically grounded way, of the possible generalizability of findings and interpretations, of the "projectibility" (Goodman, 1979/1983) of inferences based on the analyses. Our assessments of trustworthiness are as firmly grounded as those we might make of studies relying on the standard research model.

The central theoretical aim in each of the selected studies is to describe, analyze, and interpret a pattern of relationships within a set of conceptually specified analytic categories. I refer to these patterns as structures, and the studies are instances of different types of structural analysis. These structures represent a significant characteristic of the texts at a more abstract level. Their general theoretical significance depends upon whether or not the particular texts are representative samples of a general class of texts. For example, in my study of an artist-craftsman's narrative, the double structure of hierarchically ordered possible choices among types of work and the temporal ordering of actual choices is viewed as a model for analyses of the work histories of other craftspersons. Gee relates the stanza structure of a child's story, and her use of technical poetic strategies to achieve meaning and coherence, to the typical form of narratives in oral cultures. And White's discovery of the sequential structure of tropes in one paragraph of Proust's novel— from metaphor, to metonymy, to synecdoche, to irony—is interpreted by him as an instance of a general narrative strategy.

¹⁷ Many inquiry-guided studies differ not only from the experimental prototype, but from the structural analysis of narrative texts that I have examined. The specific features of, for example, ethnographies or studies of social institutions require different criteria and procedures for assessing their trustworthiness. I hope that other researchers will undertake the task of explicating their methods so that we can build a corpus of exemplars for various types of research.

In these studies, theory and analysis are in a continuing dialectic with each other and with the data, and the process is open to us. This does not mean that we would necessarily be compelled or persuaded by the findings of any particular study, or agree with a proposed interpretation. But, as I have repeatedly stressed, we are given sufficient information to make a judgment of their trustworthiness and can then decide whether or not to depend on them for further work.

This paper was written for, and from the perspective of, researchers engaged in inquiry-guided and interpretive studies. As a member of that new but growing research community, I have tried to show that we can make a strong claim for the scientific legitimacy of our work. Our collective task, to which I hope this paper has contributed, is to engage each other in vigorous debate about issues of validation as we move towards an alternative form of scientific life.

References

- American Psychological Association (1954). Technical recommendations for psychological tests and diagnostic techniques (Part 2). *Psychological Bulletin*, 51(2).
- Angoff, W. H. (1988). Validity: An evolving concept. In H. Wainer & H. I. Braun (Eds.), *Test validity*. Hillsdale, NJ: Erlbaum.
- Barnes, B., & Edge, D. (Eds.). (1982). *Science in context: Readings in the sociology of science*. Cambridge: MIT Press.
- Barnes, B., & Shapin, S. (Eds.). (1979). *Natural order: Historical studies of scientific culture*. Beverly Hills: Sage.
- Bazerman, C. (1989). [Review of *Changing order: Replication and induction in scientific practice* by H. M. Collins (1985)]. *Philosophy of the Social Sciences*, 19, 115-118.
- Bredo, E., & Feinberg, W. (Eds.). (1982). *Knowledge and values in social and educational research*. Philadelphia: Temple University Press.
- Brenner, M. (1981). *Social method and social life*. New York: Academic Press.
- Brinberg, D., & McGrath, J. E. (1982). A network of validity concepts within the research process. In D. Brinberg & L. H. Kidder (Eds.), *Forms of validity in research*. San Francisco: Jossey-Bass.
- Brinberg, D., & McGrath, J. E. (1985). *Validity and the research process*. Beverly Hills: Sage.
- Bruner, J. (1986). *Actual minds, possible worlds*. Cambridge: Harvard University Press.
- Bruner, J. (1990). *Acts of meaning*. Cambridge: Harvard University Press.
- Buske, K. (1945). *A grammar of motives*. New York: Prentice-Hall.
- Campbell, D. T. (1979). A tribal model of the social system vehicle carrying scientific knowledge. *Knowledge: Creation, Diffusion, Utilization*, 1(2), 181-201.
- Campbell, D. T., & Stanley, J. T. (1963). Experimental and quasi-experimental designs for research. In N. L. Gage (Ed.), *Handbook of research on teaching*. New York: Rand McNally.
- Carini, P. F. (1975). *Observation and description: An alternative methodology for the investigation of human phenomena* (North Dakota Study Group on Evaluation Monograph). Grand Forks, ND: University of North Dakota Press.
- Cherryholmes, C. H. (1988). Construct validity and the discourses of research. *American Journal of Education*, 96, 421-457.
- Cohler, B. J. (1982). Personal narrative and life course. In P. B. Baltes & O. G. Brim, Jr. (Eds.), *Life-span development and behavior*. New York: Academic Press.
- Collins, H. M. (1985). *Changing order: Replication and induction in scientific practice*. Beverly Hills: Sage.
- Cook, T. D., & Campbell, D. T. (1979). *Quasi-experimentation: Design and analysis issues for field settings*. Chicago: Rand McNally.
- Critical Inquiry*. (1980). On narrative. Vol. 7, pp. 1-236.

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Critical Inquiry. (1981). Critical response. Vol. 7, pp. 777-809.

Cronbach, L. J. (1984). *Essentials of psychological testing* (4th ed.). New York: Harper & Row.

Cronbach, L. J. (1988). Five perspectives on validity argument. In H. Wainer & H. I. Braun (Eds.), *Test validity*. Hillsdale, NJ: Erlbaum.

Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin*, 52, 281-302.

Dervin, B., Grossberg, L., O'Keefe, B. J., & Wartella, E. (Eds.). (1989). *Rethinking communication: Vol. 2. Paradigm exemplars*. Newbury Park, CA: Sage.

Educational Testing Service. (1980). *Test use and validity*. Princeton: Author.

Enerstvedt, R. T. (1989). The problem of validity in social science. In S. Kvale (Ed.), *Issues of validity in qualitative research*. Lund, Sweden: Studentlitteratur.

Erikson, E. H. (1950). *Childhood and society*. New York: Norton.

Erikson, E. H. (1959). Identity and the life cycle [Monograph No. 1]. *Psychological Issues*, 1(1). New York: International Universities Press.

Fehr, B. J., & Stetson, J. (1990). A bibliography for ethnomethodology. In J. Coulter (Ed.), *Ethnomethodological sociology* (Vol. 1). Aldershot, Eng.: Edward Elgar.

Feyerabend, P. (1978). *Against method*. London: Verso.

Frankel, R. M. (1983). The laying on of hands. Aspects of the organization of gaze, touch, and talk in the medical encounter. In S. Fisher & A. D. Todd (Eds.), *The social organization of doctor-patient communication*. Washington, DC: Center for Applied Linguistics.

Garfinkel, H. (1967). *Studies in ethnomethodology*. Englewood Cliffs, NJ: Prentice-Hall.

Gee, J. P. (1985). The narrativization of experience in the oral style. *Boston University Journal of Education*, 167, 9-35.

Gee, J. P. (1986). Units in the production of narrative discourse. *Discourse Processes*, 9, 391-422.

Gieryn, T. F. (1983). Boundary-work and the demarcation of science from non-science: Strains and interests in the professional ideologies of scientists. *American Sociological Review*, 48, 781-795.

Gilbert, G. N., & Mulkay, M. (1981). Contexts of scientific discourse: Social accounting in experimental papers. In K. D. Knorr, R. Krohn, & R. Whitley (Eds.), *The social process of scientific investigation*. Boston: Reidel.

Gilbert, G. N., & Mulkay, M. (1984). *Opening Pandora's box: A sociological analysis of scientists' discourse*. Cambridge: Cambridge University Press.

Glaser, B. G., & Strauss, A. (1967). *The discovery of grounded theory*. Chicago: Aldine.

Goodman, N. (1978). *Ways of worldmaking*. Indianapolis: Hackett.

Goodman, N. (1979/1983). *Fact, fiction, and forecast* (4th ed.). Cambridge: Harvard University Press.

Green, B. F. (1990). [Review of *Educational measurement*, edited by R. L. Linn (1989; 3rd ed.)]. *Contemporary Psychology*, 35, 850-851.

Harper, D. (1987). *Working knowledge: Skill and community in a small shop*. Chicago: University of Chicago Press.

Heath, S. B. (1982). What no bedtime story means: Narrative skills at home and school. *Language in Society*, 11, 49-76.

Heath, S. B. (1983). *Ways with words: Language, life, and work in communities and classrooms*. Cambridge: Cambridge University Press.

Heritage, J. (1984). A change of state token and aspects of its sequential placement. In J. M. Atkinson & J. Heritage (Eds.), *Structures of social action*. Cambridge: Cambridge University Press.

Hernadi, P. (Ed.). (1989). *The rhetoric of interpretation and the interpretation of rhetoric*. Durham, NC: Duke University Press.

Huberman, A. M., & Miles, M. B. (1983). Drawing valid meaning from qualitative data: Some techniques of data reduction and display. *Quality and Quantity*, 17, 281-339.

Jefferson, G. (1978). Explanation of transcript notation. In J. Schenkein (Ed.), *Studies in the organization of conversational interaction*. New York: Academic Press.

- Jefferson, G. (Ed.). (1989). Harvey Sacks: Lectures 1964-65 [Special issue]. *Human Studies*, 12(3-4).
- Katz, J. (1983). A theory of qualitative methodology: The social system of analytic fieldwork. In R. M. Emerson (Ed.), *Contemporary field research*. Boston: Little, Brown.
- Knorr, K. D., Krohn, R., & Whitley, R. (Eds.). (1981). *The social process of scientific investigation*. Boston: Reidel.
- Koch, S. (1976). Language communities, search cells, and the psychological studies. In W. J. Arnold (Ed.), *Nebraska symposium on motivation, 1975*. Lincoln: University of Nebraska Press.
- Kuhn, T. S. (1962/1970). *The structure of scientific revolutions* (2nd ed., enlarged). Chicago: University of Chicago Press.
- Kuhn, T. S. (1970/1974a). Logic of discovery or psychology of research? In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (3rd impression). Cambridge: Cambridge University Press.
- Kuhn, T. S. (1970/1974b). Reflections on my critics. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (3rd impression). Cambridge: Cambridge University Press.
- Kvale, S. (Ed.). (1989a). *Issues of validity in qualitative research*. Lund, Sweden: Studentlitteratur.
- Kvale, S. (1989b). To validate is to question. In S. Kvale (Ed.), *Issues of validity in qualitative research*. Lund, Sweden: Studentlitteratur.
- Labov, W. (1972). The transformation of experience in narrative syntax. In W. Labov, *Language in the inner city: Studies in the Black English vernacular*. Philadelphia: University of Pennsylvania Press.
- Labov, W., & Waletzky, J. (1967). Narrative analysis: Oral versions of personal experience. In J. Helms (Ed.), *Essays on the verbal and visual arts*. Seattle: University of Washington Press.
- Lakatos, I., & Musgrave, A. (Eds.). (1974). *Criticism and the growth of knowledge* (3rd ed.). Cambridge: Cambridge University Press.
- Langellier, K. M. (1989). Personal narratives: Perspectives on theory and research. *Text and Performance Quarterly*, 9, 243-276.
- Lather, P. (1986). Issues of validity in openly ideological research: Between a rock and a soft place. *Interchange*, 17(4), 63-84
- Latour, B. (1988). *The pasteurization of France*. Cambridge: Harvard University Press.
- Latour, B. (1990). Postmodern? No, simply Amodern! Steps towards an anthropology of science. *Studies in the History and Philosophy of Science*, 21(1), 145-171.
- Latour, B., & Woolgar, S. (1979). *Laboratory life: The social construction of scientific facts*. Beverly Hills: Sage.
- Levy, P. (1981). On the relation between method and substance in psychology. *Bulletin, British Psychological Society*, 34, 265-270.
- Lewin, K. (1931/1935). The conflict between Aristotelian and Galilean modes of thought in contemporary psychology. In K. Lewin, *A dynamic theory of personality: Selected papers*. New York: McGraw-Hill.
- Lieberson, S. (1985). *Making it count: The improvement of social research and theory*. Berkeley: University of California Press.
- Lincoln, Y. S., & Guba, E. G. (1985). *Naturalistic inquiry*. Beverly Hills: Sage.
- Loevinger, J. (1957). Objective tests as instruments of psychological theory [Monograph Supplement No. 9]. *Psychological Reports*, 3, 635-694.
- Lynch, M. (1985). *Art and artifact in laboratory science: A study of shop work and shop talk in a research laboratory*. Boston: Routledge & Kegan Paul.
- McAdams, D. P. (1985). *Power, intimacy, and the life story. Personological inquiries and identity*. Homewood, IL: Dorsey.
- McAdams, D. P., & Ochberg, R. L. (Eds.). (1988). Psychobiography and life narratives [Special issue]. *Journal of Personality*, 56(1).
- McClelland, D. C. (1984). *Human motivation*. Glenview, IL: Scott, Foresman.

- Human Studies*,
of analytic field-
ittle, Brown.
scientific investi-
l studies. In W.
iversity of Ne-
ged). Chicago:
n I. Lakatos &
). Cambridge:
usgrave (Eds.),
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en: Studentlit-
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In W. Labov,
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rsity of Wash-
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research. *Text*
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Selected papers.
eory. Berkeley:
Sage.
y [Monograph
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res and identity.
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ian.
- Mervis, C. B., & Rosch, E. (1981). Categorization of natural objects. *Annual Review of Psychology*, 32, 89-115.
Messick, S. (1989). Validity. In R. L. Linn (Ed.), *Educational measurement* (3rd ed.). New York: Macmillan.
Michaels, S. (1981). Sharing time: Children's narrative styles and differential access to literacy. *Language in Society*, 10, 423-442.
Mishler, E. G. (1979). Meaning in context: Is there any other kind? *Harvard Educational Review*, 49, 1-19.
Mishler, E. G. (1984). *The discourse of medicine: Dialectics of medical interviews*. Norwood, NJ: Ablex.
Mishler, E. G. (1986a). *Research interviewing: Context and narrative*. Cambridge: Harvard University Press.
Mishler, E. G. (1986b). The analysis of interview narratives. In T. R. Sarbin (Ed.), *Narrative psychology: The storied nature of human conduct*. New York: Praeger.
Mishler, E. G. (1989). [Review of *Working knowledge: Skill and community in a small shop* by D. Harper (1987)]. *American Craft*, 49(2), p. 22.
Mishler, E. G. (In press). Work, identity, and narrative: An artist-craftsman's story. In G. Rosenwald & R. Ochberg (Eds.), *Storied lives*. New Haven: Yale University Press.
Mitroff, I. (1974a). Norms and counter-norms in a select group of the Apollo moon scientists: A case study of the ambivalence of scientists. *American Sociological Review*, 39, 579-595.
Mitroff, I. (1974b). *The subjective side of science: A philosophical inquiry into the psychology of the Apollo moon scientists*. Amsterdam: Elsevier.
Morgan, G. (Ed.). (1983). *Beyond method. Strategies for social research*. Beverly Hills: Sage.
Morris, W. (1883/1966). Art under plutocracy. In M. Morris (Ed.), *The collected works of William Morris: Vol. XXIII. Signs of change: Lectures on socialism*. New York: Russell and Russell.
Paget, M. A. (1983). Experience and knowledge. *Human Studies*, 6, 67-90.
Polanyi, L. (1985). Conversational storytelling. In T. A. van Dijk (Ed.), *Handbook of discourse analysis* (Vol. 3). London: Academic Press.
Polanyi, M. (1966). *The tacit dimension*. New York: Doubleday.
Polkinghorne, D. E. (1988). *Narrative knowing and the human sciences*. Albany: State University of New York Press.
Prelli, L. J. (1989). The rhetorical construction of scientific ethos. In H. W. Simons (Ed.), *Rhetoric in the human sciences*. Newbury Park, CA: Sage.
Ravetz, J. R. (1971). *Scientific knowledge and its social problems*. New York: Oxford University Press.
Reason, P., & Rowan, R. (Eds.). (1981). *Human inquiry: A sourcebook of new paradigm research*. New York: Wiley.
Richards, J. L. (1979). The reception of a mathematical theory: Non-Euclidean geometry in England, 1868-1883. In B. Barnes & S. Shapin (Eds.), *Natural order: Historical studies of scientific culture*. Beverly Hills: Sage.
Ricoeur, P. (1981). *Hermeneutics and the human sciences. Essays on language, action, and interpretation*. New York: Cambridge University Press.
Riessman, C. K. (1987). When gender is not enough. *Gender and Society*, 1, 172-207.
Riessman, C. K. (1990). *Divorce talk: Women and men make sense of personal relationships*. New Brunswick, NJ: Rutgers University Press.
Roberts, R. M. (1989). *Serendipity: Accidental discoveries in science*. New York: Wiley.
Rosch, E. (1973). On the internal structure of perceptual and semantic categories. In T. E. Moore (Ed.), *Cognitive development and the acquisition of language*. New York: Academic Press.
Rosch, E. (1978). Principles of categorization. In E. Rosch & B. B. Lloyd (Eds.), *Cognition and categorization*. Hillsdale, NJ: Erlbaum.
Rosch, E., & Mervis, C. B. (1975). Family resemblances: Studies in the internal structure of categories. *Cognitive Psychology*, 7, 573-605.

- Rosenwald, G., & Ochberg, R. (Eds.). (In press). *Storied Lives*. New Haven: Yale University Press.
- Salner, M. (1989). Validity in human science research. In S. Kvale (Ed.), *Issues of validity in qualitative research*. Lund, Sweden: Studentlitteratur.
- Sarbin, T. R. (1986a). *Narrative psychology: The storied nature of human conduct*. New York: Praeger.
- Sarbin, T. R. (1986b). The narrative as a root metaphor for psychology. In T. R. Sarbin, *Narrative psychology: The storied nature of human conduct*. New York: Praeger.
- Schegloff, E. A., & Sacks, H. (1973). Opening up closings. *Semiotica*, 7, 289-327.
- Schenkein, J. (Ed.). (1978). *Studies in the organization of conversational interaction*. New York: Academic Press.
- Shapin, S., & Schaffer, S. (1985). *Leviathan and the air pump: Hobbes, Boyle, and the experimental life*. Princeton: Princeton University Press.
- Sidman, M. (1960). *Tactics of scientific research: Evaluating experimental data in psychology*. New York: Basic Books.
- Simons, H. W. (Ed.). (1989). *Rhetoric in the human sciences*. Newbury Park, CA: Sage.
- Skinner, B. F. (1961). *Cumulative record*. New York: Appleton-Century-Crofts.
- Stewart, A. J., Franz, C., & Layton, L. (1988). The changing self: Using personal documents to study lives. *Journal of Personality*, 56(1), 41-74.
- Strauss, A. L. (1987). *Qualitative analysis for social scientists*. New York: Cambridge University Press.
- Tagg, S. K. (1985). Life story interviews and their interpretation. In M. Brenner, J. Brown, & D. Canter (Eds.), *The research interview: Uses and approaches*. London: Academic Press.
- Wainer, H., & Braun, H. I. (Eds.). (1988). *Test validity*. Hillsdale, NJ: Erlbaum.
- White, H. (1987). *The content of the form: Narrative discourse and historical representation*. Baltimore: Johns Hopkins University Press.
- White, H. (1989). The rhetoric of interpretation. In P. Hernadi (Ed.), *The rhetoric of interpretation and the interpretation of rhetoric*. Durham, NC: Duke University Press.
- Wittgenstein, L. (1953). *Philosophical investigations* (3rd ed.). Oxford: Blackwell.
- Wooton, A. J. (1988). Remarks on the methodology of conversation analysis. In D. Rogers & P. Bull (Eds.), *Conversation: An interdisciplinary perspective*. Clevedon, Eng.: Multilingual Matters.
- Young, K. (1984). Ontological puzzles about narratives. *Poetics*, 13, 239-259.
- Young, K. G. (1987). *Taleworlds and storyrealms*. Boston: Martinus Nijhoff.

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