NARRATIVE'S MOMENT AND SOCIOLOGY'S PHENOMENA: Toward a Narrative Sociology

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In its urgency to establish itself as a science, sociology missed the opportunity to nurture its narrative character that the Thomas and Znaniecki research represented. Another moment now exists in an array of contradictions inherent in conventional sociological practices and the increasing acceptance and sophistication of narrative work in the human sciences. These conditions contain the potential for developing a narrative sociology in which sociology's phenomena are seen as significantly constituted by stories and in which sociological work is seen as narrative work. This article examines those conditions for purposes of opening spaces that a narrative sociology might fill.

The narrative turn in human inquiry (Mitchell, 1981) has reached the social sciences and has created a situation I refer to as *narrative's moment*. This moment is a set of conditions and possibilities through which a genuine narrative sociology might be developed. Such a sociology would encompass the sociology *of* narratives, or the study of narratives from the standpoint of sociology's domain interests, and it would more inclusively and reflexively include sociology's narratives, viewing sociologists *as* narrators and thereby inquiring into what they do to and with their's and other people's narratives. This duality of focus is at best sensitizing, with very fuzzy edges and a center yet to be created. Under current disciplinary conditions, in fact, I suspect that such a center cannot be fully grasped. However, it can be viewed in a translucent way with the help of existing work from the fields of folklore, anthropology, developmental psychology, communication, history, literary criticism, and the other human sciences. A narrative sociology thus will have the core characteristic of being very interdisciplinary and will embrace the impulse to blur genres even more than they already are.

My purpose is to contribute to a narrative sociology whose moment is signified by: (1) sociology's entering, kicking and screaming, into the post-positivistic era; (2) by the fact that three recent Presidential Addresses of sociology's largest regional societies (the Midwest, Southern, and North Central) have pertained directly to narrative and sociology (Richardson, 1988; Reed, 1989; Denzin, 1990); and (3) increasing recognition of sociolo-

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gy's methodological fetishisms (Ross, 1991; Bannister, 1987; Lieberson, 1985).[1] To those I add sociologists' turgid and still myopic recognition that they have nothing to study and theorize about unless people in some way communicate with one another. This moment is a desirable one, it rests on solid grounds, and it is entirely possible to develop a narrative sociology. Toward that end, I first provide the background and assumptions necessary to visualize how such a sociology can rest within the discipline. I then articulate, in an admittedly preliminary way, the narrative character of core sociological practices and ontological claims. The objective, as always, is to help open new spaces for understanding of human group life and to add to sociology's vibrance and relevance.

Background and Conceptualization

Intrinsic to sociology's continuous struggle with its future are tensions and contradictions that suggest the possibility of a narrative approach. A key issue is the nature of sociology's phenomena, which is one Floyd House (1934) raised over a half-century ago and has yet to be satisfactorily addressed. That phenomena, of course, is one that sociology has helped to construct, but in doing so it has constructed a skewed one. As reflected in its standard practices, sociology's phenomena are predominantly made up of rates, clusters, modal tendencies, regression curves, population parameters, aggregate patterns, and so forth, which to many reveal profound disciplinary failures.[2] One of those failures, as previously mentioned, is a reluctance to consider seriously what happens when humans communicate, which has contributed to a tacit acceptance of the defective and outmoded sender-message-receiver theory of communication (Maines and Couch, 1988). A related methodological failure is that while as a field sociology distrusts the human utterance, distinguishes between talk and behavior (obviously a very specious distinction, since talk is behavior) and then proposes itself as a science of behavior, it nonetheless methodologically relies almost exclusively on verbal data. It is common knowledge that surveys, whether using interviews or questionnaires, are the dominant means of gathering data in sociology in which researchers acquire information about what people say about themselves and others. This contradiction, combined with the increased emphasis on data analytic techniques, has led to the transformation of the meaning of the term "empirical." At its base, the term refers to features of an environment (John Dewey's "givens") that are processed and thus transformed to some degree by the sensing process itself (Dewey's "takens"). With the advent of measuring instruments, however, research is now called empirical when a researcher uses an index (sometimes of another index) of people's verbalizations about their verbalizations, conduct, or thoughts or about someone else's verbalizations, conduct or thoughts. A major consequence is that data of direct researcher experience have become correspondingly suspect, and in the name of precision and rigor, we now have a situation in which the purported study of group behavior is grounded primarily in aggregate psychological data that are so mediated by instrumentation that claims of being an empirical science have become rather dubious.

Being so quick to jump directly to the data, primarily for purposes of testing theories and propositions, sociologists by and large have ignored or buried in disciplinary myths the communication processes that are both their substance and media. Such disattention carries with it several dilemmas. One is that sociologists must use words and discursive representations in their work, although there is strong advocacy of the superiority of nondiscursive display of research findings and knowledge (Wagner, 1984; Collins, 1984). I

will explore this dilemma in some detail in the next section of this essay. Another related dilemma is that many of sociology's core concepts (personality, culture, structure, role, institution, status, norm, interaction, power, society) are concepts that also belong to other disciplines and were in use as ordinary words before the discipline of sociology came into existence (Rose, 1960). Moreover, considerable sociological knowledge and understanding has been incorporated into ordinary language and discourse (Giddens, 1984), paralleling the infusion of ordinary language into the field's conceptual structure (Blumer, 1931; 1940). This kind of fundamental blurring of sociology's boundaries underscores the fact that all social scientific work involves reactivity and that there is not and never has been a research procedure that does not affect its phenomena.

These conditions are embedded in the normal science practices of everyday sociologists, and are glossed by two very powerful influences that contribute to the inattention toward them. The first is the philosophy and ideology of rationalism that has dominated Western culture for the past four centuries. With the emergence of institutionalized science, the organizational demands of urbanization and bureaucratization, and the rise of the modern army and state came the increasing use and legitimation of quantification and the decreasing legitimation of the human utterance. At the heart of this process was the distrust of ordinary language; it was viewed as imprecise, ambiguous, evocative, and metaphorical. Correspondingly, precision was thought to come through enumeration and mathematical representation. With that division, expressed so well and forcefully by philosophers such as Locke, Hume, Leibniz, Hobbes, and Bentham, came the institutional demarcation between science and the humanities (Levine, 1985; Richardson, 1988: pp. 200–202). That demarcation inherently contained a stratification of legitimation, with science and logico-deductive reasoning being accorded superior status and credibility.

Second, the hegemonic influence of rationalism on the development of American sociology had the specific effect of retarding the development of narrative sociology. That effect is clearly evident in the methodological debates of the 1920's and 30's that were framed as reactions to the attitude-value scheme articulated by Thomas and Znaniecki's *The Polish Peasant in Europe and America* (1918–20) and to their use of narrative data (800 pages of their 2200 page, five-volume monograph were in the form of letters, biographies, and other narrative documents). With the constitution of sociology as a science at stake (Wiley, 1979; 1986), the debates centered on issues of measurement, conceptualization, ontology, and sociology's goals. Thurstone (1928, p. 547) argued that measuring instruments must be neutral; Faris (1928, p. 281) responded that methods must be subordinated to ontological characteristics, as did House (1934) some years later. Chapin (1935) argued that precision can be achieved only through quantification, a position even more forcefully echoed by Bain (1935).[3] Obviously the equation of quantification and scientific procedure was powerfully articulated in these debates, but just as powerfully was an embedded conceptualization of language. Referring to quantification, for example, Chapin (1935, pp. 479–480) stated, "It is a standard of communication. Numerical symbols are more standard and interchangeable than any other symbols. An "8" is an "8" and not a "7." But "red" is not always and every day "red." A sociology that gave primacy to the human utterance, in other words, would become "forever a bastard discipline," according to Bain (1935, p. 486), one containing " . . . words . . . and literary purple patches."

William Ogburn, however, was perhaps the most prophetic. In his 1929 Presidential Address to the American Sociological Association, he predicted that "In the future state

everyone will be a statistician, that is, nearly everyone . . . statistics will be identified with the subject matter of each social science rather than set apart as a special discipline" (1930, p. 6). In speaking of the acceptance, legitimation, and self-confirming use of statistical methods in sociological research, Ogburn's views epitomized the power of the rhetoric of precision and measurement through which sociology sought respectability and which was useful in carving out its institutional identity (Clinard, 1966). It was the beginnings of sociology's univocality which diminished the science of interpretations that Thomas and Znaniecki's formulations represented. As a consequence, the issue of the multiple meanings of discourse was not squarely faced but was seen instead as a problem to be solved by replacing words with numbers. The issue of human agency fell into neglect, and the concept of structure gained currency over the concepts of process and emergence, thus moving sociology increasingly into ahistorical theories. An irony, though, is that *The Polish Peasant* research, advocating as it did the use of narrative data, was voted by the Social Science Research Council in the 1930's as the exemplar of scientific sociology of the time (Blumer, 1939). It clearly represented the possibility for a narrative sociology that sociology's institutional practices and conventional ways of talking about itself reduced to something less than credible.

In all fairness, though, it is doubtful that Thomas and Znaniecki would have developed a narrative sociology even if they had that as a goal, which they did not. Like others of their day, they were trying to develop sociology as a nomethetic science, and in those efforts they treated human documents and written narratives only as data sources. What they lacked was narrative theory and ontology (Maines, 1992a, p. 1135), which is exactly what exists today and is essential to the emergence of the contemporary narrative moment. I now turn to that issue, and address the conceptualization of narrative and its relevance to social scientific agendas.

Scholarly work on narrative cuts across nearly all theoretical approaches, from postmodernism to rhetorical analysis, communication theory, pragmatism, functionalism, structuralism, and hermeneutics, and it is evidenced in every field of the human sciences. Accordingly, this body of work is characterized by a very healthy heterogeneity which can be brought to bear on an enormous range of questions and problems.

Despite that heterogeneity, there are common assumptions to the narrative approach (Martin, 1986). There is consensus that stories and storytelling are ubiquitous and that most if not all societal activities could not take place without narratives (Gergen and Gergen, 1988). These include socialization (Denzin, 1988), production of group solidarity (Eder, 1988; Maines, 1991), community processes (Cochrane, 1987; Lofland, 1990), planning and policy-making (Krieger, 1981), cognitive development (Mandler, 1984), cultural enactment (Howard, 1991), organizational functioning (Boje, 1991), gender (Bentz, 1989; Johnstone, 1990), and so forth (see Maines and Brigder, 1992). This range of application, of course, is most appealing to the sociologist, and it is encouraging to find in narrative analysis such a wide array of interest in issues of direct sociological concern. It also is encouraging to find a fairly broad consensus that narratives are forms of human conduct that are best conceptualized as social acts. This view can be found in the field of folklore (Dolby-Stahl, 1989), philosophy (Ricoeur, 1985), history (White, 1973), and psychology (Bruner, 1986). Perhaps surprising to sociologists, it is explicitly found also in the field of English. Barbara Herrnstein Smith, for example, asserts the sociological view that narratives are to be "regarded not only as *structures* but as *acts*, the features of which—like the features of all other acts—are functions of the variable sets of conditions

in response to which they are performed" (1980, pp. 231–232; emphasis in original). The mere acts of telling a story, as Wayne Booth (1988, pp. 174–175) notes, is an invitation to bond; "Join me, join me," the storytelling act seems to say to others.

The legitimate empiricist impulses of sociology, however, demand further specification, because all speech acts clearly are not narrative acts. I therefore propose a Simmelian approach that identifies three minimally necessary elements of narrative (McCall, 1985; see also Polkinghorne, 1988, pp. 111–113; Gergen and Gergen, 1988, pp. 20–22; Johnstone, 1990, pp. 20–33). The first element is that events must be selected from the past for purposes of focus and commentary. Second, those events must be transformed into story elements. This is done through the use of plot, setting, and characterization that confer structure, meaning, and context on the events selected. Third, a temporal ordering of events must be created so that questions of how and why events happened can be established and the narrative elements can acquire features of tempo, duration, and pace. These three elements designate narratives as empirical objects which at their core are representations of unobservables in a time/space configuration. It also is a generic conceptualization that is distinguished from related representations (e.g., the chronicle, which possesses the first and third elements but not the second), and it encompasses specific types of narratives such as the personal experience story, the saga, and urban legends (Maines, 1992a).

Of the three elements, it is the second, or emplotment, which is the most fundamental (Polanyi, 1989; Bridger, forthcoming). Stories have a point; they convey a central theme through the use of emplotment. When this element is competently used by a storyteller, the story can become engrossing and even persuasive. Walter Fisher (1987) would say the story contains "narrative probability" (coherence) and "narrative fidelity" (believability) because the teller has used those two properties in a competent manner. It is in this sense that my thinking about narratives collapses narrative and rhetoric (Condit, 1987), and places them inside the social act. That is, narratives are inherently collective processes, they pertain exactly to representations, and they frequently are political.[4]

From these considerations, a number of propositions can be specified on which a narrative sociology might be based (Maines and Ulmer, 1993). These propositions are offered for purposes of imagining the possibilities and range of a narrative sociology, and thus serve as conceptualizations precisely in the sense that Blumer (1931) discussed them. They include the following:

1. Since all socialized humans are storytellers, they are always in a potential storytelling situation when interacting with or encountering others.
2. The vast majority of all speech acts and self-representations contain at least some elements of narratives.
3. Variation in situation, audience, individual perspective, and power/authority relations will produce the universal condition of multiple versions of narrated events.
4. Narratives and narrative occasions are always potential sites of conflict and competition as well as cooperation and consensus.
5. All narratives are potentially rational accounts, but because of inherent human ambiguity and variation in linguistic competence, all narratives are ultimately incomplete.

6. Narratives exist at various levels of scale, ranging from the personal to the institutional to the cultural, they exist for varying lengths of time, and they inevitably change.
7. All social science data are already interpreted data; the uninterpreted datum does not exist.
8. All sociological facts are narrated facts insofar as they have been processed through some form of story structure that renders events as factual.
9. The act of data collection is an act of entering respondents' lives that are partly formed by still unfolding stories. Therefore, in the name of honesty, research subjects will likely tell different stories about the same thing at different times and to different people.
10. A major implication of the above nine propositions is that sociology can only be a science of interpretations and to some extent must constitute itself as an interpretive science.

These propositions naturally flow into sociology's moment, helping us to fix our gaze on its possibilities, and they also flow out of sociology's failures, helping us to overcome them by taking communication seriously. I have indicated the intrinsic social nature of narratives and have provided leads to the first aspect of the duality of focus that a narrative sociology represents. That is, nearly anything a sociologist might want to investigate can be done so from the narrative approach (Maines and Ulmer, 1993), and, further, much of that work already exists in the published literature. The remaining task is to address the second aspect of a narrative sociology, namely, sociologists as narrators, including what they do to and with narratives in the name of professional competence.

I now turn to that task, and, unable to cover all phases of it in this essay, focus most sharply on sociology's standard research practices. I hope to show in this examination how the history of rationalism is a lived, political history, how the univocality initiated in the interwar debates has permeated sociological work, and how, despite themselves, sociologists cannot help being narrators. This latter assertion naturally follows Fisher's (1987) characterization of humans as "homo narrans;" that is, if humans are inherently storytellers, so are sociologists.

Narrative Sociology and Sociological Practice

I wish to enter my material with the question of what happens during the moment when a sociologist seeks information from someone else for sociological purposes. In particular, how does that encounter, as both a social act and as a confrontation, impose limitations and create possibilities? I emphasize the confrontive aspect, not in the sense of any breach of norms of propriety, but in the sense that data collection by its very nature entails the search for information that a respondent may be unwilling or unable to provide. This question, though, raises the logically prior question of what is a person. To address the question of what the sociologist encounters, I thus must depict aspects or phases of personhood as they are relevant to the creation of a narrative sociology.

A person is a self-reflexive organism that by a fairly early age has transformed its raw experiences into abstractions (Mead, 1934). Those transformations entail several complex

processes that are not fully understood and over which considerable debate exists. Nevertheless, I propose that those transformations can be conceptualized in terms of narrative; that is, I propose that the self-abstracted person, so clearly seen in adulthood, is one who has acquired a biography and thereby can tell his or her life story. A person thus is defined as a self-narrating organism (Gergen and Gergen, 1988; Polkinghorne, 1991.)[5]

No doubt there are developmental and maturational processes that are involved. Kemper (1984) and Kuczaj and McClain (1984) have investigated the development of narrative competence in children, and propose a stage theory of gradual development beginning about age three to mastery at about age ten or eleven. That process, according to Kemper, moves from basic tasks such as plot structure to inclusion of background information to enhancing enjoyment to the ability to tell causally sophisticated stories. Mandler (1984; Mandler and Johnson, 1977) argues that the variation possible in story formation is limited by story structure, thus imposing structural limitations on narrative knowing, although others (Stein and Policastro, 1984) have argued otherwise. It nevertheless seems plausible to posit developmental limits and possibilities that, additionally, involve the emergence of memory which accompanies the emergence of selfhood. On that score, Cohler (1982) reviews evidence on what he calls "childhood amnesia." To paraphrase, it appears that changes in self and identity are accompanied by memory loss and gain, and this begins fairly early in life. That is, as we grow older, we not only lose previous "contents" but we acquire new ones, and Cohler argues that to some extent these transformations are developmentally-based. One thing this line of evidence suggests, therefore, is that humans will change their self-narrations (life stories) to some extent whether they want to or not. We will provide different versions of who we are and what we have done at different points in our lives, and we will be completely sincere and honest in telling each version. Inconsistency in self-narration is thus not isomorphic with lying or deception, but rather is an interpretive problematic.[6]

The self-abstracted person also is an organism that has acquired temporality of the self. This means that the person not only lives in temporal orders (clocks, schedules, etc.) but is one who can use time in the construction of action. G.H. Mead (1929; see also Maines, Sugrue and Katovich, 1983) placed temporality inside of social processes, which is a position more recently popularized but not created by Ricoeur (1985), who nonetheless places temporality directly inside narrative. For Mead, time was seen as non-linear, because the person can reconstruct pasts and project futures. Time is an activity that turns back on itself through the intersecting processes of cognition (memory) and sociality (keeping collective pasts alive through language and documents). It thus seems additionally plausible to conceptualize persons as self-narrating organisms who, in the process of becoming self-narrators, acquire temporality (and spatial abilities) and who therefore can abstract themselves into the past and future.

Consistent with this approach, it is important to understand that when sociologists enter the moment of contact with another human to acquire information for sociological purposes, they necessarily encounter and confront an *already interpreted person*. In this sense, all sociological data are *already interpreted data*, which means that sociology can only become a science of interpretation. My characterization of this moment of contact as a confrontation is given additional warrant when cultural and societal elements are considered. Heath (1983, pp. 184–189) shows that variation in community norms produce variation in story telling formats. One community she studied socialized children to tell what members call "true stories," which means that the community enforces a consistent

frame on experiences, and children are rewarded for "getting it right." The other community she studied, however, rewarded innovation, playfulness with stories, and embellishment. Fact is hard to find there, Heath writes, and "there is truth only in the universals of human strength and persistence praised and illustrated in the tale" (1983, p. 186). We need only to consider other contexts besides community, such as media (Carey, 1989) or the family (Stone, 1988; Martin, Hagestad, and Diedrick, 1988), to return to a rephrasing of a central principle of pragmatism, namely that self-narratives cannot be separated from collective narratives (see also Polkinghorne, 1988, pp. 14–15, 107). The moment of data collection is thus not only a confrontation with one who has already narrated him or herself but has narrated society. This position, I suggest, adds further substance to my claim that sociology's data are already interpreted data.

The next issue to be addressed pertains to the formatting and manipulation of data. How much complexity does the sociologist want or need in the information acquired? How much closure should be built into data collection? Clearly some of the answers to these questions depend on analytical purposes. To take an obvious example, if the analytical interests center on questions of population density and migration patterns, then it frequently is sufficient to gather data on the number of people per square mile (or census tract), how long they have lived at their current residence, age, marital status, gender, ethnicity, income, and so forth. These are standard demographic data which are quite useful in describing patterns of consistency and change, although it is noteworthy that these data also portray a person basically as a biological unit possessing fixed (e.g., ethnicity) or predictable (e.g., age) characteristics (Maines, 1978; for an alternative conceptualization, see Park, 1926). Nevertheless, a great deal of closure can be imposed on data collection that ignores their already interpreted quality, and so long as analytic parameters are clear, such analyses provide important information about human societies.

The point I wish to emphasize, however, is that despite such usefulness, the closure of data at the moment of their collection limits possibilities for analysis and creates the necessity for speculation. These kinds of closures and openings, I propose, stem at least from a blindness, or trained incapacity to use Dewey's phrase, to the inherently interpretive nature of sociological data. Let me illustrate.

Sociology has produced literally hundreds if not thousands of scales that can be used to study almost anything, and some researchers have become well known in part because their names become attached to a scale (e.g., the Duncan SES scale or Bem's BSRI scale). These scales simply await their use. In the past decade, there has been a flurry of studies on gender identity, most of which have used scaled data, and a substantial part of the discussion regarding those studies has centered on the adequacy of the scales and the extent to which data are methodological artifacts (Gill, Stockard, Johnson and Williams, 1987). From the standpoint of my interests, though, all of them impose closure by denying the research subjects as biographically-embedded, self-narrating persons. Consider the scale items below I have selected from a questionnaire recently used to investigate gender identity.

not at all competitive	1	2	3	4	5	6	7	8	very competitive
gives up easily	1	2	3	4	5	6	7	8	never gives up easily

not at all understanding of others	1	2	3	4	5	6	7	8	very understanding of others

The data generated from these scales will be only quantitative and will pertain to self-attributions. The analyst will look to see which items correlate with one another and which ones do not, and, depending on the particular statistics used, the data will be presented in the form of central tendencies, dispersions, or regression curves.

Not to unnecessarily probe, but it is interesting to ask what respondents must do in order to choose a number (1 through 8). One thing they can do is very little; simply go down the page and circle numbers, whether they apply or not. Assuming the task is taken seriously, though, the respondent must enter into a dialogue with his or her biography and search for situations to which the items apply. These self-dialogues typically involve considerable variation (e.g., I was competitive with some people but not others; on some tasks but not others; only when I am forced to be so, and so on). Or, their self-dialogue might reveal irrelevancies; for example, a respondent may not conceive of him or herself in terms of giving up—that item is not an aspect of the identity. The researcher, however, has absolutely no information about these self-dialogues and thus no information on what the numbers mean. Put another way, a "7" is not always a "7", as Chapin argued long ago, but instead is much like a word insofar as it is only a representation of something else that itself is only an interpretation. Hidden behind the scale items and their numbers are narrative realities that the respondent knows in one form or another but that the researcher has been trained to ignore. Perhaps ironically, those narrative realities are quite accessible. Say, for example, a respondent circles a "2", indicating little understanding of others. All the researcher has to say to generate narrative representations is "Tell me about that" (Mischler, 1986). The answer will be a story about the respondent, and that story will provide information about the situations, relationships, and self-conceptions which would then add narrative meaning to the "2". Unfortunately, however, that simple act of recognizing a person as a self-narrator rarely happens.

The scales are used because they are thought to increase precision and scientific rigor. I am not alone (see Cicourel, 1964) in pointing out, however, that the opposite can occur. In imposing closure, that is, in building data reduction into the data collection process, the researcher is put in the position of having to speculate about the phenomena being studied. Gender identity, to stay with our example, is not something made up of the scale items, but is a set of relations centering on historically-embedded processes of public and personal politics.[7] To one degree or another, everyone knows that, although not all would phrase it that way. And, as a self-narrating person, the researcher, whether man or woman, knows it, but as a researcher, that knowledge is denied. What I am driving at is that the historically-embedded ideologies justifying the production and use of research instruments thought to be neutral and the norm of researcher detachment, both of which point to the paradox of non-intervention through intervention (i.e., research), place the researcher in the position of having to speculate about the phenomena simply because the phenomena themselves were not directly studied.

I assume the conclusion I have drawn is easy enough to see, but to press my point about speculation to a more dramatic moment, I wish to comment on an influential article on data analysis by Clifford Clogg (1982). The problem Clogg tried to solve is a chronic one

in survey research pertaining to unequal proportions of missing data to survey questions because of differences in question wording. Specifically, what can be done when, say, 7 percent of the respondents of one sample but 29 percent of another sample indicate "don't know" to a question? Clogg's solution is sophisticated and statistical, and is based on the contention that a "don't know" response represents a *real attitude*. Based on that contention, he outlines a series of steps drawn from assumptions necessary for the statistics used (e.g., attitudes can be represented on a continuum by four equidistant points) in which the "don't know" responses are translated into other responses. What his procedure does, in effect, is to statistically *create* information that better satisfies the statistical assumptions necessary for the comparison of samples. If we agree with Clogg's initial assumption that "don't know" responses represent real attitudes, however, there are two possible interpretations of his procedures: either he has only solved a statistical problem and has offered a procedure that masks pure speculation (i.e., he has no empirical data, only the procedure), or he has decided that the respondents' information (i.e., "don't know" responses are real attitudes) should be changed by researchers because they create data analytic problems. Either way, the speculative nature of this sort of sociological work in the name of precision and empirical science is abundantly clear.

It seems justified to assert that the delegitimation of narrative data beginning in the 1930's and continuing today has resulted in sociology engaging in substantial speculation of the sort just described. A narrative sociology would minimize such speculation by respecting the complexity of human relations and group life. That respect would begin with the recognition of multiple realities that are rendered meaningful in personal and collective narratives. By locating data reduction *after* instead of *during* data collection, however, the researcher will be faced with an abundance of detail which typically contains contradictions (Plummer, 1983). The virtue of this approach is that the researcher has access to the contradictions and thereby is on firmer empirical grounds than without them, but it increases difficulties in drawing conclusions across cases. Questions of reliability and validity thus remain as important ones for a narrative sociology that embraces generalizing strategies. I therefore turn to those issues in search of a conceptualization of reliability and validity that is grounded in the narrative character of human living.

My question here pertains to the limits and possibilities stemming from the act of studying self-narrating organisms for purposes of establishing credible representations in the form of sociological analysis. I understand issues of reliability and validity to pertain exactly to matters of credibility of sociological accounts. The problem is that while sociology has developed technical conventions for estimating reliability and validity, it has not grappled with the fundamental fact that these two important elements of research are located inside of ideological, political, administrative, and technical processes that render them as social objects that we then call "reliability" and "validity." That is, these are not objects that do things, such as merely tell us when to have confidence in research results, but instead are social productions themselves that are the consequences of human action (Mischler, 1990). Put another way, the confidence that reliability and validity scores tell us to have in research results rests on the prior confidence that researchers have in such scores, *per se*, as well as in the means of producing them. To understand reliability and validity, we therefore must look to human activity rather than to the technical procedures for producing scores.

The customary distinction between validity and reliability is that the former pertains to truth statements about the empirical world while the latter pertains to the degree of

consistency in results produced by data gathering instruments. Validity thus addresses the question of the correspondence between sociological representations and those events represented, and reliability addresses the question of whether the method will produce the same results again. The relations between the two are asymmetric; measures may be consistently in error, and therefore high reliability can be achieved anywhere between the poles of absolute validity and absolute invalidity (Deutscher, 1973, p. 106). These possibilities have posed chronic and worrisome problems in social research, and researchers have typically sought to solve those problems by distinguishing different types of validity and reliability (e.g., external validity, internal validity, face validity, etc.) and by looking to technical improvements in the measures and instruments. The underlying agenda to these attempts has been to search for certainty, which is the search contained in the legacy of the logic of positivism and natural law. Although technical improvements have increased confidence in research results, however, the quest for certainty has been a dismal failure. Part of the reason, as others have argued (Blumer, 1931; 1940; Gergen, 1976; Cicourel, 1964; Deutscher, 1973), is that we are dealing with an unstable empirical world that will not yield uniformities despite the adequacy of our measures. My conceptualization will build off that line of thinking to consider additionally the nature of sociological concepts, persons as self-narrators, and the recent work of narrative scholars such as Jerome Bruner (1986) and Polkinghorne (1988).

My position rests on the contention that the "empirical world"—the world of objective facts and doings—may well exist but that it cannot be directly known. What we can know is solely a function of human interventions, mediations, and constructions. This idea, of course, is an old one that leads us to think of issues of validity as involving comparisons of multiple constructed realities and issues of reliability as pertaining to the consistency of accounts. The first question to be faced thus pertains to the nature of the mediation between the person and the environment. Dewey (1986; 1926) and Mead (1934) pointed to perception, language, and coordinated action as the primary mediators. These create adjustive relationships between the person and the environment, allowing humans to solve problems and create social contexts not previously existing. This is the world of commonsense concepts, which are simultaneously based on and influenced by perception, and as practical accomplishments, they contain numerous taken-for-granted meanings, ambiguities, and anomalies that are accepted as normal and usual. This everyday life world is where persons as narrators exist and from whence they come. This mediated existence, according to the analysis of Edward Bruner (1986) applies even to personal, subjective experiences, since, as G.H. Mead argued much earlier, our self-referential experiences are known to ourselves largely through social conventions and their variations.

Given that the empirical referents of sociological study consist of interpretations, mediated conduct, and commensense conceptions, the next question pertains to what sociological concepts can accomplish in the production of valid and reliable accounts. This question is important because concepts are the basic tools used for theory construction, and they represent the categories of knowledge toward which assessments of validity and reliability are directed. Ambiguity in concepts, it is generally conceded, results in ambiguity in theory and knowledge, with their attendant problems of reliability and validity.

Herbert Blumer (1931; 1940) was one of the first to systematically study the concept. His position was that the ideal is the development of definitive concepts, or those which are inductively built up to the point that they refer to a common class of empirical

instances. If these kinds of concepts can be constructed, there will be clarity insofar as they always refer to the same things; sociological verbalizations (concepts) will match the empirical world of human conduct. The only procedure that has come close to this goal, Blumer argued, is operational definitions, or those concepts based on the consistent use of definitions of phenomena that are a direct function of measuring instruments. Those definitions produce standardized results. However, they have no content. That is, concepts of this sort do not refer to a class of phenomena because they are tied directly to measuring instruments rather than grounded in phenomena.

Given the instabilities and variations in the social world, Blumer concluded that the best (not the ideal) sociologists could do is to construct sensitizing concepts. These would direct researchers toward different classes of phenomena, but could not be isomorphic with them. However, he got closer to the hermeneutic nature of sociological research in his analysis of the nature of sociological observation.

> It may be argued that the designation of an act as being respectful, hateful, aggressive, etc., is actually an inference and so is not properly a part of the observation. That it is an inference is, I think, unquestionable, but it is an inference that is fused immediately into the observation itself. This is true of every act of observation; even the observation or designation of a physical act is in the nature of a judgment or an inference (Blumer, 1940, p. 715).

Observation and interpretation, Blumer is saying, are part and parcel of the same thing.[8] This condition creates difficulties for estimating validity for a number of reasons, including the fact that such inferences and interpretations make their way inside measures themselves.

These sorts of considerations, it seems, call for alternative conceptualizations. If observations of the empirical are partly made up of interpretations, if the empirical is itself interpretive, if concepts cannot be definitive, if commensense concepts fuse with sociological concepts, and if technical procedures for assessing validity and reliability are partly ideological accomplishments, then what is the nature of sociological knowledge and knowing?

Jerome Bruner suggests an answer with his distinction between two modes of thought (1986, Ch. 2). One mode he calls paradigmatic and logico-scientific. It is concerned with verifiable truth, and uses formal rules of categorization and instantiation to produce consistent and noncontradictory statements, initially in the form of testable hypotheses, in pursuit of dependable knowledge. In contrast, the narrative mode is concerned with verisimilitude, or probability. It consists of believable stories or accounts and focuses on action, agency, and consciousness which are processed by a story structure. Of the two, Bruner states, much less is known about the narrative mode. On that score, I would agree, but I also take partial exception to Bruner's distinction on the grounds that both modes are interpretive processes which differ only in how those processes are formatted (Altheide, 1988). If my previous discussion of the philosophy of rationalism, the ideological and political discussion of the philosophy of rationalism, the ideological and political economic nature of sociology's conception of itself as a science, and my ontological characterization of persons and the empirical world as inherently interpretive have any credence, then it must be concluded that Bruner's paradigmatic mode of thought is no less interpretive

than his narrative mode. As such, we are forced to recognize the narrative character of paradigmatic thought as well as narrative thought.[9]

This view suggests that whether an account is regarded as valid is a function of the social contexts and conventions that the members of those contexts use to construct validity as a criterion for truth claims (Heath, 1983). Note that I do not say whether an account is valid, but whether it is *regarded* as valid, a distinction clearly in the debt of Thomas and Znaniecki's concept of the definition of the situation. Polkinghorne gets close to this dimension of context by arguing that narrative research uses the ideal of a scholarly consensus as the test of verisimilitude" (1988, p. 176). While he is correct, this is the character of validity tests in any setting. That is, a community of *any* sort sets its criteria for validity claims and then uses those criteria for establishing validity. It matters little whether the community is composed of sociologists, auto mechanics, artists, drug dealers, or college undergraduates, except that the criteria and distribution of events will vary.

Before moving to the next issue of narrative sociology, I provide concrete example of research that illustrates this narrative conception to validity as well as touching on my previous discussions of sociology's distrust of the human utterance. I choose Latané and Darley's (1968) classic study of bystander effects in which they investigated the extent to which group contexts influence whether people will intervene to help others in emergency situations. In their design, they set up three experimental situations: (1) the research subject was alone in a room, (2) a research subject in a room with two confederates instructed to display passive responses, and (3) three strangers in a room, all of whom were subjects. The researchers forced smoke into the room in each situation to simulate fire—the emergency—and their data took the form of observing whether the experimental subject reported the smoke. They found that 75 percent of the subjects in the alone condition reported the smoke, while only 10 percent did so when in the presence of the two passive confederates. On the basis of these results, Latané and Darley concluded that the group context influenced subjects not to intervene.

While the idea of group effects is a standard and credible one in sociological research, it is interesting to note how the researchers interpreted the post-experimental interviews, in which they asked the subjects what they thought happened. They report the following aspects of subjects' narratives.

> . . . each subject gave an account of what had gone through his mind during the smoke infusion . . . Subjects who had reported the smoke were relatively consistent in later describing their reactions to it. They thought the smoke looked somewhat "strange" . . . [However] subjects who had not reported the smoke also were unsure about exactly what it was, but *they uniformly said that they had rejected the idea that it was a fire*. Instead, they hit upon an astonishing variety of alternative explanations, all sharing the common characteristics of interpreting the smoke as a non-dangerous event (1968, p. 219) (emphasis added).

In the face of these verbal accounts, Latané and Darley offer the following.

> Despite the obvious and powerful report-inhibiting effect of other bystanders, subjects almost invariably claimed that they had paid little or no attention to the reactions of other people in the room. *Although the presence of other people actually had a strong*

> *and pervasive effect on the subjects' reactions, they were either unaware of this or unwilling to admit it* (1968, p. 220) (emphasis added)

The situation before Latané and Darley is a fairly typical one in social research—a conflict between behavioral results and research subjects' accounts of their conduct—and, I might add, a conflict that has yet to be resolved, although triangulation might hold some promise (Denzin, 1989, Ch. 10). Nonetheless, I note the following features of this case. First, Latané and Darley discounted the subjects' narratives. Second, those narratives pertained to subjects' definitions of their own situations and what did or did not influence them. Third, the researcher's central hypothesis was that group situation would reduce intervention. Fourth, their data only showed outcomes; no data were produced regarding the influence process, although they were in the position to make inferences. From these features of the study, I conclude that their vested interest in confirming their hypothesis (see Mahoney, 1976) led them to discount subjects' narratives, which paradoxically were their only source of information regarding the influence process, and to offer in their place a theory (a form of narrative) of group effects that contained the property of believability by virtue of its correspondence with their previous studies. Stated another way, sociological distrust of ordinary narrative accounts served as a community criterion that Latané and Darley implicitly used to construct the element of believability that became embedded in their alternative narrative that they proffered as a valid account.

This illustration from the research literature brings me to the final issue relevant to narrative sociology. This issue conceives of sociologists as storytellers who use a practical, scientific rhetoric. I will only touch on this feature, since others such as Simons (1978), Edmondson (1984), McCloskey (1985), Brown (1987), Gusfield (1976), and Richardson (1988) already have established that scientific work is not only a technical but rhetorical accomplishment. However, I do wish to make a few comments and then present some information regarding the interpretive character of statistical work.

As others have argued, statistical displays are formatted presentations to a community that, understanding the format, defines the display as legitimate information. I regard these displays as types of narrative events that are rendered meaningful by rhetorical devices used to construct them. Although Gephart (1988) and others have identified some of these devices, most of which allow the researcher to move between parts of discursive analysis, I wish to focus briefly on the relation between data presentation and data interpretation in order to emphasize narrativity.

I have selected more or less at random two adjacent articles from the *American Sociological Review*, both of which use sophisticated statistical procedures for their data analysis. Each uses a standard device, which I call "let's pretend," that is, the suspension of disbelief. Bailey and Peterson (1989) studied the relation between murder and capital punishment, and presented their information in eight pages of data and thirteen pages of text. McAdam (1989) studied the individual consequences of social and political activism, and presented his information in three pages of data and thirteen pages of text. Although the proportions of text to data are slightly different, it is evident that an enormous amount of research information has been displayed for readers. My point will be that in order to handle those data interpretively, the authors have in effect asked readers to pay attention only to what they narratively highlight.[10]

Bailey and Peterson, for example, present a table (Table 2, p. 731) containing eighty-four pieces of data information (a difference of means test with twenty-eight rows and

three columns), and then in their findings section write, "The average rate of homicide does not decline significantly during high execution publicity months (month *t*) or the months that follow. In short, the comparative analysis provides no reason to question the conclusion that capital punishment and homicide are unrelated" (1989, p. 729). What they tell us is that there is no relationship between homicide rates and execution publicity, but in telling that to their readers, they used the device of glossing. They displayed all their data, in other words, but did not interpret them all. Words and phrases such as "average," "in short," "no reason to question," and "significantly," none of which have any scientific properties whatsoever, were used to induce readers to share in that gloss by pretending that there were no other interpretations possible. With the device of glossing, the authors were able to create a piece of their narrative and then move along in their analysis.

McAdam uses a different device, which is similar to how anecdotes are used in stories. This device is most clearly shown with reference to Table 7 (p. 758) which contains logit regression on sixteen variables predicting marital status. The table shows that one variable, "participation in freedom summer (yes/no)" is statistically significant. McAdam thus writes, "Significantly, participation in Freedom Summer is clearly the best predictor of the subject's marital status. Indeed, it overwhelms all the other variables in the model" (1989, p. 757). While these two sentences are correct in the technical sense, they nonetheless contain a narrative function. That function is given at least in terms such as "clearly," "best predictor," and "overwhelms," again none of which are scientific concepts. They induce the reader to ignore questions about the other variables and whether the model itself was adequate by focusing attention on only one factor. That factor—the predictor variable—thus acquires an anecdotal quality insofar as it stands for the overall process of influence, and readers are implicitly asked to pretend that the statistically significant factor is the only important one.

Gephart (1988, pp. 59–60) shows some very interesting findings along these lines in his comparisons between statistical values and words used to refer to those values. He compared a range of articles using parametric statistics to study their rhetorical aspects. His procedure was to categorize regression coefficient values that were similar (e.g., values between .7 and .9) and compare them to their corresponding written interpretations (e.g.,"high" correlation). I quote the summary of his findings.

> . . . it became clear that the general practice for interpreting quantitative results is to link adjectives and adverbs to numbers to thereby transform the numbers into meaningful results. For example, PS [parametric statistics] papers used adjectives such as small, slight, substantial, high, and/or stable to describe values. These adjectives were often coupled to adverbs, for greater precision, for example, "unusually or highly stable." Relations between values were described in terms of fairly good agreement, essentially no difference, and differences that were small . . . *Terms that are similar were observed to be applied to different numeric values and similar values were linked to different descriptive terms*. Indeed, the value one author considered "substantial" was often the same or less than 0.1 discrepant from what another author labeled "relatively small" (1988, p. 60; emphasis added).

What Gephart's findings show, of course, is that scientific meanings are not inherent in numbers or statistical values. Rather, meanings must be imputed to those values by using words which are necessary to interpret them for a community of readers. However, there is no necessary correspondence between the numbers and words. Stated another way,

"high" or "significant" are not always statistically "high" or "significant," as Chapin would have predicted, thus revealing the inevitable measure of ambiguity that comes with the equally inevitable narrativity and interpretive work in sociological analysis.

CONCLUSIONS

I have argued in this essay that narrative's moment exists in an array of conditions and practices that constitute the possibilities for the development of a narrative sociology. That development as I envision it would occur minimally along two interrelated lines, or what I have called the duality of focus. First, it would recognize that a substantial portion of sociology's phenomena is made up precisely of stories. As such, any of the standard interests claimed as central to sociology can be approached through a narrative ontology. These include socialization processes, organizational structure and functioning, power and authority relations, demographic migration, international relations, gender and age relations, urban political economies, and so forth. Contrary to the suspicions that some might voice, this focus would not contain an inherent "micro" bias, because, as I have articulated, narratives are intrinsically collective acts and exist at any level of scale.

Narrative sociology's other focus would be on sociologists as narrators. This focus would see sociologists as spinners of professional tales that we call theories (Davis, 1974), as practitioners who are skilled at arranging narrative elements into what we call journal articles and research reports (McCloskey, 1990), as people who are perhaps unknowingly caught up in metanarratives that we call the culture of western rationalism (Brown, 1987), and as everyday life folklorists who can tell various stories of their group they call the discipline of sociology (Merton, 1980).[11] I have pushed this second aspect of narrative sociology the hardest and have purposefully focused on issues typically associated with quantitative analysis. My assumption has been that if some of the least obvious sociological practices can be shown to inherently involve narrative practices, then it is easier to imagine the narrative possibilities in other phases of sociological work.

As a construct, "narrative's moment" objectifies the fact that the social is grounded in communication processes, that communication itself is social, and that all sociological work is communicative work. The disciplinary, and to some extent disciplined, neglect of these simple facts perhaps stems from their being so obvious that as a field sociology passes right over them. One of the great benefits of focusing on narratives is that they are one form of communication, and by conceptualizing sociology narratively, we are forced to take communication very seriously. This includes not only writing and teaching—activities that we often deligitimize as mere issues of education—but research, theory construction, writing grants, consulting, administration, and, most importantly, sociology's phenomena. Accordingly, a narrative sociology would create a new sociological consciousness that raises new questions and revives old ones. What kinds of narratives should we study? Which ones should we tell? How can we establish narrative probability and fidelity? These three questions alone cut deeply into the moral, political, ideological, pedagogical, and organizational practices of sociology. Importantly, their answers require the interdisciplinary perspective and resources that a narrative approach represents, thereby challenging head-on sociology's myth of the mainstream. Out of that challenge can come a dialogue that promotes greater generosity and breadth of inquiry which is essential for the honest assessment how a non- narrative and narrative sociology can co-exist.

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NOTES

1. These are only a few of the signs of the increasing acceptance of and opportunities for narrative scholarship and research in sociology. Others include the recent creation of the *Journal of Narrative and Life History* (Volume 1, 1991); the vibrance of Research Committee 38 (Biography and Society) of the International Sociological Association; the inclusion of several sessions on storytelling in the 1992 Program of the Southern Sociological Society, *the 1992 Program of the American Sociological Association, and the 1993 Program of the Midwest Sociological Society*; the establishment of the New England Symposia on Narrative Studies in the Social Sciences; the inclusion of a half-dozen sessions on narrative in the 1992 *Theory, Culture and Society* Conference; the development of research institutes such as the Institute for Interpretive Human Studies at the University of South Florida that merges sociology and communication; the creation of two new European journals, *Cultural Dynamics* (Belgium) and *Time and Society* (UK), which promote interpretive social science; and the post-Mertonian sociology of science (see Fujimura, 1991) which emphasizes interpretive and narrative processes in the scientific production of knowledge. My point in listing these is that the conditions defining what I am calling "narrative's moment" are rather broad and cut across many of the organizational, administrative, and intellectual strands that constitute sociology and social science (for additional materials,see Plummer, 1990; Maines, 1992a; Abbott, 1992; and McCall and Wittner, 1990).

2. It goes without saying that many if not most sociologists, such as Alexander (1987), would disagree with the label "disciplinary failures." As Wiley (1987) points out, however, critical commentary along these lines has been fairly common in sociology. For some of the flavor of recent criticism, see Coser (1975), Smith (1974), Wardell and Turner (1986), Denzin (1986), and the exchanges between Neil Smelser, James Beckford, Norbert Wiley, Stephen Turner, and David Maines in *The American Sociologist* 21(Fall), 1990 on the future of sociological theory.

3. I traced out in some detail the methodological debates stimulated by Thomas and Znaniecki's research in the longer version of this essay, but have had to delete that material in this version. See Maines (1990).

4. An interesting illustration of all these elements can be found in the comparison of Murray's "Losing Ground" and Jencks' "How Poor are the Poor?" (Finsterbusch and McKenna, 1990, pp. 208–226). At issue is the assessment of whether welfare programs harm or benefit program recipients. Each author examines the same events (e.g., income, marital status) pertaining to a hypothetical couple named Harold and Phyllis occurring in the same time frame (these correspond to my first and third narrative elements), but arrive at drastically different conclusions about Harold and Phyllis' actions (e.g., how many children she will have; whether he is motivated to get a job). These

different conclusions are precisely different emplotments. In addition to showing the centrality of emplotment and meaning, this case also suggests the following: (1) some story themes are encased in larger ideological metanarratives (conservative and liberal, in this instance), (2) narratives are part and parcel of large scale political economic processes, (3) persuasiveness is not only a function of story-telling competence but derives as well from inducing ideological and cultural contexts, and (4) social scientific analyses are merely one form and instantiation of narrative, a point I will develop later in this essay.

5. See Bochner and Ellis (1992) for an interesting empirical description of how narrative is at the heart of the process in which a dyad moves from a condition of two subjectivities to one of intersubjectivity. This process is of central significance in sociological inquiry insofar as it pertains directly to the processual and dynamic nature of the social bond.

6. Such inconsistency bears directly on matters critical to attitude-behavior research and theory. Pestello and Pestello (1991) appropriately emphasize the evaluative and situational dimensions of these interpretive problematics. I would offer the proposition that behind every attitude lies a story waiting to be told in its support, which is a proposition that is consistent with the Pestellos' argument.

7. Note that the discussion here does not pertain to self-concept but to identity, which, following Stone (1962), is a social location. That is, it takes at least two people to establish an identity, which, once established, locates the person in a social structure. By that token, identity is inherently historical, public, and political. Berenice Fisher (1991, pp. 93–96) understands this point and has applied it to the analysis of gender identity, as has the Personal Narratives Group in their edited volume, *Interpreting Women's Lives: Feminist Theory and Personal Narrative* (1989).

8. As an aside, although not irrelevant to points made in this essay, there is some happy evidence that sociology may be catching up with Blumer. In the face of a persistent criticism of his analyses of concepts over the years, it is interesting that recent issues of *Perspectives: The Theory Section Newsletter* (14(2/3), 1991), which is the official newsletter of the American Sociological Association's theory section, contained several commentaries on formalism in sociological concepts, and nearly all of them in one way or another adopted the positions that Blumer articulated a half century ago. Displaying a kind of collective amnesia, so common in scholarship, of course, none of the authors thought to mention Blumer's analyses.

9. This is only a minor quibble with Bruner's distinction, and I suspect that he actually would agree with me. "In the end, then," he writes, "the narrative and paradigmatic come to live side by side" (1986, p. 43).

10. Actually, I would assert categorically that all researchers using any research strategy employ the "let's pretend" device, not merely those using quantitative approaches. Field workers, however, tend to be more explicit in acknowledging themselves as a research tool (Johnson, 1975; Maines, 1992b), and therefore certain types of reactivity and other interpretive processes are easier to assess with field research approaches. Relatedly, it is easier to visualize and articulate the narrative character of field research, as Van Maanen (1988), Young (1991), Mischler (1991), and Richardson (1990) show. The most obvious reason for this is that field data are typically displayed in a narrative form, but perhaps less obvious is that field researchers are probably more prone to see themselves as narrators (Davis, 1974) than are the traditional quantitative analysts, especially those working with large secondary data sets. Abbott (1992), however, has been developing a very interesting approach that he calls "narrative positivism" that is compatible with process-oriented methods such as time-series analysis and event history analysis. I suspect the compatibilities at a general level are broader than Abbott suggests, to include recent developments in life history approaches (Maines, 1992a).

11. I understand that I have characterized theorizing and knowledge production as narrative activities in a somewhat casual manner. While I feel I am substantially correct, however, at least for purposes of stimulating dialogue, see Robert Antonio (1991) for an intelligent discussion that makes some very useful distinctions about narrative and truth.

REFERENCES

Abbott, A. 1992. "From Causes to Events: Notes on Narrative Positivism". *Sociological Methods and Research* 20: 428–455.

Alexander, J. 1987. *Twenty Lectures*. NY: Columbia University Press.

Altheide, D. 1988. "Computer Formats and Bureaucratic Structures". Pp. 215–230 in *Communication and Social Structure*, edited by D. Maines and C. Couch, Springfield, IL: Charles C. Thomas.

Antonio, R. 1991. "Postmodern Storytelling Versus Pragmatic Truth-Seeking: The Discursive Bases of Social Theory". *Sociological Theory* 9: 154–163.

Bailey, W. and R. Peterson. 1989. "Murder and Capital Punishment: A Monthly Time-Series Analysis of Execution Publicity". *American Sociological Review* 54: 722–743.

Bain, R. 1935. "Measurement in Sociology". *American Journal of Sociology*. Chapel Hill: University of North Carolina Press.

Bannister, R. 1987. *Sociology and Scientism: The Quest for Objectivity, 1880–1940*. Chapel Hill: University of North Carolina Press.

Bentz, V. M. 1989. *Becoming Mature: Childhood Ghosts and Spirits in Adult Life*. Hawthorne, NY: Aldine de Gruyter.

Blumer, H. 1931. "Science Without Concepts". *American Journal of Sociology* 36: 515–533.

———. 1939. *An Appraisal of Thomas and Znaniecki's The Polish Peasant in Europe and America*. NY: Social Science Research Council.

———. 1940. "The Problem of the Concept on Social Psychology". *American Journal of Sociology* 45: 707–719.

Bochner, A. and C. Ellis. 1992. "Personal Narrative as a Social Approach to Interpersonal Communication". *Communication Theory* 2: 165–172.

Boje, D. 1991. "The Storytelling Organization: A Study of Story Performance in an Office-Supply Firm". *Administrative Science Quarterly* 36: 106–126.

Booth, W. 1988. *The Company We Keep*. Berkeley: University of California Press.

Bridger, J. Forthcoming. "Time and Narrative" in *Encyclopedia of Time*, edited by S. L. Macey. Hamden, CT: Garland Press.

Brown, R. H. 1987. *Society as Text*. Chicago: University of Chicago Press.

Bruner, J. 1986. *Actual Minds, Possible Worlds*. Cambridge, MA: Harvard University Press.

Carey, J. 1989. *Communication as Culture*. Boston: Unwin Hyman.

Carr, D. 1986. *Time, Narrative, and History*. Bloomington, IN: Indiana University Press.

Chapin, F. S. 1935. "Measurement in Sociology". *American Journal of Sociology* 40: 476–480.

Cicourel, A. 1964. *Method and Measurement in Sociology*. NY: Free Press.

Clinard, M. 1966. "The Sociologist's Quest for Respectability". *The Sociological Quarterly* 7: 399–412.

Clogg, C. 1982. "Using Association Models in Sociological Research: Some Examples". *American Journal of Sociology* 88: 114–134.

Cochrane, T. 1987. "Place, People, and Folklore: An Isle Royale Case Study". *Western Folklore* 46: 1–20.

Cohler, B. 1982. "Personal Narrative and Life Course". Pp. 205–241 in *Life Span Development and Behavior* (Vol. 4), edited by P. Baltes and O. Brim. NY: Academic Press.

Collins, R. 1984. "Statistics Versus Words". Pp. 329–362 in *Sociological Theory*, edited by R. Collins. San Francisco: Jossey-Bass.

Condit, C. 1987. "Democracy and Civil Rights: The Universalizing Influence of Public Argumentation". *Communication Monographs* 54: 1–18.

Coser, L. 1975. "Presidential Address: Two Methods in Search of a Substance". *American Sociological Review* 40: 691–700.

Davis, F. 1974. "Stories and Sociology". *Urban Life and Culture* 3: 310–316.

Denzin, N. 1986. "Postmodern Social Theory". *Sociological Theory* 4: 194–204.
———. 1988. "The Alcoholic Self: Communication, Ritual, and Identity Transformation". Pp. 59–74 in *Communication and Social Structure*, edited by D. Maines and C. Couch, Springfield, IL: Charles C. Thomas.
———. 1989. *The Research Act*. Englewood Cliffs, NJ: Prentice-Hall.
———. 1990. "Presidential Address on the Sociological Imagination Revisted". *The Sociological Quarterly* 31: 1–22.
Deutscher, I. 1973. *What We Say/What We Do*. Glenview, IL: Scott-Foresman.
Dewey, J. 1896. "The Reflex Arc Concept in Psychology". *The Psychological Review* 3: 363–370.
———. 1926. *Experience and Nature*. NY: Dover.
Dolby-Stahl, S. 1989. *Literary Folkloristics and the Personal Narrative*. Bloomington: Indiana University Press.
Eder, D. 1988. "Building Cohesion Through Collaborative Narration". *Social Psychology Quarterly* 51: 225–235.
Edmondson, R. 1984. *Rhetoric in Sociology*. London: MacMillan.
Faris, E. 1928. "Attitudes and Behavior". *American Journal of Sociology* 33: 271–281.
Finsterbusch, K. and G. McKenna (eds.). 1990. *Taking Sides: Clashing Views on Controversial Social Issues*, 6th ed. Guilford, CT: Dushkin.
Fisher, B. 1991. "Affirming Social Value: Women Without Children". Pp. 87–104 in *Social Organization and Social Process: Essays in Honor of Anselm Strauss*, edited by D. Maines. Hawthorne, NY: Aldine de Gruyter.
Fisher, W. 1987. *Human Communication as Narration*. Columbia, SC: South Carolina University Press.
Fujimura, J. 1991. "On Methods, Ontologies, and Representation in Sociology of Science: Where do we Stand?" Pp. 207–248 in *Social Organization and Social Process: Essays in Honor of Anselm Strauss*, edited by D. Maines. Hawthorne, NY: Aldine de Gruyter.
Gephart, R. 1988. *Ethnostatistics*. Newbury Park, CA: Sage.
Gergen, K. 1976. "Social Psychology, Science, and History". *Personality and Social Psychology Bulletin* 2: 373–383.
———. and M. Gergen. 1988. "Narrative and the Self as Relationship". *Advances in Experimental Social Psychology* 21: 17–55.
Giddens, A. 1984. *The Constitution of Society*. Berkeley: University of California Press.
Gill, S., J. Stockard, M. Johnson, and S. Williams. 1987. "Measuring Gender Differences: The Expressive Dimension and Critique of Androgyny Scales". *Sex Roles* 17: 375–401.
Gusfield, J. 1976. "The Literary Rhetoric of Science: Comedy and Pathos in Drinking Driver Research". *American Sociological Review* 40: 16–34.
Heath, S. 1983. *Ways With Words*. Cambridge: Cambridge University Press.
House, F. 1934. "Measurement in Sociology". *American Journal of Sociology* 40: 1–11.
Howard, G. 1991. "Culture Tales: A Narrative Approach to Thinking, Cross-Cultural Psychology, and Psychotherapy". *American Psychologist* 46: 187–197.
Johnson, J. 1975. *Doing Field Research*. NY: Free Press.
Johnstone, B. 1990. *Stories, Community, and Place*. Bloomington: Indiana University Press.
Kemper, S. 1984. "The Development of Narrative Skills: Explanations and Entertainments". Pp. 99–124 in *Discourse Development: Progress in Cognitive Development Research*, edited by S. Kuczaj. NY: Springer-Verlag.
Krieger, M. 1981. *Advice and Planning*. Philadelphia: Temple University Press.
Kuczaj, S. and L. McClain, 1984. "Of Hawks and Moozes: The Fantasy Narratives Produced by a Young Child". Pp. 125–146 in *Discourse Development: Progress in Cognitive Development Research*, edited by S. Kuczaj. NY: Springer-Verlag.
Latané, B. and J. Darley. 1968. "Group Inhibition of Bystander Intervention in Emergencies". *Journal of Personality and Social Psychology* 10: 215- 221.
Lieberson, S. 1985. *Making it Count*. Berkeley: University of California Press.

Levine, D. 1985. *Flight From Ambiguity*. Chicago: University of Chicago Press.
Lofland, L. 1990. "History, The City, and the Interactionist: Anselm Strauss, City Imagery, and Urban Sociology". *Symbolic Interaction* 14: 205–223.
Maines, D. 1978. "Bodies and Selves: Notes on a Fundamental Dilemma in Demography". Pp. 241–265 in *Studies in Symbolic Interaction*, edited by N.K. Denzin. Greenwich, CT: JAI.
———. 1990. "On Narrative and Sociology's Phenomena." Paper presented at the Narrative of the Human Sciences Conference, University of Iowa.
———. 1991. "The Storied Nature of Health and Diabetic Self-Help Groups". Pp. 185–202 in *Advances in Medical Sociology*, edited by G. Albrecht and J. Levy. Greenwich, CT: JAI.
———. 1992a. "Life History". Pp. 1134–1138 in *Encyclopedia of Sociology*, edited by E. Borgatta and M. Borgatta. NY: Macmillan.
———. 1992b. "Theorizing Movement in an Urban Transportation System by Use of the Constant Comparative Method in Field Research". *The Social Science Journal* 29: 283–292.
——— and C. Couch. 1988. "On the Indispensibility of Communication for Understanding Social Relationships and Social Structure." Pp. 3–18 in *Communication and Social Structure*, edited by D. Maines and C. Couch. Springfield, IL: Charles C. Thomas.
——— and J. Bridger. 1992. "Narratives, Community, and Land Use Decisions". *The Social Science Journal* 29: 363–380.
——— and J. Ulmer, 1993. "The Relevance of Narrative for Interactionist Thought" in *Studies in Symbolic Interaction*, edited by N. K. Denzin,in press. Greenwich, CT: JAI Press.
———, N. Sugrue, and M. Katovich. 1983. "The Sociological Import of G. H. Mead's Theory of the Past." *American Sociological Review* 48: 151–173.
Mandler, J. 1984. *Stories, Scripts, and Scenes: Aspects of Schemata Theory*. Hillsdale, NJ: Lawrence Erlbaum.
——— and N. Johnson. 1977. "Rememberance of Things Parsed: Story Structure and Recall". *Cognitive Psychology* 9: 111–151.
Martin, P., G. Hagestad, and P. Diedrick. 1988. "Family Stories: Events (temporarily) Remembered". *Journal of Marriage and the Family.* 50: 533–541.
Martin, W. 1986. *Recent Theories of Narrative*. Ithaca: Cornell University Press.
McAdam, D. 1989. "The Biographical Consequences of Activism." *American Sociological Review* 54: 744–760.
McCall, M. 1985. "Narratives as Representation". Unpublished manuscript, Department of Sociology, Macalester College.
——— and J. Wittner. 1990. "The Good News About Life History". Pp. 46–89 in *Symbolic Interactive and Cultural Studies*, edited by H. Becker and M. McCall. Chicago: University of Chicago Press.
McCloskey, D. 1985. *The Rhetoric of Economics*. Madison: University of Wisconsin Press.
———. 1990. "Formalism in the Social Sciences, Rhetorically Speaking". *The American Sociologist* 21: 3–19.
Mead, G. H. 1929. "The Nature of the Past". Pp. 235–242, in *Essays in Honor of John Dewey*, edited by J. Coss. NY: Henry Holt.
———. 1934. *Mind, Self and Society*. Chicago: University of Chicago Press.
Merton, R. 1980. "On the Oral Transmission of Knowledge". Pp. 1–35 in *Sociological Traditions From Generation to Generation*, edited by R. Merton and M. W. Riley. Norwood, NJ: Ablex.
Mischler, E. 1986. *Research Interviewing. Context and Narrative*. Cambridge, MA: Harvard University Press.
———. 1990. "Validation in Inquiry-Guided Research: The Role of Exemplars in Narrative Studies". *Harvard Educational Review* 60: 415–442.
———. 1991. "Representing Discourse: The Rhetoric of Transcription". *Journal of Narrative and Life History* 1: 255–280.
Mitchell, W. J. T. 1981. *On Narrative*. Chicago: University of Chicago Press.

Ogburn, W. 1930. "The Folkways of a Scientific Sociology". *The Scientific Monthly* 30: 300–306.
Park, R. 1926. "The Urban Community as a Spatial and a Moral Order". Pp. 3–18 in *The Urban Community*, edited by E. Burgess. Chicago: University of Chicago Press.
Pestello, H. F. G. and F. Pestello. 1991. "Ignored, Neglected, and Abused: The Behavior Variable in Attitude-Behavior Research". *Symbolic Interaction* 14: 341–351.
Personal Narratives Group (eds.) 1989. *Interpreting Women's Lives: Feminist Theory and Personal Narratives*. Bloomington: Indiana University Press.
Plummer, K. 1983. *Documents of Life*. London: George Allen and Unwin.
———. 1990. "Herbert Blumer and the Life History Tradition". *Symbolic Interaction* 13: 125–144.
Polanyi, L. 1989. *Telling the American Story: A Structural and Cultural Analysis of Conversational Storytelling*. Cambridge, MA: MIT Press.
Polkinghorne, N. 1988. *Narrative Knowing and the Human Sciences*. Albany: SUNY Press.
———. 1991. "Narrative and the Self-Concept". *Journal of Narrative and Life History* 1: 135–153.
Reed, J. S. 1989. "On Narrative and Sociology". *Social Forces* 68: 1–14.
Richardson, L. 1988. "The Collective Story: Postmodernism and the Writing of Sociology". *Sociological Focus*21: 199–208.
———. 1990. *Writing Strategies: Reaching Diverse Audiences*. Newbury Park, CA: Sage.
Ricoeur, P. 1985. *Time and Narrative*. Chicago: University of Chicago Press.
Rose, E. 1960. "The English Record of a Natural Sociology". *American Sociological Review* 25: 193–208.
Ross, D. 1991. *The Origins of American Social Science*. NY: Cambridge University Press.
Simmons, H. 1978. "The Rhetoric of Science and the Science of Rhetoric". *Western Journal of Speech Communication* 47: 37–43.
Smith, B. H. 1980. "Narrative Versions, Narrative Theories". *Critical Inquiry* 7: 213–236.
Smith, D. 1974. "The Ideological Practice of Sociology". *Catalyst* 8: 39–54.
Stein, N. and M. Policastro. 1984. "The Concept of Story: A Comparison Between Children's and Teachers' Viewpoints". Pp. 113–155, in *Learning and Comprehension of Text*, edited by H. Mandle, N. Stein, and T. Trabasso. Hillsdale, NJ: Lawrence Erlbaum.
Stone, E. 1988. *Black Sheep and Kissing Cousins: How Our Family Stories Shape Us*. Harmondsworth: Penguin Books.
Stone, G. P. 1962. "Appearance and the Self". Pp. 86–118 in *Human Behavior and Social Processes*, edited by A. Rose. Boston: Houghton-Mifflin.
Thomas, W. I. and F. Znaniecki. 1918–20. *The Polish Peasant in Europe and America*. Chicago: University of Chicago Press.
Thurstone, L. 1928. "Attitudes can be Measured". *American Journal of Sociology* 33: 529–554.
Van Maanen, J. 1988. *Tales of the Field*. Chicago: University of Chicago Press.
Wagner, D. 1984. *The Growth of Sociological Theory*. Beverly Hills, CA: Sage.
Wardell, M. and S. Turner (eds.) 1986. *Sociological Theory in Transition*. Winchester, MA: Allen and Unwin.
White, H. 1973. *Metahistory*. Baltimore: Johns Hopkins University Press.
Wiley, N. 1979. "The Rise and Fall of Dominating Theories in American Sociology". Pp. 47–49 in *Contemporary Issues in Theory and Research*, edited by W. Snizek, E. Fuhrman, and M. Miller. Westport, CT: Greenwood Press.
———. 1986. "Early American Sociology and The Polish Peasant". *Sociological Theory* 4: 20–40.
Young, K. 1991. "Perspectives on Embodiment: The Uses of Narrativity in Ethnographic Writing". *Journal of Narrative and Life History* 1: 213–243.