The Journal of Mind and Behavior
Spring 1988, Volume 9, Number 2
Pages 97–112
ISSN 0271–0137

Are "Dialogic" Data Positive?

Salomon Rettig

Hunter College

Studies of laboratory research in the natural sciences have shown the significance of cross-experimenter dialogue in the determination of scientific facts. Behavioral and social scientists have largely ignored that role in the construction of scientific facts. A dialogic data base differs epistemogically from strict behavioral observations because of its retroductive and dialectic character. Its symbolic nature calls for hermeneutic efforts designed to achieve and assess consensual rather than empirical validation. Its ultimate aim is social organization rather than prediction and control. In view of this distinction, experimental research of human behavior must show the integration of the empirical and dialogic bases of behavioral data so as to more accurately reflect its constructive nature.

Let us suppose there is a person by the name of Episte who is very knowledgable without being aware of it. He knows what to do and is very successful. A question to be asked is why it may become important for Episte to realize what he knows since he performs so well without such inquiry.

As long as Episte is successful there is little need for him to become aware of his know-how. However, should he need to change his performance, or desire to share his knowledge with others, for example, to transmit it to his children, the situation would radically change. He would begin to ask himself specific questions about his behavior and its success. By comparing his behavior with that of others he might detect that some individuals are doing things quite differently. That is, those who are successful may be doing things his way but those less successful are doing things differently. It might even dawn on him that he is doing things the "right" way while others are doing things the "wrong" way.

The above vignette suggests at least three different considerations which may call upon Episte to examine his level of knowledge: (1) the awareness of his own outcomes, (2) the comparison of his performance and outcomes with those of others (social comparison), and (3) the desire to share his know-how with others.

The first consideration clearly places the observational emphasis on Episte's

Requests for reprints should be sent to Salomon Rettig, Ph.D., Department of Psychology, Hunter College of CUNY, 695 Park Avenue, New York, NY 10021.

own behavior. He probably learned gradually to perform well, by means of behavioral feedbacks which modify his inputs so as to improve his rate of success over time. No special cognitive elements needed to be present for such behavioral feedback to operate (although they would certainly improve his rate of learning). However, social comparisons and the desire to share information introduce a very different factor, the presence of others. Such presence calls for communication (e.g., dialogue), which in turn presupposes the formulation of some common concepts. Mere behavioral displays, such as modeling, can transmit only relatively simple behaviors. The introduction of dialogic interchanges appears to create a somewhat different epistemic process than that of ordinary (monologic) inquiries in that dialogues usually do not have stable, predictable outcomes (Mitroff and Sagasti, 1973). Hence cybernetic models, for example, are hardly applicable to such interchanges. Two persons addressing one another tend to continuously modify each other, thereby setting off a dialectic inquiry system (Churchman, 1971). This dialectic may start with a person stating an empirical observation.

A second observer, by questioning or challenging the accuracy of the initial observation, may cause the original observer to modify the initial statement so as to converge more closely with that of the second observer. Over repeated cycles of observation and dialogue some integrated response emerges which separates facts from artifacts. At this point dialogue and observed entity become separated, the entity assuming the characteristics of an independently existing object or event while the dialogue fades away. Observation and dialogue have given birth to a new entity at the point of stabilization provided by observer consensus. Such cycling of empirical observation and dialogic exchanges has been repeatedly shown to take place in the laboratories of the natural sciences: Lynch's (1985) study of axon sprouting; Knorr's (1980) study of protein generation in plants; and Latour and Woolgar's (1986) observations in the neuroendocrinology laboratory (see also Knorr-Cetina and Mulkay, 1983). Given both the prevalence as well as the generative nature of dialogic data, and their role in the constructive process of laboratory inquiry, scientific facts cannot be separated from the communicative acts of the observers who have stabilized and interpreted the observations. Furthermore, any observational consensus reached in a laboratory is not independent of the consensus embedded in the published works of other scientists. In other words, of course, scientific facts are as much a product of observations as are the discourse and consensus about those observations.

It will be argued here that the above described constructive nature of fact-finding in the laboratories of the natural sciences is equally applicable in the laboratories of the behavioral and social sciences, although in the latter the dialogue may have to be somewhat more attuned to those whose behavior is being observed (Knorr-Centina, 1981). It has been argued by others (Apel, 1980; Habermas, 1971), that the immediate aim of "dialogic" knowledge con-

struction is not strategic, geared to prediction and control, but is aimed at creating understanding and agreement. Its ultimate aim, of course, is to study the social organization of human affairs.

In making here a differentiation between the empirical and dialogic inputs in science, one is reminded of the separation between the *Geisteswissenschaften* and the *Naturwissenschaften* introduced by Dilthey (see Rickman, 1969), which was so prominent on the continent several years ago. While the controversy it generated has not totally subsided, it is becoming increasingly clear that such a separation in science is simply not possible. In making a distinction between empirical inputs and dialogic inputs to scientific knowledge, especially in the behavioral and social sciences, there exists a need to conceptually recognize this differentiation in order to assess its implications. Critical concepts in psychology, for example, such as validity, stimulus-response chaining, and reinforcement may assume a somewhat different character when they are based, even partly, on interpersonal agreement rather than on environmental stimuli. As has clearly been shown in the ethnographic studies of natural science laboratories, cross-observer dialogues are not strictly disinterested, nor are they guided entirely by empirical criteria. Consensus on what constitutes a scientific fact is also based on epistemic needs such as the integration of an observation with the published research findings of others, and the degree to which a finding constitutes a scientific resource. Latour and Woolgar (1986) found at least four different types of dialogues among researchers in neuroendocrinology laboratories which have a direct bearing on the veracity of observations: (1) dialogues which refer to "known facts," usually those recently established by other investigators, (2) dialogues which assess whether the research methods employed were correctly executed, (3) dialogues about theoretical implications and the direction the current research would take, and (4) dialogues which evaluated the research strategies and psychological make-up of other researchers in light of the present findings (pp. 160–163). These four types of dialogue were viewed as in addition to constantly ongoing interpretations of what was actually observed in their own laboratory. Lyotard suggests that the rules of research follow dialectics which consist of a dialogue toward denotative consensus rather than mere technical agreement. Such a dialogue is made possible by the empirical status of the referent object as well as by the parity of the researchers, a parity based on credibility rather than on power (Lyotard, 1984, p. 28).

All scientific observations are made by humans and their artifactual extensions. Nevertheless, a distinction can be made, based on whether empirical inputs constitute the primary base for knowledge construction and the dialogue merely serves to clarify the empirical observations, or whether the dialogue is generative in the construction of scientific facts. The latter frequently seems to be the case in the behavioral and social sciences where empirical inputs assume more of a secondary role since they are often unstable,

partial, or unavailable. Shotter (1981), for example, has argued that for a social order to emerge, autonomous individuals must be able to make and justify "non-observational" self-ascriptions such as "I don't believe you" or "I've changed my mind." Such self-references must be taken seriously despite the absence of empirical observations to warrant such exclamations. Since human beings are not born with a species-specific way of life, such exclamations make it possible for a social order to emerge and be maintained while meeting the changing exigencies of daily life. For heuristic purposes one can articulate the above distinction as consisting of two worlds, a world of object-to-object relations which is largely indifferent to, or independent of human affairs [Popper's World 1], and a world of human-to-human relations in which human existence is the singular relevant focus of study [Popper's World 2] (Popper, 1979). Each of these two worlds structures scientific observations somewhat differently. World 1 largely follows Euclidean geometry in which the units of measurement are based on physical stimuli. World 2, however, follows hyperbolic geometry that stresses sensory responses, such as just noticable differences (Heelan, 1983). Nevertheless, both maintain a close correspondence with each other.

In what follows, an attempt will be made to analytically expand the distinction between an empirical (Euclidean) and a dialogic (hyperbolic) data base. Such analysis may not only suggest different "methodologies" but will hopefully illuminate somewhat separate epistemological assumptions underlying both. Although scientific objects do not exist independent of the theoretical models which create them, symbol (word) and object are not empirically equivalent; symbols necessarily require interpretation.

Hermeneutics

Habermas (1971) as well as others (Apel, 1980; Knorr-Centina, 1981; Ricoeur, 1982) have called attention to the critical role of systematic interpretations in scientific constructions. For Ricoeur (1982) hermeneutics is the process of conversing with a text, whether archaic or contemporary. While written texts appear to have fewer problems than spoken ones, both presume an understanding of semantic meaning, whether carried by syntax, pragmatics, or external empirical referents. Both also presume an already existing pre-interpreted textual context within which the new text is embedded. Thus, even semantic knowledge of the language may at times prove insufficient since certain pre-understandings may be missing. Such insufficiency has been shown to be responsible for misunderstandings in scientific laboratories as well as in the ethnographic accounts of anthropologists (Geertz, 1983).

A major problem in hermeneutics addresses itself to the position of the interpreter, especially his/her own pre-conceived understanding (see Palmer, 1969). Is the role of the interpreter in analyzing historical texts more that of

the translator who deciphers the text in its original historical, cultural, and/or local setting without contemporizing it, or does a superior interpretation demand that the text be seen through the eyes of a current (and local) perspective? Does a good interpretation perhaps consist of the "fusion of horizons" (Gadamer, 1975) of the text and the interpreter? No consensus has emerged on the above questions. However, in the laboratory of the behavioral scientist this question has rarely been asked since interpretation is ordinarily not seen as problematic. While verbal instruments such as questionnaires may be assessed for their internal validity, most other verbal interactions such as instructions, explanations, clarifications, as well as the assessments of observations are presumed to be commonly understood by the experimenters and also by the subjects.

A difficult problem of interpreting spoken texts, as in ongoing dialogues, is the subtle shifting of grounds of the participants as they listen to, incorporate, and subsequently modify their own position in the light of the encounter while still maintaining semantic continuity (Garfinkel, Lynch, and Livingston, 1981; Lynch, 1985; Schenkheim, 1978). This dialectic is a continuous, bi-directional process which, in the case of natural science laboratory research, eventually reaches an asymptote connoting agreement on an observation. It is doubtful that in the absence of unequivocal empirical inputs in the laboratories of behavioral scientists such observational asymptotes are generalizable to whole populations. "Observations" of textual facts, such as agreements reached by the participants in a dialogue, whether subjects or experimenters, are possible only after careful consultation with all parties. Thus, the assessment of consensus among subjects in a human experiment, for example, may require an additional dialogue between experimenters and participants, a so-called "double hermeneutic" (Giddens, 1976), before it can be considered to be veridical. Such secondary dialogue clarifies mutual understandings for both—the participating subjects as well as the observers—since each can correct the other in this process. Otherwise it is doubtful that independent observers of the same discourse would interpret social facts similarly.

The ethnographic studies of natural science have shown that in view of the dialogic character of laboratory findings replications of experiments are rarely duplicate copies of each other. They are usually "replications" which involve something that is different, such as an altered method which hopefully leads to the same findings (Collins, 1975; Mulkay and Nigel, 1986). Behavioral scientists in human laboratories also rarely duplicate their experiments. Copies of previous experiments are extremely difficult to produce because of the intervention of spontaneous texts in such experimentation (e.g., conversations, questions, explanations). However, repeated studies can converge in their results, though this leaves the assessment of convergence problematical. If the theoretical model is broad enough or imprecise enough, convergence may

be more easily achieved than in highly axiomatic systems. What is more often the case in such studies is that such convergence is attempted post-experimentally (retroductively), by making the theory fit the data. Under the hypothetico-deductive system, such post-observational interpretations of convergence call for further testing, thus repeating the above circle.

Another helpful notion in the interpretation of texts consists of the so-called "hermeneutic circle" whereby meaning is extracted by reciprocally relating parts to wholes and wholes to parts (e.g., observations to theory and theory to observations) each modifying the other. Thus, for example, human subjects in a laboratory may attempt to guess the overall purpose of the experiment. As the experiment progresses each detail is judged in terms of this overall hunch. Details, as they emerge during the experiment, in turn modify such guessing. Deceptions of subjects are often called for to break this circle of guessing in order to avoid biased findings. Such deceptions usually take place by means of elaborate verbal "explanations" (Baumrin, 1985). Following participation in an experiment, subjects are then instructed not to discuss the research with others. Natural scientists rarely have to contend with such textual considerations in their observations.

Recognition of the importance of dialogic inputs in human laboratory studies might suggest a somewhat different design. Subjects' conjectures could be made explicit and incorporated into the research, for example, observing how such conjectures are modified in view of subsequent laboratory manipulations. Such an approach may help apportion the degree to which laboratory findings are the result of experimental manipulations versus subject fabrication, or the convergence of both. At least this approach would focus attention on the construction of human behavior in the laboratory rather than force behavior into a Procrustean bed in which human subjects are incorrectly assigned a status of nonreactive objects.

Invariance and Causality

In view of the interpretive nature of the dialogic "data base," questions arise as to the scientific quality of such data. How can an unstable, dialectically forged inquiry system assume nomothetic invariance? Is in fact the aim of dialogic inquiry the acquisition of nomothetic knowledge? It certainly constitutes a major aim of the empirical sciences. According to Hempel (1942) "the main function of general laws in the natural sciences is to connect events in patterns which are usually referred to as *explanation* and *prediction*. . . . In the case of an explanation, the final event is known to have happened, and its determining conditions have to be sought, . . . in the case of prediction . . . the initial conditions are given, and their 'effect'—which in the topical case has not yet taken place—is to be determined" (pp. 35–38). For Hempel, nomothetic laws relate causes to effects, thereby making explanations and

predictions possible when either one of them is not given. In the real world of natural science, however, matters are rarely as simple as Hempel indicated. Natural laws require auxiliary statements to help interpret whether a set of empirical findings falls under the covering laws. The auxiliary statements are ordinarily not directly derivative from the body of theory which guides the empirical investigations, but are contextual (retroductive) explanations (Putnam, 1979). Moreover, Putnam observes, ". . . in a great many important cases, scientific theories do not imply predictions at all" (1979, p. 425). In view of the necessary reliance on auxiliary propositions, disconfirmations of predictions do not necessarily constitute falsification in the Popperian sense, since the auxiliary propositions could have been wrong. Instead of falsification "normal science exhibits a dialectic between the desire to solve problems . . . and the testing of new hypotheses" (1979, p. 431).

In contrast to empirical inquiry, dialogic inquiry is frequently directed at a *reconstruction* of events, i.e., what is it that was actually seen in the laboratory? Reconstruction in the laboratory frequently employs analogy and inferences about what is already known or intuited (Knorr, 1980). Gilbert and Mulkay (1980) interviewed 34 biochemists and had each discuss at least two of their published scientific papers. The interviews contrasted sharply with the publications in that the published accounts tended to emphasize objectivity, the linear development of hypotheses, and the cumulative nature and internal consistency of the research. This continuity in reporting was achieved by de-emphasizing the role of the scientist as the agent of the scientific discovery, by the selective organization of the facts presented, and by discounting alternative interpretations, so as to "let the facts speak for themselves." Empirical research on the reconstruction of information in the behavioral sciences has shown that the observation of social events is clearly a function of both the original stimulus as well as interpretive "attributes that go far beyond the information given in the original stimulus" (Hastie, Park, and Weber, 1984, p. 186). These are schematic rather than copy attributes.

According to Habermas, the reconstructive sciences "aim at the explicit, systematic reconstruction of implicit, 'pre-theoretical' knowledge" (cited by McCarthy, 1981). Rational reconstruction is the attempt to give an explicit account to a prior experience such as a "practically mastered, pretheoretical" know-how (McCarthy, 1981, p. 276). Since reconstruction deals with "symbolically structured reality" of covert, or so-called "deeply structured" knowledge, according to Habermas, it may have to be "maeutically" extracted, that is, by means of "questioning the subject with the aid of systematically arranged examples" (McCarthy, 1981, p. 278). From this description it is clear that the data base of reconstructed information, though future oriented, resides in the past rather than in the present; and that the aim of reconstruction is to systematize historical events. The significance of contemporary history in the social and behavioral sciences has been pointed out by others

and need not be repeated here (Gentner and Grudin, 1985; Gergen, 1973; Keniston, 1971; Pepitone, 1981; Samelson, 1974; Sampson, 1978). Gergen (1973), for example, has called attention to the non-repeatability and instability of historical inquiry. Such knowledge cannot be cumulative since it cannot transcend its historical boundaries. However, what is of concern here is the question of nomotheticity underlying historical reconstruction. Although Hempel (1942) himself argued against any differentiation between explanations in history and in the empirical sciences, historians do seem to be realizing a difference between their reconstructive inquiries and the more current constructive knowledge building efforts emerging from natural science laboratories. Nevertheless, they do see themselves as having sufficient evidence to make causal inferences. The difference, according to Dray, lies in the fact that historians, when reconstructing past events, observe antecedents and consequences simultaneously rather than successively, "for we may judge the applicability of a concept by noticing what comes *after* as well as what comes before, that to which it is said to apply. We see the significance of historical events by noticing what they *lead* to, as well as what they arise of" (Dray, 1959, p. 406).

Some psychologists who have been aware of this problematic difference have argued that causal analysis in psychology requires both an assessment of the "configured structures" underlying behavior as well as "a historical grasp of the particular and changing configuration" (Manicas and Secord, 1983, p. 402). Others, however, argue against any form of nomothetic invariance and causal inference in psychology. According to Meehl (1970), laws of human behavior are *accidental universals* rather than nomologicals. Accidental universals are pseudonomologicals because they are history dependent and consist of descriptive structures such as types and traits.

A dialogic data base which consists predominantly of linguistic reconstructions requires hermeneutic analysis and synthesis. Such a data base by itself, even when accurately interpreted, can be said to have only an existential base, since it is symbolic, unstable, and dialectic. The major difference between an existential and a nomothetic law, according to Popper (1965), is that natural laws are falsifiable while existentials are not, unless they contradict a natural law. Hence, contradiction and falsification, which are intolerable in the epistemic process of nomothetic inquiry (induction, deduction) can be intrinsic to rational dialogic inquiry, and can lead to retroductive reasoning (Hanson, 1979) that is reverse reasoning. When encountering contradictory findings reversed reasoning makes the inferring of a modified hypothesis possible. In view of the unpredictability of the dialogue, one of two possible groundings is called for to attain invariance—empirical validation through repeated observations as in the natural sciences, or with reconstructive material, consensual validation. Behaviorists such as Skinner and Spence, who were concerned with the scientific status of covert behavior, have refer-

red to the latter as truth by agreement or intersubjective agreement (Zuriff, 1980). The relationship between these two forms of validation appears to be inversely proportional: as clear empirical referents become less available, greater reliance is placed on consensual processes.

Consensus

Social psychology has produced a number of classic laboratory experiments illustrating the influence of intersubjective consensus on what is perceived to be real. Consensual validations of reality have been formed by normative means (Sherif, 1936), by group discussion (Lewin, 1953), and by pressures toward conformity (Ash, 1956). Furthermore, Festinger (1954) forcefully argued for the critical function of social comparison in the construction of social reality. Such research effort would indicate that the process of consensus formation represents a very important aspect of knowledge construction in human laboratories. Yet, with the exception of Lewin's seminal work, these studies as well as many more recent experimental studies of consensus—in relation to decision making in experimental juries (Penrod and Hastie, 1979), in small group performances (Kulik and Taylor, 1980), and in attribution theory (Hansen and Donoghue, 1977; Ross, Greene, and House, 1977)—have had to introduce artifactual factors, including hidden confederates, fabricated group pressures, and presumed consensus. In addition to the fabricated nature of such agreements, it has also been shown that consensus preceding decision making (for example, whether or not to accept the research findings of others), entails a number of considerations which are not always conducive to rational consensus. These include conformity of decisions to expectations, consensus slanted to action, and a desire to challenge existing policies (Weiss and Bucuvalas, 1980). Hence, these studies offer relatively little understanding of how rational consensus is actually formed in the absence of decision pressures and artifactual laboratory manipulations.

Dallmayr (1981) has called attention to four types of human interactions, each of which would give rise to a different form of consensus: (1) *communalism*, based on affective ties established among reciprocating individuals (consensus here would be primarily a reflection of communion among members); (2) *association*, based on cognitive-instrumental agreements designed to maximize certain outcomes; (3) *movement*, based on charismatic authority aimed at achieving monadic unity; and (4) *community*, which is attained via benign diversity and is directed at attaining "collective emancipation." At least two of these interactions, association and community, seem to be more rationally based and given to the empirical study of embedded dialogue than communalism and movement.

Rational consensus is dialogically achieved in face-to-face interaction. That is, participating subjects are actively oriented towards one another (Lynch,

1985), though the consensus reached may be covert. This negotiative process of consensus formation is in contrast to pre-existing structures imposed upon participants (e.g., instructions, scientific apparatus, or laboratory routines). The consensus must be freely produced in such a way that each subject has an equal chance to participate (McCarthy, 1973). Furthermore, the emphasis on the active construction of consensus on the part of participating subjects does not allow for an observer or group of observers to do so on their own, for example, by comparing subject A's responses with those of subject B. The act of consensus construction in fact suggests that the observers (experimenters) become participants in the dialogue so as to gain access to the tacit process of consensus formation. Since the actual achievement of consensus may be covert, it cannot be binding on the participants. However, when it is made explicit, participants should be able to provide some sort of (reconstructive) account of it, that is, to give reasons for it (Semin and Manstead, 1983).

In a series of experimental studies on ethical risk taking, it was found that group dialogue which preceded predictions as to whether socially undesirable conduct (theft) would take place under various circumstances, changed the interpretation of the ethical dilemmas portrayed. While individuals focussed their attention on the utility value of potential censure that would ensue from engaging in the socially undesirable behavior, group dialogue emphasized predominantly the utility value of the gain enjoyed by the perpetrator (Rettig, 1966). This collective shift in emphasis from potential censure to potential gain remained unknown to the participating subjects and could only be ascertained by a multi-dimensional scaling method which post-experimentally separated the various sources of variance that significantly contributed to the dialogue. Here it seemed that a tacit, yet reasoned, consensus took place in small, face-to-face ad hoc groups despite the fact that the participants, strangers to one another, were under no pressure to attain unanimity. They in fact did not attain unanimity in their judgements. This tacit operation of such a group shift toward a consensus of gain was replicated in different studies (Chapko, 1972a, 1972b; Rettig, 1969, 1972; Rettig and Turoff, 1967).

In one of these studies a confederate was openly instructed to hinder the process of consensus formation by discursively disagreeing with whatever was said by other subjects. In the control conditions subjects were either told nothing (in one condition), or a confederate was openly instructed to facilitate the consensus formation process by discoursively agreeing with whatever other subjects stated. As expected, both control conditions replicated the group shift toward a consensus of gain, while the "hinder" condition did not (Rettig, 1972). Interestingly enough, when in an earlier study an entire discussion was tape recorded and played back to another group, no such "gain" consensus took place (Rettig and Turoff, 1967). This finding would suggest that the availability of information by itself is not sufficient for consensus

formation; subjects must actively participate in the dialogue to achieve consensus. Chapko (1972b) also found the emergence of a group consensus toward gain in studies involving economic assessments. His study pinpointed the factor producing this shift, namely rhetoric. While these studies appear to be similar to other studies of increased risk taking in groups, they should not be confused with them. The ethical problems studied required no decision to be made, nor was there any consistent shift towards higher risk taking in groups. Items depicting high gains did shift in a more risky direction; however, an equal number of items with low gains shifted in the opposite directions, thus weakening any risky shift.

In addition to active participation, the attainment of rational consensus appears to be a function of various aspects of discursive expressions including sincerity, clarity, appropriateness, and veridicality (Habermas, 1971). Participants in a dialogue are more likely to achieve consensus if (1) descriptive statements that are made actually reflect the existing state of affairs; (2) normative statements do indeed establish the acceptability of actions and their norms; (3) expressive statements genuinely demonstrate transparency of self-presentation; (4) evaluative statements establish preferability; and (5) explicative statements use symbolic expressions accurately (Habermas, 1981). While the intrusion of non-rational factors such as perceived decision pressures, deception or bias, and demand characteristics may at times not be totally avoidable, their distortive effects are not likely to be identical across these different forms of consensual validity. Hence, the analytic criteria suggested above may help evaluate not only any consensus achieved, but also any consensus failed. Subjects may have to be queried in very detailed ways, perhaps maeutically, about the process of consensus formation. Such a procedure may also contain suggestions about improving the formation of consensus. Furthermore, studies of consensus should be separated from studies of decision making.

Realizing that subjects' dialogues are often reconstructive, the task of assessing consensus may ultimately involve a secondary probing using systematic retroductive "uncovering" of "deeply structured" dialogic material embedded in the subjects' reports. While multi-dimensional scaling may be one way of assessing such latent material, it has the distinct methodological disadvantage, in common with other human laboratory studies, of an artifactual a priori structuring of textual stimuli.

Conclusions

Episte's desire to share his competent performance with other people raised a host of unanticipated problems which he would have to understand and master. Most of these problems arose because of his need to engage in a dialogue with others in order to convey his know-how to them; simple

behavioral demonstrations would not be adequate. The need to dialogue calls forth a number of reconstructive cognitive processes such as recall, analogy and comparison, as well as some form of retroductive reasoning—explaining outcomes by reciprocally relating behavior and concepts. Episte's mastery of this challenge would benefit not only those he wished to instruct, but also himself, since it would force him to give accounts of his behavior, something he did not have to do before. Such accounting would transform his tacit competence into an overt and scrutinizable event. His behavior would serve as a constant guide to his reasoning. However, Episte is not a scientist and is not concerned with the generalizability of his behavior.

Ethnographic studies of research in natural scientific laboratories have shown that dialogic data constitute a significant component in what is discovered. Experimentation with human subjects in laboratories also entails dialogue, whether in the instructions provided, the explanations given, or the findings evaluated. As in natural science laboratories, dialogue is required to arrive at a consensus about what is observed. The textual material embedded in such dialogues calls for interpretation.

When empirical events are unstable, partial, or for that matter historical, human-subjects research can be exclusively a matter of dialogue, as occurs during interviews, psychotherapy, as well as in ethogenic, ethnographic, and ethnomethodological accountings. Such studies often rely on reconstructed material, that is, on accounts of behaviors that have taken place in the recent or not so recent past. Such reconstruction, though consistent or invariant, nevertheless engages epistemic processes which are not strictly nomothetic. Instead of the maximization of prediction and control as a test of theoretical soundness, reconstructed data are founded on a consensus that certain events did indeed lead to or cause subsequent events, with both premises and outcomes known at the time of reconstruction. The aim of such retroductive reasoning is to understand what has happened rather than to predict what is about to happen, although the two are not unrelated. The importance of pinpointing and making sense of what has happened extends itself across all human endeavors, including the natural and behavioral sciences, since it makes the rational regulation of behavior possible.

In view of the centrality of consensus in the processing of dialogic data, it is surprising that social and behavioral sciences have heretofore not addressed themselves intensively to the process of consensus formation. The reasons for this deficiency may be that consensus often emerges without awareness. A more historical explanation may be that positivism essentially does not differentiate between epistemic processes underlying bodily behavior and those unique to dialogic behavior, such as retroductive and dialectic reasoning. Hence no special need was perceived to differentiate between cognitive action, such as the formation of consensus, and strategic action such as decision making. Rational consensus is formed in face-to-face encounters

by the participants in a dialogue, rather than by observers. It may or may not lead to further action. One way of extracting latent consensus may be by means of post-experimental multi-dimensional scaling of dialogues.

The role of consensus in the validation of social reality, in the laboratory or elsewhere, calls for such factors as clarity and honesty of expression, as well as veracity and appropriateness. According to Habermas (see McCarthy, 1973), these criteria together make up "communicative competence." While none of these forms of social validation is new, and the processes they refer to are pervasive, the explication of the distinction between dialogic and empirical processing of data could help assess their combined role in all scientific endeavors. Scientific facts, in the laboratory or elsewhere, are the product of dialogically active constructions which iteratively relate empirical observations to theoretical comprehension, rather than to nomothetic covering laws.

References

Apel, K.O. (1980). *Towards a transformation of philosophy.* London: Routledge and Kegan, Paul.

Ash, S. (1956). Studies of independence and conformity: a minority of one against a unanimous majority. *Psychological Monographs, 70,* No. 9.

Baumrin, D. (1985). Research using intentional deception. *American Psychologist, 40,* 165–174.

Chapko, M. (1972a). Source of increased emphasis of gain in predicted ethical risk taking. *Proceedings of the 80th Annual Convention of the American Psychological Association, 7,* 219–220. (Summary)

Chapko, M. (1972b). *The sources of increased emphasis of gain in predicted ethical risk taking of groups.* Unpublished doctoral dissertation, City University of New York.

Churchman, C.W. (1971). *The design of inquiring systems.* New York: Basic Books.

Collins, H.M. (1975). The seven sexes: A study of the sociology of a phenomenon, or the replication of experiments in physics. *Sociology, 9,* 205–224.

Dallmayr, F.R. (1981). *Twilight of subjectivity.* Amherst, Massachusetts: University of Massachusetts.

Dray, W. (1959). 'Explaining what' in history. In P. Gardiner (Ed.), *Theories of history* (pp. 403–408). Glencoe, Illinois: Free Press.

Festinger, L. (1954). A theory of social comparison processes. *Human Relations, 7,* 117–114.

Gadamer, H.G. (1975). *Truth and method.* New York: Seabury Press.

Garfinkel, H., Lynch, M., and Livingston, E. (1981). The work of discovering science construed with materials from the optically discovered pulsar. *Philosophy of the Social Sciences, 11,* 131–158.

Geertz, C. (1983). *Local knowledge: Further essays in interpretive anthropology.* New York: Basic Books.

Gentner, D., and Grudin, J. (1985). The evolution of mental metaphors in psychology: A 90-year retrospective. *American Psychologist, 40,* 181–192.

Gergen, K.J. (1973). Social psychology as history. *Journal of Personality and Social Psychology, 26,* 309–320.

Giddens, A. (1976). *New rules of sociological method.* London: Hutchinson.

Gilbert, N., and Mulkay, M. (1980). Context of scientific discourse: Social accounting in experimental papers. In K.D. Knorr, R. Krohn, and R. Whitley (Eds.), *The social process of scientific investigation: Sociology of the sciences* (pp. 25–52) [Yearbook Volume 4]. Dordrecht, Holland: Reidel.

Habermas, J. (1971). *Knowledge and human interest.* Boston: Beacon Press.

Habermas, J. (1981). *The theory of communicative action: Reason and the rationalization of society* (Volume 1). Boston: Beacon Press.

Hansen, R.D., and Donoghue, J.M. (1977). The power of consensus: Information derived from one's own and others' behavior. *Journal of Personality and Social Psychology, 35,* 294–302.

Hanson, N.R. (1979). Alternatives to received view. In F. Suppe (Ed.), *The study of scientific theories* (pp. 151–166). Urbana, Illinois: University of Illinois.

Hastie, R., Park, B., and Weber, K. (1984). Social memory. In R.S. Wyer and T.K. Srull (Eds.), *Handbook of social cognition* (Vol. 2, pp. 149–212). Hillsdale, New Jersey: Erlbaum.

Heelan, P.A. (1983). *Space perception and the philosophy of science.* Berkeley: University of California.

Hempel, C.G. (1942). The function of general laws in history. *Journal of Philosophy, 39,* 35–48.

Keniston, K. (1971). Psychological development and historical change. *Journal of Interdisciplinary History, 2,* 330–345.

Knorr, K. (1980). *The manufacture of knowledge: Toward a constructivist and contextual theory of science.* Oxford: Pergamon.

Knorr-Cetina, K. (1981). *The manufacture of knowledge: An essay on the constructivist and contextual nature of science.* Oxford: Pergamon Press.

Knorr-Cetina, K., and Mulkay, M. (Eds.). (1983). *Science observed.* London: Sage.

Kulik, L., and Taylor, S.E. (1980). Premature consensus on consensus? Effects of sample based versus self-based consensus information. *Journal of Personality and Social Psychology, 38,* 871–878.

Latour, B., and Woolgar, S. (1986). *Laboratory life: The construction of scientific facts.* Princeton: Princeton University Press.

Lewin, K. (1953). Studies in group decisions. In D. Cartwright and A. Zander (Eds.), *Group dynamics* (pp. 287–301). Evanston, Illinois: Row, Peterson.

Livingston, E. (1986). *The ethnomethodological foundations of mathematics.* London: Henley, Routledge and Kegan Paul.

Lynch, M. (1985). *Art and artifact in laboratory science.* London: Routledge & Kegan Paul.

Lyotard, J.F. (1984). *The postmodern condition.* Minneapolis: University of Minnesota.

Manicas, P.T., and Secord, P.F. (1983). Implications for psychology of the new philosophy of science. *American Psychologist, 38,* 399–413.

McCarthy, T.A. (1973). A theory of communicative competence. *Philosophy of Science, 3,* 135–156.

McCarthy, T.A. (1981). *The critical theory of Jurgen Habermas.* Cambridge, Massachusetts: Massachusetts Institute of Technology Press.

Meehl, P.E. (1970). Nuisance variables and ex post facto design. In M. Radner and S. Winocur (Eds.), *Minnesota studies in the philosophy of science* (Vol. 4, pp. 373–402). Minneapolis, Minnesota: University of Minnesota.

Mitroff, I.I., and Sagasti, F. (1973). Epistemology as general system theory: An approach to the design of complex decision-making experiments. *Philosophy of the Social Sciences, 3,* 117–134.

Mulkay, M., and Nigel, G. (1986). Replications and mere replications. *Philosophy of the Social Sciences, 16,* 21–37.

Palmer, R.E. (1969). *Hermeneutics.* Evanston, Illinois: Northwestern University.

Penrod, S., and Hastie, R. (1979). Models of jury decision making: A critical review. *Psychological Bulletin, 86,* 462–492.

Pepitone, A. (1981). Lessons from the history of social psychology. *American Psychologist, 36,* 972–985.

Popper, K.A. (1965). *Conjectures and refutations.* New York: Harper.

Popper, K.A. (1979). *Objective knowledge: An evolutionary approach.* Oxford: Clarendon.

Putnam, H. (1979). Scientific explanations. In F. Suppe (Ed.), *The structure of scientific theories* (pp. 424–433). Urbana, Illinois: University of Illinois.

Rettig, S. (1966). Group discussion and predicted ethical risk taking. *Journal of Personality and Social Psychology, 3,* 629–633.

Rettig, S. (1969). Locus of control in predictive judgments of unethical behavior. *Proceedings of the 77th Annual Convention of the American Psychological Association, 4,* 427–428. (Summary)

Rettig, S. (1972). Trust and locus of control in predictive judgments of unethical behavior. *Proceedings of the 80th Annual Convention of the American Psychological Association, 7,* 221–222. (Summary)

Rettig, S., and Turoff, S.J. (1967). Exposure to group discussion and predicted ethical risk taking. *Journal of Personality and Social Psychology, 7,* 177–180.

Rickman, H.P. (1969). *Wilhelm Dilthey.* London: Paul Elek.

Ricoeur, P. (1982). *Hermeneutics and the human sciences.* Cambridge: Cambridge University.

Ross, L., Greene, D., and House, P. (1977). The false consensus effect: An egocentric bias in social perception and attribution processes. *Journal of Experimental Psychology, 13,* 279–301.

Samelson, F. (1974). History, origin myth, and ideology: Comte's 'discovery' of social psychology. *Journal for the Theory of Social Behavior, 4,* 217–231.

Sampson, E.E. (1978). Scientific paradigms and social values: Wanted—a scientific revolution. *Journal of Personality and Social Psychology, 36,* 1332–1343.

Schenkheim, J. (1978). *Studies in the organization of conversational interaction.* New York: Academic Press.

Semin, G.R., and Manstead, A.S.R. (1983). *The accountability of conduct: A social psychological analysis.* London: Academic Press.

Shotter, J. (1981). Telling and reporting: Prospective and retrospective uses of self-ascriptions. In C. Antaki (Ed.), *The psychology of ordinary explanations of social behavior* (pp. 157–181). London: Academic Press.

Sherif, M. (1936). *The psychology of social norms.* New York: Harper.

Weiss, C.H., and Bucuvalas, M.J. (1980). Truth tests and utility tests: Decision-makers' frame of reference for social science research. *American Sociological Review, 45,* 337–350.

Zuriff, G.E. (1980). Radical behaviorist epistemology. *Psychological Bulletin, 87,* 337–350.