

Von Osten's Horse, Hamlet's Question, and the Mechanistic View of Causality: Implications for a Post-Crisis Social Psychology

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Every science subsumes within it hidden assumptions that, while not empirical in nature, give meaning to empirical relations. Social psychology has borne the brunt of attacks regarding previously hidden assumptions about the mechanistic paradigm of empirical science. It appears that the field is undergoing a gradual shift in orientation, in part as a result of an erosion of confidence in the old paradigm. This paper attempts to review aspects of the attacks in order to show how they provoked a reappraisal, still under way, of the nature of empirical social psychology and the limits of sociopsychological knowledge. The question is raised as to whether such a reappraisal might lead to a profoundly pluralistic social psychology consistent with a more liberalized definition of empirical science.

Over the past two decades, social psychology has come under attack both from within and without the discipline. This paper is an attempt to put these attacks in sociohistorical perspective by interpreting some familiar events in American social psychology within a framework of confluent intellectual developments. The discussion is not intended to be a further critique of social psychology, nor does it contain a specific prescription for change. However, on the assumption that such a reexamination might contain the seeds of a new and more valid research style, this paper draws attention to current ideas which, while not precisely or explicitly formulated, seem to suggest a more pluralistic direction for a post-crisis social psychology.

Hidden Assumptions

Every science contains within it a hidden assumption—Apel (1982; Note 1) calls them regulative principles—which together constitute an invisible system of presuppositions. An example in social psychology is the idea that humans are sociocentric (or “social animals”), by which it is usually meant

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that society is preprogrammed or "wired into" human nature. There is ample circumstantial evidence for this Aristotelian assumption in the artifacts and other such remains of prehistoric peoples and their cultures to suggest that there never was a period on earth when humans did not interact in groups and societies (Zajonc, 1971). Nevertheless, it is true that habitual destruction by humans of other members of the human species has also been a fact of life throughout civilization. Thus, in order to fully comprehend the rich mosaic of human experience, social psychologists rely on an array of presuppositions instead of just a single regulative principle. Humans are sociocentric, but social behavior is presumably also full of egocentrism, hedonism, rationalism, and irrationalism among other apparent contradictions.

Each individual theory and method can be conceived as a mosaic map of hidden assumptions like these, although the emphasis on particular or interconnected principles varies from one procedure or postulate to another. For example, some self-report methods, such as opinion polling and attitude measurements, make the rationalistic assumption that the conscious views and judgments of human beings have extensive logical influence on their beliefs and behavior. On the other hand, theories postulating that behavior is fixed by arbitrary or random or irregular experiences of reward and punishment give greater weight to irrationalism, and to egocentrism insofar as they assume that social learning never results in a genuine transformation of motives (see Allport, 1968).

One characteristic of such assumptions, implicit in these examples, is that they provide investigators with vantage points from which, consciously and unconsciously, to concretize or objectify what otherwise might be dismissed as a succession of accidental events without substance or purpose. Another characteristic is that insofar as regulative principles imply their own standards which go beyond the visible relations between human beings, they are not immediately refutable on empirical grounds. That is to say, social psychologists cannot theorize about human experience without falling back on such assumptions, but at the same time they cannot use experience alone to render these assumptions false.

From this viewpoint, one fundamental position acquires an enormous significance in terms of the picture of social psychology that has emerged during the past two decades. Something resembling an incomplete system of patterns, that is, a system comprised of epistemological assumptions based only on what a particular mechanistic reconstruction claimed science to be, was the picture that emerged. The principal assumption, namely a simple mechanistic view of causality, ostensibly led investigators to (a) search for single explanatory principles and (b) resolutely defend experimentation as the method of choice for doing science. Briefly, in the late-nineteenth century, spurred on by the extraordinary successes in physics and chemistry, a number of leading experimentalists sought to imitate a classical reconstruction of

mechanistic natural science as a way of tidying up the clutter of subjective psychology. Twentieth-century psychology developed in large part out of such a pronaturalist or naturalistic idealization, inspired by the idea that the human system, like the physical systems of Galileo, Newton, and their intellectual predecessors, operated in terms of a push-and-pull machine that stressed action by contact (see Pepper, 1942/1970). It followed that the behavior of such a "machine" ought to be subject to causal laws so that, under rigorously positivistic experimental conditions, determinate results might be expected.

Coming along as it did in the heyday of the industrial revolution, this mechanical analogy with its emphasis on objective, value-free, universal evidential facts and efficient causality must have seemed an intoxicating epistemology for addressing questions of great complexity that had puzzled social philosophers since antiquity. With the notable exception of one or two forays into field theory (e.g., Lewin, 1935), the new physics of Maxwell, Planck, and others was virtually ignored by behaviorists and then neobehaviorists who were transfixed by several forms of a classical metaphysics that was already passing into history. In learning psychology, for example, researchers who first embraced a Cartesian mechanical conception later made it seem more plausible by gradually increasing the explanatory power of the Pavlovian and Hullian reflex models (see discussion by Lowry, 1971). In social psychology, where the line from a mechanistic conception to a theory of homeostasis or balance first twisted around classical biological assumptions, the notion of *stasis* (not of change) ultimately became a central proposition, suggesting that human nature, like a machine, was unchanging in its average state (Harré and Secord, 1972; Rosnow, 1981). The mechanistic analogy is also characteristic of much of present-day cognitive psychology's conception of mind as an analogue to a computer (cf. Lazarus, 1982).

This is not to say that all mechanists were (or are) experimentalists or that all mechanists were (or are) insensitive to the idea of social change. Nevertheless, as a consequence of these developments, and of the great nineteenth-century advances in knowledge in the biological sciences (from which Wundt and others took inspiration) that resulted from the introduction of stricter experimental procedures into the "life sciences," the experiment reigned supreme as the absolute ideal of mechanistic scientific psychology. The idea was even circulated that the experimental study of human behavior could proceed with the same disinterested curiosity with which physicists studied the refraction of light and chemists studied the properties of substances and elementary forms of matter. In this view, value judgments, the distinction between "good" and "bad," and personal feelings presumably had no place in science, which instead was considered as essentially an instrument of defining everything in quantitative materialistic

terms (Weisskopf, 1972).

As I shall attempt to show, there were three assumptions of this mechanistic model which became subject to rebuttal in social psychology, namely the impartiality, moral neutrality, and universality of the scientific method as applied to social behavior. By the 1930's, psychologists were finding support for these assumptions in the objectivist assertions of the logical positivist view (Toulmin and Leary, in press). Yet as early as 1927, the Hawthorne study raised questions about the possibility that psychological investigators were working with dirty test tubes as a consequence of subjects' awareness that they were being studied (Roethlisberger and Dickson, 1939). Similar questions were later reiterated by Rosenzweig (1933) and then by Sarbin (1944), who invoked the indeterminacy principle to make the point that the classical conception was an insufficient model for the logic of prediction in personality and social psychology. In these fields, it was argued, predicting human behavior was limited by reactive observations which imposed uncertainties on the behavior observed. This, Sarbin argued, was reminiscent of the situation in quantum physics and in biology where the very act of measurement may negate the possibility of observing some phenomena as they would have occurred had they not been observed.

It was not until the mid-1960's, however, that many experimental social psychologists began to pay more than just lip service to these criticisms which had been periodically raised. The artifact work of Orne, Rosenthal, and others, to which I now turn, was in retrospect not simply an attack on the failure of experiments by focusing on the understanding of "contaminating factors" or an effort to rule out systematic errors, as it is usually said, but also an implicit assault on the impartiality assumption of mechanistic natural psychology. In challenging this assumption, this work prepared the way for further assaults exposing the possibility of other problematic assumptions.

The Impartiality Question

In spite of a prevailing belief in the impartiality of experimental observations, there had always been an undercurrent of feeling that psychological observations were subject to unconscious observer bias. The case of the horse named "Clever Hans," which was the focal point of a famous scientific commission in 1904, was in later years to be widely recognized as having been a clue to the fact that even nonhuman subjects were susceptible to observer effects and unconscious suggestion (Pfungst, 1911/1965). Hans, as every psychology major has been taught, captured the imagination of the German public and European psychology as a result of his apparently remarkable feats of reasoning performed by tapping his hoof according to a code taught to him by his owner, one Wilhelm von Osten. As it turned out, the horse was certainly clever enough, but this cleverness was due

to his "perspicacity" and not to his reputed ability to reason. Observers' postural movements were unintentional signals to Hans to tap his hoof, although von Osten held to his anthropomorphic belief in the horse's rationality in the face of this adverse finding (cf. Sarbin, 1981).

However, it was not until the 1960's with the rise of cognitive psychology, that much of experimental psychology became overtly receptive to the possibility that subjects' cognitions about the purpose of psychological research could influence their behavior in the laboratory in ways unforeseen by investigators (Suls and Rosnow, 1981). Such a stance, presumably, would have been inconsistent with positivistic tenets of methodological behaviorism, which restricted usable data to observable responses and dismissed cognition as a variable of less than scientific significance (cf. Hilgard, 1977). Yet the common disguising of experimental intent (even if not always successful) showed some implicit awareness of the issue of contamination by "subject-artifacts."

The events of the 1960's are well known and widely documented, and I shall mention them only in passing. Orne (1962), in an influential article on demand characteristics (a term derived from the Lewinian concept of *Aufforderungscharakter*), argued through an ingenious series of demonstration experiments that these inadvertent task-orienting cues could account for spurious findings in laboratory experiments in psychology. He also argued—although this turned out to be an overgeneralization—that the typical subject was altruistically motivated to help science and human welfare in general. Given a strongly felt conflict between their perceptions of demand characteristics and presenting a good appearance, even many volunteer subjects (the typical subjects observed by Orne) were subsequently found to be more ego-defensive than altruistic in their experimental reactions (e.g., Rosnow, Goodstadt, Suls, and Gitter, 1973). It was eventually recognized that because of multiple motives operating prior to, as well as within, any particular experiment (some taking priority over others but not always in a predictable fashion), postexperimental assessment of subjects' perceptions of the meaning of the experiment was often essential in order to ensure the inferential validity of the experimenter's conclusions (Adair, 1982; Adair and Spinner, 1981).

Also influential during this period was Rosenthal's (1966) book on experimenter effects. Rosenthal meticulously documented a substantial amount of evidence showing interactive and noninteractive biasing effects of experimenters. The former, he argued, had to be expressly taken into account when doing laboratory experiments in psychology, especially in view of the possibility that the experimenter's expectancy or tacit hypothesis could serve as a self-fulfilling prophecy of the subject's responses.

In laying claim to the status of a natural science, many experimentalists had proceeded from an idealization that failed to take fully into account the wider

implications of studying a conscious human being within a social setting. However, to the extent that this artifact work (itself largely experimental!) was valid and robust, it appeared that experimenters had a lower or smaller measure of control over the circumstances of their laboratory experiments as a result of systematic errors that derived from the conduct and context of human subject research, than they had formerly believed.

In the case of laboratory experiments, the reason for this diminished control had to do with the important implications in experimental design of the distinction between random and systematic errors. When subjects were randomly assigned to treatment conditions, it could usually be assumed that any differences among the subjects that appeared as random errors would cancel one another. Some subjects might be more intelligent than other subjects, or taller or more sociable, etc., but it was assumed that random assignment would achieve comparability by equating the average unit within each treatment group. However, in the case of subject-and-experimenter-artifacts, the equivalence achieved by random assignment did not automatically mitigate the effects of systematic errors, which were not self-cancelling.

The idea that new ways had to be devised for assessing, reducing and possibly avoiding this problem led to widespread feelings of uneasiness, since many investigators interpreted their suspicions as challenging the trustworthiness of *any* experiment in human psychology. Particularly in social psychology it seemed that experiments were highly susceptible to subject-and-experimenter-artifacts. The experimenter-bias attack, it was said, worried experimenters to the extent that it exerted additional pressure to find simpler problems that could be handled with "doubly buffered precision" (Festinger, 1980). When the artifact assault came into direct conflict with cognitive dissonance theory at the peak of its popularity, the worst fears of many social psychologists seemed confirmed. The conflict resulted when it was asserted that a central result of dissonance theory was not due to the claimed manipulation of dissonance, but instead to the subjects' shared motivation to behave in a way that would not have caused them to be unfavorably evaluated. That is, it was said that they had modified their behavior in order to give the experimenter a favorable impression of themselves (Rosenberg, 1965).

McGuire (1969) discussed the essentially relativistic and ambiguous criteria that led to certain variables being called "artifacts." He proposed that there was a natural progression in the life of an artifact consisting of three stages. In the first stage the researchers seemed ignorant of the variable producing the artifact and denied its existence when the possibility was pointed out to them. In the second stage, which began with the acceptance of the existence of the phenomenon of an artifact, and when its implications became undeniable, the researchers gave a great deal of attention to developing ways of coping with its potential contaminating influence. In the

third stage researchers became interested in the artifact as a theoretical concept in its own right, not as a nuisance variable to be eliminated but as an independent variable with interesting possibilities for revealing new insights about human potentialities. By the late-1960's, it was clear that much of experimental psychology, including experimental-social, had passed out of the ignorance stage and had entered the coping and exploitation stages (Rosenthal and Rosnow, 1969).

Over the following decade, the concept of an artifact entered the mainstream of psychology and the research on artifacts, whatever its real or apparent failings, became common knowledge in this and other fields. This was also a period when concerted efforts were made to patch up the experiment, e.g., by developing integrative models and strategies for dealing with artifacts collectively rather than on an individual basis (Barber, 1976; Finkelstein, 1976; Rosnow and Aiken, 1973; Rosnow and Davis, 1977; Silverman and Shulman, 1970). This is not to say that the use of artifact controls was necessarily widespread, in spite of the fact that the problem of the "social psychology of the experiment" had apparently entered into the thinking of many investigators (cf. Cochrane and Duffy, 1974; Suls and Gastorf, 1980).

The Moral Neutrality Question

Even as experimenters were learning to cope with the artifact crisis, a second wave of criticisms swept over them. Just as a machine is amoral by its very nature, so it was perceived by many researchers that scientific psychology, with its roots in mechanistic natural science, must be value-free to preserve the ideal of objectivity in operational methodology (Leary, 1980). Caught up in the moralistic temper of the late-1960's and 1970's, however, criticisms were now directed against laboratory experiments in several areas of psychology on grounds that they exploited powerless subjects with abusive deceptions (Kelman, 1968, 1972, 1977). Psychology, as indeed all science, had by the mid-1970's changed from an endless frontier to an ideology of limits laid down in self-imposed guidelines and federal rules and regulations (cf. Holton, 1978). "Prior restraint" became the watchword of the decade for researchers who were becoming accustomed to having review boards peer over their shoulders (Gray, Cooke, and Tannenbaum, 1978).

Once more there was a gradual shift away from the absolutism of the traditional conceptualization. Where previously conventional wisdom drew a distinction between the pro-naturalist view of methodological behaviorism with "what is" and the concern of morality with "what ought to be," these matters no longer seemed relevant or valid. Now it was thought by many researchers that such a distinction might represent a false dichotomy set up

as a hypostatization of the tension and discord during the *Methodenstreit*, the battle of the methods, which was carried out during the most formative years of the behavioral and social sciences (cf. Birchall, 1978; Huff, 1981). "To be or not to be" concerned with the moral implications of the ethical conduct of human subject research—Hamlet's question applied to the methodology of scientific psychology—was becoming an overriding consideration of social psychologists and other investigators who felt themselves pressured as they weighed the costs and utilities of each treatment procedure.

Compliance with the new human subjects regulations presented a dilemma for experimenters who also had concerns about minimizing artifacts (Schuler, 1982; Suls and Rosnow, 1981). In part as a consequence of the artifact work, the problem of subjects' knowledge of the true intent of the research had been generally regarded as sufficiently threatening to the validity of much research that deceptions were (and apparently continue to be) routinely employed in social psychology (Gross and Fleming, 1982; Stricker, 1967). However, not only did it seem impossible to many investigators to do some kinds of research because of strictures on the use of deception, thus preventing the possibility of replicating or extending certain classic experiments with new artifact controls, but it appeared that strict compliance with the "ten commandments" of the American Psychological Association code would further exacerbate the problem of demand characteristics and subject role behavior (e.g., Resnick and Schwartz, 1973). Many investigators felt caught in an impossible dilemma between the scientific imperative of validity and the societal imperative of informed consent. McNemar's (1946) dictum about psychology having become a "science of the behavior of sophomores" seemed too sanguine in the light of this new development, which made it appear that scientific psychology was becoming only a science of informed volunteers.

Cost-benefit guidelines developed by professional and scientific organizations began to be widely heralded as a means to analyze the morality of human subject research. Implicit in these analyses was a conceptual model which could be seen as insufficient since it failed to consider the costs and benefits of *not conducting* a given study. The failure to conduct a study that could be conducted was as much an act to be evaluated on ethical grounds as was the conducting of a study. The investigator whose research might reduce violence or racism but who refused to do the research because it involved deception or an invasion of privacy, had not solved an ethical problem but only traded one problem for another. The cost-benefit approach required a balancing of considerations so that society and science did not become equal losers in the resolution of the artifacts-ethics dilemma (Rosenthal and Rosnow, Note 2).

Within the context of this modified analysis, there seemed to be a number

of ways for improving the overall cost-benefit ratio without compromising particular ethical dictates, such as the insistence on the use of volunteer subjects. Investigators were concerned because they were convinced that the validity of their studies might be jeopardized by the use of volunteer subjects. It seemed that there was no way to accurately predict the biasing effect that might result when the population and behavior of interest were not reflected by the volunteer subjects. However, on the basis of research to uncover the reliable differences between volunteers and nonvolunteers, it now seemed possible to predict the nature of some inferential errors resulting from the use of volunteer subjects simply from the evidence at hand (Rosenthal and Rosnow, 1975; Rosnow and Rosenthal, 1976).

To illustrate, suppose an investigator was interested in the effects of some unusual treatment on the dependent variable of psychological adjustment or pathology. Since there was evidence to suggest that volunteer subjects, especially in clinical investigations, tended to be more maladjusted than nonvolunteers, any unusual manipulation designed to increase psychological adjustment should be judged as more effective than it would actually be in the general population. The problem was one of ceiling effects, in that maladjusted subjects had more room in which to improve than did normal subjects who started closer to the ceiling of adjustment. In this case, the use of volunteer subjects would be predicted to increase the likelihood of the type I error.

Just as it seemed possible to shore up the experiment once again, the resolution of this latest crisis was swept under by another tide of criticisms, the universality question. The artifact work had taught social psychologists that the experiment was more human and subjective than many had perhaps believed. The cost-benefit dilemma demolished illusions about the amorality of positivistic psychology as a sociopsychological enterprise. The universality question now fostered a dialogue of ideas regarding the profoundly relativistic nature of the evidential status of sociopsychological facts in general.

The Universality Question

Among those in the forefront of this latest assault was Gergen (1973, 1977, 1978), who in a series of provocative essays laid siege to the empirical basis of social psychology as an objective experimental science. Science, he argued, was characterized by exact nomothetic laws which referred to events that could be recreated in any laboratory. Patterns of human conduct, however, were seen by Gergen to be primarily dependent upon internal, symbolic capabilities that, being unreliable and unpredictable, were beyond the scope of objective scientific investigation. The evidential status of sociopsychological facts, he argued, was like a still-photograph of some fleeting event that

passed into history at the very moment the picture was taken (his "social psychology as history" thesis). The controversy and wide range of responses generated by Gergen's view have been discussed in his recent book (Gergen, 1982). Here I shall attempt to paraphrase only some of the counterarguments directed at two of the more central positions revived by him concerning the prevailing assumptions of social psychology.

One set of counterarguments dismissed Gergen's thesis as merely a throwback to the old claim that the nomothetic bias of the nomological physicalism of scientific psychology had led to the neglect or rejection of the idiographic approach of studying single individuals. To be sure, the contention that each person is unique was itself a generalization and therefore by definition nomothetic in nature (Franck, 1982). However, Gergen attacked the possibility of there being *any* universal laws in social psychology, inasmuch as human experience seemed to him particularistic and time-bound. Those investigators who grasped this aspect of his thesis answered that unrepeatability did not also mean unlawfulness. We can throw coins in the air and get a specific pattern when they fall. We may not be able to repeat the identical pattern, but we can reasonably state a statistical "law" (in the form of a probability table for total number of heads and tails) for the relationship or probability distribution nonetheless. Further, in a much earlier article refuting particular objections such as those revived by Gergen against determinism, Grünbaum (1952) inveighed against the old claim that only human behavior was unique. All particulars in the world are unique, Grünbaum pointed out, whether they are human beings or whether they are physical objects or events like trees or light flashes or ticks of the watch. Certainly every individual is unique by virtue of a distinctive combination of characteristics not precisely duplicated in any other individual, but then so is every tick of the watch unique since no two ticks can be simultaneous with a given third tick.

Another set of counterarguments to Gergen's thesis concerning the pretensions of social psychology to the status of a science asserted that Gergen's was a misconception or limited conception of science, particularly his view that scientific events were constant and repeatable at will as opposed to human events which were in perpetual flux. In the life science of epidemiology, it was noted, researchers had come to understand that powerful drugs to combat virulent diseases could lead to hardier strains of the diseases through spontaneous mutations which then became immune to the drugs. In social psychology, the "diseases" of racism and prejudice might be seen in a similar light. While also requiring a conceptual outlook recognizing that human events, being reactive, were therefore changeable, it was argued that this merely necessitated a widening of the conceptual boundaries of social psychology, perhaps along pre-mechanistic lines, not an abandonment of its scientific aspirations (cf. Rosnow, 1978). Moreover, it was not *always* possible

even in natural science to repeat and authenticate every laboratory event at will. Collins, writing in another context (Note 3), mentioned how British scientists, in the early 1970's, had encountered great difficulty when trying to repeat laser experiments. It turned out that the replication called for tacit knowledge acquired through subjective experience. By analogy, tacit knowledge pertaining to rat handling is different in animal social psychology and pharmaceutical science, in the same way that the definition of "normal science" in these fields requires different things to be measured or held constant.

No matter that there may have been lapses in Gergen's extreme existentialist thesis, his attack on the evidential status of sociopsychological facts struck a resonant chord in many areas. With his revolutionary essays frequently serving as counterpoint or as a springboard, arguments against the old faith ranged from calls for anarchism and methodological nihilism to relativism and rationality. Some social psychologists opted for a new faith based on the idea that facts and truths were like situated actions—to borrow Mills' terminology (1940). Others argued for an ethnomethodological social psychology with emphasis on societal norms and cultural relativism, thereby emulating a similar tide of advance in sociology and social anthropology (cf. Dixon, 1977; Hiley, 1979; Zimmerman, 1978). Still others turned to an alternative model of human beings as rule-followers, to make the argument that social psychology would be better served by modeling itself after some of the aspects of disciplines other than natural science, such as linguistics or hermeneutics. On the other side of this debate, defenders of the traditional optimistic view of experimental social psychology reiterated old claims about the laboratory experiment being the *sine qua non* of science, the argument being that the experiment was like some high-intensity game that magnified interactions and churned up the level of activity in ways simply not possible by other means of investigation (e.g., Berkowitz and Donnerstein, 1982).

So far this discussion has consisted of a recapitulation of more-or-less familiar intellectual developments in an attempt to show how they provoked a reappraisal of the nature of empirical social psychology and the limits of sociopsychological knowledge. In the following discussion I try to show how these developments may also have consequences for a further liberalization of social psychology by edging away from the absolutism and experimental compulsiveness of methodological behaviorism and redefining the field to be consistent with the advances that I have discussed.

Liberalization of Social Psychology

What has become clear to many investigators as a consequence of these critical advances is that the evidential status of sociopsychological facts is not simply commensurate with, or reducible to, a mechanistic view of efficient

causality. What is not clear to many, however, is the actual scientific status of social psychology or whether any conceptual or empirical approach on its own merits might be sufficient to provide an adequate understanding of social behavior. Since the very definition of science has for some time been in a state of disarray, the question of social psychology's status as a science turns out to be an extraordinarily amorphous one.

Previously I alluded to the tradition of empirical investigation which in the late-nineteenth century captured the imagination of Külpe, Ebbinghaus, Titchener, and other students of Wilhelm Wundt. Wundt himself, to be sure, maintained that purely experimental methods were limited to a restricted range of human activities in which the rigorous demands of experimental control and phenomenal authenticity could be adequately met, and he excluded the "higher mental functions" of social (or cultural) psychology from the experimental domain entirely (cf. Blumenthal, 1980; Danziger, 1980; Leahey, 1981; Leary, 1980, 1982). But it was this experimental fetishism of early investigators (Wundt's admonition notwithstanding) which pointed psychologists, and then social psychologists, along a path characterized by what Toulmin and Leary (in press) have called "the cult of empiricism in psychology." One aspect of the further liberalization of social psychology has been the realization that empiricism does not simply mean that the only acceptable generalizations in science are those based on direct observations. Were scientists in any field to proceed only on such a limited assumption, they would never be willing to accept some very fundamental laws of science, e.g., Newton's first law of motion: a body not acted on by any force will continue in a state of rest or uniform motion in a straight line forever. No scientist has ever seen "a body not acted on by any force," much less observed its "motion in a straight line forever" (Cohen, 1931/1978).

I do not mean to imply that a scientific system of sociopsychological laws exists that is in any way comparable in power to Newton's laws of motion. However, one aspect of the liberalization of social psychology seems in part to be based on the assumption that, in view of the artifact problem and thus the uncertainty or indeterminacy of empirical generalization, the evidential status of our scientific laws always requires a leap of faith. The social psychologist's faith, like the physicist's, is not simply the blind faith of the "true believer" who accepts some doctrine of reality out of loyalty or fidelity to a cause, that is, the person who says, "I know I believe this but I'm just not sure exactly why." Nor is it merely the straight-line result of logical truth or rational analysis alone or of a rigorous empirical process of experimental observation.

Instead, it is a faith based on a cumulative (pluralistic) corroboration of evidence, a sense of proportion, and an empathic and coherent aesthetic assessment of the validity basis of human understanding (cf. Apel, 1967; Pepper, 1942/1970). By an aesthetic assessment, I mean (a) that as in the arts

the object of this faith is an abstract image (a likeness) or a representation (a substitution such as a metaphor) of reality and (b) that sociopsychological truths may be as much aesthetic as empirical, emotional as rational. Just as Michelangelo began with an aesthetic idea in creating his *Pieta*, so did Newton in developing his laws of motion and so do social psychologists employ abstract images and representations of reality in constructing their middle-range theories and concepts (cf. Gombrich, 1971; Nisbet, 1976).¹ That scientists normally speak of the "elegance" or "beauty" of their facts or equations or theories suggests that scientific truths always are partially assessed in the context of emotions in relation to aesthetics; that is to say, these assessments are concerned with emotion and sensation as much as with pure observation or pure intellectuality.

Not that art and science are indistinguishable from one another. While both are continually evolving, we do not usually think of the arts as progressive in the "negative" sense that we do of science (Margolis, Note 4). Clearly the explanatory canons of modern science are more advanced and transcend those of earlier periods in being more powerful, more precise, and better integrated. The arts seem to be more cumulative than progressive; that is, one great school seems equally significant as another on critical aesthetic grounds, the primary differences being in terms of style and taste as defined by cyclical sociohistorical denominators. Newton's science, of course, was not only different from Aristotle's but (according to certain criteria) was superior to it. Similarly, social psychology seems progressive in the sense that, like the essentially negative form of progress in other scientific fields (Margolis, Note 4), it advances by destroying much of its own past. It has been recently noted, for example, that none of the early explanatory work on cooperation and competition would be acceptable by current standards because of deficiencies in those studies (experimenter effects, etc.) which were only later uncovered (Deutsch, 1980).

This also reminds us that social psychology, like other sciences, progresses via skepticism, the root of all investigation and discovery, in which hunches are subjected to intellectual and empirical examination rather than subjugated to the dictates of authority. To be relevant for appraising the justifiability of beliefs, doubts must be formulable. The artifact advances discussed previously put investigators in a position to make their doubts formulable as

¹Nisbet (1976) makes the strong assertion that there is a basic unity of the creative act as found in the arts and in the sciences. This process which begins with an abstraction is hardly ever reached through the use of empirical methods, he maintains. However, it may be fair to say that some abstract ideas also evolve as final products that are shaped through a dialectic between observation and common sense. The growth of the very idea of imagination, with its one foot in the empirical and the other in the ideal, would be an example. Engell (1981) notes how this essential idea, which he traces from eighteenth-century British empirical thought through nineteenth-century Romantic art and literature, was a way of uniting the human spirit and emotions with the concrete reality of nature.

quite particular challenges to the truth of what they formerly believed. In this respect, the experience of social psychologists has been no different from the experience in other fields, such as physics, which have undergone their own identity crises.

What can this further reveal to us about what a post-crisis social psychology might eventually look like? Elsewhere I have discussed in some detail how the crisis of confidence experienced by physicists led to greater theoretical and empirical diversity as it was conceded that the mechanical model of Newtonian physics, in spite of limitations, was nevertheless valid within a diminished sphere (Rosnow, 1981). The classical model came to be seen as a "closed theory" which was directly anchored in experiences that set a decisive limit to its field of application, beyond which other concepts and propositions were needed to form the basis of other explanatory systems. The realm of Newtonian theory was thus divided among an array of theories, Newton's mechanical conceptualization being one of these. Each comprehensive formulation was also recognized as an idealization of reality which restricted or stylized reality by foregoing all those features that could not be captured by that particular paradigm.

It is impossible in this space to justice to the visions and specters conjured up by the wide range of alternative ideas in the air in social psychology and related fields. In raising the possibility of a profoundly relativistic and pluralistic shift in social psychology, many investigators still find a role for experimentation but have resorted to new strategies for exploiting it in more insightful ways—there is, of course, an important place for experimentation in testing generalizations and providing pointer readings which help delineate the ecological domain of relationships in the here and now (Mook, 1983; Rosnow, 1981, pp. 98 f.) For example, there is the stress on cognitive responses as mediatory factors during exposure to persuasive communication (Petty, Ostrom, and Brock, 1981), the recent emphasis on sensations as being relative to the contexts in which they are made and the situational dependencies of social attitudes in sensory psychophysics (Wegener, 1982), and, of course, the development of quantitative meta-analysis for reexamining experimental relations (Eagly, 1978; cf. Glass, McGaw, and Smith, 1981; Rosenthal and Rosnow, 1984 for statistical procedures). There has also been a recent proliferation of nonexperimental or nonlaboratory analyses of the impact of historical contexts on human behavior and of the part that social structure must play in explaining behavior (e.g., Argyle, Furnham, and Graham, 1981; Gergen and Gergen, 1983; Guttentag and Secord, 1983; McGuire, 1976; Simonton, 1976).

It is this "contextualist" (or realist interactionist) theme which keeps recurring in books, articles, and talks addressed to possible nonmechanistic alternatives in the behavioral sciences (cf. Georgoudi, 1983; Israel and Tajfel, 1972; Manicas and Secord, 1983; McGuire, 1983; Meyrowitz, in press;

Rosnow, 1981; Sarbin, 1977). By contextualist or realist interactionist, I mean an experiential, interpretive orientation which recognizes the unity, plurality, spontaneity, and ecological dependency of behavior. Human actions, being dynamic, dramatic, active, are thus interpreted within a hierarchical context of a priori (e.g., historical and experiential) and a posteriori (teleological) events and assumptions, according to this essentially explanatory or hermeneutic (as opposed to a strictly predictive or causal) orientation. Like a statement or message that makes complete sense only in terms of the total situation in which it occurs, much human behavior is comprehensible only as part of a knowledge structure consisting in part of social experiences that influence (and are influenced by) the behavior in question.² Where, when, and before whom the behavior is performed, as much as the reasons, purposes, and strictly causal agents of the behavior, are mutually complementary explanatory factors.

There are, to be sure, shared assumptions of the mechanistic and contextualist paradigms. The most basic assumption of both positions is that empirical confrontation is essential for defining or corroborating the validity and meaning of scientific propositions. However, where the mechanistic position envisions empiricism as a justificatory confrontation in which contextual dependencies are treated as mere error variation, the contextualist position regards it more as a discovery process to make clear the meaning of empirical relations relative to the contexts in which they are identified (McGuire, 1983). Together they form a constituency of ideas, and thus excluding either position will give an incomplete picture of the essential relativistic nature of human beings both as active thinkers and knowers but also as reactive organisms in certain circumstances.

Conclusion

The word "crisis," from the Greek *krisis*, a division or separation, implies a crucial turning point. Such a point in the development of modern physics came about when a fundamental postulate of classical mechanics, asserting that each physical system could be localized in time and space, was thrown into doubt. The uncertainty that followed when it was realized that the psychological subjective feeling of space and time was already an invention and not, as Newton and others assumed, something absolute possessing an independent existence, bred further confusion about other fundamental concepts and propositions. The resolution of this crisis of confidence, which led to a reconceptualization of space and time, ultimately resulted in a shifting of perspective rather than in a negation of the mechanistic point of view.

²Such a view invites comparisons with certain classical conceptions both in philosophy and psychology (e.g., Pepper, 1942/1970; Rosnow, 1978, 1981).

In physics this condition was crucial for preparing the way for the promulgation of further abstractions by which it was then possible to redefine the theoretical interstices of natural science. My attempt here has been to try to show how, in perhaps the same way, the succession of changes in the reality shaped out of developments that have already struck at the foundations of much of mechanistic natural psychology might result in a nonmechanistic model of social behavior, or possibly an array of admissible models reflecting a truly pluralistic social psychology that is sensitive to the contexts and mechanics of human experience. The effects of this revolutionary change have only just begun to reverberate in European and American social psychology. It remains to be seen whether they will produce a paradigm shift, a new synthesis, or whether they will be subjugated by adherents to the status quo only to become the rallying cry for revolutions yet to come.

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