The Obsolete Scientific Imperative in Social Work Research

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In a misguided attempt to be scientific, social work has adopted an outmoded, overly restrictive paradigm of research. Methodological rather than substantive requirements determine the subject matter to be studied. As a result, important questions and valuable data go unresearched. The assumptions and postulates of this prevailing "scientific" model of research and evaluation are examined; their roots in the logical empiricist tradition are described; criticisms of these assumptions are discussed; and alternative, less restrictive approaches to research are suggested.

Over the last three decades the profession of social work has increasingly declared its traditional model of research to be insufficiently scientific and has replaced it with discrete canons of scientific acceptability, which are used to evaluate service models and research findings.[1] Although the assumptions underlying these changed criteria for service models and research have been abandoned by most philosophers of science, they are rarely examined or criticized in the social work literature.[2] This paper examines the major assumptions of the prevailing "scientific" model of research and evaluation, describes their roots in the logical empiricist[3] tradition, presents some of the criticisms of these assumptions which have been advanced by philosophers of science, and suggests an alternative approach to the design and evaluation of social work research.

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Because the requirements of logical empiricism have been used prescriptively to limit the permissible range of research questions and data and to define service effectiveness, the issue of their validity is crucial for social work and the social sciences in general. Winifred Chambers describes "the hold of some of the more positivistic demands on the social sciences" and concludes, "While some social scientists have tended to give up hopes of being scientific in the face of these strictures, others have gravitated toward statistics, extreme behaviorism, and purely descriptive studies in an unquestioning effort to meet the rigor and precision of [logical empiricist] standards for a science—at the expense, frequently, of simplifying the issues or narrowing the range of significant inquiries."[4] In his 1975 presidential address to the American Sociological Association, Lewis Coser warns that "preoccupation with method largely has led to neglect of significance and substance. And yet, our discipline will be judged in the last analysis on the basis of the substantive enlightenment which it is able to supply. . . . If we neglect that major task, if we refuse the challenge to answer these questions, we shall forfeit our birthright and degenerate into congeries of rival sects and specialized researchers who will learn more and more about less and less."[5]

Social work has yet to begin the critical examination of logical empiricist assumptions and methodology which is increasingly found in the literature of psychology and sociology.[6] One prevalent assumption is that certain methods of data gathering are "objective," that is, distortion free, and therefore able to generate data which accurately reflect reality. For example, tape recorders, videotapes, and observers not involved in the planning of research or the execution of treatment are often asserted to yield value-neutral, generally truthful data about social interactions. William Reid, for example, says that "the trend toward greater use of direct and electronic observation of program operations, such as individual and group counseling services, can be expected to continue, in response to the need for the most accurate data possible. Most researchers have regarded with skepticism practitioners' records of program events, but until recently little else was available."[7] In this model, data must be observable in certain restricted ways, specifically, they must be converted to measurable form.[8] Behind the requirement of quantification lies the more fundamental assumption that observed and theoretical entities can and should be sharply distinguished. In an article proposing "Criteria for Evaluating Research," Harris Goldstein instructs the reader to "note whether or not the researcher has clearly separated his theoretical concepts from his empirical ones and whether or not he has indicated directly which are which."[9] Similarly, in an article setting forth "Principles of Measurement," Kogan asserts that "the researcher shuttles between the real world and the world of concepts. The real world provides his empirical

evidence, the world of concepts a scheme or map for 'making sense' out of the portion of the real world which he is seeking to account for, explain, or predict. The conceptual scheme or map is known as a theoretical model."[10]

According to the prevailing model of social work research, data which are assumed to be "empirical" or "objective," that is, thought both to mirror reality and to appear similar to all normal people, straightforwardly "ground" theoretical constructs in reality.[11] In consequence, atheoretical research is said to be both possible and desirable. For example, in an article on "Evaluating One's Own Effectiveness and Efficiency," Bloom and Block say, "In the approaching era of ecumenical therapeutic practices, it would seem preferable to have a theory-free measurement procedure. All practitioners, regardless of their theoretical orientation or the specific techniques they use, require objective evaluation to help them attain the goal of their intervention, however defined."[12] In her study of social work outcome research, Katherine Wood asserts that practitioners who bring a preexisting theoretical orientation to their work are unethical: "The worker may not, professionally or ethically, force the data into a preconceived theoretical orientation focused on only one of the [intrapersonal, interpersonal or systems] dimensions—as the researchers and practitioners in many of the studies appear to have done."[13] Wood does not, however, discuss the effects on research and practice of the unquestioned adherence to a rigidly controlling methodology.

A related postulate is that, although research cannot proceed without an attempt to organize these "empirical" data into concepts, these concepts are meaningless unless their direct link to the data from which they derive can be manifested by specific operations or measurements. The very meaning of the concepts is often thought to be synonymous with these operations. According to this postulate, theories and concepts which do not lend themselves to definition by measurement operations cannot be studied fruitfully. Kogan, for example, says that if a theoretical model is to be useful, "there is the necessity of specifying rules connecting at least some part of the conceptual system with the world of reality."[14] Goldstein posits that "scientific methods indicate a preference for concepts which can be defined operationally."[15] This requirement that concepts be definable by quantitative measurement operations has significantly restricted the scope and nature of the questions studied in current social work research. For example, broad treatment and program goals have been criticized as hard to define operationally and, therefore, deemed inferior or unacceptable—an example of the use of methodology to beg a substantive question.[16]

In addition to the assumptions of the unproblematic nature of observation and the need to define concepts in terms of these "reality-based" perceptions, a third important assumption is that good science

inherently involves predictions.[17] Norman Polansky, for example, says, "The whole aim of research in the profession, therefore, is to improve our feeble ability to predict the course of events. Our concern is with knowledge for use; foresight is crucial to effective practice. It is no wonder that successful prediction is the major criterion of whether a law one thinks he has discovered is 'valid.' This is as true for science in general as it is for each profession."[18] Edward Mullen asserts that prediction is even more important than explanation. He criticizes social work practice models for having "too often elevated the goal of explanation over the goal of prediction" and says that, given the imperfect state of our knowledge, we should choose models "that have a good probability of guiding us toward predictable results with our clients, even though these models may not be very satisfying when it comes to understanding causation or the complexities of the interactions we are working with."[19]

Thus the prevailing model of social work research posits a hierarchy of research designs which runs the gamut from least to most scientific and is ordered by the extent to which the criterion of prediction and its concomitant requirements—such as experimental manipulation, control groups, and randomization—are satisfied. As a result, ex post facto research is considered less rigorous than a "true" experimental design and is relegated to exploratory endeavors.[20] Similarly, studies in which the experimental variable cannot be manipulated for other reasons, such as ethical considerations, are not considered good science. The social work research literature is replete with statements to this effect. As early as 1950, Blenkner said that without prediction and experimental manipulation, "nothing is really established except that one has done a more or less adequate job of describing one's sample."[21]

The problem is not that these assumptions about what constitutes good science and hence good social work research never lead to useful knowledge, but, rather, that they are used normatively, rather than descriptively, to prescribe some research methodologies and proscribe others. An example is Reid's prescription for "empirical" models of practice, that is, "models whose general development and case applications are substantially based on 'hard' data, such as measurements of observed behavior. . . . An essential characteristic of these models is their emphasis on phenomena that can be observed and specified with considerable objectivity and precision."[22] Reid's requirement that these models be "testable" makes normative the assumptions of value-free data, operational definitions to connect these data to conceptualizations, and prediction as equivalent to explanation. He describes "testable models" as those "whose targets for change, interventions and outcomes can be operationalized in terms of indicators that can be reliably and validly measured. Such models should also incorporate procedures for obtaining accurate data. To facilitate an understanding

of the connection between input and outcome, practitioners' activities must be capable of producing some change within a limited period of time."[23] The prescriptive nature of current social work conceptualizations of science is exemplified by Reid's suggestion that, rather than have practice determine the form of scientific inquiry, the demands of science, as he perceives them, should determine the nature of practice: "In short, testability should be made an important criterion in determining what social workers should do. Practitioners have always taken the position that the requirements of science should be adapted to existing forms of practice, no matter how difficult it might be to apply scientific methods to the study and improvement of that practice. The author is suggesting that this position be reversed and that service models be adapted to scientific requirements wherever feasible."[24]

Logical Empiricism

The conceptualizations of science which shape the prevailing model of social work research derive most directly from the logical empiricist philosophy of science, which flowered in Vienna in the 1920s and was most in vogue between 1920 and the mid-1950s.[25] The logical empiricists participated in the quest for certainty that characterizes much early twentieth-century philosophy.[26] They hoped to establish epistemological guarantees for science in order to ensure that scientific findings would reflect a reality uncolored by the preconceptions or biases of the human mind.[27] A cornerstone of this effort was the logical empiricists' mistrust of theory and their belief that perceptions of the observable properties of material things were incontrovertible because they directly mirrored reality. Theoretical concepts were considered meaningless unless they were connected to physical observations by logical structures; these structures were understood to be truth preserving, that is, they organized experience without adding to it. Thus the only meaningful statements were either observational or logical. All other propositions, for example, propositions about hidden causes of observables, were considered metaphysical and, therefore, meaningless. From this point of view theories are nothing more than abbreviations for physical observations.

From these epistemological assumptions the logical empiricists derived a number of prescriptions for meaningful scientific inquiry. The most important of these will be summarized briefly.

Correspondence rules (operational definitions).—Because of the belief that, in order to preserve the truth inherent in physical observations, concepts and definitions (theory) must be tied to these observations by

logical operations, the logical empiricists embraced Percy Bridgman's correspondence rules. In 1927 in *The Logic of Modern Physics* Bridgman said, "*The concept is synonymous with the corresponding set of operations.*"[28] Thus operational definitions had three functions in the logical empiricist program: (1) They were thought to define theoretical terms in a truth preserving way by combining observations and logical (mathematical) manipulations; (2) therefore, they were supposed to guarantee the meaningfulness of theoretical terms, and (3) they specified the admissible experimental procedures for applying a theory to phenomena.[29]

The symmetry thesis.—The symmetry thesis asserted that explanation and prediction were formally the same. A scientific explanation was the prediction (deduction) of an event from applicable hypotheses (general laws) plus specific experimental (initial) conditions. The predicted event was thus supposed to be the conclusion of a deductive argument of the syllogistic form: (1) If X (initial conditions), then Y (event to be predicted); (2) X is present; (3) therefore Y (the predicted event) must occur. In a later development, statistics were used to show that, although it might not be possible to demonstrate that an event was necessitated by deductive logic, it could be shown to be highly probable.[30]

The requirement that a hypothesis be "testable" by controlled experiment is merely a statement of the viewpoint that any valuable explanation is in the form of a prediction. The insistence that prediction is the only valid type of explanation assumes, of course, that the universe is governed by causality, that is, that a state is methodologically necessitated by former states and that this necessity is made possible by a law of nature.[31]

The business of science is the justification, not the discovery, of theories.—The logical empiricists argued that scientific logic was deductive logic because deductive logic preserved truth. The only nondeductive part of the scientific enterprise was supposed to be the discovery of hypotheses, which was not considered a process of logic but of psychological creativity. The business of science was to justify theories (test hypotheses by prediction), not discover them; logical empiricists considered the context in which those hypotheses were generated irrelevant. As Carl Hempel, an early member of the Vienna Circle, said, "Although no restrictions are imposed upon the *invention* of theories, scientific objectivity is safeguarded by making their *acceptance* dependent upon the outcome of careful tests. These consist in deriving, from the theory, consequences that admit of observational or experimental investigation, and then checking them by suitable observations or experiments."[32] The emphasis on science as an enterprise of justification had some important consequences. In the first place, the result of the logical empiricists' exclusive focus on the justification rather than the discovery of theories was that their

philosophy of science was normative and prescriptive. It legislated standards for good science which were not and are not met by ongoing scientific work. These standards have been used to criticize practicing scientists rather than to describe actual scientific practice. Second, the history of science was ignored by the logical empiricists, who thought that, because there could be no logic of discovery, the processes by which science advances are the province of the psychologist and the sociologist rather than the philosopher of science.[33]

Reductionism.—If the logic of science is deductive and if theories are merely abbreviations for observations, it follows that complex theories should reduce into simpler ones that are closer to basic observations. William Wimsatt defines the logical empiricist postulate of reductionism as the belief that upper-level (more complex) entities, properties, theories, and laws must be deducible in terms of the properties, laws, and relations of the lower-level (less complex) theory. Upper-level entities are thus seen as "nothing more than" collections of lower-level entities, and upper-level laws and causal relations are understood as "nothing more than" abbreviations for and results of lower-level laws and causal relationships.[34] The belief that social work models can and should be reduced to simplified, quantified, time-limited, experimentally "testable" models without any loss of valuable information is an example of the influence of the logical empiricist belief in reductionism on social work research.

Criticisms of Logical Empiricism

Although the social work research literature continues generally to embrace logical empiricist assumptions and postulates, philosophers of science have long abandoned them as universal principles. Frederick Suppe, who moderated a major conference on the philosophy of science and logical empiricism, concludes,

> Consider the classical philosophical theses that an absolute causal account can be given of phenomena, that ultimate laws of a deterministic sort can be gleaned from natural phenomena, and that some rockbed of perceptual certainty is necessary to gain a firm knowledge of the world. All three of these theses are false and hopelessly out of date in terms of the kinds of theories now coming to dominate science. . . . It is from ancient antecedents in religion and philosophy, not from ordinary experience, that these fallacious doctrines have been drawn and have received sanction for so long a time.[35]

Similarly, Wimsatt says, "Many of the philosophical assumptions on which the emphasis on statistical and quantitative methodology have

been based have been challenged and often given up by many recent philosophers of science. These challenges do not deny the usefulness of such approaches, or even their necessity to answer some kinds of questions, but rather challenge the idea that any useful research must conform to these methodological constraints."[36]

Criticisms of the theory-observation distinction.—The logical empiricist attempt to guarantee the truthfulness of science by positing a sharp distinction between theory and observation and taking physical observations to be unimpeachable representations of reality has been attacked by most contemporary philosophers of science (and even abandoned by one of the architects of logical empiricism, Carl Hempel). The most common argument against the existence of inherently truthful observations is that there can be no direct or untainted perceptions because all observation is shaped by theory.[37] The weltanschauung philosophers of science[38] argue that all science proceeds from one's world view and, therefore, "one's knowledge and beliefs may influence what facts one is able to determine observationally. Persons accepting different theories for a given range of phenomena thus may be able to observe different facts, and so may disagree on what the facts are which a theory must accommodate."[39] People holding different theories about the same object do not even see the same thing. Norwood Hanson suggests that at dawn Tycho Brahe would believe the sun rose and Kepler would think the earth moved so the sun came into view. If perception is theory dependent, observation is not epistemologically "safe." As David Hull points out, "No 'fact' is sufficiently brute and pristine to be infallibly insulated against the possibility of error. For example, no observation seemed more direct and free from interference than the observation that the earth did not move."[40] Anthropologists and psychologists call the belief in the validity of sense data "phenomenal absolutism," which they define as "one ubiquitous and misleading attribute of naive conscious experience, namely, that the world is as it appears,"[41] either to the unaided human perceptual apparatus or through the most elaborate of instruments. Optical "illusions" are cited as proof that the experiences of objectivity and certainty which accompany visual perception are often misleading and also give no clues to the inferences on which they rest.[42] Anthropologists, linguists, and psychologists have documented cultural differences in perception.[43] Also, studies have shown that when people blind since birth have their sight restored as adults, they cannot comprehend but must learn to interpret visual stimuli.[44]

The logical empiricists postulated that theory and observation could be sharply differentiated, but philosophers now argue that this distinction, too, is theory dependent and untenable. For example, the observation language of one epoch of physics is the theoretical language of another: Rigid bodies are observable entities in classical physics and

theoretical entities in relativity theory. Subjective states are observable entities to psychodynamically oriented investigators and theoretical (even metaphysical) entities to methodological behaviorists. Furthermore, theory determines what is to count as an instrument of observation, and, as new instruments of observation are acknowledged or old ones are discarded, the categories of observation and inference expand and contract accordingly. Jerry Fodor says,

> The line between observed and inferred entities is just as hard to draw as the line between observation and inference. It is, indeed, the same line. I think that there is a strong temptation to say that the larger viruses are not inferred entities any more—specifically because of the electron microscope. Yet it is possible to maintain that the argument from shadows on the plate to viruses on the slide is fully as complex as the argument from spectroscopic results to vegetation on Mars. What we decide to call an observation is in part determined by what we feel comfortable about calling an instrument of observation.[45]

Some psychologists and social workers reject empathy as a valid instrument of observation, while other investigators in their fields argue that empathy yields important and reliable information.

In modern physics, as the physicist-philosopher David Bohm emphasizes, even the distinction between instrument and observed entity is problematic: "The 'quantum' context calls for a new kind of description which does not make use of the potential or actual separability of 'observed object' and 'observing apparatus.' Instead, the form of the experimental conditions and the content of the experimental results have now to be one whole, in which analysis into disjoint elements is not relevant."[46] An eminent contemporary physicist, Arthur Eddington, concludes, "We have found that where science has progressed the farthest, the mind has but regained from nature that which the mind has put into nature. We have found a strange footprint on the shores of the unknown. We have devised profound theories, one after another, to account for its origin. At last we have succeeded in reconstructing the creature that made the footprint. And Lo! It is our own."[47]

One implication for the social sciences is that sharp distinctions between data and data gatherer may be impossible. As psychologists are beginning to recognize, participant observers in the field are not inherently any more unreliable than "disinterested" researchers in the laboratory.[48] The question becomes which kind of observer will produce more fruitful information about a particular problem or theory.

The abandonment of the assumptions (1) of the unimpeachability of physical observations, (2) of a sharp distinction between theory and observation, and (3) of a sharp distinction between observer and observed has far-reaching consequences for the prevailing model of social work research. In an ironic attempt to make social work research more scientifically acceptable, many authors have unquestioningly em-

braced as doctrine presuppositions that the "hard" sciences abandoned years before. For example, it has been clear since 1927 that quantum theory cannot be understood in terms of observable entities.[49]

Social work's attempt to establish a dichotomy between values and knowledge, between what William Gordon calls a picture of the world "as it *is*," "derived from the most rigorous interpretation [one] is capable of giving to the most objective sense data he is able to obtain," and a picture of the world as we "wish or fantasy or prefer it to be"[50] is based on the now discredited belief that certain sense data are epistemologically privileged. Contemporary philosophers see values, or subjective responses to events, as an inherent part of knowing or science. If what the logical empiricists call facts are reports rather than direct representations of physiological experiences,[51] then "objectivity" can be seen to be a particular species of report or viewpoint in which the observer is regarded as a constant. The "objective researcher" is thus as much a fiction as is value-free knowledge. Electronic and similar observational means accomplish a high level of reproducibility, but this is not equivalent to truth. It is frequently asserted that verification of factual assertion rests on a high degree of observer consensuality, but, as Edward Bixenstein points out, "This is tautological if verity is a matter of what humans are persuaded, and does not inhere in events."[52] Thus replication is not the safeguard many authors have supposed, particularly in the usual case in which the replicators have the same expertise and interests. Robert Rosenthal, a psychologist who has done pioneering studies of the biases inherent in the experimental method, says, "Within any area of behavioral research the experimenters come precorrelated by virtue of their common interests and any associated characteristics. Immediately, then, there is a limit placed on the degree of independence we may expect from workers or replicators in a common vineyard."[53] A contemporary definition of objectivity is suggested by Paul Schmidt, who says, "Relative to some chosen frame of reference identical empirical descriptions will be given by trained scientists. . . . Objectivity does not refer to how nature really is but to how scientists find it in a given context."[54]

The "objective" recording of behavior is a convention in which not only is the researcher regarded as a constant, but also the subject matter and the method of investigation are seen as separate. However, social facilitation researchers like Gadlin and Ingle have found that "the experimenter, whether physically present or electronically represented, functions as an audience (spectator) who influences the behavior of his or her subject in ways that extend well beyond the structure of the experiment and the manipulation of variables."[55] Other sources of experimenter effects include biosocial characteristics (such as age, sex, or race), attributes (such as anxiety, hostility, warmth, and status) and the experimenter's expectation (hypothesis). For example,

one study demonstrated the power of experimenter expectation by presenting subjects with nonsensical geometric figures to which "correct" and "incorrect" labels had randomly been assigned. Experimenters had the key but were warned not to let this influence their presentation. To a significant degree the subjects chose the items marked "correct" on the key.[56]

If the convention of assigning the researcher a constant function is misunderstood as reality, not only will significant bias go unnoticed, but a fruitful source of knowledge, the inevitable interaction between researcher and client, will go unstudied.[57]

Criticisms of operationalism.—The logical empiricists thought that the primary function of operational definitions was to connect observed and conceptualized (inferred) entities so as to guarantee that concepts preserved the truth inherent in physical observation.[58] Therefore, the entire rationale for defining concepts operationally falls with the abandonment of the belief in theory-free and value-free observation. In addition, philosophers have leveled other, more specific criticisms against the requirement of operationalism. If, as Bridgman said, "the concept is synonymous with the corresponding set of operations,"[59] the number of concepts is multiplied beyond all reason. We certainly do not want to say that weight is one concept when measured with a balance scale and another when measured with a spring scale.[60] Also, it takes theory to decide when an operation is the same and when it is not: Is an IQ test taken with 200 people the same operation as one taken by oneself with an examiner? Ned Block and Gerald Dworkin explain that "operationalism goes wrong in construing as a linguistic stipulation a theoretic inference that a particular interaction between a thing and a device is a measurement."[61]

Given the impossibility of establishing epistemological guarantees for science, the restrictions required by operationalism guarantee nothing and proscribe much potentially fruitful inquiry. As David Hull puts it, "Operationalism was intended as a cathartic to purge physics of all non-empirical wastes, but it proved to be so strong that the viscera were eliminated as well."[62]

Criticisms of the symmetry thesis.—Contemporary philosophers have roundly attacked the logical empiricist doctrine that explanation and prediction are formally the same and, therefore, that there is no true scientific explanation without prediction. Jaegwon Kim, Stephen Toulmin, and others point out that, although prediction and explanation share the same form, they have very different functions: prediction proves the existence of an event, whereas explanation helps us understand it. Explanation, Toulmin argues, is a much more important scientific undertaking than prediction. Although theories are used to predict, their main function is to provide explanations of recognized regularities. Prediction or forecasting "is a craft or technology, an

application of science rather than the kernel of science itself."[63] The Babylonians were able to predict certain natural events with astounding accuracy, even though their explanations of these events were thoroughly inaccurate.[64] Also, explanation is more useful than prediction, because only explanation can tell us how to operate under changing conditions.[65]

One implication of recognizing the different functions of explanation and prediction is that ex post facto designs are no longer deemed inherently inferior. Many sciences achieve understanding without prediction or retrodiction. For example, in evolutionary theory the object of scientific inquiry is not an event (such as the extinction of a species) but the search for relevant premises. Many scientific explanations inherently lack the power to predict—such as explanations of the occurrences of earthquakes, the emergence of a new biological species, or the phenotype of second generation pea plants—but once the event occurs it can be explained.[66] The behavioral sciences are often taken to task because human behavior does not lend itself to specific predictions. But behavioral science theory may provide valuable explanations for individual or group behavior without necessarily being able to predict the exact timing or nature of this behavior.

Another shortcoming of the symmetry thesis is its dependence on an outmoded notion of causality. In other words, prediction is not explanatory if the predicted event cannot be shown to be caused by the experimental conditions. However, neither causality nor noncausality can ever be proved because we can never be sure we have exhausted all possible explanations. Henry Margenau describes causality as "a methodological, nonempirical regulative maxim which belongs to the metaphysical domain."[67] In the eighteenth century, David Hume concluded that causality was a projection of our conditioned expectations,[68] and contemporary philosophers like Norwood Hanson agree that causality, like observation, is theory dependent: "Causes certainly are connected with effects, but this is because our theories connect them, not because the world is held together by cosmic glue."[69] Einstein wanted to believe that "God does not play dice."[70] Stephen Hawking, whose work on black holes has made him one of the most respected contemporary physicists, asserts that "not only does God throw dice, He throws them where they cannot be seen."[71] Furthermore, prediction is impossible in much of modern physics, a development which has drastically eroded classic notions of causality. Hanson says that elementary-particle theory "requires that the nucleus of every unstable isotope be identical with every other nucleus of that type. . . . But these nuclei delay in an unpredictable way (another part of the theory requires that); so the decay cannot be conceived of as a caused event. . . . This leads physicists to say unrepentant things about the collapse of the law of causality in modern science."[72] Quantum mechanics thus ele-

vates chance to the status of a fundamental principle of nature and introduces a new kind of causality which does not allow the prediction or entail the predetermination of one event on the basis of another but, rather, couples probability distributions of whole aggregates of events. The name of this new relation is stochastic or statistical causality.[73]

One final criticism of the symmetry thesis is that it does not correspond to the way in which scientific theories actually evolve. For example, if practicing scientists had embraced prediction as the only adequate form of explanation, Einstein's equation of mass and energy ($E = mc^2$) would not have been accepted until the atom was split in 1932.[74]

As the classic notion of causality becomes less and less tenable, logical empiricists such as Hempel have tried to salvage the symmetry thesis by demanding that, if an explanation did not necessitate an event, it must at least predict it with a high degree of probability (usually the probability has to be greater than .95). Wesley Salmon persuasively argues that since prediction and explanation have different functions, statistical probability, no matter how high, will not necessarily turn a prediction into an explanation. Salmon's examples include the highly probable but nonexplanatory predictions that if we bang pots and pans, the tigers will stay out of Times Square, and that if a man takes his wife's birth control pills, he will not become pregnant. He suggests that statistics be evaluated by the criterion of statistical relevance: "Statistical explanations need not be regarded as inductive arguments, and . . . a high probability is not required for a correct statistical explanation. If a high probability is not the desideratum, what can we offer as a substitute? The answer is statistical relevance. . . . To say that a certain factor is statistically relevant to the occurrence of an event means, roughly, that it makes a difference to the probability of that occurrence—that is, the probability of the event is different in the presence of that factor than in its absence."[75]

Criticisms of the emphasis on justification.—Another tenet of logical empiricism which has been generally abandoned by contemporary philosophers is that science should be concerned only with the justification, not the discovery, of theories because there is a logic of verification but no "logic of discovery." However, the logic of science is not a purely deductive process in which consequences are deduced from hypotheses and checked against observation. When we assert that observational evidence supports a hypothesis, the hypothesis is a conclusion, not a premise. As Salmon says, "The inference *from* observational evidence *to* hypothesis is surely not deductive. If this point is not already obvious it becomes clear the moment we recall that for any given body of observational data there is, in general, more than one hypothesis compatible with it."[76] Thus all scientific activity, including hypothesis testing, can be shown to embody induction. Induction itself

is no guarantee of truth, as evidenced by the ever-present possibility that a counter example will falsify a generalization.

Karl Popper attempts to dispense with the problem of induction by substituting falsifiability rather than confirmation of hypotheses as a method of verification that will be entirely deductive. However, deductive inferences are specific and descriptive rather than generalizable, so that the moment Popper begins to choose between unfalsified hypotheses by turning to evidential corroboration, he necessarily employs induction rather than deduction. Salmon concludes, "The basic trouble with the hypothetico-deductive inference is that it always leaves us with an embarrassing superabundance of hypotheses. All of these hypotheses are equally adequate to the available data from the standpoint of the pure hypothetico-deductive framework. Each is confirmed in precisely the same manner by the same evidence."[77]

Other philosophers attack the assumption that it is possible to make sharp distinctions between discovery and justification. Brown, for example, says that "when we credit Galileo or Newton or Einstein or Bohr with having made scientific discoveries, we only consider those hypotheses which they had good reasons for entertaining to be discoveries. The context of justification is thus part of the context of discovery and no sharp line can be drawn between discovery and justification."[78]

Criticisms of reductionism.—Wimsatt argues that the logical empiricist postulate of reductionism not only ignores the scientist as decision maker and problem solver but also sets in place a series of biases that are very difficult to detect because they are built into its assumptions. He identifies what he calls biases of conceptualization, in which the world is divided into the system being studied and external forces in accordance with interest, intuition, or jurisdictional criteria (a molecular geneticist is unlikely to consider social forces as part of the subject matter of his discipline). He says that the impossibility of analyzing exhaustively or exactly the behavior of the system in its environment and the consequent need for simplifying assumptions result in biases of observation and experimental design and also in biases of model building and theory construction. A reductionist is thus led to understand the behavior of his system in terms of the interaction of its parts as he conceives them.[79] These biases are perpetuated by inertia, perceptual focus, and perceptual reinforcement. "Inertia" refers to the fact that some assumptions are so common that they are not examined, particularly because these assumptions are likely to result in increased analytic tractability.[80] Perceptual focus is described by Wimsatt as follows:

> Model-building activity is performed against a background of presumed mechanisms operating in the interaction of presumed units. If the presumed units are very well entrenched in a given area, there is a strong tendency to describe and to think about even phenomena at other levels of organization in

terms of these units. In traditional evolutionary theory and even at present, the most obvious unit is the individual organism—the unit which our everyday thought and our perceptual apparatus naturally predisposes us to consider. . . . Consequently, there is a strong tendency to see, and to talk about groups of organisms as *collections of individuals, rather than as unitary entities*. This is true even for colonies of social insects, whose interdependencies extend even to reproductive specialization, making the metaphor of the colony as an organism perhaps more revealing in evolutionary terms than the view of it as a collection of organisms.[81]

Wimsatt also identifies perceptual reinforcement, the phenomena that "one bias may act in such a way as to hide the fact that another bias is a bias, and conversely."[82]

The assumption that complex models should be translated into simpler ones for research purposes is made by social work authors who propose that complex treatment models and situations can be simplified or reduced to time-limited treatment situations and quantifiable indicators of complex psychological events without a substantial loss of information or a significant change in the subject under study. However, as Hanson points out, "Complexity is not confusion. When analysis results in destroying complexity in the name of clearing up confusions, to that extent it destroys the concept in question."[83] Wimsatt concludes, "Now for pragmatic as well as for theoretical reasons, reduction in science is better seen as the attempt to understand the explanatory relations between different levels of phenomena, each of which is taken seriously in its own right, than as an unending search for firm foundations at deeper and deeper levels in which, as Roger Sperry so aptly put it, '. . . eventually everything is explained in terms of essentially nothing.' "[84]

In his 1979 presidential address to the American Association of Sociologists, H. M. Blalock cautions against "an important kind of temptation, namely that of substituting relatively simple operational indicators for theoretical constructs without paying careful attention to the underlying measurement model and required simplifying assumptions,"[85] and concludes that "I do not believe we can simultaneously achieve generality, accuracy, and simplicity. Therefore we must give up one or another of these desirable characteristics. If we opt for simplicity, and if social reality is in fact complex, we shall inevitably be misled."[86]

Prescriptions and proscriptions.—The normative tone of much research literature derives from the logical empiricist belief that its methodology could provide a truth guarantee for scientific results and weed out unscientific (nonwarrantable) results. However, the logical empiricist model is itself not open to test because, claiming to be prescriptive rather than descriptive, it disallows actual examples from the history or current practice of science as falsifying instances of the model. Wimsatt

argues that "models of scientific activity, even normative ones, should be subjected to empirical tests like any other scientific hypotheses, in this case by detailed analysis of cases from the history of science and from current science."[87] When this has been done, the logical empiricist model shows little or no relationship to how scientists actually work. According to Herbert Simon, concerns with how scientists ought to proceed are "interesting questions of philosophy, but they turn out to have relatively little relation to the actual behavior of scientists—and perhaps less normative value than has been supposed."[88] Similarly, Wimsatt suggests that we must "follow the Kantian maxim: 'Ought implies can.' "[89] If a model of scientific endeavor requires computational power far beyond our means, such a model cannot be accepted as having normative import because it would be irrational to follow it. Any acceptable normative model must provide a more accurate description of scientific capability.

In addition, because of the problems with "objectivity" described above, no single epistemology or methodology can guarantee truth in science. Michael Scriven insists that "there is no possibility that the social sciences can be free either of value claims in general or of moral value claims in particular, and the arguments which suggested that, for their own good they should be, were themselves metascientific value claims."[90]

The social work research literature has generally embraced the prescriptive approach of logical empiricism. Instead of recognizing that each body of knowledge has its own problems and that a good design for one is not necessarily the best for another, many social work authors have prescribed unitary standards for good science.[91] This approach has arguably inhibited social work from developing a discipline-specific body of knowledge. A similar retardation in theory development has occurred in sociology[92] and, as Jerry Fodor points out, in psychology:

> Psychological metatheory has remained seriously underdeveloped. With a few important exceptions, its history during the second quarter of this century has been an attempt to work out a variety of behaviorism that would satisfy the constraints imposed on psychological explanation by an acceptance and application of empiricist (and particularly operationalist) views of general scientific method. The better known accounts of psychological explanation have thus often failed to reflect the most important movement in current philosophy of science: the attempt to determine the consequences of rejecting key features of the empiricist program. . . . These have recently come into question among philosophers of science who have realized that these doctrines are by no means indispensable to characterizations of scientific explanation and confirmation and that philosophical accounts that exploit them may in fact seriously distort the realities of scientfic practice. Yet it is upon precisely these views that much of the implicit and explicit metatheory of American experimental psychology appears to rest.[93]

Both the failure of practitioners to utilize research and the tension between researcher and practitioner, the subject of numerous articles,[94] are products of this normative approach. Rather than advocate that research be tailored to actual practice, authors have demanded "objectivity," "hard" data, time-limited treatment, simplified models, and predictive designs, with the result that there has in fact developed a genuine, though unnecessary, conflict between service and research goals.

Alternatives

Numerous alternatives to the logical empiricist view of science have been suggested, all of which reject the logical empiricist assumption that the truthfulness of scientific results can be guaranteed. Consequently, contemporary philosophers concentrate on the practicing scientist as decision maker and problem solver rather than on the derivation of absolute prescriptions for all science. Wimsatt says, "We cannot have an adequate philosophy of science without putting a realistic model of the scientist as decision maker and problem solver back into our model of science."[95] The focus should be functional and dynamic rather than normative and, as Chambers says, "move away from the simple question of whether to apply or withhold the term 'scientific' and place it more appropriately on issues as to what kinds of insight . . . sciences can offer us and how their investigations can be furthered."[96] Thus the primary consideration is not whether or to what extent a theory is correct but whether it is an improvement—not whether this theory is better confirmed but whether it is better.[97]

An alternative approach which embraces and accounts for such complexities has been developed by Herbert Simon, who won a Nobel Prize for his work. Simon concludes that "scientific discovery is a form of problem solving,"[98] and, using computer analogs, he shows that very few problems of interest to scientists are amenable to formal procedures, that is, to algorithms (ways of systematically finding the solution that is best or maximal by some criterion).[99] He suggests a "principle of bounded rationality" which asserts that almost all significant problems are of such complexity that we cannot solve them exactly, and that, therefore, in order to find workable solutions, we introduce various simplifying and approximate techniques which he calls "heuristics":[100] "Problem-solving searches are selective in that they generally explore only a miniscule fraction of the total (and usually immense) number of possibilities. In most cases of interest, the selection of the paths to be

searched is not governed by foolproof, systematic procedures, but by rules of thumb we call *heuristics*."[101] Examples of heuristics include the decisions to employ only data which are quantitatively measurable and to consider motor behavior superior to verbal behavior as a validating criterion of constructs.[102]

Unlike the logical empiricists, Simon includes "unmanageable" problems in his view of science by concluding that they can be resolved even though they are not formally solvable: "In most problem-solving domains of everyday life, however, and even in many formal ones, like chess . . . a modest number of possible solutions can be considered, and there is no way of telling whether a given solution is the best, since many other possibilities must, perforce, go unexamined. In these domains, human problem solvers and the computer programs that simulate them do not search for the 'best' solution, but for a solution that is 'good enough' by some criterion. Heuristics that proceed on this basis are sometimes called 'satisficing' heuristics."[103] Because the failures and errors producing any heuristic are not random but systematic, once a heuristic is understood, it can be made to fail: "Given this knowledge of the heuristic procedure, we can construct classes of problems for which it will always fail to produce an answer, or for which it will always produce the wrong answer. This property of systematic production of wrong answers will be called the bias(es) of the heuristic."[104] Modern philosophers of science recognize that these biases are inevitable and, consequently, concentrate on reducing rather than eliminating them. Wimsatt says, "There is no cookbook way of removing, detecting, or correcting sources of bias. One can lie with statistics as easily (and often, unfortunately, far more convincingly!) as without them, and major studies by the best people in the field (whatever the field) still turn up with their share of flaws. Science cannot be made error-free. The most we can hope for is the kind of critical interaction among people with different biases that will make each of us better aware of our own biases."[105]

Levins suggests that bias can be reduced by the "search for robust theorems." To counteract biases in any given model, he proposes building families of alternative models of a given phenomenon based on different simplifying assumptions. Because of their different assumptions, these models will produce different consequences and predictions, but there may be consequences which are true for all models. Levins calls those results that appear independently of the details of any particular model "robust theorems" and concludes that "our truth is the intersection of independent lies."[106] Wimsatt points out that two disadvantages with Levins's approach to reducing bias are, first, that we may be in a situation in which not even one model is available, and, second, when a number of models make a well-disguised assumption, it is not always possible to tell if models are in fact independent.[107]

Wimsatt suggests that another way of reducing scientific bias is "multilevel reduction analysis," in which, because the system-environment boundary is changed in going from one level to another, the biases of the same heuristics will lead to different simplifications when applied to a system at different levels of abstraction and organization.[108] For example, in the kind of "objective" data gathering advocated in the current social work literature, the generally unrecognized heuristic is the view of the researcher or his electronic agent as a constant and the client and his world as the only object of study. While this heuristic is valuable for certain purposes because it is designed to enhance reproducibility, never to expand the system-environment boundary to include the researcher-client relationship is to deprive social work of much valuable data and an important corrective to the bias inherent in the "experimental" heuristic. Because of social work's historic emphasis on the person in his situation, it is particularly ironic that more attention has not been paid to the psychosocial processes of the investigator-client relationship—not, it should be emphasized, with an eye to eliminating bias therefrom but for the purpose of gleaning new and significant knowledge about social treatment or planning. Practitioners are not inherently any more biased in their description of treatment processes than are tape recorders or observers. Practitioners and tape recorders introduce different biases, and for many evaluative purposes, the bias accompanying electronic recording may be much more detrimental to our understanding than that resulting from a practitioner's reports.

Psychologists concerned with this issue have stressed the valuable information that can be gained by treating clients as informants rather than as uninformed subjects.[109] Martin Orne, an investigator who has extensively studied subjects' conscious and unconscious attempts to respond to what they perceive as the demands of the experimental situation, concludes, "It never fails to amaze me that some colleagues go to the trouble of inducing human subjects to participate in their experiments and then squander the major difference between man and animal—the ability to talk and reflect on experience."[10]

It should be clear that the belief that theories can easily be compared because everyone is talking about the same data in the same language cannot be universally assumed. Suppe shares the pessimism of the weltanschauung philosophers about the possibility of assessing the merits of competing theories: "The proponents of two competing theories will be unable to agree on which facts the competing theories must accommodate if they are to be adequate. . . . Which facts are relevant to assessing the adequacy of a theory will be a function of which aspects of the phenomena the theory describes and which questions it is committed to answering. It is perfectly conceivable that the proponents of different competing theories may disagree on what sorts

of questions ought to be answered by an adequate theory for a particular range of phenomena."[111]

Conclusion

A crucial difference between the logical empiricists and contemporary philosophers of science is that, while the former tried to reduce scientific problems and methodology to simplified and supposedly "safe" proportions, the latter try to reduce inevitable bias by studying as many aspects of a problem from as many vantage points as possible (by comparing the work of different individuals, focusing on different levels of abstraction or different environment-system boundaries, or employing different theoretical models, etc.). If scientific objectivity is inherently impossible, contemporary models, which attempt to account for biases in the context of recognizing and including complexities, seem obviously superior to the logical empiricist attempt to solve epistemological problems by simply declaring much of the work of practicing scientists unscientific. Blalock stresses that sociologists must stop trying to eliminate bias and instead recognize that "the essential point is not that assumptions can or should be avoided but that they need to be made explicit. . . . Each measurement strategy requires the use of theoretical assumptions, only some of which can be tested."[112]

This paper has described the extent to which social work and behavioral science authors who insist on accepting as truly scientific only that research comprised of "hard" data, "objective" measurements, operational definitions, and experimental (predictive) designs base their prescriptions on logical empiricist assumptions and postulates. The adoption of the logical empiricist view of science has had the grave consequence of prohibiting researchers from studying many important questions, using much valuable data, and researching social interactions in all their complexities. If current alternatives to the logical empiricist program are adopted, such as those being developed by Mitroff, Salmon, Simon, Wimsatt, or the weltanschauung philosophers, social work and other behavioral science researchers will no longer try to meet logical empiricist requirements for science by studying only those problems and theories which meet these requirements. Rather, they will select research questions because of their importance to the field and will determine appropriate data and methods of data gathering not on the basis of unquestioned assumptions about "objectivity," but on their rational relation to the theory or problem under study. Explanation will be recognized as having a separate function from prediction, and criteria for prediction will not be applied to

explanation. Therefore experimental (predictive) designs will not be regarded as inherently superior and ex post facto designs as inherently inferior; rather, designs will be adapted to the theory or problem in question, and the value of a design will be determined by whether it provides a useful explanation of significant phenomena. Statistics will be used to explain, not "prove," an event; therefore the test of whether statistics are significant will not be high probability but "statistical relevance"—whether a factor makes a difference in the probability that an event will occur.

Furthermore, rather than prescribe a single acceptable methodology in a quixotic attempt to eliminate bias, researchers will embrace different theories, methodologies, levels of focus (macro or micro), and kinds of data and data gathering. This will reduce inevitable bias by rotating the perspective on the system under investigation.[113] Finally, research will be conceptualized descriptively and functionally rather than normatively—it will adapt to relevant problems rather than dismember them to fit notions of good research.

Because of its failure to promise bias-free results, this alternative to the logical empiricist view may make some researchers uncomfortable. In the face of the findings of the last two decades of the philosophy of science, however, the desire to cling to logical empiricist theories and methodology can be understood psychologically as the wish for a certain, knowable world but should not be mistaken for proof that such a world exists. Like physics, genetics, and mathematics, social work and the other behavioral sciences must accept that reality cannot be perceived either directly or in its full complexity and, therefore, that science represents our best efforts at solving important problems for which there can be no guaranteed or permanent solutions. In Margenau's words, "If the history of science teaches anything at all it is that there are no eternal verities which man can grasp and hold forever."[114] We must learn to live with and use rather than continue to deny the complexities and ambiguities inherent in scientific activity.

Notes

1. See, e.g., Joel Fisher, "Evaluating the Empirical Base of Clinical Practice" (Occasional Paper no. 1, Jane Addams College of Social Work, Chicago, 1979), pp. 7–8; Mary Macdonald, "Social Work Research: A Perspective," in *Social Work Research*, ed. Norman Polansky (Chicago: University of Chicago Press, 1960), pp. 1–23; and Helen Witmer, "Science and Social Work," *Smith College Studies in Social Work* 14 (1943): 222–30.

2. Although these assumptions are widely accepted, some social work authors have questioned them. See, e.g., Aaron Beckerman, "Differentiating between Social Research and Social Work Research," *Journal of Education for Social Work* 14 (1978): 9–15; John Crane, "Utilizing the Fundamentals of Science in Educating for Social Work

Practice," *Journal of Education for Social Work* 2 (1966): 22–29; Samuel Finestone and Alfred Kahn, "The Design of Research," in *Social Work Research*, rev. ed., ed. Norman Polansky (Chicago: University of Chicago Press, 1975); Henry Maas, "Research in Social Work," *Encyclopedia of Social Work*, 17th ed. (Washington, D.C.: National Association of Social Workers, 1977), 2:1183–93; Martin Rein and Sandra Tannenbaum, "Social Science Knowledge and Social Work Practice," in *Sourcebook on Research Utilization*, ed. Allen Rubin and Aaron Rosenblatt (New York: Council on Social Work Education, 1979), pp. 189–219; and Joseph Vigilante, "Between Values and Science," *Journal of Education for Social Work* 10 (1974): 107–15.

3. Logical empiricism derives from empiricism, the belief that all certain knowledge comes from experience, and from logicism, whose central tenet is that propositional logic (which includes mathematics) is truth preserving (Harold Brown, *Perception, Theory and Commitment: The New Philosophy of Science* [Chicago: University of Chicago Press, 1977], pp. 15–29). The term "logical empiricism" has been chosen over "logical positivism" because it is more inclusive. Logical positivism refers specifically to an early and even more extreme form of logical empiricism, whose major doctrine is the verification theory of meaning—the thesis that a proposition is meaningful only if it can be empirically verified, that is, if there is an empirical method for deciding if it is true or false. The difficulty is that "scientific laws, which are formulated as universal propositions, cannot be conclusively verified by any finite set of observation statements" (ibid., p. 23). The logical positivists who became logical empiricists gave up the strict verificationist theory of meaning and instead adopted the requirement that all meaningful propositions had to be tested by observation and experiment (ibid., p. 23).

4. Winifred Chambers, "Clinical Interpretations and the Debate over the Scientific Acceptability of Psychoanalysis," (Ph.D. diss., University of Chicago, 1975), pp. 2–3.

5. Lewis Coser, "Presidential Address: Two Methods in Search of a Substance," *American Sociological Review* 40 (1975): 698.

6. E.g., see James K. Cole, ed., *Nebraska Symposium on Motivation*, vol. 23 (Lincoln: University of Nebraska Press, 1976); Daryl Bem and Andrea Allen, "On Predicting Some of the People Some of the Time: The Search for Cross-situational Consistencies in Behavior," *Psychological Review* 81 (1974): 506–20; Roy Bhaskar, "On the Possibility of Social Scientific Knowledge and the Limits of Naturalism," *Journal for the Theory of Social Behaviour* 8 (1978): 1–28; Edward Bixenstein, "The Value-Fact Antithesis in Behavioral Science," *Journal of Humanistic Psychology* 16 (1976): 35–57; H. M. Blalock, "Presidential Address: Measurement and Conceptualization Problems," *American Sociological Review* 44 (1979): 881–94; Coser; Howard Gadlin and Grant Ingle, "Through the One-way Mirror: The Limits of Experimental Self-Reflection," *American Psychologist* 30 (1975): 1003–9; Michael Gorman, "Towards a Unification of Physics and Psychology," *Etc* 35 (1978): 400–407; Patrick Horan, "Is Status Research Atheoretical?" *American Sociological Review* 43 (1978): 534–41; Russell Keat, "Positivism, Naturalism, and Anti-Naturalism in the Social Sciences," *Journal for the Theory of Social Behaviour* 1 (1971): 3–17; Brian Mackenzie, "Darwinism and Positivism as Methodological Influences on the Development of Psychology," *Journal of the History of Behavioral Sciences* 12 (1976): 330–37; Ian Mitroff, "Psychological Assumptions, Experimentation, and Real World Problems," *Evaluation Quarterly* 2 (1978): 235–59; Thomas Olshewsky, "Dispositions and Reductionism in Psychology," *Journal for the Theory of Social Behaviour* (1975); 129–44; Joseph Royce, "Psychology Is Multi," in Cole, ed., pp. 1–63; Duane Schultz, "Psychology: A World with Man Left Out," *Journal for the Theory of Social Behaviour* 1 (1971): 99–107; Laurie Wiseberg, "The Statistics Jungle: Measuring War, Plague, Fire and Famine," *Society* 12 (1975): 53–60.

7. William Reid, "Developments in the Use of Organized Data," *Social Work* 19 (1974): 590; see also Anne Shyne, "Casework Research; Past and Present," *Social Casework* 43 (1967): 467–73. The point here is not that electronic or third-party observation is never valuable but that it is erroneously asserted to be the only credible source of therapeutic observation. Interestingly, although the methodological behaviorists followed the logical empiricists in eliminating subjective experiences from the scope of science, the radical behaviorists are moving toward a recognition of the need to include subjective experiences and perceptions in scientific accounts of human behavior; see, e.g., John and Janet Baldwin, "Behaviorism on Verstehen and Erklaren," *American Sociological Review* 43 (1978): 335–47.

8. See, e.g., John Schuerman, "On Research and Practice Teaching in Social Work," in Rubin and Rosenblatt, eds., p. 145; Jerry Turem, "Research Priorities in Social Work Education: A Communication to Colleagues," in ibid., pp. 33–35; and Katherine Wood, "Casework Effectiveness: A New Look at the Research Evidence," *Social Work* 23 (1978): 451.

9. Harris Goldstein, "Criteria for Evaluating Research," *Social Casework* 43 (1962): 476.

10. Leonard Kogan, "Principles of Measurement," in Polansky, ed., rev. ed. (n. 2 above), pp. 71–72.

11. Stuart Kirk and Joel Fischer, "Do Social Workers Understand Research?" *Journal of Education for Social Work* 12 (1976): 63. See also Harry Butler, Inger Davis, and Ruth Kukonnen, "The Logic of Case Comparison," *Social Work Research and Abstracts* 15 (1979): 4.

12. Martin Bloom and Stephen Block, *Social Work* 22 (1977): 130. An example from the sociological literature is the claim that status attainment research is theory free. For a good discussion and rebuttal see Horan (n. 6 above).

13. Wood, p. 453. See also Lillian Ripple, "Problem Identification and Formulation," in Polansky, ed. (n. 1 above), pp. 24–47. For an example from the psychological literature of the belief that operational ("response invariant") variables are value free, see Lois Shawver, "Research Variables in Psychology and the Logic of Their Creation," *Psychiatry* 40 (1977): 1–16.

14. Kogan, p. 72.

15. Harris Goldstein, "Making Practice More Scientific through Knowledge of Research," *Social Work* 7 (1962): 110.

16. E.g., see Wood; and Martin Kushler and William Davidson II, "Using Experimental Designs to Evaluate Social Programs," *Social Work Research and Abstracts* 15 (1979): 27–32. For a critique of this viewpoint, see Ian Mitroff and Thomas Bonoma, "Psychological Assumptions, Experimentation, and Real Life Problems," *Evaluation Quarterly* 2 (1978): 235–59; and Robert Weiss and Martin Rein, "The Evaluation of Broad-Aim Programs: Experimental Design, Its Difficulties, and an Alternative," *Administrative Science Quarterly* 15 (1970): 97–109.

17. See, e.g., Harris Goldstein, *Research Standards and Methods for Social Workers*, rev. ed. (Wheeling, Ill.: Whitehall Co., 1969), p. 7, and Norman Polansky, "Research in Social Work: Social Treatment," in *Encyclopedia of Social Work*, 17th ed., 2:1206.

18. Norman Polansky, "Introduction: Social and Historical Context," in Polansky, ed., rev. ed., p. 2.

19. Edward Mullen, "The Evaluation of Social Work Progress" (Occasional Paper no. 1, Jane Addams College of Social Work, Chicago, 1979), pp. 24–25.

20. See, e.g., Richard Stuart, "Research in Social Work: Social Case Work and Social Group Work," in *Encyclopedia of Social Work*, 17th ed., vol. 2; Peter Rossi, "Research in Social Work: Social Policy," in ibid., 2:1204; Francis Caro, "Research in Social Work: Program Evaluation," in ibid., 2:1201; David French, *An Approach to Measuring Results in Social Work* (New York: Columbia University Press, 1952); and Kushler and Davidson, pp. 27–32. When a social work author recommends a "quasi-experimental" design, it is nearly always because a "scientific," i.e., "experimental," design cannot be used for some logistical reason or because the state of a particular body of knowledge is not considered sufficiently developed for "rigorous" testing, rather than because the author doubts the hierarchical ordering of research designs (see, e.g., Ann Shyne, "Evaluation in Child Welfare," *Child Welfare* 55 [1976]: 5–18). Mitroff and Bonoma make the same point about Campbell and Stanley's discussion of quasi-experimental designs (pp. 246–48).

21. Margaret Blenkner, "Obstacles to Evaluative Research In Casework: Part II," *Social Casework* 31 (1950): 99. See also Aaron Rosen and Enola Proctor, "Specifying the Treatment Process: The Basis for Effectiveness Research," *Journal of Social Service Research* 2 (1978): 25–26.

22. Reid (n. 7 above), p. 589.

23. William Reid, "Social Work for Social Problems," *Social Work* 22 (1977): 377.

24. Ibid., p. 378.

25. See n. 3 above.

26. E.g., Bertrand Russell, Alfred North Whitehead, Gottlob Frege, the phenome-

nologists, the early Ludwig Wittgenstein, and Alfred Ayer all sought unsuccessfully a certainty that would solve the epistemological ambiguities stemming in most troublesome form from the Kantian paradigm that true reality can never be known because sense perceptions are invariantly distorted by the imposition of innate mental categories. For a good summary, see W. T. Jones, ed., *A History of Western Philosophy* (New York: Harcourt Brace Jovanovich, Inc.), vol. 4, *Kant to Wittgenstein and Sartre*, 2d ed. (1975).

27. Bixenstein (n. 6 above), p. 38.

28. P. W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan Co., 1927), p. 5.

29. Frederick Suppe, "The Search for Philosophic Understanding of Scientific Studies," in *The Structure of Scientific Theories*, ed. Frederick Suppe (Urbana: University of Illinois Press, 1974), p. 17.

30. Brown (n. 3 above), pp. 58–60.

31. Henry Margenau, "The Philosophical Legacy of Contemporary Quantum Theory," in *Mind and Cosmos*, ed. Robert Colodny (Pittsburgh: University of Pittsburgh Press, 1966), p. 353.

32. Carl Hempel, "Recent Problems of Induction," in Colodny, ed., 116.

33. Dudley Shapere, "Meaning and Scientific Change," in Colodny, ed., p. 46.

34. William Wimsatt, "Reductionistic Research Strategies and Their Biases in the Units of Selection Controversy," in *Scientific Discovery: Case Studies*, ed. Thomas Nickles (Hingham, Mass.: D. Reidel Publishing Co., 1980), p. 214.

35. Suppe (n. 29 above), p. 283; see also Suppe, "Theory Structure," in *Current Research in Philosophy of Science*, ed. Peter Asquith and Henry Kyberg (East Lansing, Mich.: Philosophy of Science Association, 1979), pp. 317–38; and Gunther Stent, "Limits to the Scientific Understanding of Man," *Science* 187 (1975): 1052–57.

36. Wimsatt to Edward Mullen, spring 1978, School of Social Service Administration, University of Chicago.

37. Norwood Hanson, "Logical Positivism and the Interpretation of Scientific Theories," in *The Legacy of Logical Positivism*, ed. Peter Achinstein and Stephen Barker (Baltimore: Johns Hopkins University Press, 1969), p. 74. Some social work authors disclaim the existence of pristine observations while they simultaneously embrace logical empiricist tenets based on the assumption of theory-free observation; see, e.g., Goldstein, *Research Standards and Methods for Social Workers* (n. 17 above), p. 17.

38. This group includes Thomas Kuhn, Paul Feyerabend, and Norwood Hanson. For a discussion of this viewpoint see Suppe, ed., pp. 125–90.

39. Suppe, "The Search for Philosophic Understanding of Scientific Theories," p. 212.

40. David Hull, "The Operational Imperative: Sense and Nonsense in Operationalism," *Systematic Zoology* 17 (1968): 445.

41. Marshall Segall, Donald Campbell, and Melville Herskovits, *The Influence of Culture on Visual Perception* (Indianapolis: Bobbs-Merrill Co., 1966), p. 4.

42. Ibid., p. 6.

43. Ibid., see also Benjamin Whorf, "Science and Linguistics," in *Psycholinguistics: A Book of Readings*, ed. Sol Saporta (New York: Holt, Rinehart & Winston, 1961), pp. 460–67.

44. Ibid., p. 79.

45. Jerome Fodor, "Materialism," in *Materialism and the Mind-Body Problem*, ed. David Rosenthal (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971), p. 132.

46. "Science as Perception-Communication," in Suppe, ed., p. 382.

47. Quoted in Morris Kline, *Mathematics: The Loss of Certainty* (New York: Oxford University Press, 1980), p. 341.

48. See, e.g., Robert Rosenthal, *Experimenter Effects in Behavioral Research* (New York: Appleton-Century-Crofts, 1966). See also n. 55 below.

49. Margenau, p. 351. See also Harold Morowitz, "Rediscovering the Mind," *Psychology Today* 14 (1980): 12–19; and Steven Rosen, "Toward a Relativization of Psychophysical Relativity," *Perceptual and Motor Skills* 42 (1976): 843–50. For an excellent nontechnical summary of recent developments in physics and their philosophical implications, see Gary Zukav, *The Dancing Wu Li Masters: An Overview of the New Physics* (New York: William Morrow & Co., 1979).

50. William Gordon, "Knowledge and Value: Their Distinction and Relationship in Clarifying Social Work Practice," *Social Work* 10 (1965): 34.
51. Bixenstein (n. 6 above), p. 35.
52. Ibid., p. 50. See also, Albert Scheflen, "Classical Biases and the Structural Approach to Research," *Etc* 34 (1977): 290–313.
53. Rosenthal, p. 324. See also Rosenthal, "Interpersonal Expectations: Effects of the Experimenter's Hypothesis," in *Artifact in Behavioral Research*, ed. Robert Rosenthal and Ralph Rosnow (New York: Academic Press, 1964): 182–279; and Rosenthal, "Biasing Effects of Experimenters," *Etc* 34 (1977): 253–64.
54. Paul Schmidt, "Models of Scientific Thought," *American Scientist* 45 (1957): 148.
55. Gadlin and Ingle (n. 6 above), p. 1007. See also Leo Goldman, "A Revolution in Counseling Research," *Journal of Counseling Psychology* 23 (1976): 543–52; Milton Rosenberg, "The Conditions and Consequences of Evaluation Apprehension," in Rosenthal and Rosnow, eds., pp. 280–350; Schultz (n. 6 above); Paul Wachtel, "Psychodynamics, Behavior Therapy, and the Implacable Experimenter," *Journal of Abnormal Psychology* 82 (1973): 324–34; and Wachtel, "Investigation and Its Discontents," *American Psychologist* 35 (1980): 399–408.
56. Rosenthal, *Experimenter Effects in Behavioral Research*, p. 135.
57. Ibid., p. 401.
58. See, e.g., Hull (n. 40 above), pp. 438–57.
59. Bridgman (n. 28 above), p. 5.
60. This extreme version of operationalism is often modified so that different operations need not always result in different concepts, but this attempt to salvage operationalism merely introduces the kind of theoretical judgments it was supposed to eliminate, such as those necessary to decide which operations measure the same concepts; see, e.g., Norman Polansky, "Introduction: Social and Historical Context," in Polansky, ed., rev. ed., pp. 23–24.
61. Ned Block and Gerald Dworkin, "IQ: Heritability and Inequality," *Philosophy and Public Affairs* 3 (1974): 352.
62. Hull, p. 440.
63. Stephen Toulmin, quoted in Suppe, ed. (n. 29 above), p. 128.
64. Stephen Toulmin, *Foresight and Understanding* (New York: Harper Torchbooks, 1961), p. 30.
65. William Wimsatt, seminar at the School of Social Service Administration of the University of Chicago, 1978.
66. Jaegwon Kim, "Inference, Explanation, and Prediction," *Journal of Philosophy* 61 (1964): 365.
67. Margenau (n. 31 above), p. 340.
68. David Hume, *On Human Nature and the Understanding*, ed. Antony Flew (New York: Collier Books, 1962), pp. 76–91.
69. Norwood Hanson, *Patterns of Discovery: An Inquiry into the Conceptual Foundations of Science* (Cambridge: Cambridge University Press, 1958), p. 64.
70. Quoted in Nigel Calder, *Einstein's Universe* (New York: Viking Press, 1979), p. 141.
71. Quoted in *Science News* 117 (February 16, 1980), p. 104.
72. Hanson, p. 92.
73. Margenau, p. 353.
74. Calder, p. 14.
75. Wesley Salmon, *Statistical Explanation and Statistical Relevance* (Pittsburgh: University of Pittsburgh Press, 1971), pp. 10–11.
76. Wesley Salmon, "The Foundations of Scientific Inference," in Colodny, ed. (n. 31 above), p. 153. See also Clyde Noble, "Philosophy of Science in Contemporary Psychology," *Psychological Reports* 35 (1974): 1239–46.
77. Ibid., pp. 249–50. For a thoroughgoing critique of falsifiability, see Hilary Putnam, "The 'Corroboration' of Theories," in *Philosophy As It Is*, ed. Ted Honderich and Myles Burnyeat (New York: Penguin Books, 1979): 349–80; and Ian Mitroff, "Systems, Inquiry and the Meanings of Falsification," *Philosophy of Science* 40 (1973): 255–76.
78. Brown (n. 3 above), p. 130.
79. Wimsatt, "Reductionistic Research Strategies and Their Biases in the Units of Selection Controversy" (n. 34 above), pp. 230–35.

80. Ibid., p. 248.
81. Ibid., p. 249.
82. Ibid., p. 250.
83. Hanson (n. 37 above), p. 77. The issue of reduction is an important one in sociology because the question of whether sociology can or should be reduced to psychology is obviously a critical one; see Murray Webster, "Psychological Reductionism, Methodological Individualism, and Large Scale Problems," *American Sociological Review* 38 (1973): 258–73.
84. Wimsatt, "Reductionistic Research Strategies and Their Biases in the Units of Selection Controversy," p. 252.
85. Blalock (n. 6 above), p. 883.
86. Ibid., p. 882.
87. William Wimsatt, "Reduction and Reductionism," in Asquith and Kyberg, eds. (n. 35 above), p. 7. See also Ian Mitroff and Tom Featheringham, "On Systemic Problem Solving and the Error of the Third Kind," *Behavioral Science* 19 (1974): 383–91.
88. Herbert Simon, "Scientific Discovery and the Psychology of Problem Solving," in Colodny, ed., p. 23. When it is taken into account, the history of science indicates that scientific methodology, including definitions of logic and rigor, are time and culture dependent; see Kline (n. 47 above) and Larry Laudan, "Historical Methodologies: An Overview and Manifesto," in Asquith and Kyberg, eds., pp. 40–54.
89. Wimsatt, "Reduction and Reductionism," p. 7.
90. Michael Scriven, "Logical Positivism and the Behavioral Sciences," in Achinstein and Barker, eds., p. 201.
91. See n. 21 above.
92. Coser (n. 5 above), pp. 691–700.
93. Jerry Fodor, *Psychological Explanation: An Introduction to the Philosophy of Psychology* (New York: Random House, 1968), pp. xiv–xv.
94. See, e.g., Sidney H. Aronson and Clarence C. Sherwood, "Research vs. Practitioner," *Social Work* 12 (1967): 89–96; Scott Briar, "Toward the Integration of Practice and Research," in *Future of Social Work Research*, ed. David Fanshel (Washington, D.C.: National Association of Social Workers, 1980): 31–37; Francis Caro, "Evaluative Researchers and Practitioners: Conflicts and Accommodation," *Journal of Research and Development in Education* 8 (1974–75): 55–62; Reid, "Social Work for Social Problems" (n. 23 above), p. 378; Fredrick Seidl, "Making Research Relevant for Practitioners," in Fanshel, ed., pp. 53–62; and Edwin Thomas, "Research and Service in Single-Case Experimentation: Conflicts and Choices," *Social Work Research and Abstracts* 24 (1978): 20–31.
95. Wimsatt, "Reduction and Reductionism," p. 8.
96. Chambers (n. 4 above), p. 3.
97. Wimsatt, seminar (n. 65 above).
98. Simon, p. 22.
99. Herbert Simon, "Thinking by Computers," in Colodny, ed., p. 15.
100. Wimsatt, "Reductionistic Research Strategies and Their Biases in the Units of Selection Controversy," p. 220.
101. Simon, "Thinking by Computers," p. 12. See also Amos Tversky and Daniel Kahneman, "The Framing of Decisions and the Psychology of Choice," *Science* 211 (1981): 453–58.
102. In the behavioristic heuristic, intentionality is not considered, and the fact that we can lie with gestures as well as with words is rarely mentioned; see Else Frenkel-Brunswick, "Psychoanalysis and the Unity of Science," *Proceedings of the American Academy of Arts and Sciences* 80 (1954): 271–347 and Olshewsky (n. 6 above).
103. Simon, "Thinking by Computers," p. 16.
104. Wimsatt, "Reductionistic Research Strategies and Their Biases in the Units of Selection Controversy," p. 220.
105. Wimsatt to Mullen (n. 36 above).
106. Quoted in Wimsatt, "Reductionistic Research Strategies and Their Biases in the Units of Selection Controversy," p. 251.
107. Ibid., pp. 251–52.
108. Ibid., p. 252.
109. Gadlin and Ingle (n. 6 above), p. 1008.

110. Martin Orne, "Demand Characteristics and the Concept of Quasi-Controls," in Rosenthal and Rosnow, eds. (n. 53 above), p. 153.
111. Suppe, ed., p. 211.
112. Blalock, p. 888.
113. C. W. Churchman and Ian Mitroff, among others, have attempted to systematize and conceptualize different perspectives on scientific inquiry. They suggest at least five different approaches to science, each of which has its particular strengths and weaknesses; see, e.g., Churchman, *The Design of Inquiring Systems* (New York: Basic Books, 1971); and Mitroff, "Systems, Inquiry, and the Meanings of Falsification" (n. 77 above).
114. Margenau (n. 31 above), p. 355.