Recent developments in the history, philosophy, psychology, and sociology of science raise serious challenges to our traditional notions about the decisive power of experiments in the development of scientific knowledge. These developments suggest that the power of an experiment is only as strong as the clarity of the basic assumptions which underlie it. Such assumptions not only underlie laboratory experimentation but social evaluation research as well. A dialectical methodology is proposed for assessing the influence of key assumptions in both settings. Among other conclusions, analysis of the role and influence of key assumptions suggests an additional source of experimental error, termed the error of the third kind, or E III. E III is defined and discussed as the probability of conducting the "wrong" experiment when one should have conducted the "right" experiment.

PSYCHOLOGICAL ASSUMPTIONS, EXPERIMENTATION, AND REAL WORLD PROBLEMS

A Critique and an Alternate Approach to Evaluation

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Every science, no matter how well-advanced or developed, rests upon a base of key, "zero-level" assumptions. Because these assumptions play such a vital role in directing inquiry, and because it is impossible to verify each of these base postulates with any complete or

AUTHORS' NOTE: *The authors wish to express their gratitude to their wives and their graduate students, two classes of individuals especially good at reminding us of our background assumptions. Order of authorship was randomly determined; contributions were equal.*

EVALUATION QUARTERLY, Vol. 2 No. 2, May 1978

final assurance (Duhem, 1914; Popper, 1962), it is important to articulate and confront them from time to time to assure ourselves that they remain plausible within a continually changing context. In certain cases, some assumptions are regarded as so basic that we believe they are in need of virtually continual inspection and challenge.

We would venture to suggest that psychology has gone too long between reexaminations of its key ideas regarding the concept of the experiment. Recent developments in the history of science (Brush, 1974; Westfall, 1973), the philosophy of science (Churchman, 1971; Feyerabend, 1975), the social psychology of science (Mitroff, 1974a, b; Mitroff and Fitzgerald, 1977), and operations research (Mason, 1969; Sagasti and Mitroff, 1973), when taken together, serve to literally force a critical reexamination of many of our ideas regarding basic research via experimentation. However, given that the broad foundation of experimental method underlies almost every psychological subdiscipline, the scope of such a reexamination is an unwieldly one without some a priori restrictions. Because of our own concerns and interests, the bulk of this paper concentrates on experimentation and quasi- experimentation as it is currently employed in social psychology.

The choice of social psychology as an exemplar is not random. Social psychology encompasses a particularly interesting applied focus along with the more traditional practice of "basic" research (Meehl, 1972). It provides a satisfying cross-sectional slice of research settings as a bonus. In the form of social evaluation research (e.g., Streuning and Guttentag, 1975) and the corollary methodology of quasi-experimental tactics, social psychology has taken its first tentative steps beyond the laboratory toward the accumulation of scientific knowledge about the processes and outcomes of social programs (e.g., Wortman, 1975). In addition, there exists a small but continuing body of less evaluatively focused social psychological field studies on both micro- and macro-social processes. Since social evaluation research has its feet buried in the same logic of experimentation, we reexamine some basic ways of thinking about quasi-experimentation and other "real world" knowledge-gathering tactics as well.

In brief, this paper serves to challenge, or at least direct some needed critical attention toward the basic assumption that experiments are adequate arbiters of cause-and-effect linkages, no matter how well designed or executed. To the contrary, we argue that much rethinking is in order regarding the confidence we place in experiments with respect to their power of disconfirming our theories. Next, we attempt to show that "applied" social psychological research, primarily the program evaluation variant, is neither that different in terms of its experimental base nor in its ordinary conduct (as has been contended by some) from "basic" or laboratory research, and therefore, that these unusual conclusions about experiments apply in the evaluation area as well. The Campbellian validity notions about quasi-experimental tactics as these apply to program evaluation are challenged as adequate mechanisms for monitoring data-gathering in applied settings. Finally, we present some alternative methods for "doing science," especially real world evaluation, under the changed set of zero-order assumptions we advocate.

Lest we be misunderstood at the outset, we are in no way calling for the abandonment of the concepts and methodologies of the controlled experiment. Nor are we intending to downgrade the substantive accomplishments of those who, like Campbell (e.g., Campbell and Stanley, 1963), have so creatively contributed toward our concepts and methods of knowledge accrual and have directly faced the research problems engendered by a sticky, gelatinous reality. Rather, our examination of social psychology's base assumptions calls for a broadening of our ways of thinking about experimentation. To borrow a term from Chomsky (1957, 1966), we wish to explore the "deep structure" of experimentation which both plagues yet constitutes the richness of scientific inquiry.

THE MYTH AND THE ASSUMPTION OF THE PURE EXPERIMENT

In recent years considerable attention in the philosophy of science has been focused on what has come to be known as the Duhem or D-thesis for short (Duhem, 1914; Laudan, 1965; Mitroff, 1973; Quinn, 1969; Wedeking, 1969). According to the nineteenth-century historian of science Duhem (1914), and contrary to the scientific thinking of his age, there could be no such thing as a completely decisive, crucial falsifying scientific experiment. While according to the then-prevalent view of science, it was felt that there could be no conclusive *verification* of an hypothesis, it was felt that there could be a conclusive *falsification* of an hypothesis (Popper, 1965). Falsification supposedly followed the simple schema of logical reasoning known as *modus tollens*:

$$\text{If } H \rightarrow 0 \text{ (by hypothesis)}$$
$$\text{and if } \sim 0 \text{ (from experimental data)}$$
$$\text{therefore } \sim H \text{ (by modus tollens).}$$

That is, given a certain hypothesis, H, which implied that if H were true we ought to observe a set of consequences, 0, and also given that upon carrying out a proposed experiment one observed not 0 or ~ 0, then one was justified by modus tollens (one of the valid forms of logical reasoning known as the "rule of negative inference") in concluding that the hypothesis H was false or that $\sim H$ was true.

Duhem forcefully pointed out that the above reasoning was a gross oversimplification and distortion of the processes of science. The scientist never tests a single hypothesis in isolation from other hypotheses. When the scientist tests an hypothesis, he by necessity tests it against a background of auxiliary hypotheses, assumptions, and even underlying metaphysical ideas (Popper, 1962). Duhem argued that a more accurate representational scheme for falsification should read as follows:

$$\text{If } (\Sigma H_i) \rightarrow 0 \text{ (by assumption and theory)}$$
$$\text{and if } \sim 0 \text{ (by experimental test)}$$
$$\text{therefore } \sim (\Sigma H_i) \text{ (by modus tollens).}$$

That is, when the observations of experiment (~ 0) were in disagreement with the prior predictions of theory (0), all that one was legitimately entitled to conclude was that *someone* of the H_i making up the *system* of hypotheses, possibly infinite in number, was false. But, which particular hypothesis was in error, if it was indeed only one, could never be determined with complete assurance by experiment alone.

Duhem's point was not that the falsification of scientific theories and hypotheses never occurred, but rather that disconfirmation was an inherently ambiguous process. If the scientist wanted to falsify a single hypothesis, then by necessity he either had to assume or to know by some other means than experiment alone the truth of all the other possible hypotheses entering into the experiment. Since the number of background assumptions was potentially infinite, very few assumptions could be tested out prior to the conduct of the actual experiment itself.

Duhem's position could well be summarized by saying that experimental research gives rise to as many contentious policy disputes, if not more, than it attempts to settle. Scientists with different theoretical orientations could well be expected to entertain different background assumptions (e.g., gestalt versus structuralist versus functionalist), a fact which would prevent the resolution of even directly contradictory results.

Duhem went further and argued that even if the hypothetical situation of a single hypothesis H which implied a behavioral consequence 0 were true, experiments would still be inconclusive. Assume for example that there existed the result of some experiment which demonstrated $\sim$ 0 to be the case. Even in this hypothetical situation, Duhem claimed the scientist is justified in concluding $\sim$ H *if and only if* he could show that there existed *no* set of plausible auxiliary assumptions which could "save the hypothesis."

The scientist must show that there existed *no* additional plausible hypothesis A such that the system of (H + A) was sufficient to imply $\sim$ 0. Duhem neither asserted nor meant to imply that it was possible to find an "hypothesis-saver A" for every H, but rather that "The burden of proof is on those who deny H to show that there does not exist an A which would make H compatible with $\sim$ 0." And, "unless [one can prove that there does not exist a suitable A], a scientist is logically justified in seeking some sort of rapproachment between his hypothesis and the uncooperative data" (Laudan, 1965: 298). Unless one can show that there is no suitable A, a scientist is justified in the continued pursuit of his favorite hypotheses (Mitroff, 1974a, b) even though there might be a considerable body of negative data which argue against them. If anything, it appears that it is social (e.g., peer pressure—David, 1971) and psychological factors, rather than mere logical ones alone which eventually halt the continued pursuit of an hypothesis (Mitroff, 1974a, b). These include theoretical fads and disenchantments, peer evaluation of one's interests, and even what the grant agencies will support as "interesting" work.

The conclusion that emerges is that one of science's most basic processes, hypothesis testing and the supposed gradual elimination of false ideas, has a certain fundamental open-endedness to it. If experiments were irrefutably conclusive, and if observations by themselves implied or allowed any single unambiguous interpretation (Feyerabend, 1975; Hanson, 1965), then the rejection or the acceptance of an hypothesis would be a clear-cut matter (Churchman, 1971; Mitroff, 1973). Conversely, all scientists would be eventually forced to the same common interpretation of results and of necessity, eventually hold the same background assumptions.[1]

The effects of the D-thesis cause us to be more skeptical of the contentions of authors like Rossi who believe in the "the singular power of controlled experiments as *the* model of evaluation studies" (1972: 29, our emphasis). Our conclusion is not that controlled experiments are worthless in themselves and thereby irrelevant either to social science or to evaluation. Rather, the compelling force and power of every controlled experiment is no better than our ability to inspect and to raise challenges to the underlying background assumptions on which it rests. As Churchman has put it: "There is no such thing as a 'crucial test' in empirical science unless the [scientist] is willing to make some very strong systematic presuppositions" (1971: 136).

As much as we have needed a methodology for designing controlled experiments, our conclusion is that we may need even more a methodology for uncovering and analyzing the effect of different underlying background assumptions. We shall say more about what such a methodology might look like after we have confronted another implicit zero-level assumption of more recent origin.

THE ASSUMPTION OF BASIC DIFFERENCES BETWEEN SOCIAL PSYCHOLOGY AND SOCIAL EVALUATION

Generally, evaluation research may be defined as the conduct of social scientific inquiries, usually in the context of some institution, corporation or agency, where the investigatory purpose is a functional assessment or tracking of some unit subsystem (Bonoma, 1976). Social or public (i.e., tax-based) sector evaluation correspondingly focuses on some program (Cook, 1966; Grobman, 1970; Moores, 1973; Taylor, 1973; Wortman, 1975), policy (Evans, 1972; Weiss, 1972, 1973; Wozniak, 1973) or service provided by a "social" agency (DuBois and Mayo, 1970). The functional assessment sought is an index addressing the efficiency of service delivery or program operation. In both cases, "the essence of evaluation is attribution" (Evans, 1972: 634), where attribution is understood as a trained observer's scientifically guided judgment about program worth (cf. Scriven, 1967) or program design (Wortman, 1975).

It has become somewhat fashionable in social evaluation literature to emphasize the uniqueness of evaluational undertakings as compared to traditional social psychological inquiry (cf. Streuning and Guttentag, 1975). We have no quarrel with those (e.g., Sechrest, 1973; Proshansky, 1974) who would separate social evaluation from the remainder of social psychology, beyond a degree of dismay and a portion of fear. The dismay is generated by any new attempt to shatter our already fractionated discipline still further. The fear, though, is caused by a recognition of pendulum-swing oscillations between proponents of either "side" of the sort that Hull once characterized (in another context) as "metaphysical and theological controversy" (1935: 492). It is true that those with evaluation concerns historically have been adjudged as less "mainstream" than basic researchers (cf. Marx and Hillix, 1963; Samuels, 1973). It is equally true that, primarily through the efforts of social evaluation pioneers (e.g., Argyris, 1970; Lewin, 1951; Scriven, 1967), " 'applied concerns' . . . are no longer seen as the sordid options of mental cripples" (Koch, 1971: 672). However, just such a history makes the hypothesis plausible that current segregatory attempts may represent less de natura differences in problems settings than the zealous slogans of a new and rising sect.

A review of the literature (Bonoma, 1976) identifies four major issues purporting to demonstrate substantive differences between the settings, problems, and conduct of social evaluation and traditional social psychological researchers. In overviewing these, Bonoma attempted to apply some obvious tests of homomorphism. He claimed that if social evaluation writers are correct in their conclusion of noncomparability with traditional experimental excursions, their pursuits would be seen to involve different (unique) sorts of settings and encounter different (noncomparable) kinds of difficulties. If, however, the issues cited by evaluation specialists appear to occur also in the traditional domain, even if with differential absolute frequencies or emphasis, we may conclude that the two efforts are homomorphic (i.e., pattern-matched; they "map") and thus are not *substantively* different. In other words, the differences are of degree, not of kind. We briefly review Bonoma's arguments here.

The first issue raised cites the attributive and advocative nature of social evaluation pursuits as compared to traditional ones (cf. David, 1971). It is claimed that evaluations are specifically designed to operationalize the articulated values and goals of a program. They are, therefore, explicitly conducted in order to judgmentally determine to what extent these values have been satisfied. Since traditional research is said to aim toward being "objective" and "value-free," social evaluation should be regarded as a conceptually distinct endeavor from traditional research (e.g., Charlesworth, 1973; Guttentag, 1973; Koen, 1973; Krause and Howard, forthcoming; Weiss, 1973).

The issue is both difficult and tenuous since it disappears in all but the most extreme polar comparisons between the two settings. Further, earlier evaluation writers have amply demonstrated their ability to carry out attributive research within the traditional mold (cf. Scriven, 1967). Once it is appreciated (as the preceding discussion of the D-thesis helps to make clear) that traditional research is never value-free, then it is not a question of values versus no-values, but of making explicit our value biases in research or else suffering the consequences (Barber and Silver, 1968). Also, in both traditional and social evalution pursuits, the cycle of inquiry is the same: deduction of hypotheses (from a theory or program), test, and subsequent modification (of the theory or the program). Both are attributive, both are value-bound, and both employ the standard "inquiry cycle" (Marx and Hillix, 1963) in pursuit of knowledge. On this "judgmental attribution" issue, "basic" social psychology and social evaluation appear to be homomorphic.

The second argument contends that there exists a multiplicity of concerned parties to social evaluation settings with vital interests in the design, collection and disposition of data relevant to any program or project. These parties, because of formal or informal role positions, ordinarily possess conflicting values and preferences about any research endeavor. Consequently, they still often attempt to influence the researcher with respect to the focus, design and conduct of the evaluation (Argyris, 1970; Evans, 1972; esp. Krause and Howard, forthcoming; Roston, 1972; Taylor, 1973; Weiss, 1973). Consequently, a grand conflict of research interests may ensue, with the researcher either ineffectively caught in the middle or else enlisted as a partisan for some factional causes. It is claimed that the common existence of such conflicts renders the traditional research model emphasizing dispassionate objectivity incapable of implementation and rigorous experimenter control impossible (Guttentag, 1973; Krause and Howard, forthcoming; Weiss, 1973).

It is not clear that the existence of metaconflicts in social evaluation settings renders these distinct from traditional social psychological research settings. For example, the university as a setting for basic research is also quite well described by the metaconflict paradigm (cf. Wolfe, 1971). The assistant professor at a large university, like his evaluator counterpart, must also contend with competing research factions, whether from the chairman's discouragement of "unfundable projects," students' reluctance to play captive guinea pig, or just the preferences of a journal editor who regards the investigator's interest area as passé. These conflicts may not be as directly related to research results, but they affect the research process nonetheless. This is not to say that either traditional social psychological research or social evaluation studies are vacuous proclamations bought by the most potent power broker in either setting. Traditional or evaluational, research is always to some degree a negotiated compromise. This political aspect of "doing science" has persisted from the time of the Greek scientists and their patrons to the present.

The third, political sensitivity, argument asserts that social evaluation researches are always carried out within, and affected by, the political or bureaucratic system in which the target program is embodied. Consequently, social evaluation efforts often fall victim to system sensitivities having no direct relevance to the research project, but which may impede investigatory efforts (e.g., Bonoma, 1977; David, 1971; Evans, 1972; Taylor, 1973; Weiss, 1973). For example, research designs potentially yielding information that would reflect unfavorably on program admnistrators and sponsors may be rejected, or unflattering findings may be suppressed. Even the decision of which program to evaluate is political, since the more successful programs are more likely to be subjected to scrutiny because of pressure from public officials needing favorable political ammunition.

Again, however, there appears to be no lack of good homomorphism between the social evaluation arena and traditional social psychological settings. Anyone who has been a nontenured part of an academic psychology department is well aware of the particularly potential nature of his continuing appointment (Wolfe, 1971). Structurally, the size and composition (e.g., sex and ratios) of the department, and even the secretarial assistance provided are all political decisions that are "irrelevant to," but impact on, the research endeavor. Functionally, some problem areas are considered highly threatening by administrators (e.g., race, gun control or pornography research), and may be discouraged. And, strong legitimation effects exist as well, as when a prominent investigator opens a new area and determines for others a "worthwhile" problem. It is unpleasant and not very tactful to raise these points about traditional research. Unless they are at least broached, however, we are in danger of further fractionating a discipline just to maintain a set of convenient fictions.

The final difference posed is the instability argument. It is said that social evaluation research is performed on programs or projects that are inherently unstable, since they are designed to be both adaptive and evolutionary. That is, services and programs are client-centered—they exist to deliver service, and hence innovate, adapt, change, and mutate constantly in order to meet that aim. Thus, traditional emphases on the constancy of measurable phenomena and the establishment of controllable treatments are simply not applicable to social evaluation pursuits, since these display temporal and system instabilities rendering them immune to the sorts of research forays usually launched by traditionalists (Evans, 1972; Guttentag, 1973; Krause and Howard, forthcoming; Weiss, 1973).

Clearly, one could appear to the field research tradition in classical social psychology to establish homomorphism between the adaptive systems and investigatory difficulties of evaluational and traditional endeavors (cf., e.g., Webb et al., 1966). However, similarities can be found using very "basic" examples as well. Those readers familiar with the study of choice behavior via some formal economics model will recognize all the instabilities claimed as province by social evaluation writers inherent in this "lab" research setting. Concerning structural factors, no one is quite clear what utility or probability functions look like. Regarding adaption and evolution, human decision makers show a shocking lack of controllability and a maximum of adaptation (Bonoma and Schlenker, 1978).

The essential point is just that humans, quite pleasantly, are unstable (i.e., adaptive) systems of the most refined order. Regardless of the point or level of application, *any* investigatory effort that deals with their behaviors must necessarily and simultaneously encounter system-change parameters.

SOCIAL PSYCHOLOGY AND SOCIAL EVALUATION: AN ASSUMPTIONAL BOTTLENECK

These comparisons allow us to speculate that there quite possibly exists a fair-to-good degree of homomorphism between the nature and context of both traditional social-psychological and social-evaluation research settings. However, our counterarguments emphasizing the similarities rather than differences between social evaluation and traditional social psychological research in no way weaken the evaluation researchers' contention that the existence of conflicts, sensitivities, and instabilities degrades the applicability of the experimental method to these settings. Rather, they may be correct in this hypothesis, but for the wrong reason. Since social evaluation settings appear to be homomorphic to traditional concerns, their contention that system conditions often make the experimental method impossible to implement opens this Pandora's box for *all* social psychology to a greater or lesser degree. Our comments here, as in our previous discussion of the D-thesis, are directed toward both settings.

If one accepts the initial assumption that social events are at least partially orderly (i.e., causally produced), then it can be demonstrated (cf. Kaplan, 1964) that such phenomena can be most efficiently studied by way of the experiment, given certain initial conditions. The existence of the appropriate "initial conditions" is exactly what writers such as Gergen (1973), Koch (1971), and Newell (1973) question, and contrasting their views with the arguments of social evaluation researchers shows the latter arguments to be specific forms of the general questions raised by the former. For our purposes, the question translates as, "Does the existence of metaconflicts of interests, political sensitivities and a focus on unstable phenomena degrade the applicability of experimental investigatory strategies in traditional as well as evaluation applications?"

Contrary to Schlenker (1974), we believe the answer must be a qualified "yes." That is, and in partial agreement with Gergen and Koch, it is true that the existence of conditions such as those cited by social evaluation specialists often renders experimentation, with its requirements of rigorous control, impractical or impossible. Moreover, this is often the case in social evaluation as well as traditional social-psychological research settings.

Even the more sophisticated experimental designs may be basically incompatible with the sytems constraints existing in social evaluation settings. This is because of the nature of the experiment, which is designed to serve as a "snapshot" of effects produced under specified system states. Taking such a picture is of little value if the subject changes immediately after exposure, and of very little value at all if such changes are the result of systematic differences in powerful factors assigned to "error variance" (e.g., political sensitivities) completely outside the realm of experimental interest. Therefore, the usual strategy of inquiry, which includes forming a rudimentary "map" (i.e., theory) of the investigatory area and then exploring this map via experimentation, may be degraded for the reason that the entire map is nonrandomly affected by its enclosure within progressively larger systems.

In fairness to social evaluation authors, we must agree that greater degradation of the experimental method often is experienced in program evaluation than in the study of dyadic behavior. However, the low level and power of traditional social psychological laws (Gergen, 1973) attests at least partially to the existence of a homomorphic dilemma within the traditional domain as well. The settings and problems of the one cannot logically be segregated from those of the other on these grounds. It can be concluded that no investigatory-explanatory approach that ignores the nexus of systems in which the phenomenon of interest is embedded can produce generalizable knowledge. Further, it may be expected that both traditional and evaluational experimental pursuits will again in feasibility with (1) the methodological sophistication of the researcher, and (2) the articulation of general principles of cross-system influences (e.g., Grinker, 1967).

ASSESSING BACKGROUND ASSUMPTIONS IN TRADITION AND EVALUATION DOMAINS: ERRORS OF THE THIRD KIND

If it is (a) in fact plausible that the methodology of the pure experiment as it is practiced in basic social psychology cannot offer unambiguous comment on the falsifiability of hypotheses, and (b) once it is accepted that the difficulties that plague basic attempts in this regard also plague evaluational pursuits, then the primary difficulty facing the researcher in either domain is that of developing some way to explicate and assess the plausibilities of differing background assumptions that underlie and support the investigatory context. For example, a mental health unit may be evaluated because the researcher feels that treatment often leads to cure, or contrarily, because the investigator thinks "mental illness' does not exist and treatment facilities just waste public funds. These differences in assumptions do no harm in a strict pre-

Duhem world in which hypotheses are strictly and experimentally falsifiable. In a not so clear world they may bias results as is discussed below.

In this light, we see the quasi-experimental logic, method, and metatheory that has been offered by Campbell and others (e.g., Rossi, 1972) as in one sense an unfortunate development that has obscured some fundamental issues common to both basic and evaluation pursuits by focusing attention on methodological difficulties and remedies. Campbell's (e.g., 1957; Campbell and Stanley, 1963) offering of the various threats to internal and external validity when the experimental requirements of randomization and control cannot be met, for example, implies that if we make our (real world) evaluation studies quasi-experimentally sophisticated then we can rest assured that our data are (almost) as valid and scientifically precise as those generated from a "pure" experiment. One is reminded of *The Republic* and true experimentation as reality, while quasi-experimentation yields only shadows of varying validity on the walls of the cave. The trouble is, of course, that the best of pure experimentation may not be a valid path to truth at all without attention to the background assumptions brought to the investigation by the researcher.

In short, the Campbellian validity threat categories, and the remedies therein suggested, may be thought of as themselves embodying one particular set of background assumptions about experimentation. These collectively suggest that if "threats" are removed then the investigator may evaluatively operate with near "pure experimental" impunity in the advocative, political, unstable, and evolving "real world" with as much (or nearly as much) assurance as he could gain in his laboratory. We believe, to the contrary, that the assessment of the "validity" of any experimental or quasi-experimental tool requires a preceding methodology for uncovering the underlying background assumptions supporting the research process in the first place. Consequently, in our opinion, it also requires a different set of evaluative categories than those formulated by Campbell for judging the validity of the research process as it might be alternatively characterized and directed by different assumptional universes.

The validity issues raised by Campbell are appropriate for consideration once a given set of background assumptions has been accepted. For example, once one has already decided upon (a) from which theoretical and paradigmatic (Kuhn, 1962) perspective the experiment will be conducted, (b) what the independent and the dependent variables will be, (c) what the subject population will be, (d) the mode of collecting data and processing it, (e) what shall count as "observational data" and the criterion of testing with regard to what has previously been decided upon as the hypotheses under test, then one can consult validity cautions and evaluate an experiment thereby.

Looked at another way, the issues raised by Campbell are appropriate for attempting to minimize the usual Type I (E I) and Type II (E II) errors associated with hypothesis testing, and to control for the factors that can potentially jeopardize any "nonpure" experimental design. The assumptional dialectic we prefer, however, raises up to consciousness a more fundamental type of error that has been termed the error of the third kind of E III (Mitroff and Featheringham, 1974; Mitroff and Turoff, 1974). E III can be defined as the *probability of solving the "wrong" problem when one should have solved the "right" problem.*

When we are dealing with it at the assumptional level, we are dealing with one of the widely neglected aspects of scientific method, problem *definition* or problem-*forming* (Raiffa, 1968). The issues raised by problem definition and the concept of E III fundamentally concern the *strategic* level of the scientific method and or problem-solving (Mason, 1969). The problem here is basically one of "defining what the problem 'is' or what will be considered as such" (Mitroff and Featheringham, 1974). On the other hand, the issues raised by the concepts of E I and E II and the all-too-typical treatments of experimental design basically concern the *tactical* level of problem-solving. The problem here is one of finding an optimal solution to an already well-formed or well-defined problem.

DEALING WITH ASSUMPTIONAL PROBLEMS

There are strong indicators that an appropriate methodology for defining problems and for explicating background assumptions is emerging from a variety of fronts. Recent developments in such disparate fields as the history of science (Holton, 1973), philosophy of science (Churchman, 1971; Feyerabend, 1975; Mitroff, 1973, 1974b), sociology of science (Mitroff, 1974a, b), psychology (Levine, 1974), and operations research (Mason, 1969; Sagasti and Mitroff, 1973) all point in the same direction. This is that an appropriate methodology of the kind we are seeking is fundamentally dialectical in character.

In a series of provocative papers, Feyerabend (e.g., 1975) has mounted a sustained and impressive series of arguments against what has been termed "naive empiricism" (Churchman, 1971). For our purposes, naive empiricism can be defined by the following series of statements: (1) scientific data is *independent* of theory, i.e., that data (0) can be collected without having to presuppose the prior existence of some theory or hypothesis (H) with regard to the phenomenon under scrutiny. (2) Scientific data is *neutral* with respect to theory; its basic meaning is independent of any particular theory, i.e., that 0 is not a function of H. (3) Data alone are sufficient to test a theory.

In contradistinction, Feyerabend takes sharp opposition with each of these statements of philosophical belief. First, by means of plentiful historical examples and philosophical arguments, he shows that scientific data is neither independent of nor neutral with respect to theory; i.e., that $0 = f(H)$. While the relationship between a particular theory and a set of data need not be isomorphic, scientific theory "guides" the collection of scientific data in the sense of implying what data are relevant to collect. Stronger still, data can not even be unearthed in the first place without the presumption of some theory, no matter how implicit and loose that theory may be in its development or formal expression.

Feyerabend's argument then takes a truly radical turn. If data can only be unearthed with the prior presumption of theory, then *the existence of only a single theory* with regard to some phenomenon (no matter how well confirmed and successful that theory has been in the past, e.g., Newton's laws) *can be detrimental* to the continued testing of that theory. That is, because of the potentially severe incestuous relationship between theory and data, tests of data unearthed by a theory can unwittingly act to confirm the very theory it should instead be challenging.

Feyerabend's conclusion is that the most severe testing of a theory or set of hypotheses H_1 occurs when it is pitted against the data that only can be uncovered with the help of some strong rival theory or hypotheses H_2 with regard to the same phenomenon that the first theory is attempting to explain. Thus, the appropriate way to do science is consistently to look for "critical experiments" that help to discriminate between competing sets of assumptions. The critical experiment notion, though, loses its "criticalness" in this view, since unless two competing views exist it is less than satisfying to experiment. The testing of any theory H_1 in science therefore requires the existence of *at least one other* strong rival theory, H_2. Put in the form of the modus tollens, the test procedure of a scientific theory or hypothesis reads according to Feyerabend as follows:

$$\begin{array}{ll} \text{If } H_1 \rightarrow 0\,(H_1) & - \; H_1 = \text{Preferred theory} \\ \underline{\text{and if } \sim 0\,(H_2)} & - \; H_2 = \text{Nonpreferred or competing theory} \\ \text{then } \sim H_1 \text{ or } H_2 & \end{array}$$

Without pursuing this model further here, it provides an additional line of argumentation as to why the simple notion of falsification as discussed earlier in this paper fails to hold (Mitroff, 1973).

From the history and social psychology of science it thus appears evident that not only does science advance through a dialectical process whereby scientists who are committed to opposing theories do battle with one another (Mitroff, 1974b), but that one of the most distinctive characteristics of the outstanding scientist is his marked preference for dialectics within the realm of science (Holton, 1973).

Finally, the adversary of judicial conception of scientific method has emerged from two other independent sources. One is psychology (Levine, 1974) and the other a combination of management science (MS) and operations research (OR). In the field of OR/MS, the conception of dialectical inquiry has not only evolved to the point of operationalization, but it has even undergone some preliminary field (Mason, 1969) and laboratory investigations (Mitroff et al., 1974). The dialectical method is schematically illustrated in Figure 1.

Unlike "normal science" (Kuhn, 1962), which takes intense disagreement between opposing groups of scientists as a sign of the breakdown of the normal inquiry path, the method illustrated in Figure 1 begins *only after initial* recognition of significant conflict between two or more parties or points of view. Further, because there is opposition between two ore more points of view, the method does not begin by assuming that any one of them is "true" or "false," "right" or "wrong," but that each viewpoint may be picking up a part of a yet-to-be-determined "correct" viewpoint.

By means of a specifically developed (Mason, 1969; Mitroff and Featheringham, 1974) and integrated multivariate intervention, the

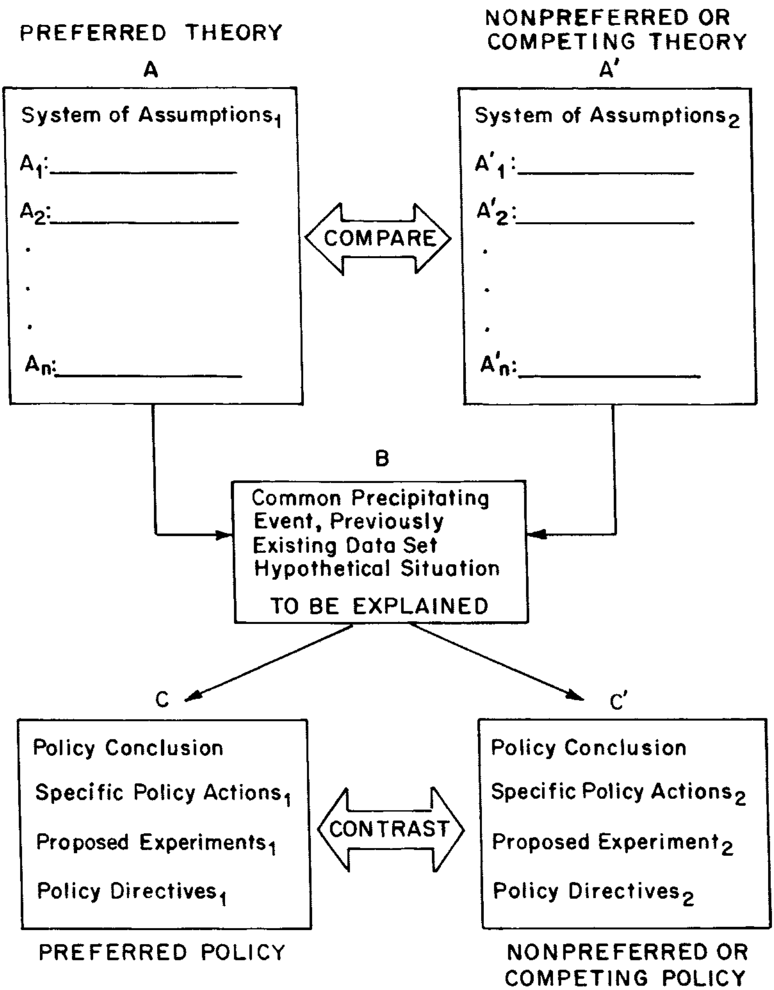

Figure 1: A Dialectical Inquiry System

assumptions underlying the two viewpoints can be, as much as possible, shaped into sets of contrary assumptions. That is, for every assumption A1 that is characteristic of the one viewpoint (A), there is identified an opposing assumption A1′ characteristic of A′ such that A1 and A1′

are contrary propositions. In logic, two propositions A1 and A1′ are regarded as contraries if one proposition is "true" then the other must be "false," but where the two propositions can both be false. That is, A1 and A1′ are exclusive but they are not exhaustive.

Given an initial sorting of the two viewpoints into sets A1 and A1′, the next step of the dialectical inquiry process consists of the simultaneous application of each individual element of A1 and A1′ to the elements of some common data bank B. The elements of B may be an hypothetical event, some past or current situation, a previous data set, the results of current experimentation, or whatever. The role of the data bank B is extremely crucial. B must not only consist of items that are regarded as relevant to either viewpoint, but it must also be capable of differing explanations.

The purpose of the data bank B is to further draw out the differences between A1 and A1′ and to illustrate the critical role that A1 and A1′ play; i.e., that by itself B is without meaning. B only acquires or takes on meaning by being coupled to a point of view, A or A′. Another way to put this is to say that the basic disagreement is not over the status of B, as the relative holders of viewpoints A1 and A1′ believe, but over A1 and A1′ themselves. By explicitly and formally witnessing that more than one set of policy conclusions or experimental interpretations C and C′ can follow from the same data bank B, a decision maker, policy maker scientist, or judge who is not a bought-and-paid-for captive of either viewpoint will be in a better position to understand the issue under debate than if he or she were permitted to listen only to the "expert" but partisan advice of either viewpoint alone. Thus, the real beneficiary of a dialectical debate may more likely be someone who is not thoroughly committed to either position alone than one who is so committed. In Bayesian terms (Mitroff, 1971), if the proponents of A and A′ are deeply committed to their positions, then nothing anyone can say can alter these probabilities of complete acceptance for their own positions and a complete rejection of their opponent's position.

If the reactions of a decision maker toward the assumptions A1 and A1′ embodied in each position can be expressed in decision-theoretic terms, then an "objective" criterion can be formulated for choosing between the two viewpoints even though the values that enter into the criterion are based on a decision maker's subjective feelings, attitudes, and beliefs toward each position (Bonoma, 1975; Mitroff and Featheringham, 1974). In effect, the objective criterion represents an operationalization of E III (Mitroff and Featheringham, 1974). The criterion can tell a decision maker what his probability of belief is in the falsity of one viewpoint given his presumed original belief in the truth of the other.

Omitting the mathematical computations possible using a dialectical method for evaluation, an example might be the following. Assume that a state mental health program administrator has firm evaluation data showing (1) that a group of catchment areas implementing new first-line services (community mental health centers, outpatient counseling, visiting social workers, and so on) are effectively deferring 10% of the expected admissions to second-line facilities (hospitals) on a comparative baseline basis. Further, (2) the first-line community services are effectively reducing the per patient costs of health care by drawing on community resources to pay for such services. However, (3) recidivism is extremely high in locally treated patients, and (4) the community programs have come under much fire for providing poorer quality services than are available at state facilities. The administrator must decide to abandon, continue, or expand the program for the next budget period.

The usual approach involves the administrator factoring in his own biases with the (unknown) biases of the researcher and making a decision. Our approach suggests a different data-gathering *and* policy-making process. Before assuming the data bank showing effective deferral but high recidivism is unbiased, both the researcher or policy maker should move in two directions. They must move backward from the data to confront both their own preferred background assumptions (i.e., first-line services are cheap and effective) to construct competing but plausible set of counterassumptions (i.e., first-line services are cheap but ineffective). Second, they must forward from the data to construct a set of policy options consistent with their preferred assumptions (expand the services) and with the competing assumptions (upgrade the services, or discontinue them).

Through this process, the researcher and then the policy maker can make intuitive and sensible comparisons of (1) the plausibility of their assumptions, (2) the fit between assumptions and data, (3) the fit between data and policy recommendations, and (4) the fit between policy and assumptions. All these comparisons are necessary in addition to "good" data for enlightened policy decisions.

SOME CRITERIA FOR EVALUATING OUR RESEARCH ASSUMPTIONS

The categories and criteria usually involved in assessing the research process focus exclusively on properties ascribed to either the experimental process itself or to the behavior of subjects and experimenter within their respective roles. In this "ideal type" analysis, the experiment is compared as it matches up to the triple requirements of control, manipulation, and randomization; the investigator is measured as regards his conduct when his or her behaviors may suggest alternative explanations for subjects' behaviors (e.g., experimenter effects); and, subjects are compared to an ideal, dispassionate, naive human actor who is neither trying to fulfill nor harm the experimental purpose.

The assumptional evaluation process we have proposed here as a prerequisite to any other types of "validity" assessment for experimental practice requires that we focus the same microscope on the researcher's likes and dislikes, openness or inflexibility, role, and other characteristics as these are intertwined with his or her ability to display a "constructive alternativism" regarding the research process. This microscopic analysis is proposed not from any motivation to make the investigator look less dispassionate than he or she might like to present himself, for dispassion in the research process is a well-recognized absurdity (Mitroff, 1974a, b). Rather, our assumptions about science and any particular behavioral phenomenon we choose to investigate are so inextricably tied up with our own personalities as researchers that examining one requires an examination of the other. It should be noted here that the dialectical inquiry approach essentially converts the "basic" inquiry process into one much closer to that conflict-filled and multiply-biased environment usually described by the evaluation researchers as a difficulty in their work.

Table 1 presents an intuitive, speculative, and thus tentative, listing of some things one might like to think about, and hopefully investigate more in the future, when one is examining or evaluating the relationship between one's underlying assumptions and one's research endeavors regardless of the "basicness" or "evaluativeness" of the research setting. We list several categories into which assumptional evaluations might be partitioned; among these are the general psychological state of the researcher, his tenacity in holding assumptions about the data, his selection of a data bank, and his inferential processes in reaching conclusions from the data.

TABLE 1
Evaluative Criteria for Explicating and Examining Different Background Assumptions

Criterion Categories	*Properties Relating to Proponents of Different Views*
A. General Psychological	1. preferred methodology and/or "style" of doing research
	2. general attitudes toward science
	3. ability to tolerate conflicting scientific ideas
	4. degree of self-reflectiveness: awareness of own personality
	5. willingness to be cross-examined, probed
	6. receptivity to other world-views
B. Pertaining to Assumptions	7. willingness to state and inspect underlying assumptions
	8. degree of commitment to assumptions
	9. hostility toward competing assumptions
C. Pertaining to Data Bank	10. selective perception; functional fixity
D. Pertaining to Conclusions	11. degree to which conclusions are argued to and the way in which they follow from data bank to assumptions
	12. tendency to draw firm conclusions from consensually inconclusive results

The categories and the criteria that are appropriate for evaluating assumptions pertain to a process, that of a dialectical inquiry or debate, and to the properties of the proponents of each of many viewpoints, as well as those of a decision maker who may be the adjudicator between them (Levine, 1974).

CONCLUSIONS

It is our major contention that the role of the experiment in social psychology needs to be reexamined in the ways we have suggested. Basically, the entanglement of our methods with our assumptions allows no separation between them, any more than it allows any purely empirical "falsifications" of data. Additionally, the problems faced by the research process are the same whether encountered in the tightly specified world of 2 x 2 factorial designs, or in the political cast of the evaluation arena. In fact, the latter setting actually serves to make some of the interrelationships between theory, data, and assumptions more clear because of its frank partisan and advocative nature.

We conclude that there is a strong need for some methodology that will allow us to compare and contrast the background assumptions in the data-collection process. The dialectical system of inquiry, in which every test performed on some data is a "critical" one, appears to provide some promise here. It is all too easy to believe that one should compare one's research methods to some ideal-type of "valid," pure experimentation model, and then conclude that if some correspondence between one's preferred methods and the ideal is achieved no more concern need be given to the validity of the results.

In both the real world of evaluation and the more tightly controlled one of the laboratory, we believe that "validity" cannot be even approached until one learns to question his or her assumptions as closely as he or she questions the rigor with which data was generated. Such an ability to question requires an openness and flexibility that is not found in many individuals, scientists or otherwise. The dialectic inquiry system is a device designed to insure that this flexibility can be obtained through the collective efforts of a number of scientists who each believe passionately in their own preferred set of background assumptions.

NOTE

1. Notice that none of the foregoing arguments have rested upon a presumption of sloppiness or incompetency on the part of either science or scientists. The argument says that even the best scientists, the most well-designed, and the most carefully executed experiments will be subject to inconclusiveness. This condition arises *not* because of any laxity or contentiousness of experimenters, but because of the fact that different experimenters bring different expectancies, past histories, and ideas regarding science to bear on the specific subject matter of their inquiries.

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