Social Theory, Social Programs, and Program Evaluation: a Metatheoretical Note*

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Program evaluation attempts to assess the consequence of deliberate efforts to intervene in ongoing social processes. Efforts to understand and explain social processes and social change constitute social theory. The evaluation of social programs provides an opportunity both to test existing theories of social change and to discover new theory. Current notions about theory in the evaluation literature are reviewed and cues from that literature provide the basis for suggested directions in the relationship between the design of programs, their evaluation, and theory.

Current applied social scientists may see deeper significance in their work if they recognize that it is neither peripheral nor new foliage but that, on the contrary, it emerges from the deepest taproots of their disciplines and has the most venerable tradition. (Gouldner, 1957:102)

As Gouldner reminds us, there is nothing new or peripheral about the application of social science to practical issues in society. The lack of novelty should surprise no one. There is a familiar tradition of denigration (among academic colleagues) for those sociologists who stoop to efforts to make their knowledge useful. There is, however, less recognition of the relevance of such applied work to theoretical development.

One such activity—called "program evaluation"—attempts to assess the consequences of deliberate efforts to intervene in ongoing social processes. Those deliberate efforts often consist of "programs" designed to help the sick be well, endow the ignorant with wisdom, motivate the incompetent to be successful, and encourage the naughty to be good. The social sciences all attempt to understand how social processes work: how they remain stable and how they change. The explanations which result from these attempts constitute social theory. This paper does not propose any specific theory although one is discussed as an example of how an appropriate kind of theory can inform program design and evaluation. The paper is metatheoretical in the sense that it considers the relationship between theory in general and the design and evaluation of social programs.

There is a body of sociological theory dealing with the nature of organizations and their structure and that body of theory illuminates a number of methodological problems in evaluation research. I have used organizational theory elsewhere as a point of departure in considering one problem in program evaluation (Deutscher, 1977). This paper concentrates on theories related to social change, be-

cause they seem most relevant to the design of programs which intend change. I narrow the consideration to micro-theory for reasons discussed below.

Although there have been some efforts to develop theories of evaluation per se (Alkin, 1972), these are not created here on the grounds that (1) those efforts consist largely of systems analyses which may result in useful procedural models, but generally do not attempt to explain the process and (2) the present model views program evaluation as a field test of basic social science theory of change rather than as a discrete discipline requiring its own body of theory.

Attentiveness to Theory in Program Evaluation

Theory, in itself, is a matter which rarely receives attention among program planners or evaluation researchers. A recent study of federal evaluation programs notes that "only slightly more than 18 (43) of the total studies were evaluating programs reported as having some underlying conceptual or theoretical framework" (Bernstein and Freeman, 1975:42). Rossi (1972:42) regrets that: "A large part of the problem presented by broad aim programs lies in the absence of reasonable social science theories which could serve as a guide to the design of social action programs." Rossi is pessimistic about the current state of social theory and thus its capacity for guiding program design. He concludes in despair that funds ought to be provided to diverse sets of enterprises in the hope that trial-and-error experimentation may hit upon some effective program. Campbell (1971:10) shares Rossi's discouraged estimate of the current state of social theory: "We have no elegantly successful theories that predict precisely in widely different settings."

I am no more sanguine than Rossi and Campbell about the viability of current social theory, but is it so lacking as to leave no alternative but trial and error? There are social scientists who have worked extensively in problem areas and who have generated, if not a unified theory, at least plausible middle-range theories. Cressey (1958) for example, has recommended that correctional programs be designed along lines suggested by theories of crime causation. Breedlove (1972:66) cites *Retrieval from Limbo* (Gantner et al., 1967) as an example of a theoretically rooted evaluation which makes an outstanding contribution to practice theory.

An illuminating exception to the general inattention to theory among evaluation researchers is found in the attack by Cain and Watts (1972:74) on the Coleman Report. Of the two criticisms offered by these analysts, one is purely theoretical: "The specification of the theoretical model," they write, "is inadequate to support the regression analysis used in testing the model." They focus upon Coleman's failure to provide a rationale for the inclusion of some variables, the exclusion of others, and the kinds of relationships anticipated. Although we might disagree (as Coleman does) on what constitutes an adequate theoretical model, their concern is nevertheless justified.

Coleman (1972) responding in the same vein as both Rossi and Coleman, argues that economists have neatly specified models to work with while the state of theory in sociology is not that well developed. But of greater importance is the difference between Coleman and his critics in notions of what constitutes "policy recommendations." Coleman argues that the economist, dealing with known

variables, is concerned with estimating their relative values and making recommendations about program input on the basis of those values. Sociologists on the other hand seek to discover theory in a mass of messy data: "The policy results of their research lie in such things as uncovering important processes that had previously been unknown or ignored in policy" (Coleman, 1972:99). The importance of this debate lies primarily in the example it sets. It is a rare item in the evaluation research literature: a discussion of theoretical issues.

Among those social scientists involved in the evaluation of social programs, the attention of some is drawn to theoretical questions. Although writing of educational programs, Suchman (1971:46) addresses all social programs when he insists that, "There must be some theoretical basis for linking the program to the objectives. The question 'Does it work?' presupposes some rationale as to why one might expect it to work." Suchman insists that there can be only two possible sources for program failure: either the program itself is ineffective or the theory providing a causal link between program and objectives is faulty. Although their observation derives from experience with health programs, both Angrist (1973) and Rieker (1971) attribute "the rarity of intended, expected or measurable effects" in evaluation research to the lack of theory.

In specific problem areas, we find Cain and Hollister (1972:130) concerned about evaluating community programs. They write of "an overriding primary need for better theories of community structure" and conclude that "without theory it is hard to know what facts or data we should be collecting." McDill et al (1972:171) point to "the theoretical aspects" as one explanation for the failure to document the effects of compensatory education programs, and refer to "the embarrassing position of applying solutions and cures to . . . still unknown problems" (McDill et al., 1972:180). As one reads Scot and Shore's (1974:57) agonizing report of their efforts to evaluate the New Jersey Negative Income Tax experiment, the need for theory leaps out as a major source of their difficulty. They see theory primarily as a useful guide for the selection of variables and, like Rossi, bemoan the lack of any firm theoretical framework in sociology. Without emphasizing it, Jones and Borgatta (1972:49) acknowledge the need for theory in social work programming and hope that the social science disciplines will eventually provide *"grounded theory* upon which to base our interventions and against which we may evaluate their effectiveness" (italics in original). What Jones and Borgatta do emphasize is the improvement of research technology and the clarification of practice goals as the major sources for improvement of future evaluation research. Breedlove (1972:55) challenges this assumption, arguing instead that "the underlying problem for . . . evaluation research, is the failure to give serious attention to the development and testing of practice theory."

There is another side to the coin. Hyman and Wright (1967), like Cressey, believe that social scientists with a firm grip on their substantive field do have a theoretical reservoir from which to draw. They suggest that the evaluator can find guidance in the theoretical orientation of the relevant social disciplines: "Sociology, social psychology, and anthropology provide guides to subregions concerning groups which would be salient to the evaluation of programs aiming at group change" (Hyman and Wright, 1967:200). Although no social scientist would question the desirability or even the necessity for theoretical grounding of social programs and their evaluation, many such as Campbell, Rossi, Cain and Hollis-

ter, Coleman and Scott and Shore entertain strong reservations about the viability of existing theory. There are, however, a few like Hyman and Wright, Brooks or Cressey, who have more confidence in the current state of theory. Their optimism hinges primarily on the quality of middle-range theories in substantive areas. In an effort to pursue possible linkages between theory and programs, let us turn to an abstract consideration of how social theory and social programs may be related.

The Relationship between Programs and Theory

A theoretically rooted program is one which is based on the notion that change can be explained in a certain way and will therefore occur if certain actions consistent with that explanation (theory) are taken. Brooks (1971:55) has suggested that Community Action Programs provided the opportunity to test basic social science theory with hypotheses derived from it being translated into programs: "Each component project of a community action program is, in effect, a test of a hypothesis about the causes of poverty. . . ."

Rarely is the opportunity provided to put our theories to such a clear-cut test, but those who actually design programs often entertain their own implicit theory of change. Evaluators need to tease out and make explicit whatever theory underlies a program design. What kinds of ideas about social process and social change make practitioners confident that their program will change people in intended ways?

All programs are built around either implicit or explicit theories about how social change occurs. Such theories may be narrowly specific to particular programs or broadly applicable to any kind of social intervention—as, for example, in the case of Rein and Miller's (1970) theory that the creation of a demonstration project is in itself a strategy for change. Even those cynical efforts to create, for political expedience, the *appearance* of doing something, are theoretically rooted. They imply a theory of social stability built around the concepts of "co-optation" and "cooling out." The process of cooling out has been documented on an interpersonal level in Goffman's (1962) study of confidence games. The fact that many programs are politically motivated does not provide grounds for dismissing them theoretically.

For the social scientist, the evaluation of a program which is based on a specified theory becomes a familiar if not routine task: It becomes basic research under field conditions. Traditionally most of our research involves theory testing. It is something we know how to do and for which we have a deductive logic to guide our methodology. Cressey (1958:764) addressed himself precisely to the interplay between theory and research when he considered criminal corrections programs: "If correctional work were scientific," he argues, "utilization of each correctional technique would be an experiment designed to test the validity of a theory of crime causation." The other side of the coin suggests the manner in which programs can provide important theoretical increments: "Perhaps in the long run we will find that some technique introduced for nontheoretical reasons 'works.' If this occurs, then we can, by working backward, develop [discover] a plausible theory of crime causation" (Cressey, 1958:764). Drawing a parallel to

the relationship between theory and research, Empey (1964:57) also notes this reciprocity between theory and social action programs.

It is no accident that the most extensive thinking about the relationship between theory and programs appears to take place among those working the field of criminology. It is an area to which sociologists have turned for basic theoretical formulations from Durkheim through Sutherland and since. It is also an area of continuing interest to a variety of legislative, administrative, and judicial enterprises at all levels of government as well as to the populace in general. The combination of theoretical interest, empirical research, and community concern account, in large part, for the extensive literature on theory and practice. This concern is not, however, the exclusive domain of criminologists. For example, it is clearly articulated by Grandy (1975) in reference to educational programs. Based on his belief that programs are usually developed on the basis of an implicit theory, he sees evaluation as one way of formulating and testing hypotheses as well as part of the process of theory verification (Grandy, 1975:350).

I do not intend to suggest that anything called "theory" in the social sciences can be blindly applied to the design and evaluation of social programs. Some theories are not very good either because they do not explain enough to make much difference, or they have no empirical basis (i.e., they do not relate to the world as we know it with our senses), or they are not plausible (i.e., they do not make much sense). Furthermore, there are some theories of social change which are not relevant to most programatic efforts to induce changes regardless of whatever other merits those theories may have.

Macro-theories of social change in sociology and anthropology are among these otherwise meritorious theories which are largely irrelevant to intervention programs (some exceptions to this general irrelevance are discussed below). They are irrelevant because their central concepts are on the level of "society" or "culture," and programs, as we understand them in the United States, do not usually address societal or cultural issues. Rather, they are designed to change individuals within a given socio-cultural milieu. Although there are occasional programs designed to improve smaller systems such as health care, police systems, or "communities" (cf. Voth, 1975), it is among social psychological (micro-) theories of change that ameliorative programs are most likely to find plausible explanations and guidelines for program design. Theories about how people change their minds, their feelings, or their behavior seem most appropriate to the problem. Typically, a program is designed to help fit deviant individuals or deviant segsents of the population into the main stream of the existing social system. If programs do resemble my description so far, then it is possible that they and their evaluation are inherently conservative activities. I have considered this issue elsewhere (Deutscher, 1976).

A Social Psychological Theory and Its Program Implications

Most intervention programs are designed to change how people believe or feel or talk or behave. It follows that an appropriate theory for program design and evaluation should be one which explains how people change in these respects. Evidence from a variety of sources suggests that there is no necessary relationship

among these several ways of orienting one's self to a social phenomenon. That evidence comes from laboratory experiments, field experiments, surveys, and ethnographic and participant-observation fieldwork. It is interdisciplinary in that it is found in the work of psychologists, sociologists, and anthropologists, not to mention novelists, journalists, poets, and newspaper columnists. I have presented and analyzed much of that evidence elsewhere (Deutscher, 1973). The simplest correct statement which summarizes all such evidence is that attitudes and behaviors are not *necessarily* related—nor is attitude and behavior change.

This lack of relationship has been occasionally recognized by evaluators such as Freeman and Sherwood (1971:269) who warn against the use of attitude scales as substitutes for measures of overt behaviors. It is, however, more common to find in the evaluation literature the untenable assumption that attitude change and behavior change are one and the same thing or that one more or less automatically follows from the other. Cain and Hollister (1972:113) for example, suggest that, "over the long run, *but not necessarily in the short run*, attitudes will closely correlate with the more tangible performance indicators" (emphasis theirs). This statement not only assumes a non-existent relationship, but confounds the error: if anything, a relationship is less likely to persist into the future than to exist in the present, because both attitudes and behaviors are constantly changing and they change independently of each other.

It is possible to induce anti-criminal, anti-drug, anti-smoking, anti-suicidal or anti-whatever attitudes in a sample of criminals, addicts, smokers, suicide attempters or whatever. But it does not necessarily follow that those whose attitudes have changed will quit smoking or attempting suicide or whatever. The reverse is equally true: behavioral change may be induced in any of these areas without concomitant attitudinal change—a basic premise of behavioral modification. In fact, this may be what happens in such programs as Alcoholics Anonymous or Synanon—examples to which I shall return. For the moment I wish only to make a preliminary theoretically derived recommendation for programs and their evaluation: they should consciously choose and focus upon what is considered the most relevant dimension. If it is deemed important to reduce prejudice, then an attitudinal choice is appropriate. If, on the other hand, the action program is concerned primarily with discrimination in such areas as education or employment then it is overt actions to which the program and its evaluation should address itself.

There is neither logic nor evidence to support the evaluation of a program designed to change what people do, on the basis of what they say! The reverse is equally true. There is, then, a theoretical and logical absurdity to the position of those such as Cain and Hollister (1972:113) who opt for behavioral outcome measures for all program evaluations, "on the practical grounds of choosing outcomes which may be more accurately measured." To what advantage is accurate measurement of outcomes which are not relevant to the aims of a program? Angrist (1973:10) discussing the evaluation of housing projects provides one example: "Program planners may seek attitudinal changes in designated population segments without any behavioral requirements—for example that tenants be more satisfied with the housing maintenance but not that they lower complaints about maintenance." Heberlein (1973) like Angrist, understands this issue. He rejects flood control programs designed to change attitudes (e.g., education) in part be-

cause attitudes towards flooding are unrelated to the behaviors of persons residing in flood plains.

A type of micro-theory of change which seems appropriate in the design and evaluation of many programs is a situational sociology. On the basis of considerable evidence, a situational sociology assumes that people alter what they say, what they think, what they feel, what they do, as they alter their definition of the social situation in which they find themselves. In the calculus from which the act emerges, people take into account who is there, what kind of a place it is, the purpose of the gathering, and what they think others may expect of them. This being true, people may speak or think or act or feel in very different ways depending on how they define the immediate situation in which they find themselves. Although his emphasis is on objective features of the social environment rather than on subjective definition of it, Moos (1974) begins to move in this theoretical direction in his evaluation of treatment environments.

Any program which is designed to implement (and to test) situational theory finds itself automatically sensitive to the volatility of whatever changes it may induce. A change which takes place within a situation provided by a program can be expected to persist only under two general conditions: persons who change as a result of defining new situations, the elements of which have been provided by a program, will remain in a changed state only as long as (1) they remain in the program situation, or (2) they can define salient life situations outside of the program in the same manner that they defined the program. Moos' (1974) research on psychiatric treatment programs provides considerable documentation for this.

Situational sociology is not an appropriate theoretical framework for some kinds of programs. Heberlein (1973) has argued persuasively that social structural changes are necessary if effective reduction of human tragedy during natural disasters is to be achieved. Brooks (1971), in his discussion of community action programs, points out that enduring changes in communities and societies are not produced quickly or easily. In fact, as he suggests, it may take a generation to estimate the effects of a community action program. Different kinds of programs are based on different (implicit or explicit) theories of change and thus require different kinds of evaluation efforts. Weiss and Rein (1972) complain that their evaluation of Mobilization for Youth failed because they tried to estimate efforts to change agencies by measuring changes in individuals. It is a serious error to apply interpersonal theory to programs which intend to change organizations. To detect the effectiveness of altering communities (as Heberlein, Brooks, Voth, and Weiss and Rein attempted) requires considerable amounts of time. Anticipated time frames provide useful clues as to what kind of theory may be relevant. It is instructive to consider that it takes a long time to alter public opinion (Hyman and Wright, 1967), to gain effective changes in community health (Freeman and Sherwood, 1971) or to assess the impact of Head Start programs (McDill et al., 1972). When Angrist (1973) expresses concern about short-run situational effects, it is to such programs that this concern is rightly directed. But when programs are designed to change the minds or the actions of individuals, then situational effects are what situational sociology is built upon and they need not be short-run.

The rapid erosion of program effects is a familiar phenomenon. One example

is provided by the follow-up studies of Head Start, virtually all of which find initial differences between experimental and control groups "largely gone by the end of the first year of school (Williams and Evans, 1972:253)." But erosion is not necessary if one takes situational theory seriously. The apparent success of Synanon in the reform of drug addicts is, I suspect, based on the insistence that the addict is made aware of the fact that he can never leave Synanon—*physically* —without running serious risks of re-addiction. If one is to remain drug free, one must live at Synanon forever. Alcoholics Anonymous is a bit more subtle, but it can be seen as operating on the same principle. Along with others like themselves, alcoholics come to see themselves in a different way—they are coerced (albeit sometimes gently) into redefining themselves as drunks. Not only are they drunks in their own estimate, but they come to define themselves as people who will always be drunks, and it is that definition and its constant reenforcement which helps them not to drink—at least for today.

Alcoholics Anonymous helps people to arrive at a new definition of themselves and to constantly remind themselves of who they are, regardless of the physical surroundings in which they find themselves. Members of A.A. may frequent bars, attend cocktail parties and keep booze at home, but their definition of themselves as alcoholics, as long as it remains the pivot around which their lives revolve, enables them to define all of these situations differently from other participants. Nehemia Jordan once pointed out to me that people behave in the voting booth much the same as they behave in the presence of a political polster because both situations are defined the same, including the anonymity of the voter (respondent) and the statistical nature of the vote (response)—you are unimportant as a person; only as a number are you of interest to this election (poll). Employing his symbolic interactionist theory, which underlies much of the present discussion, Blumer (1948) has analyzed the manner in which similar definitions of the situations can be expected to elicit consistency in attitudes and actions.

It follows from a situational sociology that, although attitudes cannot be expected to conform to behaviors, and attitudes or behaviors elicited at one point of time in one situation cannot be expected to be the same as those elicited at another point of time in another situation, sometimes there may be consistency. Attitude change may persist and may even lead to behavior change (or vice versa) *if the situation at the later point of time is defined in the same manner as the earlier one.* A situational theory provides a plausible explanation of change, has empirical support, and can be directly applied to program design. Furthermore, to the extent that such a theory underlies a program, it is amenable to testing, that is, to evaluation, using the same deductive methods customarily employed by social scientists. We do have traditional techniques for testing theory with data!

Situational theory is proposed as an example of a type of theory which is appropriate for guiding the design and the evaluation of social programs which intend to alter the actions or attitudes of sets of individuals within the society. Such programs and such a theory are severely limited in their failure to take into account larger issues of social change. Why are situations defined as they are by members of one society or segment of a society and defined differently by others? How can a society or segment of a society be altered so as to provide a different range of definitions for members to choose from? But if situational theory fails to address

such issues, so too do most social programs (Deutscher, 1976). To the extent that programs tend to be situational, then for better or for worse, it is appropriate for evaluation theory to be situational.

Making Implicit Theories Explicit

Although the social scientist may on occasion find it possible to design the program which is to be evaluated, such opportunities are rare. Therefore, traditional deductive theory-testing as an evaluation procedure is not going to be possible on most of the occasions when evaluation is called for. Two decades have passed since Gouldner (1957:94–95) urged us to consider the explanations and hypotheses which laymen exhibit in their dealings with social problems. To ignore practitioner theory would, according to Gouldner, not only be poor public relations but would also betray the traditional debunking functions of sociological analysis. He urges the applied social scientist to draw competing hypotheses from practitioners and other nonscientists. Although it might be ideal to implement simultaneously a number of programs based on competing scientific and lay theories, such an opportunity is unlikely. Because the social scientist is rarely able to design the program which is to be evaluated, alternative theoretical procedures would be helpful.

There is an alternative which has several advantages over the customary procedure of imposing a theory on a set of data. Rather than beginning with a theory, we can begin with a quest which Glaser and Strauss (1967) have described as the discovery of theory. Such a quest imposes difficult detective work on the part of the social scientist, but when this inductive procedure is completed, the scientist can relax in the familiar techniques of deductively testing it. One of the advantages of this alternative then is that it provides the means for pursuing the originally preferred methodology, but in such a way that "input" problems in the program are not problematic. In order to discover the actual theory on which a program is based, one must explore what is in fact in the program. Thus, an ongoing program can be evaluated theoretically regardless of the apparent absence of theory in its design.

The idea of discovering a theory assumes that one is there to be discovered— an assumption not readily acceptable to all social scientists, but central to the work of some phenomenologists such as Garfinkel. His concept of ethnomethodology takes as its point of departure the assumption that everyday members of any society are able to engage in meaningful interaction and to organize themselves in more or less effective ways because they operate on a set of shared, but often taken for granted rules, which constitute a working theory (Garfinkel, 1967). It is toward the discovery of those rules and that theory that Garfinkel and some of his students are moving in their ethnomethodological research. Wieder's (1973) analysis of a halfway house for addicts provides an example of a truly ethnomethodological evaluation.

In this sense, an ethnomethodological stance is what I am suggesting as an alternative. Let us discover the kinds of (usually) implicit theories—the rules which when taken together constitute a workable explanation of social processes —employed by those clinicians, practitioners, or educators who create operating programs. This is what Campbell (1971:10) seems to imply when he observes

that "the guesses of the experienced administrator and politician are apt to be on the average as wise as those of social scientists."

In order to pursue this alternative, it is helpful, but not necessary, to assume that honest people with years of experience involving hundreds or thousands of patients, clients, or students have evolved an understanding of what processes are most likely to alter the attitudes or behaviors of what kinds of people. Not all practitioners are equally competent or equally sensitive to the responses of those they attempt to help, but good or bad, there must be some underlying theory imbedded in whatever programs they may devise. They believe their programs are worth trying because they believe they have learned how to change people. The evaluator, having made these theories explicit, can put them to the test. This alternative method also permits the discovery of new theories of social change in the programs created by experts operating on their own implicit theories. Program evaluation provides the unusual opportunity of a built-in field test of such theories in the same setting and at the same time as they are discovered.

Summary and Conclusions

There are three dimensions to the problem under consideration. The design of social programs should ideally be based upon some theory of social change. To the extent that a program has been designed in this manner, its evaluation becomes a more or less routine research operation, testing the given theory. Such a procedure provides both an appropriate evaluation of the program and a test of the social theory on which the program is based.

I have suggested, as an example, one body of theory which appears to be relevant to the design of many social programs as we know them in the United States. Program designers and program evaluators however, can generate programs derived from whatever body of theory that may seem appropriate to them. The notion of program evaluation as theory testing is not contingent upon any particular theory.

In the context of the types of methodology conventionally taught in graduate departments, the testing of theoretically derived hypotheses is not problematic. But the final alternative strategy suggested in this paper implies methods which are not so widely applied and understood and perhaps not so well developed as they need to be. To discover practitioner theory requires some form of participant observation, especially when the practitioners are not self-conscious about their theory. Although the methodological rationale has been spelled out (Glaser and Strauss, 1967), the procedural techniques remain mystifying to most social scientists. Bogdan and Taylor (1975) have done much to demystify participant observation for the beginning field worker. Nevertheless, most social scientists are uncomfortable with procedures which are not well codified, the outcomes of which are not predictable, and which are extremely demanding in terms of time, intellect, and imagination. Such procedures are expensive in all of these terms, because once the investigator has teased out the underlying theory which guides a program, the evaluation of the program remains incomplete without testing that theory.

The evaluation of social programs is sometimes denigrated as less than honorable work for the social scientist, yet as Gouldner suggests in the lines which open

this paper, it is an activity which "emerges from the deepest taproots of their disciplines and has the most venerable tradition." Furthermore, deliberately designed social programs which intend to produce social change provide the best field opportunity available to the social scientist for testing and developing social theory. Such an activity is hardly less than honorable work for the social scientist.

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