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Evaluation as Social Technology[1]

INTRODUCTION

So far I have argued that the value of outcome evaluation can be increased if type, validity, and usefulness are considered separately as distinct aspects of any given evaluation plan. Each of these three elements contains implications for the other two, but those implications will not be clear unless each component part is analyzed separately. There is another aspect of the problem which must also be considered, namely, the basic philosophical model of knowledge seeking upon which outcome evaluation is based. This is a consideration which cuts across elements of type, validity and usefulness, and deals with basic approaches to social research. How are questions formulated? How are variables chosen? What decision rules are used to weigh evidence, draw conclusions, and make recommendations? The answers to these questions reflect a philosophical model of research, and the model chosen can have far-reaching effects on the ultimate value of any research project. There are three main aspects to the argument about to be developed. First, there are crucial differences between scientific and technological models of knowledge development. Second, these differences have profound implications for the practical value of research. Third, evaluation is far more of a technological than a scientific pursuit.

Evaluators strive to make their work as useful for decision making as possible. This desire is not an aberrant phenomenon in the history of social science, as there is a long tradition of trying to make social research responsive to the needs of society. It seems reasonable to consider the "evaluation usefulness" issue as part of this tradition, as there is no reason to assume, a priori, that evaluation is uniquely different from all other aspects of social research. In fact, a survey of critiques of relevance in social research will prove extremely enlightening and applicable to the case of evaluation. It will become clear that a good many of the problems which impede the relevance of social research will be lessened by reconceptualizing evaluation as a technological rather than a scientific process.

WHY IS SOCIAL RESEARCH NOT RELEVANT? A REVIEW OF CRITIQUES

There are four types of explanation for the lack of relevance of social research.[2] Each type presents a different aspect of the problem, and all have some bearing on the choice of appropriate evaluation activity.

Cultural/Historical/Sociological Roots of Irrelevance

The course of social research is profoundly influenced by the greater social milieu in which the work is embedded, and theories generated in one setting are not easily transplanted to other settings. Further, those who perform social research can be said to have a "culture" all their own with shared values, a unique vocabulary, special interests, relatively distinct boundaries, its own reward system, and many other aspects of social groups. The needs and objectives of those who share that culture are often at variance with the needs and objectives of other groups. Even when there is agreement, communication is often difficult, and serious misunderstandings are common. All of these issues contribute to the problem of lack of relevance.

Gergen (1973) argues that while the methods of social research may be scientific, theories are often based on "acquired dispositions" which reflect the thinking of society about particular issues. If there is any truth to this statement, the relevance of social science will depend at least in part on the speed and direction with which social scientists change their theories relative to changing contemporary history. Irrelevance is likely if the change processes for society at large are out of step with the change processes within the social scientific community.

Moscovi (1972) makes essentially the same point. He argues that the content of American social psychological theories are uniquely American, and as such are not applicable to countries with other social systems or different basic mechanisms for organizing economic and social life. Our theories are culture bound and are not necessarily applicable when transposed to new surroundings.

For both Moscovi and Gergen, social theories are a reflection of the culture in which scientists work, and consequently, any change in the cultural context may lead to a lack of "fit" between existing theories and their surrounding societal context.

Another aspect of the culture problem lies in differences between scientists and the rest of society in terms of vocabulary, goals, epistemological beliefs, reward systems, time frame for work completion, and the like. Morell (1977b) has developed the notion that these differences amount to a "culture gap" between social scientists and the administrators of social programs.

This "culture gap" is an important aspect of the lack of relevance because the social scientists' work simply does not fit into the world of program planning and administration. There are too many differences in terms of requirements for knowledge, beliefs in the quality of different types of knowledge, questions which are considered important, and reward systems.

As an example of this problem, Lorenzen and Braskamp (1978) have conducted research which indicates that of three different types of information -- political, cost benefit, and scientific-administrators in mental health settings attend only to the cost-benefit data. Their evidence suggests that this may be true regardless of the nature of the problem for which the information is collected, or the "true" best fit between problem and information. Unfortunately our best theories of client success in mental health settings do not involve cost-benefit issues. Although some work has been done in this area (for example, Newman, Burwell, and Underhill 1978), cost-benefit aspects of client improvement in mental health are certainly not the major thrust of current mental health research. In this case both the theories of mental health upon which evaluations are based, and the major interests of evaluators, seem to be at variance with the needs and interests of decision makers.

In sum, one important reason for the lack of relevance of social science may lie in a lack of congruence between the body of social scientific knowledge and the requirements for knowledge which emerge from the larger societal context.

Irrelevance Imposed by the Basic Strategies of Science

The basic goal of science is to discover what is true (Popper 1965, chap. 3). This goal is pursued by means of theory development and through research. There are inherent elements in both of these strategies (theory development and research) which may result in the inapplicability of social scientific work to the understanding of in vivo social phenomena. I will deal first with issues related to methodology and then turn the discussion to the constraints imposed by the nature of theory.

Many arguments have been advanced to the effect that the most powerful scientific procedures are also the hardest to apply in a manner which will be applicable to non-research situations. Argyris (1975) claims that many experiments require the researcher to exert a high degree of control over his or her subjects, and to advocate one particular, limited, well-defined course of action. He further argues that experiments involve minimal amounts of real time learning by both subjects and researchers. Argyris' argument is that because of these constraints, results obtained from experimental research may not be applicable to similar, but non-experimental situations.

Tajfel (1972) cites conditions under which experiments will be applicable to real world situations, but the conditions he requires are rarely met. As examples, Tajfel believes experiments must be interpreted in light of knowledge about the social science context of research and the effects of researchers' and subjects' expectations on research results. This type of information is rarely available and difficult to obtain.

A different approach to the problems caused by research methodology is taken by Fdwards, Guttentag, and Snapper (1975). Their basic claim is that there are fundamental limitations imposed by the traditional approach to hypothesis testing, and that those limitations preclude evaluation from providing a "usable conceptual framework and methodology that links inferences about states of the world, the values of decision makers, and decisions" (p. 140). This critique was developed specifically within the context of evaluation, but presumably it would also apply to other (but not all) applied research situations.

While some of the problems of lack of relevance in social science emanate from the methodology of science, still others emerge from the nature of scientific theory. An essential element of theory building is the development of models of phenomena which *are and must be* simpler than the actual working of that phenomenon under natural conditions. (See for example, Kaplan 1964, chap. 7.) The "simplistic models" idea is based on the notion that all events are caused by a very large (in fact, an infinite) number of factors. Although some of those causal factors may be much more immediate and powerful than others, the actual number is limitless. Since it is beyond human capacity to explain an infinite number of factors, our models will always reflect a simpler picture of reality than is actually the case. Another aspect of the problem is practical. Our models are often likely to exclude factors which we may know influence an event. Those factors are left out because including them would obscure the relationships among particular variables of special interest. Given the scientist's need to construct simplistic models of reality, it is by no means surprising that those models often fall short when called upon to predict or explain events which occur in uncontrolled, unsimplified settings.

Another aspect of the problem is that theories can be said to have the properties of range and accuracy (Bunge 1967). Range refers to the number of elements which the theory explains. Accuracy refers to the precision of those explanations. Bunge advances the notion that there may well be a trade-off between these two dimensions, and that an increase in one may well lead to a decrease in the other. If there is any truth to this assertion, it is easy to see why many theories in science are found wanting when called upon to explain social events. The complexity of real life events may result in a lack of consonance between the artificially simplified event for which a theory was developed, and the uncontrolled event which must be explained. In a sense, the range of the theory may be extended beyond the precise range for which it was originally developed, with an attendant decrease in the accuracy of explanation. Since many theories are usually based on simplistic situations, or are developed for specific contexts, the use of theory in diverse practical settings is very likely to result in a stretching of the theory's range, with a consequent decrease in accuracy.

Callahan (1962) has documented a dramatic example of the misapplication of theory. In a carefully reasoned and well-researched analysis, he has shown that a considerable drop in educational quality resulted from the wholesale transfer of business management concepts into the field of educational administration. Such transplanting was rampant in the early part of this century, and in Callahan's judgment, the results were tragic. Callahan does not argue that good management practice should be excluded from educational administration. He does claim that management requirements are different in education and in the profit-making sector, and that theories developed in the latter cannot be transposed in wholesale fashion to the former. In a sense, the range of business management theory was stretched considerably. The resulting predictions from that theory were not only inaccurate, but extremely counterproductive.

In sum, there are certain requirements of scientific methodology and theory which are likely to result in a lack of applicability of scientific findings to the general course of events. Although it is conceivable that these problems may be ameliorated, it is impossible for them to be eliminated, as they derive from fundamental aspects of the scientific research enterprise.

Irrelevance as a Function of Choice of Topics for Study

One important criticism about the irrelevance of social research is that researchers are devoting their time to the study of the wrong variables. This is not an issue of the relevance of theory to a particular time and place, nor is it an issue of methodology. It is a criticism of the choice of topics of study-a critique of people's interests and priorities. Goodwin and Tu (1975), for instance, wonder why more psychological studies are not done concerning attitudes about taxation, or on other policy-relevant matters. A second example may be drawn from income maintenance research. Berk and Rossi (1977) criticize the evaluation of such programs for an overemphasis on "work disincentive," and for downplaying factors such as effects on improved health, or the enrichment of leisure time. They argue that although information on work disincentive may meet the major priorities of Congress, it may not make for an accurate assessment of income maintenance programs.

A common theme runs through all of these arguments. Social research as it is presently constituted has not yielded explanations that are useful in understanding or predicting complex social systems or behavior, and the reason for the failure is based on a mistaken choice of problems, variables, or questions.

Irrelevance as a Function of the Impotence of the Social Research Enterprise

Arguments have been advanced to the effect that evaluation research has not been helpful in solving social problems simply because the social research enterprise does not have the power to help with such problems. Rossi (1972) for instance, claims that many current social problems are extraordinarily difficult to solve, and that only small gains can be expected from attempts at their solution. He argues that much of what is easily done has already been accomplished, and that which is left is highly resistant to solution. Rossi (1972, p.226) cites the example of illiteracy:

> Dramatic effects on illiteracy can be achieved by providing schools and teachers to all children: Achieving a universally high enough level of literacy and knowledge, so that everyone capable of learning can find a good spot in our modem labor force, is a lot more difficult.

If there is any truth to Rossi's assertion, it is little wonder that research has not been helpful in pointing directions to solutions. There may be no good solutions, and all the science and all the research in the world will not change that fact.

A similar problem is alluded to by Carol Weiss (1973) in her writings on the political nature of evaluation research. Weiss argues that attempts to solve social problems are forged in the political arena, complete with all the vested interests and influence patterns which are part and parcel of the political decision-making world. If there is any truth to her analysis, scientific knowledge will be related in only the most tangential manner to attempts at solving social problems. The reason for the weak and indirect relationship is that political dynamics allow only those solutions which are based on single-cause models. Any research which indicates the need for solutions based on multiple-cause models will be disregarded. In this case, research is irrelevant not in the sense that it cannot point to better solutions, but in the sense that it will not be called upon to do so.

Another aspect of the problem is that our best methodologies may be least appropriate to the problems at hand: Berk and Rossi (1977) argue that the methodology of applied social research is far more highly developed for the study of individuals than it is for the study of organizations. They cite the case (p. 81) of studies of alienation, where:

> ... there is a conventional scale for measuring the alienation of individuals, but not for the alienating tendencies of work organizations.

To the extent that meaningful solutions to social problems will emerge from studies of social structure, evaluators find themselves with inadequate methodological tools for the work they must do.

A FRAMEWORK FOR SOLUTIONS

Each of these criticisms of lack of relevance in social research has some validity, and each sheds light on a different aspect of the problem. Taken together, they constitute a formidable obstacle to the conduct of relevant social research on a systematic and ongoing basis. An efficient way of overcoming this obstacle is by means of a single overriding framework which could lead to a solution to each of the variety of problems involved. If such a perspective could be found, we would have a powerful and simple means of furthering the development of relevant social research. The purpose of this chapter is to argue that the technological approach to research is just such a model.

There are important differences between science and technology, and many problems of relevance will be solved or lessened if social research is based on the technological model. In particular, that aspect of research called "outcome" evaluation will benefit from such a formulation. The next section will detail the crucial differences between science and technology. The discussion will then show how the technological model will help evaluation avoid or reduce the problems which cause irrelevance in social research.

DIFFERENCES BETWEEN SCIENCE AND TECHNOLOGY

A review of the literature on the nature of science and technology reveals four types of differences between the two enterprises. Each difference has its own implications for the use of evaluation, and each highlights different aspects of the relevance problem.

Theory in Science and Technology

An analysis of scientific and technological theory by Bunge (1967) makes it clear that there are profound differences between theory as a guide to practical action and theory as a model for understanding nature in as truthful and accurate a manner as possible. In fact, the requirements for good theory in each realm are often incompatible, if not antithetical. Incorrect or inaccurate theories often lead to correct predictions, as often has been true, for example, in the case of weather forecasting. Thus, although accurate prediction may be a valid criterion for judging the value of technological theory, it may be misleading as an aid in the discovery of truth.

Testing theories for truth or accuracy must often take place in settings which are deliberately removed from the noise and distractions of naturalistic settings, as such distractions may make it impossible to investigate subtle (or even not so subtle) relationships among variables. Further, the truth seeking endeavor does not put the scientist under any obligation to test his or her theories under naturalistic conditions. In terms of truth-seeking work, there is no logical imperative to do so. On the other hand, the ultimate test of theory as a guide to action is whether the theory proves useful in the face of the very same real world noise which is deliberately removed from the truth-seeking endeavor. Consequently the developer of technological theory is duty bound to test the practical consequences of his or her theories.

The difference is crucial because it relates to the way in which researchers orient their thinking, choose their projects, and organize their work efforts. When truth seeking is the goal, work may or may not force researchers to confront issues of practicality. When guides to action are being sought, practicality becomes a central, organizing principle of an entire research effort. Technological theories may still have to be developed in artificial settings, but they *must* be evaluated in light of real world events. This is not true in the case of science.

The search for guides to action might actually be hampered by seeking too much truth. As Bunge (1967) points out, lens designers rely on the wave theory of light-a theory which has been outdated since early in the twentieth century. The wave-particle theory of electromagnetic radiation., with its attendant quantum mechanical considerations, is far more true than is the outdated wave theory. But the use of quantum mechanics would enormously complicate the job of the lens designer, have no practical benefit for the design of better lenses, and waste enormous amounts of time. (On the other hand, if only practical needs were taken into account, the development of quantum mechanics would have been greatly impeded.)

As an example, in the behavioral sciences, consider an evaluator's use of psychotherapy change measures which are reliable and valid, and whose validity is based on a well-tested theory of psychopathology. Those very same measures may also be impractical for use in ongoing service agency settings. The reasons for such impracticality may be many. Measures may be accurate only if used by specially trained personnel. A measure may be time-consuming and not fit with the hectic schedule of service providers, thus leading to improper use of the measure. A scale may be usable only in abbreviated form because of limitations of space on existing forms or the programming requirements of a management information system. Those problems take on considerable significance if an evaluation objective is to develop an ongoing evaluation system for use within an agency. In such cases the feasibility of a measure becomes at least as important a factor as reliability and validity. In fact, it might be useful to sacrifice some scientific rigor for the sake of practical application. The result of such a trade-off may be a measure which does not give the highest possible quality information, but which will be extremely useful to program planners, administrators and service providers. As in the case of lens design, the "scientifically better" or ,'more true" approach has less practical value than the "scientifically inferior" method.

In sum, the differences between technological and scientific theory relate to issues of prediction, contexts of application, and strategies of theory development. In all these cases it is likely that theories which are useful in a technological sense might be counterproductive to the development of theories which are powerful guides to action.

Search and Decision Strategies

There are important differences between science and technology in terms of how problems or variables are chosen for study, the requirements for accuracy of results, and the type of evidence which constitutes a solution to problems or questions.

The Choice of Topics. Given that all phenomena have an infinite number of causal factors, which aspects of a problem should be studied in any given research setting? The criteria for answering this question are different for science and technology, and as a result different aspects of a problem are likely to be studied depending on whether one approaches the issue from a scientific or a technological viewpoint. The technologist will be interested in those causes which are most immediate, or most powerful, or most manipulable within the constraints of a specific real world context of action. The scientist will be most interested in studying those causes which will clear up a conceptual difficulty, or which will help determine the truth of a speculation, or which will advance the development of theory.

There is no reason to assume that scientific and technological criteria for choosing variables will necessarily lead to the same point. In fact, it is easy to see how the search for truth may focus attention on variables or phenomena which have little or no practical value. Consider the example of the search for cross-situational consistencies in behavior. Efforts at understanding and predicting behavioral consistency have considerable theoretical importance in social psychology, and enormous efforts have been put into research on the topic (Bem and Allen 1974, Mischel 1973). The general (and highly simplified) conclusion is that cross-situational consistencies in behavior probably do exist, but only in highly constrained circumstances. Further, the prediction of people's behavior requires careful assessment of various aspects of social situations and, probably, of individuals as well. Given the amount of precise information needed to predict individual behavior, it does not seem reasonable that theories of behavior consistency will have much practical use in understanding highly complex social programs. Even if all the necessary measurements could be made (which is not likely at this point in our social psychological knowledge), it is not likely that they could be made in the hectic world of social service. Service providers are too busy, measurement tools are too delicate, and the client population is too diverse. There are legitimate and important theoretical issues to be studied in the area of behavior consistency, and the topic does have considerable importance. On the other hand, theories of behavior consistency are not useful guides to the choice of variables in evaluation settings, the scientific value of those theories notwithstanding. Given the amount of substantive debate and measurement problems involved, a consideration of variables important in behavior consistency research is almost certain to focus attention on issues which have minimal importance for people who plan large-scale programs.

Levels of Accuracy. Once problems are chosen it becomes necessary to decide on the level of accuracy which will be accepted for results and solutions. Here too, technology and science are different. Within the limits of available instrumentation, the person engaged in the scientific search for truth must attempt to make results and solutions as accurate as can be obtained. (This does not mean using inappropriate measures, such as angstrom units for astronomical research. It does mean an attempt to be as precise and accurate as possible within the problem context in which research is being conducted.) The applied researcher, on the other hand, has the liberty of specifying, in advance, an acceptable degree of accuracy (Ackoff, Gupta, and Minas 1962). Further, that degree of accuracy can be considerably less than the greatest amount of accuracy which is attainable. In other words, those engaged in applied research can specify the precision of a solution which is "good enough," and that determination usually depends on practical considerations which result from the context in which research results must be applied. The search for truth, on the other hand, does not allow such liberties.[3] Scientific results are never good enough unless they are as good as they can be. The technologist who operates on the scientific model is likely to invest time, effort, and resources in pursuing levels of accuracy which are unnecessary and, perhaps, even misleading as guides to practical action.

A good example of this problem can be found in the area of psychological testing. Clinicians have long known that the full battery of psychological diagnostic tests (WAIS, MMPI, etc.) provide far more detailed and multifaceted information than can possibly be used for overall assessment. When judgments have to be made, much information must be glossed over or ignored. The area of diagnosis is simply too imprecise to make good use of all the information which the science of psychometrics is able to provide. When complex tests are used, some information must be ignored. Every jot and tittle of a psychological profile cannot demand attention. From a scientific point of view, however, precision and multidimensionality are much to be desired. Specific research hypotheses about personality cannot be investigated with imprecise tests, and small differences among groups of people may have major consequences for one or another theory of personality.

Types of Evidence. A third important difference between decision strategies in science and technology deals with the relative importance of refutation and confirmation. Agassi (1968) points out that traditionally science relies heavily on the concept of refutation. Since it is impossible to investigate all possible instances of a phenomenon, it is impossible to state unequivocally that a particular assertion is true. On the other hand, it takes only one disconfirming instance to demonstrate that an assertion is false. Thus in the search for scientific truth, disconfirmation and the refutation of assertions are much more important than confirmation or attempts to prove that hypotheses are true.[4] I Not so in the world of technology, which is tied at least as much to the practical world of political decisions as it is to the realm of science.

A further element in Agassi's (1968) argument is that the conduct of technological investigation often involves the commitment of resources to the solution of a problem of considerable social significance. Resources are always scarce, and decision makers are in great need of guidance as to which of a number of innovations are likely to be most successful. The aim is not to prove things true, or to show that one solution is certainly the best, but to eliminate the worst alternatives and to choose from the ones which are likely to prove most beneficial. In that sense, confirmation becomes a very important concept. Each time a solution has been shown to lead to an acceptable outcome, the faith of the public and of decision makers in that course of action will increase. Technological endeavors may be seen as efforts at confirming expectations about particular programs or course of action, while the chief role of science is to provide empirically testable explanations about states of nature. The main criterion for a good technological study is its potential to accurately affect our perceptions of the probability of success of a particular innovation. In contrast, the main criterion for a good scientific study is its potential to test, via the mechanism of disconfirmation, assertions about the true state of nature.

According to Agassi (1968) these differences can lead to important practical problems if the wrong model is followed. In science, standards of criticism can be raised as high as possible in any given situation. The more stringent the criticism, the better, as **it** is truth which is at stake. In technology, however, it is possible to raise standards of criticism too high, and in so doing, impede the implementation of needed innovations.

Summary. The search and decision strategies of science and technology are different in three crucial areas. Differences exist between them in terms of how problems or variables are chosen for study, in the ability to specify levels of precision and accuracy, and in the importance of designing research so as to confirm expectations of success or to disconfirm a stated hypothesis. These differences add up to entirely different methods of approaching problems and conceptualizing important issues.

Key Elements and Goals in Science and Technology

Agassi (1966) believes that as a minimum technology includes elements of applied science, problems of the implementation of research findings, and issues relating to the maintenance of existing systems. Wiesner (1970, p. 85), in a discussion about making psychology more relevant to the problems of society claims that:

> Technologists apply science when they can, and in fact achieve their most elegant solutions when an adequate theoretical basis exists for their work, but normally are not halted by the lack of a theoretical base. They fill in gaps by drawing on experience, intuition, judgment and experimentally obtained information.

Sporn (1964), in an analysis of the nature of engineering (a field which is certainly closely related to technology) echoes the sentiments of Weisner. Sporn claims that engineering empiricism can provide a guide for action in cases where a theoretical base is lacking. To do so, the practice of engineering must include elements of science, tools, methods, systems and social organization.

Writings on the question of goals in science and technology make it quite clear that the objectives of the two endeavors are not the same. SkoIimowski (1966) claims that the goals of science are to investigate reality, enlarge knowledge, acquire truth, and study "what is." Technology, on the other hand, seeks to increase the efficiency of given techniques, create a reality of our own design, and in general, is far more concerned with what "ought to be" than it is with investigating "what is."

The difference between science and technology is best summed up by Jarvie 1972) who wrote: "The aim of technology is to be effective rather than true, and this makes it very different from science."

In a discussion of the methodology of engineering, Walentynowicz (1968) argues that while scientific achievement lies in the truth of a statement which might be made, engineering success is the effective solution to a proposed problem. Further, the solution must be acceptable within the constraints imposed from four sources: a particular body of knowledge, a given set of skills, a well-defined point of view, and the constraints imposed by available resources.

Although it may be argued that factors such as the lack of theory, limitations on freedom of action, and the need for non-theoretical information are operative *in any* research, a crucial difference still exists between science and technology. The entire scientific enterprise is geared toward overcoming these limitations in order to further the search for truth. The raison d'etre of technological work is to maximize solutions which must operate within practical constraints.

Summary: Differences Between Science and Technology

Although science and technology share many surface similarities, an investigation of the logical structures which underlie each endeavor reveals quite clearly that they are *not* one in the same. (A summary of crucial differences appears in Table 5. 1.) Scientific theory is very different from technological theory. Search and decision strategies are different in each realm. Rules for the acceptability of

Table 5.1. Summary of Differences between Science and Technology

	Science	**Technology**
Theory	Main use is as a guide to truth	Main use is as a guide to practical action
	Emphasis on understanding	Emphasis on prediction and control
	No obligation to test in practical settings	Must be tested for usefulness in practical settings
Search and design strategies	Study of factors which explain conceptual strategies	Study factors which are immediate, powerful, and manipulable within practical setting
	Accuracy must be as high as possible	Accuracy determined by limitations of action in real world settings
	Emphasis on refutation	Emphasis on confirmation
Key elements and goals	Theory development	All factors which help as guides to practical action (theory, intuition, experiment, social systems)
	Truth, "what is"	Efficiency, reality of our own design, "what ought to be", effectiveness

evidence differ. The goals of science and technology are different. Taken as a whole, these differences have far-reaching implications for the ways in which scientists and technologists choose problems, invest their efforts and resources, select audiences, evaluate evidence, and make recommendations. These differences are not merely alternate routes to the same place. Because of the differences between science and technology, it is quite likely that each approach will lead to entirely different strategies for the study of social problems. Further, it is likely that the subject matter of each investigation and the conclusions arrived at will also differ greatly.

So far this chapter has reviewed two main areas-critiques of relevance in social science, and the differences between the technological and scientific methods of approaching research. It remains to be shown how these problems can be reduced if evaluation research is conceptualized as a technological rather than a scientific enterprise.

ADVANTAGES OF EVALUATION AS SOCIAL TECHNOLOGY

As we have seen, basic critiques of the relevance of evaluation (and social science in general) revolve around four issues: cultural/historical/sociological factors; constraints which result from the basic approach of science; the choice of topics for study; and impotence of the social science enterprise. We have also seen that differences between science and technology revolve around questions of theory, search and decision strategies, goals and key elements. This section will attempt to synthesize this information so that evaluation can become a powerful factor in the solution of social problems. The discussion will deal first with issues of culture and history, and then collectively treat issues of constraints inherent in the basic approach of science, the choice of topics for study, and the general impotence of social science research.

Cultural/Historical/Sociological Irrelevance

This critique operates on two levels. First, there is a difference in perspective which divides evaluators from the decision makers, policy setters and administrators with whom they work. (This issue will be elaborated in Chapter 6.) This division is characterized by differences of vocabulary, belief in the power of research techniques, reward systems, objectives, and priorities. Second, there is a more general difference between the needs and social/philosophical orientation of society at large, and the theories and perspectives of social scientists. Gergen (1973) and Moscovi (1972) discuss this issue in terms of the relevance of social psychological theories to other countries and other social systems, but a similar dynamic operates in the relation between evaluators and the society in which they are embedded.

Both aspects of the problem point to the need for a mechanism which will bring the thinking and theorizing of evaluators into line with the needs, priorities, and goals of the society that surrounds them. One promising solution is the development of evaluation along the model suggested by technology rather than that suggested by science, as technology is far more responsive to the demands of its social environment than is science. This increased responsiveness is manifest in all aspects of differences between science and technology-theory, search and decision strategies, goals, and key elements.

Irrelevance Based on Theory. The value of technological theory is directly related to the usefulness of the theory as a guide to practical action. As such, technological theory must be developed within a context of responsiveness to practical issues. Theories which are true but irrelevant or weak in the real world of everyday action will be discarded, a tendency which may not exist in the scientific world where theories are judged by testability, influence on understanding states of nature, and contribution to clearing up conceptual difficulties. To the extent that evaluators plan their work on the basis of scientific theories, they run a risk of gaining knowledge about social programs which is valid, but which may also be irrelevant as a practical guide to action.

As an example, consider the evaluation of an educational program which is guided by theories of the relationship between subtle teacher-child interactions and academic achievement. Such a theory is likely to lead to the study of many interpersonal factors which may be difficult or impossible to substantially manipulate in non-research settings. Although the information which would be gained may be true and accurate, it would not be particularly useful to those who plan educational programs, administer schools, or teach classes. In contrast, consider the variables which would have been chosen had the evaluator operated on theories which relate curriculum and textbook content to academic achievement. These factors are relatively easily manipulated, and such information might be extremely useful in changing educational programs for the better.

Search and Decision Strategies. Similar problems exist when search and decision strategies are based on the scientific rather than the technological model. Variables are chosen in science for their ability to suggest theories which are accurate reflections of the true states of nature. Unfortunately, the variables which are most important for such knowledge may not be the same as those variables which are the more powerful determinants of real world events. In fact, small unexplained aberrations in the prediction of an event are often the best clues to the discovery of new truths and the furtherance of theory. Consequently a scientific orientation to the choice of problems and variables may lead to factors which are irrelevant to people whose chief interest is discovering powerful solutions to serious practical problems.

As an example, consider the debate over race and intelligence (Brody and Brody 1976; Jensen 1973). The hereditability of psychological characteristics is certainly an important issue for our understanding of individual behavior, psychological makeup, and the relationship of individuals to social systems. Consequently there is a good deal of scientific justification for studying the topic. But does the topic have any practical value for those who plan educational programs or social policy? One could cogently argue that it does not. Our society is very much based on an ethic which encourages each person to realize his or her highest potential. As a result of this ethic, much of our educational system is geared (at least ideally) to educate each student to the best of his or her ability. We know that many black children have higher lQs than white children. This is an indisputable fact which clearly emerges from the very evidence which is used to demonstrate overall racial differences. In fact, the overlap in IQ is considerable. If the average black-white IQ difference is 15 points, and the standard deviation for intelligence tests is between 15-17 points (both commonly accepted figures), 16 percent of the black population has a higher IQ than the *average* score for whites. The number of black children who have higher IQs than whites is even greater. The implication of these figures is that many students (both black and white) stand to be done a considerable disservice by an educational planning system which pays too much attention to overall differences in intelligence among racial groups. The important point is that the disservice will occur even if the average difference figure is correct, and regardless of whether the difference is caused by hereditary or environmental factors. Thus we have a case of a scientific psychological problem which deserves attention, but which may mislead educational planners if they allow their priorities to be influenced by the issue.

A related issue is the specification of acceptable levels of accuracy. An important aspect of applied research is the identification of specific levels of accuracy which will satisfy the need for a solution to a practical problem (Ackoff, Gupta, and Minas, 1962). By making such decisions evaluators automatically gear their work to the requirements of those who are primarily responsible for implementing innovations. Failure to set levels of accuracy explicitly may lead to a mismatch between accuracy obtained in an evaluation study and the accuracy that is needed by decision makers and program planners. Too much precision is useless and distracting because it does not reflect the actual amount of influence which can be brought to bear on a problem. (As in the case of diagnostic testing, where more detailed information is provided than can possibly be used for placing individuals in various programs, organizations, or modalities.)

A final issue concerning decision strategies involves the roles of science and technology in the adoption of new plans or courses of action. As Agassi (1968) has shown, standards of criticism in science should be raised as high as possible. If this is done in the technological field, however, it is unlikely that decision makers will ever have enough confirmatory evidence to be willing to risk a new solution to a persistent problem. Thus technologists must constantly consider which level of evidence will guard against the choice of poor solutions but encourage experimenting with solutions which are among the better alternatives to a problem. If evaluators do not take this issue to heart they are likely to level too much criticism at a project without a sense of why decision makers need evaluators in the first place, namely, to perform research that will give people enough confidence in a new course of action so that further constructive efforts might be attempted.

In sum, search and decision strategies in science are aimed at developing theory which is true, at honing information to the highest possible level of accuracy, and at being as critical as possible of new ideas and of the tests of those ideas. All of these three objectives may be dysfunctional in a technological sense. In technology, accurate prediction is more important than truth, levels of accuracy are situationally determined, and the function of research has a much stronger practical confirmatory function than it does in science. All of these aspects of technology force researchers to confront the needs of decision makers because technological problems are fundamentally determined by the same need context which operates for administrators and planners. The defining context of scientific problems is entirely different, as theory development is not inherently tied to practical needs.

Key Elements and Goals in Science and Technology. The planning and conduct of any research must take into consideration elements of available tools and methods, practical issues of implementation, and constraints imposed by systems and social organization. In science these elements are obstacles to be overcome in the search for truth and the development of theory. In technology they represent the researchers' response to the very same parameters and operating principles that guide decision makers in the practical world of action. In essence, the world of the technologist is bounded by the same factors as the world of administrators and policy makers. Thus the technological model of research automatically relates to the problems and needs of those who must make decisions about social programs. The universe of the scientist is not so bounded, as scientists are permitted (in fact encouraged) to construct artificial situations in which tools, resources, and the like are extended as far as possible, with no regard for large-scale everyday limitations. Consequently researchers operating on the scientific model are likely to generate information which is incompatible with the needs of decision makers.

The goals of administration and planning are effectiveness, control over situations, and efficiency, all to be maximized within the constraints of available resources, existing knowledge, and freedom of action. These objectives will be pursued by planners and administrators regardless of whether researchers attempt to help or not. Technological research is automatically attuned to the same objectives, as success in technology is determined by the extent to which resources and knowledge can be used to increase efficiency, effectiveness and control over real world settings.

In addition to identical success criteria, it is likely that pursuit of that success is done with the same intellectual tools in the world of technology and the world of everyday choice making, i.e. theory, experience, intuition, judgment and experimentally obtained information. The only difference is that the technological research approach is likely to bring a rigor to the decision process which might be otherwise lacking. Although scientists also use all of these elements in decision making, the scientific enterprise is not specifically geared to sharpening the use of those elements in others.

Thus there is a correspondence between the goals and intellectual tools of everyday decision making, and the technological approach to problem solving. This correspondence is lacking in science, which generates rewards not for how well solutions operate within practical constraints, but for how well those practical constraints are transcended in the pursuit of truth and theory development.

Summary: Technology as a Solution to Cultural/Historical Irrelevance. A telling critique of the relevance of social science is that the theories and investigations which are generated by researchers are incompatible with the requirements or basic orientation of the surrounding cultural climate. The argument is that even if the methodology of research is applicable, the subject matter of the research is not. This problem is manifest in the field of evaluation by a disparity between what evaluators study and the solutions they propose on one hand, and the needs of decision makers and the general public on the other. What is needed to solve the problem is a mechanism to make the work of evaluators compatible with the more general problem-solving needs. A powerful method of accomplishing this task is to base evaluation on a technological model. Technological work is much more closely bound to the needs of society at large than is scientific work. This relationship is manifest in differences between science and technology as they relate to the nature and use of theory, the development of strategies of search and decision making, the key aspects of science and technology, and the goals of both endeavors. In all these cases, practical factors arise which the scientist seeks to transcend in order to further the search for truth and the development of theory. The aim of technology is not to transcend those factors, but to seek novel solutions which are the best possible under the limitations imposed. Scientists are rewarded for transcending those limitations. Technologists are rewarded for working successfully within them.

Modeling evaluation on the technological system merely imposes on the researcher precisely the same constraints and sensitivities which are encountered by those who, with or without the use of research, will seek solutions to genuine societal problems. Scientists do not have those constraints, or at least, are rewarded for avoiding them. Thus while the scientific model is inherently irrelevant to practical issues, the technological model is, by its nature, responsive to changes in the need for practical solutions to practical problems. Just as planners are rewarded for their responsiveness to solutions to new problems which may arise, so too are technologists rewarded for the same responsiveness. In sum, the technological model of research is likely to bring the "research culture" and the general societal culture into alignment.

Relevance as a Function of Basic Strategies, Choice of Topics, and the Impotence of Social Research

It has been claimed that basic strategies of research do not make for practically useful information, that researchers study topics which are irrelevant in the world of practical decision making, and that in any case, the social research enterprise does not have the power to help with pressing social problems. All of these criticisms have some validity, and none can be made to disappear. On the other hand, research has the potential to discover more powerful and precise information than can be obtained from other means. Thus we are faced with the problem of finding an approach to research which will minimize the problems of applicability and relevance, and will thus allow the advantages of the research approach to be brought to bear on practical problems. Organizing research efforts as a technological enterprise will accomplish this goal.

Basic Strategies of Research. The technological model of research yields a clear difference between research which is conducted for the sake of developing new innovations, and research for the sake of testing those innovations in actual practice. This distinction provides a frame of reference which will allow evaluators to meet a great many criticisms of the relevance of their work. Discriminating between development and field testing is not an important element in science. Such a distinction is, however, a major element in technological research. The development-field test distinction touches all dimensions of differences between science and technology, and in each case the technological model yields powerful guides to increased relevance. Argyris (1975) argues that the more powerful methodological tools of social science are not applicable in many field settings. Tajfel (1972) claims that experimentation can be useful, but only if a large amount of research is carried out on factors which influence the research context. Edwards, Guttentag, and Snapper (1975) believe that the classical approach to hypothesis testing is not particularly useful for evaluation situations.

All of these critiques assume, at least implicitly, that in order to be relevant, research must be done in settings which are highly similar to the situations in which research results are meant to generalize. In other words, the research setting must be as "messy" or complex as the real world setting of interest. If this were true it may indeed be impossible (for practical and ethical reasons) to conduct adequate research on practical problems. Fortunately the history of research clearly indicates that this is *not* the case, and that the simplifications which are necessary for research can lead to practical advances. The question turns on the distinction between technological research and development on the one hand, and the field testing of new techniques on the other. The two are not one and the same.

Research and development efforts can-and usually do-take place in settings which are different from those where products will ultimately be employed. The only requirements for development research are that major elements of real world influences be approximated in the research setting, and that variables be chosen which are likely to make a practical difference within the context of those influences. These requirements constitute the essential ingredients of the research and development phase of technology, and are quite compatible with artificial research settings.

An excellent example can be found in the concept of the "lab school." In these cases, universities or research institutions help set up and run schools for the express purpose of having a context for educational research. These settings are not exactly analogous to normal school systems. Crucial differences exist in the expectations and role perceptions of staff, funding structure, the source of authority for policy decisions, selection processes for students, size of student body, and the researcher's control over everyday activity. Given these differences between ordinary and laboratory schools, there is certainly no guarantee that educational programs developed in the controlled setting will operate effectively on a wide scale. On the other hand, laboratory schools are a reasonable approximation of normal school settings, and certainly provide a useful context for the development of meaningful educational innovations.

Another phase of technological work is the field testing of proposed innovations. The purpose of field testing is to determine whether an innovation will be successful without the artificiality, special attention, and extra funding which are inherent aspects of efforts at development. Field test research is less methodologically rigorous than development research. It must be so, as the reason for field testing is to see if an innovation will function as planned when operated by practitioners under everyday conditions. It is precisely for these reasons, though, that field tests should not be carried out unless the dynamics of an innovation's workings are well understood. Without such understanding, the noise of field settings would make it impossible to attribute program effects to particular aspects of a program, or to determine why a program did not function as planned. In the language of Campbell and Stanley (1966), field test research admits a large number of plausible rival hypotheses.

Because of these rival hypotheses, data from field test research cannot be interpreted in the absence of a well-developed knowledge base. Such a base can only come from methodologically rigorous efforts during the development phase of technological research.

It may be that in the actual practice of evaluation in social service, the line between program development and field testing is blurred and that evaluators find themselves doing two tasks at once. It is easy to see why in the face of such confusion evaluation is vulnerable to the charge of not appropriately applying powerful research designs. It is difficult to extract valid development data from field test contexts, and the attempt to do so is likely to sensitize evaluation's critics to think in terms of the requirements of development research. If evaluators speak the language of development research, they are likely to be perceived as doing development research, the real nature of the work notwithstanding.

One might argue that the fault is the evaluators', as they should know better than to jam square pegs into round holes-development research designs into field test settings. It is more likely, however, that the fault lies with the system in which we are all forced to attempt solutions to social problems. Campbell's (1971) experimenting society is not a reality and we all must do the best we can in difficult circumstances. There is a political dynamic to social service funding, and evaluators are just one of the parties who are caught in the maelstrom. On the other hand evaluators do not help their image when they fail to recognize the difference between development research and the field testing of innovations-a distinction which is crucial in technology but of minor importance in science.

An excellent example of differences between development and field test research can be found in the area of drug research. Typically, the development of a new drug passes through several stages. (A more in-depth explanation of this process can be found in Calesnick 1971.) First, a likely substance is identified. This process may result from the blind testing of numerous chemical substances, or from theories of biochemical action, or from some combination of the two. Once a substance is identified as a likely candidate, it is tested on animals for efficacy, negative effects, potency, and the like. These animal tests are conducted in a fairly rigorous manner, with the use of control groups, strict regulation of dosages, and careful attention to time schedules. If the new drug still seems useful, it may be tested on humans through the use of controlled clinical trials. These trials are also conducted according to a methodologically rigorous research plan. Ideally, the study should have a no-treatment control group, several experimental groups using varying dosages, and careful attention to drug administration and measurement of effects on the subject. In addition, such a study may also match the drug against the best-known accepted drug, and will be administered according to a double-blind format. Neither the subject nor the researcher should know which drug is being administered to whom. Research of this type is labor-intensive, and is usually conducted with a relatively small number of subjects. Finally, if the drug still seems worthwhile, it may be tested in large-scale, uncontrolled clinical trials. The final stage is needed to see how the drug will work with large and diverse populations. Such studies are also needed to make sure that the drug works appropriately when administered in the absence of close research scrutiny, and in the normal working context of everyday clinical medicine.

From our point of view, there are several interesting facets to this process. First, chemical substances are picked only if there is reason to believe that they will make a noticeable and practical difference in the everyday world of clinical medicine. This is a technological-not a scientific-criterion.

Second, both development and field test studies are employed. The animal research and the randomized clinical trials are necessary in order to develop precise expectations of what the drug will do. Uncontrolled research could not produce such knowledge. There is wide variation in people's reactions to drugs. The same drug may operate differently in different populations. Individual medical history may influence a drug's effect. Individual behavior-such as seeking other medical treatment-may affect drug action. Because of factors such as these, uncontrolled trials-no matter how large-could not supply accurate and valid information on drug action. Thus, randomized controlled trials are needed in order to develop a knowledge base which will allow the effects of a new drug to be understood.

Third, controlled clinical trials are not sufficient for making decisions about using a drug as part of general medical practice. The use and effects of a drug may differ in closely controlled circumstances and in general medical practice. Thus wide-scale research is needed to approximate the usage of the drug in general practice.

Fourth, the uncontrolled clinical trials would never be acceptable as the sole methodology for testing a new drug. On the other hand, those uncontrolled trials, if used within the context of an already developed knowledge base, can supply crucial information. Without that information, a decision to employ the new drug could not be made.

Scientists who are primarily interested in understanding biochemical mechanisms would not choose to study a substance only because it might be useful in clinical medicine. Given a choice between a substance which makes a practical difference and a substance which might elucidate an unknown chemical mechanism, the scientist would choose the latter. The technologist, on the other hand, would choose the substance which would make a practical difference. Scientists would not feel compelled to conduct uncontrolled clinical trials. Technologists must conduct such trials.

There is no reason why similar dynamics should not operate in the area of education, or income maintenance, or the development of mental health programs, or any other area of social endeavor. Development research could be used to establish a knowledge base which once established, would allow the interpretation of information from wide-spread trials. Both are needed in order to develop useful social innovations.

In sum, the methodology of social research can be rigorously exercised in the cause of evaluation as long as the distinction between development research and field testing is maintained. Although this distinction is a major element in the technological model of research, it is not crucial in the scientific model. Valid development research can be conducted if three conditions obtain. First, variables must be chosen for the express purpose of being powerful enough to make a discernable difference in the settings where they are destined to operate. Although the technological model is sensitive to this requirement, the scientific model is not. Second, the research setting must include the most powerful or important factors which might mitigate against the proper action of experimental variables or programs; scientific research attempts to screen out such factors. Finally, research settings must be simplified enough to allow observation of the relationships among important variables. If these conditions are met, there is a good chance of obtaining valid information which has a reasonable chance of directing meaningful action in real world settings. As to whether such results can be translated effectively into the real world, that must await the classic field test.

Field testing is a special form of research which by its nature cannot employ the more rigorous methodological techniques. Consequently, field test research cannot be adequately interpreted in the absence of an already well-developed knowledge base. There are better and worse ways of conducting field test research, but critics of such research cannot employ the criteria used for judging development studies. Field test research must be criticized within its own frame of reference.

Once a technological model of research is adapted, the ambiguity between development and field testing disappears and the requirements for choosing appropriate research designs become clear. Rigorous experimental or quasi-experimental designs are appropriate for development research, while less powerful techniques must be employed during field testing. Each type of research can vary greatly in methodological quality, but critiques must be confined to appropriate frames of reference. Part of the ambiguity between the two types of research is due to the political reality which forces evaluators to carry out both types of work simultaneously. Another part of the problem is failure to conceptualize research as clearly belonging to one or the other category. The scientific model of research blurs this distinction, while the technological model casts the development-field test distinction in sharp relief.

The Choice of Topics. Still another reason why technological models should be invoked in evaluation is that the scientific world is a very special existence in which priorities are not set with practical value in mind. In a sense, there is no obligation to conduct field tests in order to assess practicality. It might be said that science is very much an art in which the aesthetic criteria are truth and theory development. This is not to say that science is immune to sociological forces, as this is clearly not the case. Scientists are influenced by the priorities and needs of the world around them, funding priorities do indicate the research which will be done, and science is most certainly subject to fads and fashions. There is, however, a crucial difference between the influence of societal forces on science and on technology. In the case of science, there is no *logical* connection between practical societal needs and the topics of scientific investigation. Such factors are intruders on the logical structure of science. Science as a pursuit could exist without such forces, if only the world would let it. Not so in the case of technology, which, by its *essential nature* must be responsive to practical needs. This difference shows up in the nature of theory, in the choice of topics, and in the methods of both endeavors. This is the crucial reason for adapting the technological approach to evaluation, as on all levels, it orients the researcher to an interface with practical issues, theories, laboratory models, and variables which make a difference in the real world. Most important, the criteria of truth and potential to help with theory development-the aims of science are likely to lead *away* from practical concerns, or at least, any correspondence between the two is at best coincidental.

Impotence of Social Research. Rossi (1972) claims that social research may simply be too weak an enterprise to solve current social problems. Those problems are so difficult and unrelenting that current knowledge is simply not up to the challenge. Weiss (1973) believes that the problems may be solved, but only if programs are designed and funded to deal with the multiple causal factors which we know to be operating. The current political process of program funding does not recognize this reality, and until it does, many social problems will defy solution. How might evaluation help in a world of difficult problems and inadequate solutions? Again, the technological model comes to the rescue. To date, theories in social science have not addressed powerful variables set in a context of factors which are important in the real world. One reason for this state of affairs is that scientific theories are not developed for the express purpose of guiding social innovation. Bunge's (1967) notion that theories have both range and accuracy is useful here. As the range of any specific theory is stretched, its accuracy decreases. It may be that the use of scientific theories to address practical issues necessitates stretching the range of those theories, and as a result accuracy suffers. We would be far better off with theories specifically aimed at explaining factors which make a difference in the real world.

As an example, consider the case of the multi-causality of social problems. Weiss (1973) has criticized policy makers for funding programs which are based on single-cause theories even though we are certain that those problems must be dealt with on a multi-causal basis. Although program funding may be based on single-cause models, program evaluation need not follow the lead. Evaluators might measure many of the likely determinants of a problem even though the program being evaluated is designed to influence only one of those many factors. Such an evaluation might begin to give us a sense of which factors are truly important in a practical sense, and might point the way to theories of social action which are useful for program planning. For instance, the evaluation of a program designed to increase reading speed by teaching pattern recognition might include collateral measures of parental support for students' participation, achievement motivation, study habits, and the like. The same is true for mental health, drug abuse, corrections, and many other areas of social programming. Numerous social, psychological, and economic aspects of a problem may be incorporated into the evaluation of programs based on single cause models. The power of inference may not be as great in such situations as we might wish, but the information may still be useful, and likely to lead to theories which explain program effects in naturalistic settings.

Efforts of this kind may lead to predictions which are not as accurate as those of scientific prediction, but as Ackoff, Gupta, and Minas (1962) have pointed out, technologists have the luxury of setting limits on the amount of needed accuracy. This luxury is not shared by scientists who, in their search for truth, must try always to be as accurate as possible.

We have seen that technology includes several elements which are not shared by science. These include experiment, intuition, experience, and judgment. Although scientists certainly use these intellectual tools in their work, scientific efforts are not primarily aimed at expending effort to systematically sharpen others' ability to use those tools. Technologists, on the other hand, do make such efforts. An excellent example of this process is Guttentag's work in decision theoretics, where the major thrust is sharpening the ability of program personnel to make accurate judgments about their intuitions, observations, and opinions. (See for example: Edwards, Guttentag, and Snapper, 1975.) Operations research is another example where the main thrust is to help with the prediction of events regardless of the causal factors which are involved. As a consequence of its special emphasis on experiment, judgment, intuition and experience, technologically based evaluation is likely to help administrators and policy setters choose appropriate programs, see the importance of programs based on multiple causation, and choose from all available alternatives those programs which have the highest potential for success.

Evaluation alone is by no means equal to the task of finding radically new and powerful solutions to social problems, nor is it likely to change funding mechanisms in a pronounced manner. It can, however, make a contribution to this effort, and such a contribution is a natural outgrowth of the technological model which has, as integral elements, far more than truth seeking and theory development. Although conscientious scientists may also make such efforts, their attempts flow from a sense of civic responsibility rather than from the inherent nature of their work and their research. The logical structure of scientific work is not geared to finding successful practical innovations, and it is not surprising that civic responsibility has not transcended the limitations of logical structure. A concerted effort at employing a technological model of research might lead to powerful innovations which could work within present funding limitations.

Conclusion: Advantages of Evaluation as Social Technology

Rather than being a unique phenomenon, evaluation research is merely the latest manifestation of long-standing efforts at making social research relevant to the problems of society. As such, the shortcomings of evaluation must be understood within a larger framework which encompasses the more general issue of why social science has not been as socially relevant as it could or should be. Critiques of the relevance of social science revolve around four themes: the relation of theory to changing social needs, fundamental limitations of methodology and theory, the choice of inappropriate topics for investigation, and the difficulty of social problems relative to the weakness of social research. To some degree at least, all of these problems inhibit the usefulness of evaluation, and all become less serious when evaluation is conceptualized as a technological rather than a scientific endeavor.

Although there are many surface similarities between technology and science, profound differences exist between them which have direct and serious implications for the social relevance of research. (These differences are summarized in Table 5. 1.) Differences involve the nature of theory, research strategies, the dominant intellectual tools of both endeavors, and the goals of each. Taken together, the differences mean that technology is more responsive than science to the task of developing practical innovations. The use of theory, the choice of research topics, the reward system for researchers, the organization of resources-all are attuned to developing successful course of action for those who have the responsibility of solving everyday practical problems. The central theme of science is not organizing resources for successful practical action, but rather, organizing resources for the development of theory and the discovery of truth. The scientific goals are independent of the technological goals, and on many occasions they are antithetical.

Evaluation is a form of social research which emerged from a need to test the value of society's efforts at solving social problems. As such, evaluation must generate information which will be useful to decision makers. If evaluation is to fulfill that role, it must operate on a model which is attuned to maximizing solutions within a context of ever-changing political, social and economic.

Constraints. The scientific model, with its main goals of advancing truth and theory development, is attuned to surmounting those constraints by developing artificial situations which interact as little as possible with the practical world. Although scientists as individuals (or in groups) may be interested in helping with practical issues, the logical structure of their work is oriented toward goals which are, at best, irrelevant to the development of innovations which will survive the rigors of wide-scale implementation. Hence the need for conceptualizing evaluation as technology, a system in which the reward structure and the use of resources are intimately attuned to the issues involved in developing and testing practical innovations.

For the sake of clarity, the discussion so far has cast the scientific-technological distinction in the starkest possible contrast. One must not think, however, that there is no interplay between the two endeavors, that evaluation has no relation to social science, or that evaluators should not be concerned with matters of truth or theory development. Powerful interplays between science and technology do exist, and evaluators must consider those interactions. The main point in this chapter has been that, as a dominant mode of organizing activity, it is not productive to conceptualize evaluation as science, nor to assume that the search for truth and theory development will lead to successful practical innovation. Because of evaluation's highly specific and applied focus, the organizing theme of evaluation research must be technological. Now that the point has been made, the discussion will turn to the complementary aspects of science and technology. This will be done in order to obtain a sense of how the entire social research enterprise can be brought to bear on the solution of social problems.

INTERRELATIONSHIPS BETWEEN SCIENCE AND TECHNOLOGY

What is the relationship between social technology/evaluation and social science? What does each body of knowledge hold for the other? Is there a role for social science in helping to solve social problems? These issues cannot be ignored, as an over reliance on technological evaluation to the exclusion of social science is likely to stultify the search for substantive innovations which may help solve social problems.[5]

In an analysis of the differences between science and technology, Gruender (1971) argues that because the range of scientific study is limited, it is entirely possible that technologists will, in the course of their work, discover important inconsistencies in scientific theory. These inconsistencies may pose crucial problems to scientists, who would then have to redirect their thinking or research. Presumably, a similar process could also work in reverse. Scientists may discover facts which are inconsistent with technological theories, and which may direct the technologist into modifications or corrections which would improve guides to practical action.

Bunge (1963) claims that scientists and technologists are able to inspire each other, not because of any logical relation between science and technology, but because both endeavors are critical and analytic; and in a later work (1966, p. 128), because:

> ... knowledge considerably *improves* the chances of correct doing, and doing *may* lead to knowing more (now that we have learned that knowledge pays), not because action is knowledge but because, in inquisitive minds, action may trigger questioning.

Another reason why the scientific aspects of social problems cannot be ignored is that there are crucial junctures when interaction must take place if substantive progress is to occur. In the usual course of business, new technology flows from old technology, and not from science (De Sola Price, 1965). There are, however, cases where technological limits are reached and where further progress is impossible without a basic, new, fundamental understanding of a phenomenon. According to Feibleman (I 96 1) these are the times when technology must turn to science. But which body of science is sought at such times? De Sola Price (1965) makes it clear that avant garde technology does not turn to the newest scientific developments when help is needed. On the contrary, such events are rare exceptions. In general when technology is in need of help from science, it is the generally known, or ambient, body of scientific knowledge which is invoked. (The same also holds true in reverse. When scientists need technological help they do not turn to the very newest developments in technology, but rather, to the generally known and understood body of technological knowledge.) Thus it becomes vital for social science research to continue, for in the absence of continuing scientific progress, the limits of technological understanding and action will remain stationary, or at least, stultified.

A classic example of scientific contributions to the practical world of program planning can be found in the works of Jean Piaget. Piaget considers himself a II genetic epistemologist" (Rosen 1977). The essence of genetic epistemology is the study of the acquisition of knowledge. It is a theoretical pursuit which deals primarily with how people develop the ability to know and to understand. It was not developed for the express purpose of practical application. Piaget's theories, however, have had enormous impact on curriculum development in education, to the extent that one can find mathematics textbooks which are specifically based on theoretical concepts developed by Piaget and his colleagues. (As an example of such a book, see Copeland 1970.) Prior to Piaget's theoretical work, the best efforts of educational technologists resulted in curricula which were not responsive to the actual ability of children to process abstract concepts. It is quite likely that many of those curricula demanded that children produce at a level which was impossible for them. It is not likely that these problems would have been brought to light by people in the business of developing curricula. It took knowledge which came from highly abstract and theoretical sources to do the trick. Although that theory was based on empirical research, the research was intended to further

theory, and not to help with practical problems of education or curriculum development.

Thus the argument in evaluation is not "science or technology," as both are indispensable aspects of efforts to achieve practical solutions and workable innovations. The issue is one of emphasis. If one is engaged in evaluation, the focus of the work is likely to be narrow, well-defined, and intimately tied to the working lives of service delivery personnel, administrators, and policy makers. Given this focus, the most powerful approach for conceptualizing problems and organizing work is the technological model. Technological theories concentrate on factors which make a difference in the real world. Technological research recognizes important differences between the development and field testing of innovations. Technology is specifically aimed at helping decision makers with all the phases of knowledge-theory, experiment, experience, intuition and judgment. Above all, technological action is guided by the same practical constraints which impinge on practical innovation, and success in technology is determined by the extent to which solutions are optimized with those constraints.

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[1] I picked a lot of brains in my efforts to find specific examples of issues which I discuss, and many thanks go to the people who helped with this effort. In alphabetical order, they are: Michael Kean, Hugh Rosen, Myma Shure, Jerry Siegel, Glen Snelbecker, and George Spivack. In addition, I would like to thank Lois-Ellin Datta for her excellent overall critique of this chapter.

[2] Actually, there are five types of critiques. The fifth is an explanation proposed by George A. Miller (I 969), in a discussion about the relevance of psychology. He argues that although psychology is relevant to practical problems, the relevance is not of an "instrumental" nature, i.e., the practical relevance of psychology does not depend on turning scientific concepts directly into technological applications. He claims that although a relatively direct transposition may be done in the physical sciences, it is not likely in the case of psychology. As an alternative, he proposes a type of relevance which manifests itself not through technological products, but through influencing public conceptions of what is humanly possible and desirable.

I am not dealing with this critique in the body of the chapter because my chief interest is not whether psychology (or any social science) has technological relevance, but whether evaluation can be useful if it is built on a technological model. In that sense, Miller's point is not relevant to arguments presented in this chapter. I mention it here for the sake of completeness, and because it might be useful for people who are concerned with other aspects of the relevance issue.

[3] The phrase "the se-arch for truth" is not original. It constitutes the subtitle of a fascinating work by Mario Bunge (1967) on the philosophy of science. The full title is *Scientific Research: 11. The Search* for *Truth.*

[4] This is a very brief and simplistic statement of the issue. All theories have some disconfirming instances, and isolated instances of disconfirmation are by no means

sufficient to destroy a theory. A detailed discussion of the issue can be found in Lakatos and Musgrave 1972. Disconfirmation is much more important in science than it is in technology, but the reader should not think that single instances of disconfirmation play a major role in the development of scientific theory.

[5] Actually, this is a statement of faith in the potential power of social science. It may be that, except for a few exceptions, social science will never be useful in finding solutions to practical social problems. Finch (I 961) points out that until the sixteenth century, science and engineering were very different activities which had very little to do with each other. It was not until then that scientists began to develop theories which were powerful enough to be of use to engineers. What stage are we presently at in terms of the relation between social science and social technology? The answer is not at all clear, and there are no guarantees about the course of future developments.