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Potential Pitfalls of a More Applied Social Psychology: Review and Recommendations

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Interest and involvement in applied social psychology have increased dramatically in recent years. A chorus of social psychologists has applauded and encouraged this trend toward a more applied social psychology, claiming that it will promote a variety of improvements including better theory building, the amelioration of social problems, and increased employment and funding opportunities. Although we agree that this enthusiasm is at least partly justified and that applied research is necessary, we argue that inadequate attention has been paid to the potential pitfalls of applied social psychology. Five potential pitfalls are discussed: reduced construct validity; reduced focus on mediating processes; decreased concern for probing the vast array of theoretically important causes of behavior; increased research faddism; and increased incoherence of the discipline. These five pitfalls are problems in any disciplinary study of social behavior, but we believe they are exacerbated as social psychology becomes more applied. We offer some recommendations to help social psychology avoid these pitfalls. Only by seriously attending to the potential pitfalls of a more applied social psychology can these problems be avoided.

Interest in applied social psychology has recently shown a dramatic increase. Two new series (Bickman, 1980a, 1981a, 1982; Kidd & Saks, 1980a, 1983)

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and other recent edited volumes (Deutsch & Hornstein, 1975; Stephenson & Davis, 1981) are devoted to the subject. Current introductory social psychology textbooks generally include at least one chapter on applied social psychology (e.g., Baron & Byrne, 1981; Harari & Kaplan, 1982; Penrod, 1983), and the field now even has its own textbook (Fisher, 1982b). An historical content analysis of job ads in the APA *Monitor* would certainly show an increasing demand for "applied social psychologists" and for applicants with "applied skills."

The trend toward a more applied social psychology has been met with a virtually unanimous cheer of approval. Observers have claimed that a more applied social psychology will: insure that the questions social psychologists ask are "nontrivial" (Helmreich, 1975; Kidd & Saks, 1980b; Mayo & LaFrance, 1980; Saxe & Fine, 1980); lead to more accurate principles about human behavior, because our observations will be of people in real life settings (Helmreich, 1975; Leventhal, 1980; Mayo & LaFrance, 1980; Saxe & Fine, 1980); provide an opportunity to assess the ecological validity of generalizations derived from laboratory research (Ellsworth, 1977; Leventhal, 1980); reveal gaps in existing theories, and thus guide future research (Fisher, 1980; Mayo & LaFrance, 1980); lead to the amelioration of serious social problems (Bickman, 1980b; Kiesler, 1980; Mayo & LaFrance, 1980; Saxe & Fine, 1980); increase the nonacademic employment opportunities of social psychologists (Edwards, 1975; Kidd & Saks, 1980b; Woods, 1976); and provide research funding for social psychologists with university affiliations (Kiesler, 1977; Leventhal, 1980).

The enthusiasm for a more applied social psychology is no doubt partly justified. Indeed, a commitment to integrating theory, research, and practice was the central focus of Kurt Lewin's original vision of a useful and valid social psychology (see M. A. Lewin, 1977). On the other hand, at least some of the claims that have been made for applied social psychology seem overly optimistic. Social psychologists probably do not yet know enough to contribute greatly to the "amelioration of social problems" (Leventhal, 1980). Advocates of applied social psychology may overestimate the degree to which the behavior of those observed in typical applied research projects is natural and representative. While the advantages of applied social psychology may have been overstated, it is even more problematic that the potential pitfalls of a more applied social psychology have generally been ignored. The purpose of the present paper is to describe five possible pitfalls and to begin discussion of what social psychologists can do to avoid them as our discipline becomes more applied. In the first section of the paper we describe five potential pitfalls of a more applied social psychology. In the final section we discuss possible means of avoiding these pitfalls.

THE POTENTIAL PITFALLS OF A MORE APPLIED SOCIAL PSYCHOLOGY[1]

There are several possible dangers associated with a more applied social psychology. Some of these have already been widely discussed. For instance, it has frequently been noted that applied research often does not allow an investigator the same degree of control as basic research, and thus considerable methodological skill may be required to attain internal validity in applied research (Cook & Campbell, 1979). Others have noted that preparing students for work in applied settings may require changes in traditional graduate training (e.g., Anderson & Lewis, 1978; Bickman, 1980b; Carroll, Werner, & Ashmore, 1982; Deutsch & Hornstein, 1975; Fisher, 1981, 1982a; *Society for the Advancement of Social Psychology Newsletter*, 1980a, 1980b). In this section we do not discuss these two pitfalls, since others have discussed them and relatively straightforward solutions exist. Rather, we discuss several potential pitfalls of a more applied social psychology which, in their extreme, may threaten the very nature and existence of our discipline. From a less extreme view, these pitfalls threaten to make social psychology less coherent, and to reduce our ability to study and understand important social processes.

Our discussion focuses on five potential pitfalls. First, a more applied social psychology may attend less to the construct validity of the variables investigated. Second, less consideration may be given to the processes mediating the relationships between external, easily observable variables. Third, many theoretically important determinants of behavior may be ignored. Fourth, research may become increasingly fadish, addressing whatever politically minded policy makers define as the important social problems of the day, rather than focusing on enduring questions about human concerns. Fifth, becoming more applied may make social psychology more incoherent, conceivably even leading to the end of social psychology as a distinct and formal discipline.

[1]It might seem useful for us to provide a clear and detailed definition of what we mean by "basic" and "applied" social psychology. However, we shall not attempt to do so because (1) to examine critically the various means of differentiating between the two would double the length of this paper; (2) we believe that many of the definitions in use fail to stand up to critical examination; and (3) others (e.g., Bickman, 1981b; Deutsch, 1980; Fisher, 1982a, 1982b; Kidd & Saks, 1980b; Reich, 1981) have recently dealt with definitional issues, and our discussion would therefore be largely redundant. Suffice it to say that we believe our comments apply across different definitions of "basic" and "applied," including those (e.g., Bickman, 1981b) that do not see the two as dichotomous, and that our position particularly applies the more research becomes "policy relevant." Thus we believe that the reader may use his or her own favorite definition, or intuitive sense, of "basic" and "applied."

Although our primary focus is on these five problems as potential pitfalls of more applied research, it should be noted that each of the problems also characterizes more basic social psychology to some degree. That is, we do *not* mean to imply that there is a one-to-one relationship such that the five pitfalls pertain to applied research but not to basic research. Instead, we are arguing that these pitfalls tend to be relatively more problematic in the case of more applied research and that, therefore, becoming more applied will tend to increase the severity of these problems. However, we do not believe that this relationship is an inherent one. Thus the recommendations at the end of this paper represent methods of avoiding the pitfalls of a more applied social psychology. These recommendations suggest that to avoid the pitfalls, we must conduct sophisticated, high quality applied research that maintains strong links to social psychological theory. In a broader sense, these recommendations can also be seen as suggestions for building a more coherent social psychology, incorporating both basic and applied research, from the currently fragmented pieces of the discipline.[2] In other words, the recommendations describe means for using and further developing social psychology's unique perspective in both basic and applied research (Saxe, 1983), rather than running the risk that greater movement toward application will endanger the unique perspective that is social psychology's.

Our discussion of pitfalls should not be taken as an argument against more applied social psychology. We are advocates of applied social psychology and are ourselves committed to the conduct of applied research (cf. Bryant & Wortman, in press; Mark, Bryant, & Lehman, 1983; Mark & Romano, 1982; Mark & Shotland, 1983; Shotland & Mark, in press; Wortman & Bryant, in press). Indeed, we believe that for ethical, pragmatic, and scientific reasons applied research should continue. In other words, our focus on the pitfalls of a more applied social psychology is not meant to discourage applied research, or to suggest that social psychology should focus exclusively on basic research; rather, we wish to point out that there are potential disadvantages associated with social psychology becomong more applied, that these disadvantages are potentially serious, and that as a field we can and must take steps to avoid these pitfalls. Only by considered actions can the potential of a more applied social psychology be avoided, and the promise of a more applied social psychology attained. It is in this spirit that we now discuss five potential pitfalls of a more applied social psychology.

Reduced Construct Validity

The notion of construct validity became popular in the 1950s, due to several important discussions of psychological testing (Campbell & Fiske, 1959;

[2]This view of the recommendations was emphasized by an anonymous reviewer, whom we thank for his or her comments.

Cronbach & Meehl, 1955). In Cook and Campbell's (1976, 1979) expansion of Campbell and Stanley's (1966) typology of validity for cause-probing research, construct validity refers to the validity with which generalizations about more abstract, higher order constructs can be based on concrete research operations. Construct validity can refer either to the presumed cause or to the presumed effect in an experiment. As an example, to ask about familiar modeling experiments (e.g., Bandura, 1965), "Does the concrete research operation of a child striking a Bobo doll represent 'aggression?' " is to question the construct validity of the effect.

There is a risk that in moving from basic to applied research, social psychology will give less emphasis to construct validity. In theory-testing research, knowing that X causes Y is of little value unless we know, *in theory-relevant terms,* what X and Y are. For example, in a study of communicator credibility, knowing that the manipulation caused differences in attitude change means little unless we can be sure that the manipulation involved communicator credibility and not communicator attractiveness or evaluation apprehension. Further, for basic research, *both* construct validity of the cause and construct validity of the effect are of fundamental importance (although their relative importance may differ somewhat from study to study).

Applied research, in contrast, generally places less emphasis on construct validity. Cook and Campbell (1979) have noted this, especially in the case of construct validity of the cause. As they claim, much applied research "is concerned with whether a particular problem has been alleviated by a treatment—recidivism in criminal justice settings, achievement in education, or productivity in industry" (p. 83). While the dependent variable of interest may be measured with care, little attention is given to "determining the causally efficacious components of a complex treatment package, for the major issue is whether the treatment as implemented caused the desired change" (p. 83).

Consider as an example, "Sesame Street," the educational television program for children. "Sesame Street" was first evaluated by Ball and Bogatz (1970; Bogatz & Ball, 1971), who conducted a randomized experiment in which they encouraged a group of children to watch the then new TV program regularly. They concluded that the experimental group performed better on tests of letter and number recognition and on tests of simple cognitive processes than did the unencouraged control group. Cook et al. (1975) conducted a secondary analysis of the Sesame Sreet experiment, and one of their conclusions is that the superior posttest performance of the treatment group was due in part to "encouragement to view," and not solely to "Sesame Street."

Although Cook et al. (1975) improved the construct validity of the cause by probing these two distinct components, these components still cannot be validly labeled in theory-relevant terms. We can ask how the complex package of activities called "encouragement to view" should be labeled—As increasing the value of watching? As reinforcement of actively viewing the show? As providing tasks which complement the show's content? Or as something else? We could likewise ask how the complex package of entertainment and education called "Sesame Street" should be labeled in theory-relevant terms. A similar problem arises in much applied research because in such research the critical question about the construct validity of the cause is generally whether the research operation is a good representation of the policy-relevant treatment that can be transferred to other settings. In contrast, of course, the analogous question in basic research concerns how well the operations fit the specific constructs defined by theory.

Cook and Campbell (1979) and Bickman (1981b) suggest that in applied research the construct validity of the effect will generally be much greater than that of the cause. This appears to be the case in the Sesame Street example. However, we think that in much applied research this ordering does not hold. In many applied studies, the dependent variable of interest *is* clearly specified, but its relationship to higher-order constructs remains quite unclear. For instance, one might evaluate a program designed to reduce the number of students who drop out of college (Mark & Romano, 1982). The dependent variable, retention in college, is clearly specified. However, one may ask whether improved retention rates represent an increased desire to learn, altered perceptions of the economic advantages of college training, reduced scope of alternative actions, improved social life in college, some combination of these, or something else. Similarly, in other applied research that takes as its dependent variable the extant indicators of a social problem, effects may have practical but not theoretical meaning.

We do not mean to suggest that applied research always has less construct validity than basic research, or that applied researchers are less concerned with construct validity than basic researchers. The "Bobo Doll" example illustrates that basic research can have questionable construct validity, and the "Sesame Street" example demonstrates the concern for construct validity that applied researchers often have. Our argument is simply that construct validity will *typically* be poorer in applied social psychological research than it will be in basic social psychological research—for at least four reasons. First, basic research is designed with the goal of "clean" operationalizations; in applied research this is typically a less important goal. For example, whereas basic researchers attempt to disentangle causes of behavior, in applied work, program developers often attempt to combine or "confound" numerous causes of behavior in order to create a powerful program (see Sechrest et al., 1979). Second, relative to applied researchers, basic researchers typically find it easier to provide evidence of convergent and discriminant validity (though within a single study they often do not do so beyond a simple "manipulation check"). Furthermore, basic researchers typically have a higher payoff for providing such evidence than do applied researchers. Third, applied research may be more faddish than basic research (for reasons discussed below), which inhibits the cumulative development of construct validity across studies. Fourth, the constructs on which applied researchers focus (e.g., recidivism, retention in college) will often be low-level, i.e., they are specified by practical interests and cannot reasonably be interpreted in theory-relevant terms. Thus, in some cases applied research may achieve construct validity in a technical sense, but the construct examined will be of little direct relevance to social psychology.

Because we are arguing that a more applied social psychology runs the risk of reduced construct validity, it is worth noting that others have reached quite different conclusions. In particular, Saxe and Fine (1980) have argued that applied research will "often *enhance* the construct validity of theoretical formulations" (p. 76, emphasis added). They argue that this will occur because "construct validity, for problem-based research, is easily achieved because the research . . . will be designed in such a way that it is guided by theory rather than politics or whims; the independent variables will accurately reflect the constructs" (p. 76). We agree with Saxe and Fine that applied research generally avoids triviality more than basic research, but we do not see this as a guarantee of construct validity. We believe that Saxe and Fine may have underestimated the influence of "politics and whims," especially in major social programs or pilot programs, and in applied research funded by external agencies. They are also likely to have underestimated the extent to which most social interventions involve complex packages of many variables, rather than refined representations of a single construct.

We do not mean to be totally pessimistic about construct validity in applied research. To the contrary, in a later section of the paper we present recommendations designed in part to enhance construct validity in applied social psychology. However, we do contend that there are a number of factors that make construct validity generally less important and less well-established in applied research than in basic research—unless steps are taken to avoid this pitfall. And it is important to avoid this potential pitfall, because a focus on construct validity—particularly in terms of social psychological constructs—is a major contribution social psychologists can bring to the study of social problems (Saxe, 1983).

Reduced Focus on Mediating Processes

Another potential drawback of a more applied social psychology is that it may tend to focus less on the mediating processes underlying external, easily observed relationships. This problem is closely related to that of poor construct validity. In theory development, it is important not only to know how to label X and Y, but also to know the path X travels in causing Y. Because of the increased complexity involved in studying causal chains and mechanisms, however, knowledge of process is woefully lacking in contemporary social psychology. Unfortunately, becoming more applied may further hinder the study of process by making it both less relevant and more difficult. As an example of the issue of mediating processes, consider the relationship between superordinate goals and cooperation. When the basic researcher finds that superordinate goals lead to increased cooperation, an important question typically remains to be answered, namely "How does this effect operate?" In contrast, when an applied researcher charged with increasing cooperation finds that this end has been accomplished by introducing superordinate goals, the research problem is solved and the question of process remains unanswered.

Applied social psychology places prime importance on solving pressing social problems (Fisher, 1982b), usually at the expense of understanding the causal dynamics underlying the solution. This emphasis on problem-solving naturally involves a focus primarily on outcome rather than process variables. Basic research, in contrast, is aimed at the improvement of understanding, typically by testing deductions derived from theory or from the observation of social processes. This pursuit often entails a narrower concentration on critical variables that mediate or moderate observed causal relationships.

Even in cases where the applied researcher may wish to focus on the process underlying an observed effect, the nature of many applied settings makes it difficult or extremely costly to collect extensive measures of process variables (Cook & Campbell, 1979). Conducting surveys, collecting observations, or administering questionnaires over many sites may be expensive; subjects may not be available on demand; the mediating process may occur when subjects are not "on site" (e.g., they may occur when the client interacts with his or her family); a host organization's other interests may conflict with extensive process measurement; and so on. For these and other practical reasons, little attention has been given to mediating processes in the Sesame Street example (Ball & Bogatz, 1970; Bogatz & Ball, 1971; Cook et al., 1975), in most research on desegration (Crain & Carsrud, in press), and in a variety of applied research areas (Chen & Rossi, 1983; Shotland & Mark, in press).

In short, the complexity of many applied research settings may hinder applied researchers' attempts to study the mediating processes underlying observed effects, and the goals of applied research may make it less likely that questions of process are addressed. We do not contend that there is a perfect relationship between type of research and focus on mediating process, such that all basic research has such a focus and no applied research does—only that there tends to be less focus on mediating processes in more applied research. Nor do we contend that this reduced focus on mediating processes is an inherent part of all applied research—indeed, later in the paper we make recommendations to increase the focus on mediation in applied social psychology. However, we fear that unless certain steps are followed, a movement toward more applied research will mean a decreased focus on mediating processes.

Decreased Concern for Alternative, Theoretically Important Causes of Behavior

Another potential danger of a more applied social psychology is that in applied research one may fail to investigate *alternative* causes of a behavior of interest. Those causal variables which are most likely to be ignored are ones that: affect behavior in interaction with other variables, not in the form of main effects; may be theoretically important, but are presumed to be relatively small in effect size; or are difficult or expensive to manipulate (Bickman, 1981b).

There are several reasons why alternative causes are not probed in many applied research programs. To some extent these arise because in applied research, the concern is typically for defining a solution to a problem. In many cases, this problem solving focus means developing or testing a "treatment" or "program" intended to alleviate or "solve" the problem. Inherent in this process are factors which reduce the applied researcher's attention to certain types of causes of behavior.

One class of causal variable that may be underrepresented in applied research consists of variables which influence behavior only through their interaction with other variables, particularly if the interaction involves an individual difference variable. It is generally impractical (and in some cases it may be illegal) to provide one sort of program for males and another for females, or one for internals and another for externals, for example. In addition, the relatively lower control in some applied research settings may make the investigation of interactions more difficult than it is in typical basic research.

Another reason why theoretically important causes of behavior might not be examined in applied research is that some theoretically interesting causes of behavior do not translate into practical or potentially potent treatments. Applied researchers would not, for instance, examine the relative effectiveness of live vs. televised models if it were clear that live models represented an impractical and overly expensive treatment strategy. (See Mark & Shotland, 1983, for additional examples in the context of charitable solicitations where, for instance, strategies which require multiple visits to households are generally impractical.) The sociologists Scott and Shore (1979) make a similar point, noting that by selecting independent variables that "are apt to be malleable in an applied context [i.e., are potential treatments], . . . [one has] greatly narrowed the range of primary variables for study" (pp. 230–231).

There is another, related reason why applied researchers may not undertake intensive study of alternative causes of behavior: When alternative treatment strategies exist, the better strategy is generally defined as that which provides the most cost-effective reduction of the problem. When two strategies are compared and one is found to be more cost-effective, the other is likely to be ignored. Any subsequent research will probably focus on improving or assessing the generality of the more cost-effective strategy. Thus, the relationship of the costlier treatment to the dependent variable typically remains unclarified, as, typically, do ways of making the costlier strategy more cost-effective. In this regard, Cronbach (1982) has noted that for policy research "the question is not 'Is there a difference?' but 'Which of the *costlier* features of the *original* T[treatment] can be stripped off without reducing benefits too much?' " (pp. 233–234, emphasis added).

A final possible reason why alternative causes are not adequately probed in applied research arises from the way policy makers and research funders typically define success for applied research programs. Satisficing, rather than optimizing, is typically the criterion for success in "solving" social problems. In other words, once a treatment strategy is found which reduces the problem *enough,* the problem is deemed "solved" and research funds will largely be shifted toward the solution of other problems. Imagine, for example, a research program aimed at reducing aggressive episodes in youth institutions. The research would be deemed successful when a program is found that reduces the number of fights to an acceptable level. If research were to continue, it would probably focus on improving the program or on assessing its generality, not on examining alternative causes of aggression, because the satisficing criterion has been met.

In short, in applied research, theoretically important causes of behavior might not be studied unless (a) they are seen as translatable into practical, relatively cost-effective, generally applicable treatments and (b) a satisfactory solution to the social problem of concern has not yet been found.

Contrast this with the strategy basic researchers must follow to develop comprehensive theory. Building comprehensive theory demands that we probe alternative sources of variation, even if they account for relatively small amounts of variance and apply only in specific circumstances. For instance, learning that persuasive communication delivered by attractive, credible, liked communicators leads to attitude change does not end the basic investigator's search for other valid sources of attitude change. While the applied researcher who has developed an effective solution may well concentrate only on improving its strength (Quay, 1979), the basic researcher typically continues searching for empirical evidence of other types of causal variables based on observation and on hypotheses derived from existing theory. For the basic researcher, such considerations as cost-effectiveness and whether a variable can be translated into a treatment program are irrelevant when selecting variables for study. Unless changes are made, a more applied social psychology will be more likely to ignore alternative, theoretically important causes of behavior.

Increased Research Faddism

As with any scientific discipline, social psychology operates within, and is affected by, a larger culture. It can be argued that research activities are determined in part by concerns about (and in) society, as illustrated by the concern for conformity research in the 1950s, and an emphasis on helping research in the 1970s. Whatever the reasons for the changes in focus, one might argue that progress in social psychology has been hindered by faddism – that is, by a tendency to investigate currently "hot" topics, rather than to maintain a focus on more enduring questions (Ring, 1967; Smith, 1972).[3] In any case, a more applied social psychology may increase faddism and reduce attention to classical social psychological issues. One advantage frequently cited for applied social psychology is that external parties such as federal agencies often define important social problems and provide the financial support necessary for research on these topics. This arrangement, of course, means that programmatic research on enduring social psychological questions will occur only if the funding agency so desires. As Chen and Rossi put it, "In basic research, outcome variables express the disciplinary interests of the researcher; in applied social research, outcome variables are those of interest to policymakers or other sponsors of applied work" (1983, p. 288).

To assume that the research agendas of our funders will approximate a program of basic research may be naive. This seems especially true when we consider that the allocation of research funds is a political process and is subject to the changing winds of political fortune and the changing whims of political actors. Many participants in policy-relevant research (e.g., Bevan, 1982; Tapp, 1981; Walgren, 1982; Weiss, 1973, 1977) have noted how political considerations affect the funding of applied research topics (though the same point can, of course, be made about funding for basic research). For example, the funding of criminal justice research over the past 15 years has clearly been affected by political considerations (e.g., Galliher, 1979). In-

[3] One could also argue that some of the apparent faddism in basic social psychology is not faddism in the focus of study, but in terminology or prevailing theory. For example, a case could be made that the shift from attitudes to attribution theory to social cognition does not reflect faddism, but reflects a cumulative change in the way of viewing a similar, enduring set of basic issues.

deed, we can view the Reagan administration's effect on social science research funding (Rossi, 1983) as a dramatic example of the instability that results from overreliance on the political process in defining our research agenda.

Another indicator of the greater faddism in applied research is the differential commitment to research areas in basic and applied social psychology. While they may foray into new research grounds occasionally, basic researchers typically conduct research in the same general area(s) (e.g., helping, attitude change) for many years. In contrast, applied researchers seem much more likely to move from one research area to another, largely as a function of the availability of funding. It is not uncommon to find applied researchers who have conducted research in such diverse areas as primary education, criminal justice, and health. Bickman (1981b) has contrasted his own basic and applied research activities, and notes that while his basic research on bystanders has continued over many years, in the same period his applied research has involved a large number of unrelated problem areas.

Increased Incoherence of the Discipline

One might criticize contemporary social psychology as lacking the coherence that best marks a scientific discipline (Ring, 1967; Smith, 1972). Nevertheless, social psychology can be seen as focusing primarily on the implications of what F. H. Allport has referred to as "master problems" (Brooks & Johnson, 1978; Gorman, 1981), or on issues of "human concern" (Brickman, 1980). In terms of general domains of study, the field focuses largely on (a) how individuals perceive the social world and how these perceptions affect behavior (whether the study is framed in terms of attitudes, attribution or social cognition); (b) social influence processes (e.g., attitude change, persuasion, conformity, obedience), with finer distinctions possible in terms of social psychological processes and theoretical models; and (c) group dynamics, which overlaps in part with the previous areas, and includes such topics as leadership and communication networks.

If there is nevertheless reason to believe that social psychology lacks coherence, there is even greater reason to fear that a more applied social psychology will become even less coherent (Fisher, 1982b). Because being a truly applied science means being committed to solving practical problems, truly applied social psychologists are concerned about studying basic social psychological processes only insofar as these aid in problem solving. While a Lewinian perspective suggests that theory development is important in solving problems, many applied research problems will not lend themselves to theory development. In any case, developing a coherent social psychological theory becomes secondary to problem solving as social psychology becomes more applied (Bickman, 1981b).

In addition to this decreased commitment to theory development, a trend toward a more applied social psychology may lead to a less coherent discipline due to researchers' immersion in numerous diverse social problems, each with its own history, terminology, and connection to other disciplines (Deutsch, 1975; Leventhal, 1980). For instance, the social psychologist who becomes involved in desegration research will: need to become familiar with the legal background of desegregation; be concerned with how "desegregation" is defined; examine the demographic distribution of different ethnic or racial groups within a community; study the political and public support for desegregation; and so on (Crain & Carsrud, in press; Pettigrew, in press; Stephan, 1978). In contrast, a social psychologist whose applied work focuses on energy conservation will have to become familiar with patterns of energy consumption, the economics of energy distribution, and the politics of regulation (Stobaugh & Yergin, 1979; Yates & Aronson, 1983).

We should note that we are not criticizing the interdisciplinary approach which is required to address many applied problems effectively. Indeed, one benefit of applied research is that interdisciplinary contact often occurs, and another potential benefit is that social psychologists may gain an understanding of the limits of their field. What we are citing as a pitfall is the possibility that applied researchers may lose their identification as social psychologists, and more seriously, might forget the unique perspective that is social psychology's.

We could examine additional applied areas to show how this might occur, or expand the examples of applied social psychologists working in desegration and in energy conservation to examine in greater detail how different applied settings lead to variations in the nature and level of questions addressed, the literature (and discipline of the literature) studied, and the terminology used. Instead, we will simply restate our argument: Without concerted efforts to the contrary, a more applied social psychology will lead to a less coherent discipline, in part because different applied problems lead to different interests, publication outlets, journals read, conferences attended, and terminology. While specialization occurs in basic as well as in applied research, it seems clear that the degree of diffusion, and the likelihood of losing a commitment to the general discipline of social psychology are much greater in applied than in basic research. Indeed, there may be some validity to the fear, expressed by some psychologists, that their colleagues and students who conduct applied research may "go native," and forego social psychology (Bickman, 1981b; Carroll et al., 1982).

PUTTING THE PITFALLS IN CONTEXT

We have identified five potential pitfalls that applied social psychology should attempt to avoid. We have argued that a more applied social psychology may: (1) ignore construct validity; (2) ignore mediating processes; (3) ignore theory-relevant causes of behavior; (4) become more faddish; and (5) lose a sense of coherence as a discipline. We have argued that these five pitfalls increasingly threaten social psychology as the field becomes more applied. That is, social psychology will increasingly suffer from these pitfalls if the field moves toward more applied work, unless steps are taken to avoid them. The pitfalls are of interest not only because they represent dangers that applied researchers must seek to avoid, but also because they represent stumbling blocks that make it less likely that we will enjoy the advantages which have been claimed for applied social psychology. For example, the pitfalls of reduced construct validity and reduced focus on mediating processes are serious problems in their own right, but they also make it unlikely that applied social psychology will meet such promises as guiding theory development and increasing the accuracy of our generalizations about human behavior (Mayo & LaFrance, 1980). In fact, unless the problem of reduced construct validity is adequately addressed, increased applied research could mislead theory development and decrease the accuracy of our generalizations. In a similar fashion, the other pitfalls, if ignored, make it unlikely that applied social psychology will attain its potential benefits for the field.

However, it is not the case that by becoming more applied social psychology will inevitably suffer from the pitfalls we have identified. Indeed, in the next section we offer recommendations designed to reduce the likelihood that a more applied social psychology will suffer from these five pitfalls. We believe social psychology should become more applied. In an age of increasing accountability and increasing resource limits, there are both pragmatic and ethical reasons for becoming more applied (Fishman & Neigher, 1982). Further, *if* the pitfalls we have identified can be avoided, becoming more applied can provide important scientific contributions (e.g., extending external validity, probing the boundary conditions of relationships, obtaining additional operationalizations of constructs, hypothesis generation), and the combination of more basic and more applied research can lead to the advantages of which so many have written. Thus, we would not advocate reducing social psychology's applied activities, though avoiding applied research would be one rash means of lessening the threat of the pitfalls discussed in this paper. Rather, we believe applied efforts should be encouraged, but that the enthusiasm for a more applied social psychology should not blind us to the potential problems that may result from a trend toward a more applied social psychology. Only by carefully considering the possible pitfalls can we avoid them. We hope this paper will stimulate further discussion about the pitfalls of becoming more applied. If we act to avoid the pitfalls, then a more applied social psychology may be better able to fulfill its promise.

RECOMMENDATIONS FOR A MORE APPLIED SOCIAL PSYCHOLOGY

In this section, we make five recommendations. We believe that by following these recommendations social psychology will be able to become more applied and yet avoid the pitfalls identified in the previous section.

Recommendation #1. We should not focus on problem solving to the exclusion of truly *scientific* problem solving. It is appropriate that problem solving is "the name of the game" for the policy maker. It is very problematic, however, when the social scientist adopts the policy maker's perspective. It has often been said that the three goals of science are prediction, understanding, and control. The policy maker's perspective lends itself to omitting understanding from this trinity, something a scientific discipline cannot allow. Thus, we should not be satisfied with studying the effects of some multifaceted heterogeneous treatment on some complex outcome of questionable construct validity. Rather, we must decompose the treatment and study its various components (Saxe & Fine, 1980), and study the processes by which various components operate (cf. Scriven's 1976 "modus operandi" method).

In addition to its obvious benefits for understanding, such an approach can be justified practically in that it may lead to more efficient, stronger treatments. Although applied researchers have developed heuristic models for *defining* social problems (e.g., Ovcharchyn-Devitt et al., 1981), similar conceptual frameworks are still needed for delineating effective approaches to problem *solving.* We need to develop a rationale for decomposing treatments in a convincing model of applied research, to develop a set of compelling examples of how this approach has led to better solutions of specific social problems, and thus be able to convince funders of the importance and fundworthiness of *scientific* problem solving, i.e., problem solving which focuses on constructs and processes as well as on outcomes.

Recommendation #2. We must be careful not to let others define our research problems and specify what constitutes "success" in our applied work. The field of evaluation research has produced results of practical utility, but these results cannot be assembled into a coherent view of human behavior. Evaluation research is not a discipline which creates a coherent literature aimed at a better understanding of human behavior, largely because program evaluators generally rely on funders to specifiy their research agendas. Because social psychology seeks to be a discipline capable of providing an improved understanding of human behavior, we should attempt to avoid having our research agenda controlled by funding agencies in this way.

To do so, applied social psychology needs to cultivate varied, heterogeneous sources of funding, including governmental agencies charged with problem solving, private foundations, and business organizations where appropriate. We also need to lobby traditional funders of basic research (e.g., NSF, NIMH) and our academic institutions, to convince them of the merit of funding more applied (and often more costly) research. Developing a heterogeneous, diverse source of research support may be necessary if a more applied social psychology is to avoid serious pitfalls. It may also be necessary for applied researchers to learn how to conduct applied research in field settings on shoestring budgets. Finally, it may be useful to encourage social psychologists to become more involved in the administration of funding agencies.

Recommendation #3. We must continue to develop our methodological expertise and to apply it successfully. Further, the methodological training of applied social psychologists should include a focus on process analyses and on methods of enhancing construct validity. Previous commentators on applied training needs have not adequately noted this focus. Instead, they have generally focused on two goals for methodological training: (1) obtaining skills needed to make reasonable causal inferences in more complex field settings and with data from other than randomized experiments; and (2) gaining the ability to perform various tasks other than cause-probing research, e.g., descriptive research with surveys. While these two training goals are essential, so too is the need for the technical skills that will allow applied social psychologists to enhance construct validity and conduct process analyses in applied research.

To achieve these ends will require a broad variety of metholological skills. Causal modeling techniques may be necessary for rigorous process studies (Judd & Kenny, 1981), and for furthering the convergent and discriminant validity of treatment and outcome constructs (cf. Sherif, 1977). Qualitative research methods may prove invaluable in the study of process (Cook & Reichardt, 1979). The skillful use of unobtrusive measures (Webb, Campbell, Schwartz, & Sechrest, 1966) may strengthen both construct validity and the study of process. We must also learn the skills necessary for studying the components of treatments (Quay, 1979; Scriven, 1976), and for conducting "side studies" (Riecken & Boruch, 1974; Saxe & Fine, 1980), i.e., smaller studies conducted in conjunction with the larger applied research study, and which may focus on process or construct validity.

In addition to technical skills, substantial management and accounting skills will often be critical for the applied social psychologist who wishes to conduct process studies and enhance construct validity. As a result, and because these skills will generally prove useful, the training of applied social psychologists should not ignore management and accounting techniques.

Recommendation #4. We must infuse the field of applied social psychology with theory—and good theory at that. Stressing multiple methods and interdisciplinary approaches alone is not enough; we must also emphasize the creation of empirical hypotheses in applied research (McGuire, 1969). Specifically, we must rely on theory to indicate potential courses of action, rather than using a simple "trial and error" approach to problem solving (Gergen, 1978; Helmreich, 1975; Lewin, 1951).

Numerous recommendations have been made in the past for how to improve the ability of social psychological theories to promote effective problem solving. Observers have proposed that applied social psychologists: (a) use the work of related fields, such as psychological ecology and structural anthropology, to generate theories specifying the psychological characteristics of social settings and the social consequences of different personalities (Deutsch & Hornstein, 1975); (b) develop theories linking the interaction between the person and the environment (Fisher, 1980; Lewin, 1951); (c) cultivate "contextual theories" focusing on the structural and dynamic features of social situations (Stokols, 1980); and (d) shift from theories about individuals to theories of social systems (Smith, 1973; Tajfel, 1979). While such strategies sound good in the abstract, few concrete attempts have been made to follow through on these ideas. Ironically, hard evidence documenting the utility of such approaches may be necessary before applied social psychologists are willing to commit themselves to these forms of theory development. Clearly, as the potential payoffs are large, we need to invest more effort in this direction. By improving our theories' ability to solve practical problems, applied social psychologists may not only build more valid theories, but ultimately develop more effective solutions to these problems (McGuire, 1969).

One step in this direction is to encourage the involvement of social psychologists in policy and program development. Another is to develop better techniques for teaching students to use theory in applied settings. In addition, enhanced methodological expertise will increase the ability of social psychologists to conduct theory relevant research in applied settings. Further, revised editorial policies in journals might encourage the dissemination and discussion of theoretical developments arising from applied research.

We should point out that the use of theory in applied research not only enhances theory and problem solution, but can also lead to technically better research. Chen and Rossi (1983) have recently made a similar point in arguing for a "theory-driven approach" to evaluation research. Chen and Rossi's desire to bring social science theory into program evaluations is not based on a concern for the well being of any particular social science discipline, or for the development of better theory. Rather, Chen and Rossi (1983) focus on the value that theory has for designing better evaluations. They point out, for example, that power can be increased in a criminal justice program evaluation by using existing theory as a guide for estimating recidivism rates among different subgroups.

Recommendation #5. Our institutional structures should be modified to become more attractive to applied social psychology and to facilitate the advantageous blend of basic and applied research. Several institutions could be modified. The editorial policies of our basic professional journals (e.g., *JPSP*) could be modified to provide greater recognition for applied work, and for attempts to integrate basic and applied research. Perhaps we should embrace Fishman and Neigher's (1982) recommendation "that a certain proportion of manuscripts accepted for publication [in APA journals] contain explicit consideration, both in their introductory and discussion sections, of the experiment's pragmatic importance and potential applications" (p. 543).

Academic institutions may have to accept a lower rate of publication among applied social psychologists, because carefully studying process and outcome in applied settings may require greater time and effort than traditional basic research (Carroll et al., 1982). Social psychologists can contribute to this change, for instance, by taking into account the difficulty of conducting good applied research when they participate in institutional and external evaluations for tenure and promotion decisions.

In addition, changes may be required in graduate training. As we have noted, graduate education in applied social psychology should teach students multidisciplinary research methods and "state-of-the-art" statistical techniques to improve their methodological expertise. It should teach students how to study causal processes underlying solutions to social problems, develop diverse nontraditional sources of research support, conduct cost-efficient research, build more useful, socially relevant theories, and use these theories to derive potential solutions to social problems (Deutsch, 1975). Indeed, many of these goals have already been incorporated in applied social psychology training programs such as those at Loyola University of Chicago (Carroll et al., 1982; Posavac, 1979), The University of Saskatchewan (Fisher, 1981), The State University of New York at Buffalo (Bunker, 1979), and The University of Utah (Carroll et al., 1983). By formalizing our recommendations in terms of objectives for graduate training in general, and by continuing to improve graduate training, we should be better able to avoid the pitfalls of an applied social psychology.

CONCLUDING COMMENTS

We have identified five pitfalls of a more applied social psychology: reduced construct validity; reduced focus on mediating processes; decreased concern for probing the vast array of theoretically important causes of behavior; increased research faddism; and increased incoherence of the discipline. Of course, all five of these pitfalls already characterize contemporary social psychology in varying degrees. In part, this is because of the existing level of applied research in the discipline. However, it is partly because the pitfalls also apply to basic research (though, we would argue, to a lesser degree than to applied research). Thus, the pitfalls we have described and our recommendations for avoiding them are relevant even to basic researchers, and even if social psychology does not become more applied. Indeed, even if we are incorrect in our basic assumption—that the five pitfalls characterize applied research more so than basic research—these pitfalls represent important issues of concern to all social psychologists, and our recommendations are important tasks in the future development of a social psychology that successfully integrates both basic and applied orientations.

The recommendations we have made are similar to those made by others who have commented on the trend toward a more applied social psychology. For instance, Fisher (1980) has proposed seven "touchstones" of applied social psychology, which overlap in large part with our own recommendations. However, our focus is different from that of Fisher and other previous commentators. These commentators have presented suggestions in an attempt to make applied social psychology *maximally beneficial.* In contrast, our position is that without corrective steps, it will be *harmful* if social psychology becomes more applied, in that the pitfalls we have identified will increasingly plague our field.

Almost certainly, a more applied social psychology has other pitfalls beyond the five we have discussed. Similarly, there are useful recommendations for avoiding the pitfalls beyond the recommendations presented in this paper. We hope that this paper helps stimulate discussion about the pitfalls of a more applied social psychology, as well as action to avoid the pitfalls. The promise of applied social psychology is too great for us to ignore its pitfalls.

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