Comments on the Current Status of Social Psychology

Morton Deutsch
Teachers College, Columbia University

In writing this informal paper, I find myself torn between taking a positive attitude toward the richness, diversity, and ingenuity of social psychology and a negative attitude toward its conceptual confusion, faddishness, and fragmentation. This ambivalence is common to many social psychologists and leads many of us, in our manic moods, to think "the whole world is social psychology" and, in our depressed moods, to view our field as an insubstantial, intellectual game not appropriate to serious scholars. As I start writing, I am neither in a depressed nor manic phase.

Instead, I feel a sense of frustration. The task set for this paper is precisely the task that needs to be done for social psychology: namely, to develop an integrative, over-view of its current status and prospects. Such an overview is necessary to overcome the fragmentation and disorder of the field. To do an insightful overview would require an extended, intellectually-demanding effort which is clearly beyond the very limited scope of this paper. To anticipate one of my conclusions and recommendations: not enough effort is devoted to such endeavors. The research funding policies of the federal agencies and private foundations encourage a proliferation of empirical research studies and discourage integrative, theoretical writing; these policies should be revised.

With full recognition that what follows is not a coherent overview of social psychology, let me turn to some of the specific questions that were posed in this paper.
1. Trends

Relevance. Perhaps the most notable trend in social psychology is its renewed search for relevance. I stress "renewed" because the father of modern experimental social psychology, Kurt Lewin, was deeply concerned with "relevance." Such terms as "level of aspiration," "authoritarian and democratic leadership," "group dynamics," and "action research" - which are associated with Lewin - reflect this interest. This early concern with the relevance of social psychology had an enormous impact. The conceptual apparatuses of several other disciplines - notably political science, sociology, and administrative science - were strongly influenced. In addition, it spawned a variety of consulting and training activities in various kinds of organizations and educational settings. These have included "sensitivity training," "leadership training," and "organizational development."

Although these consulting and training activities were natural (if not logical) developments from the theoretical and experimental work of Lewin and his students, a growing schism began to separate the "practitioners" of social psychology and those who were more intimately involved in research and theory-building. The academicians felt that the practitioners were too undisciplined and evangelical and that contact with them would damage the experimentalist's shaky claim to be involved in a scientific undertaking. For this, and also other reasons, the experimental social psychologist turned away from "applications" and a concern for immediate social relevance and became more deeply immersed in the laboratory. This is not to say that the laboratory experiments were insensitive to social issues: the topics of laboratory studies - group productivity, group cohesion, trust and
suspicion, social pressures and conformity, attitude change, the resolution of conflict, etc. - clearly reflect many pervasive social concerns. However, many experimentalists were not concerned about making their work appear "relevant;" in fact, some even looked down upon theorists and experimentalists who were explicitly concerned about the social relevance of their work.

Some experimentalists - by no means, a majority - became so immersed in the laboratory that they started doing experiments which were generated by artifacts of the laboratory: experiments which had no methodological, social, nor theoretical significance but which essentially spoke only to the inner circle of those concerned with minor talmudic disputes about a particular experimental manipulation.

Partly in reaction to a growing distaste for the endless experiments which were nothing but minor, insignificant variations on well-established themes and also partly under the pressures for relevance from the "student revolution" and the funding agencies, there has been a renewed concern with it among academic social psychologists. This concern has expressed itself in many forms. One form is to take social problems into the laboratory and attempt to "simulate" them. The studies on pro-social behavior (by Berkowitz, Darley and Latane, Hornstein, etc.), on crowding (Freedman), on the effects of television (Bandura, Berkowitz, etc.), on littering (Krauss and Freedman), on prisons (Zimbardo), on the effects of urban stress (Glass and Singer, Katz and Milgram, Irle), on obesity (Schachter), and on smoking (Janis and Schachter) are in this tradition. Another variation of this basic type is to take the "laboratory" into the field by creating experiments in which the subjects are unwitting participants - e.g., the
"dropped-wallet" technique of Hornstein to study experimentally the conditions which lead people to return them or the "norm-violation" techniques of Milgram and of Abelson. In such studies, the subjects do not know they are participating in an experiment and some of the issues raised by Rosenthal and Orne about "experimenter effects" are circumvented even as new ethical and methodological ones arise.

Still another form taken by socially concerned research has been that of studying on-going social institutions. Sometimes field experiments are employed, as Lieberman and Miles have done in their studies of encounter groups, as Likert and his colleagues have done in organizations, and as Schmuck has done in classrooms. A mixture of methods - experiments, observation, questionnaires, etc. - are being employed in studies of marital conflict (Deutsch), in research on the effects of different architectural designs in student dormitories (Valins), in studies of the functioning of school systems. In addition, there has been a growing methodological interest in how to study social change so as to draw reliable conclusions about its effects. The work of Campbell and his students on the methodological issues involved in turning "reforms" into "experiments" reflect this orientation.

The renewed search for relevance has, of course, benefitted from the research and theoretical paradigms that were being developed in the experimental laboratory. Research is, in many ways, more sophisticated on applied problems than it used to be. As a consequence, the researcher no longer risks a loss of scientific status by working on such problems. Nevertheless, there are many issues that must be faced with more insight than is now apparent if the knowledge being generated by social psychology is to be socially used as well as socially useful. It is evident that there is much
useful knowledge in social psychology and the other social sciences which
is not being used. An obvious implication of the foregoing is that social psy-
chology should begin to take as an important topic for research and theorizing
a topic which does not appear as such in any of its textbooks - namely, the
conditions which facilitate and hamper the use of social psychological
knowledge. Although the topic has been much neglected, a growing interest
in it is reflected in C. R. U. S. K. at Michigan and in a forthcoming
Conference on the Applications of Social Psychology being sponsored by the
Committee on Transnational Social Psychology of the Social Science Research
Council.

A further issue of importance in any discussion of a relevant social
psychology is: what is relevant? Elsewhere, in my paper "Socially Relevant
Science," I have discussed this question. Here I wish to restate Lewin's
favorite slogan: "There is nothing so relevant as a good theory." And I
add, there is nothing so irrelevant as a simulation unless one knows the
key variables in the phenomena one wishes to study. Too many social
psychologists are responding to the pressures for relevance by settling for
the pseudorelevance of surface similarities. History and everyday
experience alike testify that the appearance of relevance can be grossly
misleading. It takes hard and demanding intellectual work to get beyond
a surface understanding of the social problems confronting us.

Competence. A second trend that is characteristic of social psychological
research is an increased level of technical competence. As a reader for
several journals, I am impressed by the generally high level of methodological
skill of the investigators. Two decades ago, there were relatively few skilled
researchers; today, most of the researchers know how to do competent research. The growth in methodological competence, however, has not been paralleled by an increase in theoretical sophistication. To the contrary, I have the impression that there is less theoretical knowledge and less concern with the systemization of ideas than was true twenty years ago. Too many studies in the journals have too much methodological elaborateness for their intellectual substance.

_Diversity._ Social psychology has increasingly spread its tentacles into other fields: from physiology to economics, from animal behavior to international behavior. The range of phenomena that social psychologists are involved in studying and the diversity of research procedures which are employed have increased markedly in recent years. This has produced something akin to an "identity crisis" in several well-known social psychologists. In addition, it raises questions about the nature of social psychology. Does it have a subject-matter of its own or is it merely a connecting link between other disciplines or is it the fundamental discipline of the behavioral sciences? At the moment, the centrifugal forces induced by diversity seem stronger than the centripetal forces arising from coherent theory.

_Cognitive emphasis._ Despite the increased respect for the techniques of behavior modification, social psychology has remained under the influence of the cognitive psychologists. Several years ago "dissonance theory" was a dominant influence in research in social psychology; more recently "attribution theory" has played this role. Both "theories" are in the Heiderian or Gestalt tradition and largely attempt to explain social
behavior in terms of what goes on inside the head. The G. H. Mead tradition of explaining what goes on in the head in terms of social behavior has received, in recent years, some unexpected support from Skinner and his followers. However, these latter do not seem to have benefitted by Mead's earlier, brilliant discussion of the issues with which they are concerned.

2. Landmark studies

Everyone has his own favorite list. Proshansky's and Seidenberg's Basic Studies in Social Psychology contains many of my favorites. "Landmark studies" are studies which establish a new paradigm for research. Typically, they do this in one of several ways: (1) by establishing a research format for investigating a phenomenon (e.g., Asch's conformity studies, Bandura's modeling experiments); (2) by establishing a research technique or methodology that can be used to study different phenomena (e.g., the development of multidimensional scaling); or (3) by developing an idea which has implications in many different contexts (e.g., dissonance theory, equity theory, attribution theory). Rarely is a "landmark study" one study; it usually takes a series of studies for the new paradigm to take a grip on the field.

3. Coherent pattern

Coherence is not the typical character of social psychological research. More commonly, a research paradigm plays itself out in a horizontal elaboration and varied demonstration of the phenomena under scrutiny rather than in a deeper understanding of them. Or the paradigm under question gets bogged down in a welter of conflicting details, often dealing with peripheral issues.
In my view, this occurs because few investigators attempt to go beyond the particular idea which they start with in the attempt to embed it into a system of interrelated ideas.

4. Fashions in research

Research in social psychology, as in many sciences, has a faddist quality: researchers want to be doing research where there is the most "action." In social psychology, for reasons cited above, this often leads to a mass of repetitive elaboration of a simple research paradigm in such a way as to kill interest in it. Since it is not a matter of establishing parameters (the ideas are usually not sufficiently precise for this), the mass of researchers working in a given area - without any theoretical interest - tend to kill it by sheer monotony or by a Walter of minor conflicting findings which they generate. Then a new paradigm attracts attention and the field hops to it. Topics like "group process," "cohesion," "conformity pressures," "attitude change," "dissonance," "conflict resolution," and "attribution" become popular and fade out, leaving in their wakes some new insights and facts which stand in partial isolation from old insights and facts.

5. Promising directions

It is very difficult to think of an unpromising direction in social psychology - whether it be the research on "attribution," "conflict resolution," "prosocial behavior," "urban stress," or "moral development." However, if forced to select such an unpromising area, I would select the "risky shift." The issue is whether the promise of the promising areas will be fulfilled. This will require investigators to take a more
theoretical attitude than has been true in the past.

6. Factors holding back progress

In my view, the major factors holding back progress is using the already existing social psychological knowledge are: (1) a lack of an adequate theory about how to use scientific knowledge to bring about social change; (2) a lack of a skilled corps of translators and other social engineers to implement existing knowledge; and (3) the resistance arising from the fact that existing social psychological knowledge has widespread political implications for social changes and these are not welcome to those who favor the status-quo.

In my view, the major factor holding back the development of new knowledge which could be potentially useful is the lack of systematic theory in social psychology. This lack, in turn, results from the fact that good theory is always difficult to produce, the current state of the field is probably not yet conducive to it, and the social conditions of work do not foster favorable conditions for theorizing.

7. Promising developments

Methodologically, the most important development is the increased capacity to deal with complexity as evidenced in multidimensional scaling, path analysis, more sophisticated use of the computer, multivariate analysis, more complex models for causal analysis, and the greater capacity to do research on larger-scale phenomena. An increased ability to deal with "organized complexity" is a necessity for scientific progress in social psychology.

Substantively, the most important development simply did not occur. In my view, social psychology needs more good "facts" as well as good theory.
Barker's "psychological ecology" had the right idea but probably the wrong methods. We need more good descriptions of people and groups in various situations that are socially important. We need to know more about what goes on in schools, homes, factories, etc. We simply do not know enough about the important psychological habitats and the behaviors which occur in them.

From what I have stated earlier, it is clear that I am not impressed with the current state of theorizing in social psychology. Yet I am not pessimistic. Many of us are beginning to feel a need to do something about it. The European social psychologists are particularly discontent with the implicit assumptions of American-dominated social psychology and they are raising fundamental questions (some of this is in a forthcoming book edited by Tajfel and Israel - which I cannot accurately cite). The creative move will, in the future, be in the direction of theory. I think there is an emerging tendency in this direction which may flourish if nourished by a proper climate of support.

8. Utilization of research

This is too big a topic for this paper. However, I suggest that the work of Paul Lazarsfeld and his collaborators - who have done case studies of the utilization of basic research - be consulted. In addition, our Conference on the Applications of Social Psychology will address itself directly to this topic.

9. NIMH's Role

Training. I know a good deal about NIMH's support for training in psychology and have been very much impressed by the good judgment, sophistication, and common sense which characterizes its operation in the
training area. It has been a forward-looking program of support, oriented to the future roles of psychologists as well as to their present ones.

My crystal ball envisages a growing need for social psychologists to be able to consult and to do research in large as well as small groups and organizations. They will need to combine the research skills and knowledge of the sociological and psychological social psychologist with the practitioner skills of effective organizational consultants. Obviously, not all social psychologists need be or can be expected to be universal experts; some will have expertise in consultation and training, some in research, and some in theory or methodology. Nevertheless, there will be a need for greater versatility than in the past. Employment opportunities for social psychologists will be expanding in the various professional schools - education, business, medicine, nursing, law, and public health - and in various types of organizations. They will continue to be relatively good in the traditional areas of employment of social psychologists. Although laboratory research will be a less dominant influence than in the past, it will still be an important tool in the research training of social psychologists whose own research will primarily be in field settings; it will also be useful as a relatively economical method to explore and test ideas.

An obvious implication of this forecast is that student trainees will or should be spending more time in field settings and more time acquiring the skills preliminary to functioning effectively in such settings. This in turn suggests that the incoming graduate student will have to acquire a good basic training in psychology and other related disciplines before entering the graduate program. It also means a considerable revamping of the offerings of many graduate training programs, with a probable increase in
their per student cost.

In addition to a demand for Ph.D. level social psychologists, I believe there will be an emerging need for people at the subdoctoral level. Some of these will be "practitioners"; others will be research administrators or research assistants. As research operations and social interventions become more complex, there will be an increasing need of trained helpers of various sorts who themselves would not be independent researchers.

Research. I know little about NIMH's research support. I have had no direct connection with it either through support of my own research or through participating in the review procedures. Nor do I have the haziest notion of what types of research studies are or are not being supported by NIMH funds. Thus, I cannot make any specific comments about NIMH's role. However, I have several general comments to make about research funding for social psychology.

1. The type of support which is most readily available, short-term grants for empirical studies, tends to foster a proliferation of atheoretical, unrelated or fashion-following studies which generally contribute little to the intellectual substance of the field. Programmatic support, longer-term support is more sensible.

2. Research funding should try to encourage more theoretical thinking. Admittedly, good theoretical thinking is rare and it is difficult to identify the seeds of a good theory from a submitted proposal to do theoretical writing. Nevertheless, it is not impossible to identify a number of people who might do such thinking and to indicate the possibility of support for a couple of summers, or for an academic year for theoretical work in a
given area. Support for such activity would be considerably less expensive than that for a research grant and, for some people, more welcome and possibly more productive.

3. "Relevance" has become a key word again in evaluating research proposals. As I have suggested earlier, not everything which seems relevant is actually so and many research studies are profoundly relevant even when they do not simulate the "real world." Some obviously relevant and important kinds of studies to do include: (1) Studies which investigate the conditions which affect the utilization of theory and research; (2) Studies which characterize systematically the social psychological world of people including their psychological habitats. These studies should be done with enough consistency and regularity to develop a good data base for evaluating the social psychological effects of "reforms" and other social events; (3) Studies which guide the development of and test the consequences of social inventions. There is a need to develop new practices in every area of life - including education, architecture, industry, recreation, and religion. Research to stimulate, guide, and evaluate these practices is essential; and (4) Studies which test out the implications of theoretical ideas in varied social contexts. The basis for generalization of research findings from one context to another always resides in the theoretical ideas which provide a conceptual link between the different contexts. The more varied the contexts in which a theory is supported, the more confidence one may have in its general robustness. There is, to repeat Lewin's dictum, "nothing so relevant as a good theory."