The subtitle of my paper is somewhat misleading because I do not like the terms "basic" and "applied" as used in reference to research; they imply an invidious distinction between forms of research and they are misleading in other respects also. I much prefer to distinguish among several broad, overlapping categories: "theory-oriented," "methodologically oriented," "problem-centered," and "evaluation" research. Each of these forms needs support by social practitioners such as educators, administrators, doctors, and lawyers and by the various professional schools. Each is difficult to do well and requires first-rate intellectual work, even though they vary considerably in the types of skills and orientations they demand from the researcher.

I wish to focus my comments on "theory-oriented" and "problem-centered" research. In so doing, I do not wish to leave the impression that "evaluation research" and "methodological research" are less important. In fields such as education and administration, where "fads" and "fashions" gust through the professional schools with minor changes in the political and social climate, the importance of evaluation research should be self-evident. My impression is that: (1) there is still very little evaluation research going on; (2) much of what is called evaluation research is of substandard quality; (3) there is need for theoretical research to explicate the causal models implicit in what is being evaluated; and (4) there is considerable need for methodological research to develop increased knowledge of how to do such research effectively and economically. The need for increased methodological knowledge, and thus more methodological research, exists for all types of research.

I turn now to some comments on theory-oriented and problem-centered research. Let me emphasize that neither the locus of one's research (whether it
is in the laboratory, a school, or a factory) nor its origin (in a practical problem or theoretical idea) necessarily determines its theoretical or practical significance. Research done in one classroom may have little relevance to what takes place in another classroom, whereas research done in a factory might have much relevance to understanding what goes on in a school. Research done in any particular locus at any particular time (classrooms, factories, or laboratories) can be of use in understanding what goes on somewhere else at another time only if: (1) we have a bridge of ideas (in other words, some theory) that connect the two settings; and (2) we have knowledge of the specific characteristics of the setting in which the ideas derived from prior research are to be applied. Thus, it makes no sense to attempt to predict what kinds of social relations and self-attitudes will develop in a given classroom from what occurs in another classroom if they differ in essential respects—e.g., if one class has a competitive and the other has a cooperative grading system. On the other hand, if one has a general theory of the effects of cooperative and competitive processes, then the theory provides a bridge for seeing the similarities between cooperative phenomena even in such different settings as work and school. The basic cooperative processes will be similar even though their concrete manifestations may differ in the two settings. The theory enables one to identify conceptual similarities and differences between the settings, and knowledge of the specific characteristics of each setting enables one to provide operational definitions of the appropriate theoretical concepts.

Just as the research setting does not determine the relevance of the research conducted in the setting (the ideas or theory determine the relevance), so the setting does not determine the theoretical significance of the research. Research of no theoretical significance can be done anywhere—in the laboratory, a school, or a factory. Similarly, research of considerable theoretical significance can be done in a school, factory, or community as well as in the laboratory. However, it is often cheaper and easier to do good, theory-oriented research in the laboratory.

DIFFERENCES BETWEEN THEORY-CENTERED AND PROBLEM-CENTERED WORK

There are some characteristic differences in emphasis between research that is primarily aimed at finding solutions to a problem and work that is aimed at developing or testing theory (Deutsch & Hornstein, 1975, pp. 262-267). These differences sometimes lead to mutual suspicion and antagonism between those who are more interested in one rather than the other form of research.
The Analytic Versus the Synthetic Approach

The technologist synthesizes by combining variables whereas the theory-oriented researcher analyzes variables by isolating them. The problem-centered researcher is more likely to be inductive, to start with the concrete and move to the general. The initial focus is on the symptoms of difficulties and these become the dependent variables whose causes are to be identified and controlled. On the other hand, the theory-oriented researcher is more likely to start with a concept and move toward a concrete exemplification of it. The focus is on the independent variables and what effects they cause.

Problems do not shape themselves to fit theories or even disciplines; a mosaic of theories and disciplines must be integrated as one attempts to solve a problem. Research directed at, for example, improving the amount of mathematics learned by elementary school students would have to draw on a wide assortment of theories—those related to learning, thinking, sex-role socialization, cognitive development, group processes, social change, and so on. In approaching such a practical problem, the technologist must not only draw on different theories within his or her own discipline but must also borrow freely from other disciplines. Moreover, because there are no established procedures for combining theories to fit a given problem, technologists must often work intuitively without being able to specify precisely how they are weaving together the different theoretical ideas that they are employing.

Universities and professional schools do not generally provide the institutional framework for a productive interplay between theory-centered and problem-centered research. A productive interplay would imply a two-way flow between theory-oriented and problem-centered work; each would stimulate and nurture the other. Theorists who have a lively contact with problem-centered research would be more apt to respond theoretically to the issues raised by concrete problems of practice; problem solvers would be more apt to approach their problem-centered research with a richer background of theoretical ideas.

As I see it, the development of such a productive interplay between theory-oriented and problem-centered research requires a dual organization at graduate schools and professional schools. This dual organization presupposes that theory-centered researchers would have two loci of responsibility: one connected with their discipline, which would house and support their theory-oriented research, and a second connected with a problem-centered institute of research. The institute would bring together researchers from a variety of relevant disciplines and practitioners who are directly involved in teaching and practice in the problem area. Presumably, each graduate school and professional school would have five to ten such institutes and each would
specialize in different problem areas. Hopefully, such institutes would be
renewed after 10 years or "self-destruct" rather than be perpetuated
thoughtlessly.

There are many problems in establishing such a dual organization. The
organization and traditional practices of universities work against it.
Moreover, there are shortages of both theory-oriented and problem-centered
researchers at most professional schools. Partly, this results from the fact that
the general level of support for research as compared to instructional activity
is too low. For such a dual organization to become feasible, there will have to
be a substantial increase in long-term funding for problem-centered research.

The Skeptical Versus the Pragmatic

There is a different orientation toward truth in the two forms of research. The
theory-oriented researcher is interested in enduring truths; the problem-
centered researcher is interested in useful truths. The interest in enduring
truths leads to a skeptical attitude toward what works: Will it be as true in a
different locus or at a different time as it is today? Do we know why it works?
The interest in useful truths leads to more of a focus on "how useful is it?" than
on "why is it useful or not?"

The Emphasis on the Surprising or Interesting
as Compared to the Practical or Economic

Theory-oriented research is more concerned with standards of verification
whereas problem-centered research is concerned with standards of effective-
ness. As a consequence, theory-oriented research gravitates toward the
surprising and thus, the interesting consequences of a theory because a
theory's validity and generality seem enhanced by its ability to predict the
unusual and unexpected. On the other hand, the practitioner must be
concerned with the mundane and practical—namely, with the aspects of the
situation that can be altered with minimum cost to produce the desired
consequence. Problem-centered research must be concerned with the relative
cost-benefit ratios of alternative courses of action; theory-centered research
focuses on the development of new insights into phenomena even when such
new understanding has no apparent applicability.

The Relative Importance of
Social Skills and Self-Insights

The social skills necessary for conducting theory-centered, laboratory
research in social psychology are minor compared to those necessary for
effective applied work. The array of skills that may be required of the
problem-centered social scientist include such entry skills as tact, social poise,
and persuasiveness, all of which are necessary to initiate and establish working relations with people whose status, intellectual background, social and cultural values, and interests may be quite different from those of the social scientist. They also include the skills involved in creating a sense of trust, respect, and clarity of roles in the working relationship that is established. Moreover, social scientists who are concerned with social problems may become advocates if they want to have their research findings used to bring about social change. As advocates they will need to have the political skills necessary to identify the levers of power and the manner in which they are controlled as well as the communication skills required to obtain and hold the attention of significant audiences. They will also want to be able to communicate with their audiences clearly, vividly, and persuasively. Doing this will often require the use of techniques, media, and styles of communication that differ radically from those characteristic of academic and scientific discourse.

Thus, it is evident that greater social versatility is required of social psychologists who venture outside of the laboratory than of the ones who confine themselves to it. However, it is by no means self-evident how to help train people in acquiring the social skills necessary to work effectively as applied social psychologists. It is apparent that traditional academic courses are not a sufficient means of training. Apprenticeship experiences with a skilled practitioner and experientially based training such as that offered by the NTL Institute of Applied Behavioral Science seem to be useful supplements.

In addition to more highly developed social skills, applied social scientists need to have more understanding of themselves. They need to know how they tend to cope with such personal problems as those relating to authority, conflict, dependency, and trust. It is evident that social practitioners, like psychotherapists often must rely heavily on subjective impressions in understanding what is going on and they are more apt to use impressions effectively if they are tuned into how their own psyches work. Well-trained therapists, for example, can use their awareness of how they are feeling, anxious, bored, or feeling trapped as a clue to understanding what is happening between themselves and their patients. Similarly, insightful social practitioners may only become sensitive to the implicit demands being made upon them as they become aware of their anxiety about fulfilling their own omnipotent fantasies. Such self-awareness may be useful in formulating problems for the theoretically oriented researcher, but it is not crucial for the conduct of research.

It seems apparent that such self-awareness is not a necessary or even a usual consequence of formal coursework in psychology. The experience of a psychoanalytically oriented psychotherapy or participation in sensitivity training or encounter groups often predisposes one to an inner awareness. However, as even casual observation of one's psychoanalytic acquaintances
will confirm, the predispositions are not always evoked in contexts dissimilar to the ones in which they were acquired. Thus, in addition to some form of experiential learning, supervised practice in a relevant context may be necessary before the self-awareness becomes a useful tool. One of the roles of the supervisor in such training is to focus on the feelings, peripheral thoughts, associations, and fantasies of the practitioner-in-training as he or she engages in different phases of practice.

The Relative Salience of Ethical Concerns

The continuing concern over ethical standards in relation to conducting research with human subjects indicates that the researcher must always be attentive to the welfare of the people who participate in research. However, rarely is the theory-oriented, laboratory researcher attempting to produce more than temporary effects in the persons being studied. Hence, many of the ethical problems center about avoiding even transient negative effects and insuring that the participation of the subjects is based upon their informed consent or the consent of appropriate guardians of the subjects' interests. The power over their own fate is presumably to be left in the hands of the subject rather than in those of the researcher.

It is apparent that the ethical issues become more profound when the social researcher or practitioner is attempting to bring about individual, organizational, or societal change of a more profound and enduring character. On the face of it, the ethical value of "informed consent" seems at least as appropriate to the work of the social engineer and problem-centered researcher as to that of the laboratory researcher. Yet if the management of a firm wishes to increase worker productivity and a consultant suggests a reorganization of work patterns, who is to give the informed consent—the workers who are to be influenced or the management who desires the change? There is no a priori reason to assume that there will usually be a coincidence of interests and views about such matters between labor and management. Thus, a consultant to management who advises a client how to introduce a labor change, without the informed consent of those who will be affected, is in a dubious position with regard to this ethical value. However, it is apparent that in social engineering, implementation of the value of informed consent directed toward producing individual or social change would entail a radical restructuring and redistribution of power. A critical difference in power would disappear if the strong could influence the weak only with the informed consent of the weak.

Even if the social practitioner is willing to accept the radical implications of the value of informed consent, he or she may still ponder its feasibility. How can one adequately and sufficiently describe a complex, technical process to untrained people so that they can make a truly informed judgment about it?
Moreover, isn’t the social placebo—the conveyance of the unquestioning and confident belief that a given practice will have a given result—one of the most influential devices in the social as well as the medical practitioner’s kit?

The Social Statuses of the Theory-Oriented and Problem-Centered Researcher

The “reference groups” and rewards tend to be different for the two types of researchers. The theory-oriented researcher obtains prestige from fellow scientists in his or her discipline and related disciplines, and the rewards are largely honorific. The problem-centered researcher obtains prestige from practitioners, teachers, and administrators. Because his or her work often has clear economic value, there are more funds available to support the researcher and the research.

A COMPARISON OF A THEORY-ORIENTED AND A PROBLEM-CENTERED RESEARCH STUDY

I would like to describe two early studies of mine. One was a theory-oriented investigation (1949a, 1949b) and the other was instigated by a social problem (1951). My discussion of these studies takes a different viewpoint on theory-oriented and problem-centered research than the one I have advanced in the preceding pages. Now, I highlight the need to overcome the “blinders” that limit one’s perspective as one defines one’s research and oneself as being “theory-centered” or “problem-centered.” The emphasis on the differences between the two types of research may prevent one from recognizing the similarities between them. Much good social psychological research can contribute to both theory and practice. Moreover, research instigated by a given problem (e.g., the threat of nuclear war) may become so transfigured in its formulation and execution that it loses its specific relevance to the problem that initiated it, even as it takes on relevance to a different type of problem. In addition, it is evident that the social usefulness of a given piece of research may have a short or long life span. A study with much immediate relevance to social issues may become quickly outmoded if it has no general ideas; a study that is remote from immediate concerns but has important ideas may lead to a stream of research, which throws continuing light on a variety of social problems.

In the following discussion, I examine each of the two studies from the perspectives of their social utility and theoretical significance. I suggest that by thinking of one study as “basic” and the other as “applied,” I blinded myself to recognition of the full potentials of each. This self-imposed limitation is one that, I believe, has been unfortunately typical of social
psychology. I will also suggest that in considering the social usefulness of research one must adopt a long as well as a short time perspective. Regrettably, those most interested in making social psychology more useful have often adopted only the short time perspective (Deutsch, 1969).

A Study of Cooperation and Competition

The study of cooperation and competition was initiated under two major influences, one of which shaped its substantive focus and the other of which determined its form and its scientific goals. The substantive focus grew out of my concern about nuclear war. Like many others at the time, I thought that human life would not long survive unless the nations of the world cooperated. This thought got focused on the United Nations Security Council and was crystallized in two contrasting images: the members of the Council working together cooperatively with a problem-solving attitude or the members competing with one another to obtain a relative advantage for their own nations. I suspect that my initial concern developed this way because the United Nations Security Council was in the public spotlight and also because I was then a student at the Research Center for Group Dynamics at M.I.T. There it was natural to think of group process, group productivity, and the factors that influenced them.

As my attention shifted from the relations among nations to relations within a group, the problem took on a more generalized form. The problem was now transformed into an attempt to understand the fundamental features of cooperative and competitive relations and the consequences of these different types of interdependencies in a way that would be generally applicable to the relations between individuals, groups, or nations. The problem has become a theoretical one with the broad scientific goal of attempting to interrelate and give insight into a variety of phenomena through several fundamental concepts and several basic propositions. The intellectual atmosphere of Kurt Lewin's Research Center for Group Dynamics was such as to push its students to theory building. The favorite slogan at the Center was "there is nothing so practical as a good theory."

Thus, I turned my social concern about the possibilities of nuclear war into a theoretically-oriented investigation of cooperation and competition. In so doing, did I lose contact with my original concern? Is there any relevance at all of a theory of cooperation, competition, and an experimental study of small groups to the prevention of a nuclear holocaust? Before answering this question, let me state that my tendencies to grandiosity, although not insignificant, are under control. I have never thought that any efforts of mine, whether as a scientist pursuing systematic knowledge or as a citizen engaging in political action, would be a crucial factor in influencing the likelihood of such large-scale events. Nevertheless, I have maintained the hope that the
cumulative efforts of many individuals pushing in the same direction may have significant effects. In addition, I have assumed that the acquisition and dissemination of systematic knowledge is inherently of social value; misjudgment, evil, corruption, and the abuse of power are abetted by ignorance but are reduced by the dissemination of knowledge.

Thus, I have hoped that my intellectual work on cooperation and competition, combined with the work of other social scientists on related problems, might significantly affect ways of thinking about these types of social relations and that, as a consequence, systematic and possibly new ideas about preventing destructive conflicts among nations might emerge. Anyone wishing to accuse me of being optimistic, of course, would be right. After all, my initial theoretical and empirical work in the area of cooperation—competition centered on the differential effects of these types of relationships. Only later did I work on the factors influencing whether a cooperative or competitive relationship would develop. This later work (which has been described under such labels as "interpersonal conflict," "bargaining," "conflict resolution") is much more directly related to the question of preventing destructive conflicts. Yet, it turns out that one of the major simplifying ideas about factors affecting conflict resolution arising out of my more recent work complements my earlier theoretical analysis of the effects of cooperation and competition. Namely, the characteristic processes and effects elicited by a given type of social relationship (cooperative or competitive) tend also to elicit that type of social relationship. Thus, the strategy of power and the tactics of coercion, threat, and deception result from and also result in a competitive relationship. Similarly, the strategy of mutual problem solving and the tactics of persuasion, openness, and mutual enhancement elicit and also are elicited by a cooperative orientation.

In essence, the theory states that the effects of one person's actions on another will be a function of the nature of their interdependence and the nature of the action that takes place. Skillfully executed actions of an antagonist will elicit rather different responses than skillful actions from an ally, but a bumbling collaborator may evoke as much negative reaction as an adroit opponent. The theory links type of interdependence and type of action with three basic social psychological processes—which I have labeled "substitutability," "contexesis," and "inducibility"—and it then proliferates a variety of social psychological consequences from these processes as they are affected by the variables with which the theory is concerned. I do not attempt here to spell out how this is done. The theory has been published (Deutsch, 1949b, 1962), and my interest in this presentation is on the conditions determining the initiation of cooperation and competition rather than on their effects.

The point I wish to make is that if you take a situation in which there is a mixture of cooperative and competitive elements (most bargaining and
"conflict" situations are of this nature), you can move it in one direction or the other by creating as initial states the typical consequences of effective cooperation and competition. In such indeterminate situations, the tendency to relate cooperatively will be increased by anything that will "highlight mutual interests," "enhance mutual power," "lead to trusting, friendly attitudes, and a positive responsiveness to the other's needs," "minimize the salience of opposed interests," "lead to open, honest communication," etc. On the other hand, the likelihood of a competitive relation will be increased by attempts to "reduce the other's power," "suspicious, hostile, exploitative attitudes," "the magnification of the opposed interests," "the use of tactics of threat, intimidation, or coercion," "devious communication and espionage," etc.

I have attempted to express an idea that is still in the stage of intellectual development. If it is nurtured carefully, it may have considerable sweep and may help us to deal with social conflict productively rather than destructively (Deutsch, 1973). However, the theoretical and empirical work being done on the resolution of conflict reflects the intellectual efforts of many social scientists from a variety of disciplines, and by my emphasis on my own work I do not wish to give a misleading picture of the unique significance of my contributions. My own work is only a small part of the total scientific activity in this area, and it is the cumulative result of these diverse efforts that are beginning to have social utility.

Initially, the major utility of these accumulating efforts has been in the emergence of a mode of thinking with an array of concepts that highlight some of the central processes involved in conflicts and that provide a coherent basis of organizing the details of such processes. This has served to reduce the mystical aura of the inevitability of destructiveness often associated with conflict, it has provided new insights to many people engaged in the handling of conflict, and it has occasionally been reflected in important public statements. Thus, the historically significant speech of President Kennedy at American University on June 10, 1963, in which he outlined "A Strategy of Peace," and which signaled the start of a thaw in American-Soviet relations, was clearly very much influenced by the newly emerging social scientific mode of thinking about resolving conflict. More recently, the Kerner Commission Report on race was also enlightened by this new perspective. Under the leadership of such applied behavioral science groups as the National Training Laboratory Institute for Applied Behavioral Science, there has been a widespread application of social psychological approaches to conflict resolution in industrial and school settings.

A Study of Interracial Housing

Let me turn now to a rather different type of study, our study of interracial housing. Like the work on cooperation-competition, it was stimulated by the
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belief (Deutsch & Collins, 1951) that "the social scientist has a responsibility, not merely to further his own esthetic and intellectual pleasures in the course of research but also to contribute to the solution of important social problems [p. xi]." However, the interracial housing study, unlike the earlier one, was guided throughout by a continuous concern with social usefulness. We selected interracial relations in housing for investigation because we felt that residential segregation was of central importance to intergroup relations in general. Residential segregation by its very nature leads to a de facto segregation in many other areas such as schools, churches, banks, playgrounds, and shopping and community centers. It also usually leads to major economic disadvantages for the ghetto resident. Another reason for the focus on housing, with particular emphasis on a comparison of integrated and segregated occupancy patterns, was the realization that the Federal Housing Act of 1949 would soon give rise to many public housing developments. Public officials in localities throughout the country would be making decisions in the near future about whether these developments were to be racially integrated or segregated. (The usual practice in the past has been some form of segregation.) It was our hope that our research might have some influence on these decisions.

Our study compared integrated interracial housing developments in New York City with segregated biracial developments in Newark in an ex post facto experimental design. The developments were selected to be as comparable as possible except for their occupancy patterns. We studied behavior and attitudes of blacks as well as whites, of adults as well as children. As in all such ex post facto field studies, the design was not as tight as one would like, but we did a reasonably careful job in seeking a variety of evidence in order to test the various alternative explanations for our findings. Our findings (Deutsch & Collins, 1951), from many different angles, supported the conclusion that "from the point of view of reducing prejudice and of creating harmonious intergroup relations, the net gain resulting from the integrated projects is considerable; from the same point of view, the gain created by the segregated biracial projects is slight [p. 24]."

This conclusion was, of course, buttressed by a detailed consideration of the conditions under which it was likely to hold. That is, we did not assume that integration would always and inevitably produce favorable results. We assumed that if the projects were poorly run, dilapidated, managed by people who were bigoted in attitude or practice, or inhabited by racial groups of radically different economic status and social customs, then favorable results from integration would have been unlikely. We selected our segregated as well as our integrated projects so that they did not have these characteristics.

It is natural to ask, "What would we have done if our results had shown that segregation rather than integration produced more favorable interracial relations?" The answer is that we would have published the results but would have searched for an understanding of the conditions that caused interracial
contact to lead to antagonistic rather than cooperative behavior. A social scientist in such a situation has a dual responsibility to be honest about the results and to publish them in such a way so as not to encourage false interpretations that cater to basic prejudices.

The study was widely discussed by public housing officials and by community groups, and its findings helped a number of housing authorities to adopt a policy of nonsegregation. The Executive Director of the Housing Authority of the City of Newark, for example, wrote in a postscript to the study (Deutsch & Collins, 1951):

In supplying us with an objective picture of race relations in our project, a picture which is faithful to our own impressions, their study dramatically focused our attention and that of the community at large on matters which under the press of other business, we had tended to ignore. The study did more than help to focus attention on the basic question of segregation in housing. Perhaps its most important consequence was its usefulness to those community groups concerned with intergroup relations and civil rights...To such groups the study was an invaluable tool in creating the atmosphere which made it possible for the housing authority to adopt and execute a policy of nonsegregation. I don't know how many meetings of such groups I attended, but invariably the Deutsch-Collins study was referred to and quoted [p. 130].

Clearly, then, our study of interracial housing had some immediately useful and significant social consequences. In addition, it served to challenge a widely held opinion that "stateways cannot change folkways," which, not surprisingly, was usually interpreted as to support the status quo rather than a change in "stateways." Thus, the study along with much other research, helped to provide a supporting rationale for judicial decisions and legislation to bar segregation.

However, I must admit that this research did not contribute significantly to the ideas of social psychology. Intellectually, its merit did not reside in its theoretical innovation but rather in its systematic application of existing social psychological concepts to the important social issues of residential segregation. This is not to say that there is not a need for new ideas and theoretical advances to clarify and develop systematic understanding of intergroup relations. Indeed, some of our current experimental research has this as an objective. But I must confess that as we begin to explore some of the conceptual underpinnings in this area we are pulled in the direction of such questions as the relationship between social categories and social coordination. It takes us away from the immediate social urgencies. Our housing study was not formulated with a theoretical objective and, unfortunately, we were not alert enough to appreciate fully the wider theoretical significance of some of our findings. These findings suggested that behavioral change preceded attitudinal change. The white women in the
integrated projects often behaved in an unprejudiced manner toward their black neighbors before they felt this way. Had we been clever enough to realize the general implications of this finding we might have anticipated the major idea underlying Festinger's theory of cognitive dissonance (Festinger, 1957), which is in essence the converse of the old truism that people tend to act in accord with their beliefs. Namely, people tend to make their beliefs and attitudes accord with their actions. This is a very important idea that has major implications for theoretical and practical work in the area of attitudinal change. This idea could have emerged from our field study of interracial relations in housing but, alas, it did not.

Which Study Was More Socially Useful?

If now, many years after the completion of each of these two studies, I try to assess their social utilities relative to one another, what conclusion seems warranted? Both studies have received considerable recognition within social psychology and both have been honored by being selected for inclusion in Basic Studies in Social Psychology (Proshansky & Seidenberg, 1962). However, I clearly have a personal preference that I should confess before attempting to answer the question. My work on cooperation-competition gives me more satisfaction and I have more pride in being its author. The ideas in it are more original and more fundamental to understanding social life irrespective of time or place than the ideas in Interracial Housing. Nevertheless, I cannot point to any specific desirable social change that has resulted from my work on cooperation-competition. Whatever social benefits have derived from it have come indirectly through its contribution to the stream of ideas being developed cumulatively by the social sciences that are leading to new perspectives on conflict.

Interracial Housing, on the other hand, has made little new contribution to the theory of social science, but it has clearly contributed to desirable social change in several instances. However, let me note the obvious. Despite our study and the work of many other social scientists that have demonstrated the social and personal value of racial integration, there is little racial integration in the United States. The available statistics indicate that there is more de facto residential segregation in the United States now than there was 20 years ago. Similarly, as Pettigrew (1969) pointed out, "There is more racial segregation of schools today in the entire United States than there was in 1954 at the time of the Supreme Court decision [p. 4]." With the repeated and continuing frustration of attempts to break down the walls of custom, belief, and institutionalized practice that keep black and white separatists, it is no wonder that the achievement of racial integration is now of less relevance than the enhancement of black power. As far as one can tell, there will be no meaningful racial integration until the economic and political power of the
black people, and their white allies, is strong enough to shatter the walls of institutionalized indifference and discrimination that perpetuate their isolation and disadvantage. Black pride, cohesive black organization, and effective political alliances are keys to the enhancement of black power, chauvinism and separatism, on the other hand, provide a fragile, illusionary, and inherently self-defeating basis for group or self-esteem. Thus, although in my opinion the findings and recommendations of our study of integrated housing are still valid—integration is still preferable to segregation—history has turned a study that once seemed highly relevant into one that now appears to be somewhat beside the point.

Whatever its changing relevance for immediate social concerns, it was not inevitable that the housing study be lacking in theoretical significance; as I suggested earlier, had I been more keen-witted I might have anticipated the key notion of dissonance theory. Similarly, the study of cooperation-competition need not have been without immediate social consequence. It was conducted in an experimental format, which incidentally involved a systematic comparison of two types of classroom grading systems: cooperative and competitive. The results clearly demonstrated some harmful consequences of the competitive grading system and they could have been employed in a campaign to reexamine the prevailing system of grading. Such a campaign was not in existence nor did the thought of fostering one occur to me.

Thus, the potential usefulness of each study was unduly limited by the "blinders" I had donned unwittingly. In thinking of the cooperation-competition study as a theoretical inquiry into fundamental issues, I overlooked its immediate social significance. And in conceiving of the interracial housing study, in terms of its immediate social relevance, I was insensitive to its broader implications. I am inclined to believe that the "blindness" reflected in both instances is not unique to me but is commonplace in psychology. Psychologists doing research on how to improve the learning of a particular subject matter in the classroom rarely make significant contributions to learning theory, and those seeking to test theoretical notions rarely conduct their research in such a way as to suggest immediate consequences for learning in the school.

Obviously, "blinders" have functions. They reduce distractions and facilitate focused attention. Yet, if worn too long, they limit the scope of vision; a broad perspective is necessary to a socially relevant science. A focus on "science" that excludes "social relevance" as a distraction or a focus on "social relevance" that excludes "science" as irrelevant will in the long run be destructive to both. A society will not long nurture a science that does not nurture society. Nor will there be much to nourish society with unless there is a proliferation of systematic knowledge that is rich and diverse enough to initiate and reliably sustain complex activities in many different settings.
Let me now return to the question of the comparative social utilities of the two studies I have discussed. It should be obvious that there is no simple answer. Both types of studies are needed for a socially relevant science. Moreover, a rigid characterization of the kinds of studies into sharply different categories may be dysfunctional to both. This is not to deny that the primary social value of a theoretical study such as that of cooperation–competition may be manifest only in the long run whereas the primary utility of an applied study may be manifest in its immediate social consequences. It is evident that in considering what psychologists can contribute to society, we must be concerned with both the future and the present. In many areas of urgent social concern, we do not have enough reliable, systematic psychological knowledge to make any valid social contribution at present. Some of this knowledge can be acquired only by freely ranging investigations that seek to define and formulate the fundamental questions that must be answered before one can identify what knowledge is really relevant to a solution of a problem.

It is a truism that a demand for relevance makes no sense until one can identify what is relevant. History and everyday experience alike testify that the appearance of relevance, the pseudorelevance of surface similarities, can be grossly misleading. It is hard and demanding intellectual work to get beyond the clichés and slogans of the "establishment" or the "antiestablishment" to a fundamental and usable understanding of the important social problems confronting us. It requires the freedom to be irreverent and irrelevant to the current idols and the passing fashions. Nevertheless, a salient concern for social relevance may be a healthy component in the motivation of the work of all psychology whether they be functioning as "basic" or "applied" researchers. There may be no long-run future for any of us or for psychology unless some of the urgent social crises facing us are dealt with intelligently.

In summary, I have described characteristic differences in the research orientations involved in theory-centered as compared with problem-centered research. I have illustrated how these differences may lead investigators who adopt a theory-centered orientation to be blind to the useful social implications of their research; and also how the differences may lead problem-centered researchers to miss the potential theoretical significance of their work. However, both orientations are necessary if we are to have a productive, socially useful social psychology that is good for the long run as well as the short run. When viewed from a larger perspective the two orientations complement one another, yet they are often experienced as being in opposition. How can we produce more of a cooperative interaction between them? My main suggestion in this paper has been to propose the development of integrative institutional frameworks that would promote the
productive interplay between the two orientations because our present institutional settings foster their segregation. It should also be evident that my theory about the causes and consequences of cooperation and competition has implicit in it many other suggestions. I leave it to the reader to draw out these implications.

REFERENCES


