The Systematic Analysis of Socially Significant Events: A Strategy for Social Research

Stuart W. Cook

One of Kurt Lewin's convictions was that a study of the significant social phenomena about us is important to the development of a social science worth taking seriously. He believed that the close interrelating of social research and theory with everyday social events would, if undertaken systematically, contribute greatly to the development of basic science. It is on this point that what I shall have to say today makes contact with his life and influence.

Lewin's view on this matter was shared by those psychologists who founded SPSSI and provided its leadership during its earlier years. For many of these psychologists membership in SPSSI and choice of research problems went hand in hand. As the years have passed, this coincidence of SPSSI membership and research on significant social phenomena has become less and less frequent.

I shall try to suggest some reasons why this change has taken place and what I think might be done about it. But first I should like to make explicit my own recognition of a fact which constitutes the background for much of what I have to say. This is that I speak today not only of my own work and thinking but of that of two research groups whose achievements are, in fact, responsible for my selection for this award. One of these groups is CCI, the Commission on Community Interrelations of the American Jewish Congress, which Lewin founded; the other is the Research Center for Human Relations of New York University. Over the past fifteen years my ideas have developed inseparably from those of my colleagues in these groups; all of my research has been done in collaboration with them. I no longer know the source of some of the thoughts I shall voice; I can but call your attention at the outset to the probability that much of what is to follow originated with others. At least ten of my present
or former colleagues have been close to the research I shall describe—although not all of them may subscribe to my conclusions about it. These ten are Isidor Chein, Mary Collins, Morton Deutsch, Barbara Dohrenwend, John Harding, Russell Hogrefe, Marie Jahoda, Claire Selltiz, Rosabelle Walkley, and Daniel Wilner. For brevity's sake, however, I shall speak throughout simply of what we have done.

My purpose in this talk is to make some suggestions which may increase the amount of research related to socially significant phenomena rather than to discourage research of other types. I recognize the high pay-off potential of studies which strive from the beginning to identify explanatory processes of wide generality. Such studies typically start from a description of some construct or hypothetical process such as empathy, social perception, cohesion, or dissonance, and proceed by contriving some social environment in which the construct or process presumably operates and can be studied. Rather than reduce the number of such studies, what I desire instead is to lend support to a research strategy about which I believe we have become needlessly discouraged and disillusioned. Our faith that the psychological study of social issues would contribute not only to the constructive resolution of social problems but also to the development of psychological science has been greatly weakened. We have become increasingly pessimistic about the possibility of going beyond description and evaluation research in dealing with complex social conditions. Systematic analysis of social issues, in the sense of identifying common sets of variables that underlie apparently diverse situations and of isolating the effects of each such variable, has on closer examination seemed beyond reach. Accordingly, the feeling has grown that research on socially significant phenomena can not be theoretical; it can only be applied. As long as this impression persists we shall in all probability not enlarge upon our present minimum effort in this area. On the other hand, if the impression could be shown to be unjustified, research behavior would change correspondingly. Most of us, as a recent SPSSI self-study showed, would rather work upon the factors which weigh heavily in social behavior than on those whose import is trivial. On the other hand, most of us wish also to contribute to the building of our science. Is it possible for us to integrate these two desires?

My contention is that there is a strategy available to us that does make this possible but that it is only rarely followed. I have referred to this strategy in my title as "The Systematic Analysis of Socially Significant Events." What in general I mean by this is an approach in which one starts with the social phenomena he wishes to understand or explain or predict. By stages he develops a conceptual framework or system which permits an increasingly analytical treat-
ment of these phenomena. At the outset of his endeavor he is likely to make most use of exploratory studies and field experiments. As he progresses he adds experiments carried out in the laboratory. Throughout he utilizes such classificatory concepts and hypothetical processes as are needed to help him order and interpret his data. From a starting point of complex social phenomena he gradually moves to a more detailed and analytical concern with the causal dynamics of social behavior. Thus, at an appropriate stage in the sequence of events the development of theory begins. It is characteristic of theory growing out of this approach that it deals with the significant factors in social life which we claim to be the province of behavioral science.

The illustrative studies to which I shall refer constitute a case history of work done by my colleagues and myself on intergroup contact and attitude change. Such contact occurs in various life settings—residential, educational, occupational, recreational. It may vary from hostile to indifferent to friendly. It may be thought of as a recurring social pattern or relationship with potentials for outcomes in action and attitude which from a societal point of view are important. It exemplifies what I mean in my title by “socially significant events.”

As you might imagine, I arrived at my interest in intergroup contact and its consequences by way of the broader problems of intergroup relations, prejudice, and discrimination. The war and the post-war period brought an enhanced concern for these questions. My own work in this area began soon after the war in company with my colleagues at the Commission on Community Interrelations of the American Jewish Congress. Shortly thereafter, in his 1946 presidential address to the Society for the Psychological Study of Social Issues, Theodore Newcomb suggested what he thought to be the most likely avenue to the reduction of intergroup hostility (9). He described this as the crossing of institutionalized barriers to communication with members of the other group, with the shared support of members of one’s own group. Following Newcomb’s address, two influential reviews of intergroup relations appeared: one by Goodwin Watson called Action for Unity (13) and another by Robin Williams called The Reduction of Intergroup Tensions (14). Both reported that many practitioners in the field of intergroup relations believed that contact or personal association with the object of one’s prejudice was likely to be more effective in changing attitudes than were such alternative experiences as exposure to correct information, persuasive communication, etc. But both also pointed out that neither everyday experience nor research results uniformly supported this view, and raised questions stressing the need to consider the effects of different conditions of contact.

As we watched the evidence accumulate on such matters as
selective rejection of favorable communications regarding disliked groups, the boomerang effects of cartoons opposed to prejudice, and the self-selection involved in exposure of the already favorable to intergroup educational programs, my colleagues and I soon came to share the view that personal contact might be a more potent influence than other experiences. Accordingly, we decided to focus our work, where possible, on the contact experience. We recognized that we should exclude the type of contact that had been sought out by the participant, since in such a case the effect of the experience on the individual's actions could not be separated from the effect of the favorable attitudes he had brought to the situation. For this reason we limited ourselves to contact which had not been intended by the participants and was, in this sense, involuntary.

As might be expected, most of the studies that had been done up until that time put the question in blanket terms: Would intergroup contact reduce prejudice? Generally, one knew little more about the contact than its location, as in neighborhoods or at work, or the occupations of the participants, such as doctors or teachers. There were two exceptions to this. In 1942, Harlan, in an analysis of personal contacts with Jews as recalled by non-Jews, classified them in terms of frequency and of intimacy (5). He reported that the more frequent the contacts with Jews the greater the prejudice but that the more intimate the contacts the lower the prejudice. Somewhat later Allport and Kramer, also working with recalled contacts, noted that in some contacts both groups were equal in status while in others the minority group members were in a lower status position (2). One of their findings was that while non-Jews with considerable contact were as prejudiced toward Jews as were non-Jews with little or no contact, the picture was different when only equal-status contact was considered; persons reporting such contact were significantly less prejudiced than those who had not had such contact.

Our first working concepts, then, included on the independent variable side two dimensions on which to categorize intergroup contact: First, whether or not the contact was equal status and, second, the degree of intimacy of the contacts. Newcomb, Watson and Williams in their discussions had also suggested several other considerations which we were later to find useful. Among these were social support for the contact, competitive and cooperative factors in the contact, similarity of interests among the participants to the contact, and the initial level of prejudice and hostility.

The first step we took in carrying out our decision to study intergroup contact was, in retrospect, quite naive and not consistent with the strategy I shall conclude by recommending—that is, we proceeded immediately to plan an experiment (3). We were in touch at the time
with an organized recreational program in which the clubs or play groups were composed, by design, on an interracial basis. From the point of view of the children, however, the interracial association was unintentional; they came to the recreational center because of its excellent swimming pool and recreational facilities—facilities unduplicated in the area in which they resided. They did not come because of its interracial program. Moreover, on the two major dimensions we already had in mind, this promised to be effective interracial contact: The white and Negro children would have equal status in the play groups and the activities of the program were such that the contact should be fairly intimate.

The question we asked in our experiment had to do with the problem of generalization. The tendency to isolate or compartmentalize favorable information about persons from social groups one dislikes had often been pointed out. We assumed that one of the conditions that facilitated the compartmentalization process, especially in children, was the freedom to suppress or, so to speak, not notice the group membership of the person with whom one was associating. Since at that time we felt confident that the recreational program would produce satisfying experiences and pleasant affect for the children and would probably lead to friendships with their racially dissimilar playmates, we did not attempt to vary conditions which might strengthen or weaken this outcome. Instead we concentrated upon what might generalize the favorable relationships we anticipated to the racial groups as a whole. What we ended up with was a plan in which the adult group leaders were to watch for occasions on which particularly enjoyable experiences were being shared by white and Negro children. On these occasions the group leaders were to interject comments which called the children’s attention both to the fun they were having and to the racial differences in the group. As you can see, we thought of ourselves as counteracting the compartmentalization tendency by connecting two experiences—bringing them into conscious contiguity, you might say—namely, the experience of pleasure, on the one hand, and, on the other, the verbal labels, Negro or white, that identified the racial membership of the children partly responsible for this pleasure. There were to be 12 groups of 20 children each. Each group was approximately half white and half Negro. Six groups were to receive the experimental treatment; a comparable six groups were to serve as controls.

We were quite pleased with the plan for the experiment and, I suspect, had visions of giving learning theory the shot in the arm we thought it needed. However, the group leaders—our experimental agents—along with almost everyone else who heard about the experiment, recoiled in moral indignation. Wasn’t it elementary, they pointed out, that our objective was to teach children to enjoy play without
regard to the race, religion or national background of their associates? Then, if so, why call attention to race?

We managed to carry off the experiment although, due to this and other difficulties, it was only a watered down version of what we had intended. More detail about it would not be germane to my present theme. The point of the anecdote for these remarks is simply that in retrospect we realize that, however sound the hypothesis, such an experiment was premature. We were as yet too unfamiliar with the outcomes of contact experiences to have assumed as we did that relationships with and feelings toward other children present in the situation would change. As it developed, the weight of evidence was that for these particular children there was in fact little, if any, such change. Thus, even had we been able to introduce the experimental treatment in the magnitude intended, it is doubtful whether the generalization hypotheses under examination would have been put to test.

About at this point in our work, we began to look into interracial public housing as a possible setting for our research. Federally assisted public housing projects for low income families were being constructed throughout the country. Some of them had both white and Negro tenants. The extensive demand for housing made it seem likely that in the case of such bi-racial projects we had another instance of unintended contact. Some preliminary investigation supported this view; professional personnel in the housing field assured us that it was rare, if ever, that a tenant admitted to a project turned down his opportunity for any reason, including the knowledge of its interracial character.

We learned, however, that interracial housing projects did not all come in the same mold; among other things, they varied in what housing professionals called occupancy pattern. The Negroses might occupy one section of the projects with the whites in another, an arrangement which we later came to call area segregation. Or the Negroses might occupy one apartment building with the adjacent one being occupied by whites and the next one in line being occupied by Negroses, that is, a pattern of building segregation. Finally, there could be found instances of complete integration with white and Negro tenants occupying adjacent apartments.

As we became aware of this and other features of interracial public housing, we decided it might be wise to attempt a much more comprehensive review of experiences with it. Accordingly, one of us undertook to locate and interview individuals scattered throughout the country who had reputations for skill in this field (4). We asked them to tell us about their experiences and indicate what factors, in their judgment, had led to friction and what factors to friendly relations in projects where there were both white and Negro tenants.
The replies stressed the significance for tenant relations of such matters as the character of the neighborhood surrounding the housing project, the attitudes of management personnel, the character and timing of announcements of racial policy, the proportion of tenants from each racial group, the types of tenant activities, etc. Although there were differences among our informants in what they emphasized, many of them felt that occupancy pattern was of major importance to relations between tenants of different races. In addition it became clear to us that occupancy pattern was a basic characteristic of the housing project which established a context for studies of its other features. Accordingly, we decided to take advantage of what appeared to be a natural experiment represented by the existing variation in occupancy pattern. Our informants had said that social relations among white and Negro tenants in projects with integrated buildings would be closer and friendlier than among tenants in projects with some form of intra-project segregation. Fortunately, we found what we needed close at hand: two area-segregated projects in Newark and two roughly comparable integrated projects in New York City. We proceeded to study the tenants in these projects and emerged with clear-cut and striking findings, about which I shall have more to say in a moment (4). For now, my point is only that the study was quite successful and particularly so by contrast with our attempted experiment in interracial recreation.

Let me digress for a moment to say that during this period we became aware of something that in retrospect looks quite obvious—namely, that knowledge and experience with the phenomena we wished to study existed in unwritten form. Our thoughts about this crystallized in the context of ideas which had just been advanced by Donald Marquis in his 1948 presidential address to the American Psychological Association (7). In a talk on research planning at the frontiers of science, he lamented the prevalent practice of conducting studies unrelated to one another and argued that theoretical contributions on the frontiers would be forthcoming only if we develop the technique of program, as distinct from project, design. He pointed out that program design for the unexplored problems characteristic of scientific frontiers would profit greatly from preliminary observations to give the scientist background for his early efforts to formulate working concepts.

It seemed to us that our interviews with housing personnel who had had experience in bi-racial projects pointed the way to a particularly effective method of making preliminary observations. Selected categories of people are in a position to observe in the course of their everyday work the effect of alternate decisions and actions with respect to social relations. Such specialists acquire a reservoir of experience that can be of tremendous value in helping the social scientists to
become aware of the important influences operating in a situation. Accordingly, we decided to devote some effort to perfecting the technique of interviewing such informants. This we did in the course of two subsequent studies, one of recreation workers in settings where there were participants of different races, the second of management and union personnel who had had experience in situations where both whites and Negroes were employed. We came to call this method the experience survey and have published a brief account of it in our text, *Research Methods in Social Relations* (11).

The overall results of our first intergroup housing study are well known and I shall remind you only of their general nature. We found, as anticipated, that more neighborly or intimate contacts had, in fact, developed in the integrated projects than in the area-segregated ones. Also, white housewives in these projects reported with much greater frequency that they believed their white neighbors approved of friendly relations with Negro tenants. And, when we examined the beliefs and feelings of the housewives, we found that many more of the integrated project respondents than of the area-segregated ones showed favorable attitudes.

As we reflected on the differences between the two occupancy patterns, we realized that they involved at least two components. The first, and more obvious, was the fact that the integrated occupancy pattern led to Negro and white tenants living closer to each other than did the area-segregated—this was a proximity component. The second was that the two patterns might be interpreted by the tenants to symbolize different attitudes on the part of persons in authority as to what was proper and appropriate in race relations; the integrated might indicate approval of mixing; the area-segregated, disapproval—this was a social climate component. However, it was not possible to examine separately the effects of the two components because in the housing projects in this study they went completely together. Such an examination did become feasible in a second housing study two years later (15). In this second study, the segregated projects were building-segregated rather than area-segregated, and only about 10 percent of the tenants in any of the projects were Negroes. Under these conditions it worked out that there were white tenants in the integrated projects who lived farther from any Negro family than did some of the white tenants in the building-segregated projects. Given this range on the proximity variable within each of the two types of occupancy pattern, we were able to compare tenants within each type who differed in proximity but for whom the social norm implied by occupancy pattern was constant. The results supported the interpretation that it was proximity which gave the major push to the development of intergroup contact.

In this second study we were able to take still another step
which supported the appropriateness of the conceptual distinctions we were developing. As in the first study, we had collected evidence not only on the degree of intimacy of contacts but also on the perception of approval or disapproval of interracial associations anticipated from other tenants. While these two variables were associated with one another, there was enough unrelated variation so that we could examine the relationship of each to favorableness of attitudes. While the picture was complex there seemed little doubt that each variable was contributing to attitude change.

Meanwhile we had been involved in other studies of intergroup contact and had learned of still others. Some were raising questions about the effects of intergroup contact. Others had introduced new qualifications limiting its potential significance. On the one hand, this reinforced our sense of the shortcomings of our working concepts; on the other, it added force to the need we felt to overhaul them.

In one sense, of course, the process of conceptual re-examination was already underway. We had learned in the housing studies that we were dealing not only with variation in contact but also with variation in the contact situation. Some aspects of the contact situation, such as degree of proximity of the participants, influenced the type of contact which occurred. Other aspects, such as the perceived norm among one's own group toward relations with the other group, appeared to influence attitudes directly in addition to influencing the occurrence of contact. A first task in the overhaul, it seemed clear, was to enumerate the concepts descriptive of potentially significant variation in the contact situation itself.

Seven such concepts seemed to be necessary. I have already mentioned two of the seven: first, the degree of proximity between races provided by the contact situation and, second, the direction and strength of the norms of one's own group within the situation toward interracial association. A third I also mentioned earlier in relation to the possibility that tenants might interpret the kind of occupancy pattern they lived under as indicative of the way the Housing Authority and the project management felt interracial relations should be; generalized, this variable is the direction and strength of the expectations regarding interracial association believed to characterize authority figures in the situation. The informants in each of our experience surveys stressed this factor; they gave many illustrations of the influence exerted by the standards set by leadership figures.

A fourth concept is one which derives from the earlier idea of equal-status contact. Both Robin Williams and Bernard Kramer (6) had distinguished between two versions of this idea: One of these is equality or inequality of socio-economic and educational status; this clearly refers to attributes of the specific individuals in contact. A second is equality or inequality of the functional relationships among
people. We now realized that this second meaning of equality of status placed it among the features of the contact situation. For example, regardless of their individual characteristics, the white and Negro tenants in our studies had equality of position within the housing community. Accordingly we called this variable, relative status within the situation.

The fifth concept is related to the fact, already noted by Robin Williams, that participants in the contact are sometimes competing and sometimes cooperating. This too, it now seemed clear, was a function of contact situations or, more concretely, of the extent to which the activities characteristic of particular situations require or encourage cooperative behavior. Examples of those that require extensive cooperation are meetings of planning groups, and contests involving team play. Other situations call for, or at any rate permit, independent work by individuals. Still others make conflict and competition almost inevitable. We refer to this way of categorizing contact situations as their interdependence requirements.

The remaining two concepts were introduced to take account of one of the most frequently reported findings with regard to intergroup contact—namely, the more intimate or neighborly the association, the more favorable the attitude. We examined contact situations for differences which might lead to variation in level of intimacy of associations. In addition to the factors of proximity and interdependence requirements which might have this effect, two other variables seemed particularly relevant. We called the first of these, the acquaintance potential of the situation and the second, its implications for social acceptance.

Acquaintance potential refers to the opportunity provided by the situation for the participants to get to know and understand one another. One may encounter another person every day for months in the reading lounge of a Manhattan men's club with no more than a whispered comment about the weather. Contacts of the same proximity and frequency in a U.S.O. club invite a wide range of communication.

Two situations which are equivalent in acquaintance potential may, however, differ considerably in their implications for social acceptance. By this variable we mean the extent to which participation in a given situation with another individual implies that one is willing to accept him as a social equal, and, at least potentially, as a friend. An indication of the degree to which an activity is defined as implying social acceptance is provided by the strength of the tendency in the culture to restrict one's participation in the activity to situations in which the other participants are of equal or superior social status. To use an example: eating together implies greater social acceptance than working together; the strength of the tendency in our culture to
restrict participation in the former activity is greater than it is in the latter.

Having made the distinction between intergroup contact and the intergroup contact situation and having identified dimensions along which contact situations vary, we turned our attention to the individuals participating in the contact. Taking first the minority group participants, that is, the individuals representing the group which is the object of the attitudes under study, we had noted in our experience surveys and field studies a number of characteristics on which variation occurred. Among these were socio-economic and educational level, interests, preferences and tastes, and pleasantness of personality characteristics. We concluded tentatively that their most important aspects could be subsumed under two concepts: first, extent of dissimilarity to commonly-held stereotypes about the minority group in question, and second, extent of similarity to majority group representative in the situation in terms of background, education and interests.

Somewhat different variables seemed appropriate for describing the majority group representatives, basically because we viewed them as our research subjects. The fact that we were interested in attitude change had already called our attention to the variable of level of initial attitude. In addition, interest in character structure as related to prejudice had been stimulated by the Authoritarian Personality studies (1). Paul Mussen had reported that boys with differing personality patterns reacted differently to an interracial camping experience (8). Perhaps most fundamental was the implied question we read into this line of work—namely, are there individuals for whom hostile attitudes serve such basic needs that no type of intergroup contact will affect them?

We thus ended our first effort to overhaul our conceptual framework with a question considerably more complex than that with which we had started. From the query, “Does intergroup contact reduce prejudice?” we had moved to a question which ran: “In what types of contact situations, with what kinds of representatives of the disliked group, will interaction and attitude change of specified types occur—and how will this vary for subjects of differing characteristics?”

It was at this point that we were given the opportunity to extend our study of intergroup contact into the context of relations between foreign students and their American hosts (12). Such students were known to vary both in the frequency of their contacts with Americans and in their beliefs and feelings about them and about aspects of American life. No one knew whether the contacts influenced the attitudes, although it was rather widely assumed that such might be the case.

In line with our focus on unintended contact we became interested in the possibility of finding groups of students who were living
and studying under circumstances which differed in such a way as to lead to differences in interaction with Americans. If such differences in interaction were predicted in advance on the basis of environmental differences and if, later, differences in attitude—or even better, attitude change—were found to parallel the interaction differences, some confidence might be placed in the causal link between the two. In the absence of such a demonstration that difference in interaction followed from differing environmental influences, we could not infer that interaction led to attitude change; it may instead have resulted from it.

We started with the assumption that foreign students studying in small colleges located in small towns would encounter conditions which differed from those in large universities in metropolitan areas in ways we were now confident would encourage interaction. In the small college setting we anticipated that the students should more frequently find themselves in living quarters and in other situations where proximity to Americans would be high, proximity to other foreign students would be low, and where local norms or traditions of hospitality would support interaction. Just to live dangerously, we made the additional prediction that the large university with many foreign students, but located in a small town, would represent an intermediate case. We had, of course, the problem of locating comparable groups of students in these three settings, but, except to say that, with minor exceptions, we were successful in this, I do not have time to describe the study design in greater detail.

What I wish to do instead is to indicate the influence which this study had in refining further an aspect of our conceptual thinking. As I noted, we were confidently predicting that our three groups of students would differ in amount of interaction with Americans. On the other hand, we could say little or nothing about what differences, if any, there might be in the nature of the interaction; our concepts were as yet inadequate for this purpose. We decided to approach the task of differentiating dimensions on which instances of interaction might be usefully distinguished from each other by first reviewing the ways in which interaction might be thought to influence attitude. In the case of foreign students there seemed to be two main possibilities. The first was that the interaction would increase the students’ opportunities to acquire either more information or a different kind of information about Americans. This involves the familiar assumption that new evidence of the right sort or acquired under the right conditions will modify old beliefs. The second possibility was that interaction would produce a pleasant emotional experience, including a friendly affect toward the American participants, and that, under some conditions, this would be generalized to feelings about Americans as a group.

The kind of variation in interaction which might increase oppor-
tunities for information about the other group had received some attention earlier. Several studies, including our own, had classified contacts in terms either of frequency or intimacy. We continued to utilize both ideas in somewhat modified form. Frequency of contact we decided to estimate in two ways: One, a report of the frequency of participation in various activities with members of the other group—in this case, Americans; the other an estimate of the proportion of free time spent with members of the other group. Intimacy of contact had been used quite loosely to connote either feelings or activities and to refer to a broad range of variation from brief and superficial to extended and non-superficial; we redefined it as a more limited dimension which ran from interaction of an impersonal nature to interaction revealing personal, unique or private features of the participants. To frequency and intimacy we added variety. We quantified this in terms both of the different types of association engaged in and the different types of Americans with whom associations had occurred.

Our second possibility—namely, that some aspects of interaction might be associated with pleasant emotional experiences, gave us more difficulty. Our decision was to classify instances of contact, first, on a pleasantness dimension, running from warm and friendly thru neutral to cold and hostile. In addition we decided to arrange them along a second dimension which reflected the degree of cooperation involved since it had often been suggested that degree of cooperation was paralleled by a coordinate amount of pleasant affect.

As to the results of the study, again I have time for only the briefest sketch. Our first-line predictions were confirmed quite strongly. Foreign students in the small college, small community settings by contrast with their matched counterparts in the intermediate and larger college-community settings had more interaction with Americans as well as interaction of a more intimate or personalized nature. We were able to show that the mediating condition for this outcome was a combination of degree of proximity to Americans and the social expectations regarding interaction which characterized the settings in which the proximity occurred. When we came to our second-line predictions—namely, that degree or type of interaction would influence attitude change, the picture is far more complex. To the extent that a brief summary is possible here, it would be that the influence on attitude seems slight and that it is limited to beliefs and feelings about the more personal aspects of American social customs.

Before giving you the last step in my illustration, let me once more digress briefly to comment on research methods. We have now discussed a number of studies carried out in interracial or cross-cultural settings. In form or structure these are controlled field studies.
Such studies have two functions in the strategy I am describing. The first is that of verification. Information from experienced informants, or other sources of preliminary observations, will have suggested that the dependent variables—in our case, attitudes—are strongly influenced by certain environmental conditions. The field studies will either support or discount the suggested relationships—or, more likely, will clarify and qualify them.

The second function is to aid in the refinement of a conceptual framework. You will recall my discussion of the two concepts, proximity and norms regarding interracial associations; this is an example of the differentiation of environmental variables in the course of a controlled field study.

There is not time to deal further with our conceptual system even though, as yet, I have said nothing about concepts descriptive of relevant needs and motives, nor even of attitudes themselves. I would like, however, to say a few words about the fact that as the process of defining the variables goes forward we are also led to inferences about psychological processes through which attitude change may occur. I have already touched on three of these: first, the modification of beliefs by new information about the object of belief; second, the generalization of pleasant feeling arising from experiences with individuals to the group to which they belong; third, the internalization of the perceived norms of a social situation in which an individual finds himself. A fourth process was suggested by findings from two of our field studies. One of these studies was of a work situation in which some of the white workers were in departments in which the Negro employees had jobs of equal status, some where the Negroes had jobs of inferior status, and some where no Negroes were employed. On our attitude measures these three groups differed clearly in only one major respect. Workers in departments where Negroes had jobs of equal status most frequently expressed a willingness to accept another position where Negroes would be doing the same type of work as they. We added to this finding one from the second of our housing studies—namely, that white tenants in the building-segregated projects recommended as social policy the particular form of partial segregation under which they had lived. These findings implied that some part of attitude change, possibly that occurring in early stages of the total process, may best be viewed as a compartmentalized accommodation to or acceptance of the particular intergroup situations and activities in which the subject has participated.

With these hypothetical processes, as well as several others, before us we are seeking further clarification in the laboratory. We feel enough at home with many of the concepts in our analytical system to be confident that we can manipulate them under laboratory con-
ditions. Equally important, we feel reasonably assured that we know which variables need to be controlled and at what level.

We have been experimenting to date with a standard laboratory setting in which the subject is employed to work for the experimenter. Unknown to him, his supervisor and co-workers in this activity are confederates of the experimenter. One or more of these confederates is Negro. The relative status within the situation of the subject and the Negro confederate can be varied by the quality of job assignments made to the two individuals. Social norms approving or disapproving of interracial equality and association are created by scheduled remarks exchanged among the confederates in the subject's presence. The impersonal-to-personal dimension of the conversational interaction between the subject and the Negro confederate is controlled by governing the content of the latter's conversation. By modifying the task requirements the interracial association can be made more pleasant and gratifying; this is accomplished by insuring the success of cooperative activity between the subject and the Negro confederate and by rewarding it financially.

Since the characteristics of subjects must be known in advance of their selection and employment, a plan has been developed which makes possible pre-experimental measurement. Potential subjects are employed as test takers in a test development project, conducted by a person never identified in any way with the job activity of the experiment proper. Tests administered at this time allow selection of subjects in terms of specified levels of initial attitude, strength of need for social approval, and scores on personality variables. Several weeks following termination of the experiment the subject is recalled by the test development project for the final attitude measurement, under the pretext of examining the reliability of the tests.

Regrettably, my illustration must end at this point. While we have made satisfactory progress with the early stages of our experimentation we have as yet no results worth reporting.

I feel sure that the approach I have tried to illustrate with our own research will be clearer if before I close I sketch briefly for you how it might apply to another problem.

Many commentators have noted a disturbing and potentially dangerous lack of participation by individual citizens in civic activities and public affairs. They point to the lassitude of the citizenry with regard to community issues and to the growing importance in the American scene of relatively small but well organized pressure groups which take advantage of the general apathy in pursuing their own special interests.

Stated in these broad terms, the problem is not amenable to the approach I have been describing. The range of relevant variables is too great—one would need to consider such different matters as
the possible effects of urbanization, the mass media of communication, the nature of the educational process. The approach I am advocating requires the selection of a relatively limited pattern of social relationships that recurs in a variety of settings. Only by focusing on such a limited pattern can one hope—at least at the present stage of our science—to arrive at a conceptual framework in which the number of important variables is small enough to be manageable. Just as we selected situations of intergroup contact as providing a delimited pattern of recurring relationships within the broader field of intergroup relations, so would one need to select some delimited aspect of the broad problem of participation and apathy with regard to civic affairs.

Suppose the investigator decides to focus on participation in voluntary civic groups. Let us say, further, that he decides to limit his problem to influences determining the extent of active participation by people who are already nominally members of such groups. Within these boundaries he can hope to identify a manageable number of variables to which he can assign values in any setting he studies. However, the number will be large enough for their combined effect to provide a basis for understanding and predicting variation in participation in different situations.

Obviously, the question of participation in groups is by no means an unexplored one. Psychologists have studied many aspects of leader behavior, group and task structure, and group process. Sociologists have examined the characteristics of people who participate and the effects on participation of large-scale organizational structure. A first step for the investigator who wishes to develop a conceptual framework for studying member participation in civic groups, then, would be to survey the considerable body of relevant literature and abstract from it ideas about important variables and implications for underlying processes.

An experience survey would almost certainly be a fruitful next step. This is surely a field in which much practical wisdom has accumulated but has not been written down. Experienced and analytical informants could be sought out who would have much to tell about success and failure in maintaining responsible activity among members of community improvement groups, such as block organizations, citizens' committees for school, groups that watch-dog legislative sessions, such as the League of Women Voters, local mental health associations, etc. Being practitioners, their observations would probably be phrased in terms of activities, techniques, characteristics of individuals. They might report, for example, that it is easier to secure participation in a campaign to have a traffic light installed at a dangerous crossing than in activities having to do with the conservation of natural resources. They might tell you that members of local
chapters were more active when national organizational policy called for policy making by conventions of local delegates than when policy was set by a national directorate.

The social psychologist will be able to discern more general principles that seem to underlie such observations. Suppose he infers, for example, that what is involved in this case are members' perceptions of the probability of concrete consequences emerging from the organization's activities. Suppose further that he decides this is a variable of sufficient importance to warrant study. As a first step, he will probably seek two organizations drawing on similar populations and similar in their general nature—including the possibility that their activities might have similar consequences if other conditions were equal—but in which some aspect of the situation leads to the probability that the activities of one will, in fact, have more impact than those of the other. To locate such organizations he will draw first on the knowledge of civic groups and problems of working with them which he gleaned from his experience survey. What organizations these will be we can not, of course, predict. They might be two citizen's committees for schools, one working in a town where the Board of Education was receptive to suggestions; the other in a town where it was not. Or, they might be two local mental health organizations, one working on the development of a local clinic, the other on a national fund raising drive.

Beside selecting this presumably important variable in terms of which the situations he observes are expected to differ, the investigator will have outlined—on the basis of his review of the literature, the experience survey, and his general psychological background—other categories of variables that must be taken into account. He will, of course, want to select groups that are as similar as possible on these other variables, with the major difference between them being the extent to which their activities are likely to lead to tangible results. It is obviously unlikely that he will find groups that are matched in all these respects; however, his conceptual framework will at least have alerted him to the need for taking these other variables into account and estimating their values in each of his comparison groups.

Each such field study should lead to a further refinement and clarification of his conceptual framework and probably to explanatory hypotheses about the processes underlying the effects of certain variables. Eventually the investigator is likely to feel that his conceptualization has reached a point where it is desirable to manipulate independently variables that are typically entangled in clusters in real-life situations; then he will move to the laboratory—investigating the effects of variables derived not from some arm-chaired theoretical system but from a conceptual framework in which the concepts have evolved through observation of their importance in everyday social
relations. The operations he uses to establish his experimental variables will correspond as closely as he can manage to typical manifestations of those same variables in everyday life; as a result, there is a good chance that his conclusions will help to illuminate not only the behavior of the subjects in his laboratory but the behavior of people in life situations—which I take to be the goal of social science.

To conclude my case for the systematic analysis of socially significant events as a strategy for social research, let me summarize the advantages I have claimed for it. The selection as a research focus of a social event which recurs in many forms makes it possible to work with a limited number of concepts descriptive of the complex of determining influences which control social behavior and attitude change, a complex composed in part of environmental conditions, in part of personal characteristics, and in part of social interaction. Variables isolated in the study of such events will carry weight in influencing behavior, and the explanatory processes conceptualized in dealing with such variables will be of corresponding significance.

In the course of the development of the conceptual framework the point will be reached where experimental manipulation of some of the variables will be possible under laboratory conditions. Since this will make possible a disentangling of variables which normally change together, the laboratory work will contribute to the understanding of explanatory processes and causal dynamics. It will then be possible to incorporate this understanding into the further study of the social events which were the original point of departure.

As the conceptual framework is refined and explanatory processes are formulated, the system will approach the level of abstraction and comprehensiveness that we ask of psychological theory. Cross-connections to explanatory principles developed elsewhere will then emerge, to the mutual advantage of both the general and the more limited theoretical enterprises.

And now may I close with two brief observations. First, I have advocated a strategy for social research which is rarely followed. A natural inference from what I have said would be that I am opposed to research which does not fit the pattern I have suggested. This is not the case. Rather than to reduce the amount of such research, what I desire, as I have said earlier, is to lend support to an alternative research strategy—one in which admittedly I have more confidence because I think it more likely to help us understand the natural events in our social environment.

And, finally, although I have justified my recommendations primarily in terms of the need to see more clearly how research on complex social phenomena could contribute to psychological theory, I continue to feel that such research is justified also by the obligation to contribute to the constructive resolution of significant social issues.
On other occasions I have argued the case that research in psychology can be both socially useful and scientifically meaningful (10). My experience to date, although far from conclusive, has strengthened my faith in that position.

REFERENCES