Whatever Happened to Action Research?

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The Wright Institute

First, a word about my title: when SPSSI's program chairman really turned on the pressure to get a title from me, I had been talking with a friend in operations research who had been doing a cost effectiveness study of one of our great universities. He and his colleagues had been seeking new funding through the Committee on Basic Research of the National Science Foundation and the U.S. Office of Education. These researchers had become quite enthusiastic about their work, believing as they did that they could demonstrate its usefulness to the university. Now my friend had his answer from the committee: they were wildly enthusiastic about the proposal but, of course, could not fund it because it was not "basic."

I wondered how far this rigid separation of research and action had gone. It was clearly apparent that the term "action research" is not exactly on everybody's tongue nowadays. You may note that it appears only once in the index of the program for this convention: where it says "Action research, whatever happened to." Or consider last year's convention, the theme of which was Psychology and the Problems of Society. In the table of contents of the book under that title the only mention of action research appeared in the paper by Dr. Arthur Naftalin, former mayor of Minneapolis, who I suppose had had every reason to be problem-oriented.

1The developmental scheme for college teachers discussed herein is mainly the work of Robert Shukraft.
The contrast with the 1940s and early 1950s seemed to me striking. Particularly during the late 1940s there was an outpouring of reports of action research from the Research Center for Group Dynamics and the Commission on Community Interrelations. One, such as myself, who during that hopeful time was taken up with promoting psychoanalysis and personality research, could and did easily form the impression that action research was most certainly a dominant trend in social psychology.

Yet in 1957 when I spoke before this society about social science and social reform (Sanford, 1965), basing my presentation on the work of my colleagues and myself at Vassar College (Sanford, 1956), Brewster Smith said with surprise and, as I thought, with some pleasure: "So you are really following the old Lewin model of action research." I thought, of course, that mine was a new and improved model; but I knew it could not have existed without the work of Lewin, and was surprised to hear the latter spoken of as if it belonged to the past.

**Lewin's Model**

What was Lewin's model? I think it was well set forth in his 1946 paper on "Group Decision and Social Change" (Lewin, 1947) written for the first edition of *Readings in Social Psychology* (Newcomb & Hartley, 1947). The example he used, you will recall, had to do with changing food habits. Action research consisted in analysis, fact-finding, conceptualization, planning, execution, more fact-finding or evaluation; and then a repetition of this whole circle of activities; indeed, a spiral of such circles.

Leon Festinger told Alfred Marrow that Lewin's greatest contribution "on the abstract level may have been the idea of studying things through changing them and seeing the effect. This theme—that in order to gain insight into a process one must create a change and then observe its variable effects and new dynamics—runs through all of Lewin's work [Marrow, 1969, p. 235]." This seems very close to common sense. It is the way to solve any practical problem or to learn any skill. Yet for Lewin this kind of involvement with practical problems was a never-failing source of theoretical ideas and knowledge of fundamental social psychological relationships.

My reason for asking what has been the fate of this contribution of Lewin's is not just a fascination with history or a desire to preserve intact the work of an admired figure; I am interested in working out a model, for the integration of action and research, that is adequate for today. It may help ensure a better future for such a model if we know something of the vicissitudes of the old one.
Action Research Today

Action research is still very much alive. It has strong advocates in high places. Martin Deutsch, in his presidential address before this society last year, said that the need for knowledge of the effects of experiences upon development, as a basis for changes in policies and organizations, "clearly points to an emphasis on action programs and action research as fundamental tools of the social scientist [Deutsch, 1969, pp. 14-15]."

Programs clearly labelled action research, or which could properly be so labelled, are not hard to find. There are, for example, four major ones within walking distance of the Wright Institute in Berkeley. There is Soskin and Korchin's "after-school school," a program for offering people of high-school age what (on theoretical grounds) they seem to need for their development but do not get in school (Soskin & Korchin, 1967); there is Peter Lenrow's "collaborative problem-solving" with teachers and administrators of the Berkeley School System, an action research program with some distinctive new features (Lenrow, 1970); there is the "new careers program" of Joan and Douglas Grant which demonstrated that prison inmates can be an important source of manpower in the human-service fields (Grant & Grant, 1970); and there is the new program of Wilbur Hoff and his associates for training poor people for jobs—to be defined and instituted—in hospitals and clinics (Hoff, 1970). I should mention here too the Wright Institute's new doctoral program in social-clinical psychology (Sanford, 1970b). This program was based on some knowledge of the situation and needs of graduate students and has been guided by theory of individual development and organizational processes. If this program of action research is to flourish and at the same time provide useful general knowledge about organizational innovation and development, there must be more than a few circles of analysis, execution, and fact-finding.

My planned systematic survey of action research programs has not yet taken me beyond the Berkeley city limits, but, even so, I receive reports from time to time of highly significant programs in less favored parts of the country. The latest is Robert Sinnett and Angela Sachson's impeccable evaluation of their project for demonstrating that severely emotionally disturbed students can be provided satisfactory care in a rehabilitation living unit in a regular university dormitory (Sinnett & Sachson, 1970).

The survey of which I speak should pay particular attention to publications in the fields of public health, social welfare, and criminology. It is my impression that more action research on the
Lewinian model is done by specialists in these fields than by social psychologists. A great deal of work that is in the spirit of action research, or that is in some important respects like action research, today goes under the heading of "community psychology," as shown, for example, in Adelson and Kalis' (1970) systematic treatment of this subject. It goes on also under the heading of "evaluation research." The approaches to this subject of Donald Campbell (1969), Samuel Messick (1970), and Michael Scriven (1967) carry to a high level of sophistication the essential fact-finding feature of Lewin's model. Scriven, indeed, in distinguishing between "formative evaluation," which serves program improvement, and "summative evaluation"—appraisal of the final product—goes a long way toward filling in Lewin's circle with precise operations. Although these three authors assume a division of labor between the evaluators and those responsible for execution, their work shows beyond question that much can be learned from studying the effects of actions.

**Trends toward Separating Action from Research**

The fact remains, however, that none of this work—true action research, community psychology, evaluation research—is in the main streams of social psychology or social science generally. Although it is true that the great bulk of federal funds for social science goes to applied social science, as academic social scientists never tire of pointing out (e.g., Beals, 1970), how many of these projects could qualify as action research? The emphasis is most certainly not on the study of actions as a means for advancing science but rather on the application to problems of what is already known.

**Voices of the Establishment**

This separation of science and practice is strongly advocated by leaders in academic social science. Thus George Miller in his presidential address before the American Psychological Association said last year:

Many psychologists, trained in an empiricist, experimental tradition have tried to serve two masters at once. That is to say they have tried to solve practical problems and simultaneously to collect data of scientific value on the effects of their interventions. Other fields maintain a more equitable division of labor between scientist and engineer. Scientists are responsible for the validity of the principles; engineers accept them and try to use them to solve practical problems [Miller, 1969].

Similarly, George Albee, in his presidential address before the same association this year, according to excerpts in *Psychology*
Today (Albee, 1970), insisted on the fundamental difference between research and practice and strongly implied that never the twain shall meet. Possibly Miller and Albee do not speak for the majority of psychologists, but they certainly speak for what might be called the psychology establishment.

The psychology establishment has also spoken recently through the psychology panel of the Survey of the Behavioral and Social Sciences under the auspices of the Committee on Science and Public Policy of the National Academy of Sciences and the Problems and Policy Committee of the Social Science Research Council.

Among the thirteen recommendations of this panel is the one that "psychologists increase their research on problems related to social action programs and field experiments," and, happily, they suggest that this "may provide insights into principles of human behavior that are difficult to study in any other way [Clark & Miller, 1970, p. 133]." The panel thus legitimizes action research, and perhaps we should be grateful for small favors, but it also says that psychologists in general should have more money to go on doing just about what they are doing now. The tone of the panel's report is one of complacency about the achievements of psychology, and there is nothing in it to suggest that the present allocation of resources is about to be radically altered.

The broader social science establishment has also spoken recently through the report of another blue ribbon commission: the Special Commission on the Social Sciences of the National Science Board (Special Commission, 1969). This commission shows considerable sensitivity to the present identity crisis of social science and awareness of the need for fresh approaches to our social problems. I think Kurt Lewin would have applauded, as do I, the recommendation of federal funding for multi-disciplinary, problem-oriented institutes. But, like the other authorities just cited, this commission seems to be stuck with the science-engineering model, in which discoveries are first made (in the lab as it were) and then "applied."

What has happened to action research? I would say now that, contrary to the impression I had in the late 1940s, that it never really got off the ground, it never was widely influential in psychology or social science. By the time the federal funding agencies were set up after World War II, action research was already condemned to a sort of orphan's role in social science—for the separation of science and practice was now institutionalized, and it has been basic to the federal bureaucracies ever since. This truth was obscured for a time by the fact that old-timers in action
research were still able to get their projects funded; this after younger researchers had discovered to their sorrow that action research proposals *per se* received a cool reception from the funding agencies and were, indeed, likely to win for their authors the reputation of being "confused."

We must, I think, face the fact that the old-timers in action research have not been reproducing themselves—at least not at the usual rate for psychologists. When jobs for action research open up, as sometimes happens, trained people are nowhere to be found. Lipton and Klein, for example, report (concerning their Boston University program for training psychologists for practice and research in problems of change in the community) that a big problem was the absence of adequate role models. They write, "There are few psychologists available to undertake the direct supervision of the student seeking a community-psychology experience. Apparently few psychologists have made their way into fields of application like group relations and community relations [Lipton & Klein, 1970, p. 288]."

In attempting to summarize what I suspect is a familiar story, I would say that we have separated—and then institutionalized the separation of—everything that from the point of view of action research (everything, I would say, that in the sight of God) belongs together.

**Negative Consequences of the Separation**

Analysis of the problem, conceptualization, data gathering, planning, execution, evaluation, training—the intimate family of activities that constituted Lewin’s model has been pretty well dispersed.

The categorical separation of research from practice has had some serious negative consequences for social science. For one thing, it has cut the academic researcher off from lines of investigation that are necessary to the development of his science, for example, the study of phenomena that cannot be experimented upon in the laboratory or the study of social structures that can be understood only through attempts to change them. Again, it has laid the social sciences wide open to the charge of irrelevance, not only by students but by men of affairs. It would hardly occur to a college president to look to the social science literature for help with his problems; and, as Blum and Funkhauser (1965) have shown, social scientists are among the last people state legislators would consult about the problem of drug-abuse.

Much of the trouble comes from the fact that the separation of science from practice raised for academic men—who have a deep interest in the matter—the question of which has the greater
status; and, despite the fact that social scientific work on practical problems poses the greater intellectual challenge, the decision went to "pure" science. A result has been that brave new efforts by federal agencies to do something about the problems of society soon become bogged down. For example, when new bureaus or "study sections" on applied social science are set up, their personnel seek to avoid appearing second-class by being just as hard-nosed or "scientific" as their heroes in the academic social sciences—and end by being just as narrow. Or when centers for research and development are set up within universities, academic social scientists, who are needed to lend prestige to the enterprise, seize the opportunity to get funding for what they were doing already, and things go on much as before. This same tendency is displayed by the newer professional schools of universities—education, social welfare, criminology, public health—which become distracted from their purposes by putting in PhD programs which ape, but do not match, those of the older disciplines.

**Sterility of Segregated Experts**

Once science is split off from practice, further splits develop. We have on the one side experts in conceptualization, theoretical model-building, research design, experimentation and on the other side experts in planning, in execution, and in evaluation, each class of whom has more and more difficulty in talking with the others. Training follows the general trend of events in science: separated bureaucratically from research and from action at the funding level, it is mainly in the hands of the segregated experts. Sensitivity training, for example, a proceeding of enormous potential and with some solid achievements to its credit, has been lifted out of its natural context of social structure and social practice and rendered free-floating, apparently on the assumption that its recipients are also free-floating.

Small wonder that social science is having an identity crisis, that social scientists generally seem to be well-heeled but unhappy. We do not want to be "mere technicians" who carry out other people's purposes, as Sherif (1968) has so eloquently said; yet, we have so detached ourselves from practical affairs that we do not know enough to make decisions in important matters ourselves. There should be a large place for idle curiosity in a civilized community, yet, if we are not doing action research, we find it hard to indulge our curiosity at a time when our basic institutions seem about to fall to pieces. As liberal intellectuals we would like to make social science available to people who need it the most, that is, the poor and the oppressed, but for this the science-
engineering model—as I will show in a moment—is hopelessly inadequate. How did we get into this fix? The basic trouble is the fragmentation I have described and this in turn can be understood as an aspect of the general tendency toward specialization in modern science and scholarship. Effective social problem solving calls for multidisciplinary work, yet departmentalism seems everywhere on the increase. The psychologists' panel mentioned earlier recommended consideration by universities of the idea of a Graduate School of Applied Behavioral Science—but it was careful not to threaten the interests of existing departments and schools; it was out to show that psychology deserves more support, apparently on the assumption that what is good for psychology is good for the nation.

There is, of course, specialization within as well as among the disciplines, in consequence of which we have a fantastic proliferation of bitsy and disconnected and essentially unusable researches. Specialization is a natural accompaniment of high levels of development in science, and it has obviously led to some intellectual and practical pay-off in the past, but the compartmentalization of social scientific activities and the scarcity of efforts to pull things together calls for explanation. Like professional practice, social science has been adapting itself to the requirements of an advanced technological society—which demands more and more segregation of functions and the training of experts to perform them.

Downgrading the Person

Just as professionals in health, education, and welfare no longer treat or deal with the whole person but only with particular symptoms or functions, so psychologists in their research and theory-making focus more and more on part-functions without bothering to connect them with central structures in the person. Indeed, the very concept of the person, downgraded and ignored, seems about to disappear from the literature.

I have elsewhere, and more than once, pointed to what seem to me to be the serious consequences of this (Sanford, 1968, 1970a). Not only have we contributed to the dehumanization of our research subjects by reducing them to "respondents" for the sake of enterprises that never yield any benefit to them, and to the dehumanization of ourselves by encouraging self-definition in terms of narrow specialties; we have also been disseminating a most unfortunate image of man. What is for the social scientist now researchable, an aggregate of meaningless "behaviors," becomes for great masses of our people a conception of the self as
fragmented and externalized: one is what one can present to others in a particular situation.

It is understandable that social science, like most other human activities in our society, should accommodate itself to inexorable social and historical processes. But more than other disciplines and professions it must bear responsibility for what has happened, for it has had the knowledge and the values to permit it to see what was happening. In any case it is certainly up to the social scientists, particularly those who work in universities, to oppose the current trend; they have the necessary knowledge, prestige, access to students, and the power, for they are for all practical purposes the social science establishment.

Some Action Research in Higher Education

What is needed is a contemporary model for action research or, as I prefer to say, research-action. I want in the remainder of this paper to make some suggestions toward the development of such a model. In order to do this it is first necessary to tell something of what my colleagues and I have been doing in recent years. I shall limit myself to work in higher education. (And I might say it is good to have this more or less captive audience. When I have lectured on this subject at numerous colleges and universities during the past 15 years, psychologists, including old friends, have stayed away in droves, presumably to avoid any appearance of being mixed up with an applied, thus lowly, field.)

Research Affects its Subjects

In my 1957 address (Sanford, 1965) I argued that research had direct consequences for its subjects. Mervin Freedman, Donald Brown, Richard Jung, John Bushnell, and I had studied Vassar women longitudinally, interviewing them several times a year for four years. We became convinced that for these students taking part in this research was one of the most important influences of their college years. It also seemed clear to us that reporting our research findings to the whole campus community had a significant favorable impact on the student culture. And so I began saying that the best thing to do for our college and universities was to study them.

At Stanford, beginning in 1961, Joseph Katz and his associates carried on in the same general direction that we had taken at Vassar (Katz et al., 1968). Now there was more feedback to individuals and to the community generally. Students began using these research findings in their discussions of education and campus life in the pages of the Daily, the campus newspaper. Mean-
while, of course, we were reporting findings and making various recommendations to management, that is, to administrators and teachers. I won't say these fell on deaf ears, but clearly this is not where the action was.

It became very clear, however, that the research was influencing the campus culture. Students could not read descriptions of, say, the rating-dating pattern at Stanford, talk about this in various formal and informal settings, and at the same time go on behaving in the same way as before. Particularly striking was the fact that some of the research subjects who were interviewed over the years became leaders of the student movement for educational reform, which at Stanford in the middle 1960s was very effective.

A Student Self-Survey

In the fall of 1967 I began teaching a class in higher education at the Graduate Theological Union in Berkeley. This class continued throughout the year; in the winter quarter the students and I conceived a plan to learn about higher education through the study of students by their fellow students. In the course of preparing a comprehensive interview schedule, students examined themselves and interviewed each other. Thus they learned what were the significant questions. We fully expected that students would report changes in themselves as a result of being interviewed, and that the program as a whole would have a significant impact on the school.

I reported on this work at the APA meetings in 1968 and last year in the American Psychologist (Sanford, 1969). Fifty students, approximately one-half the student body, were interviewed, yielding a large amount of quantifiable data on life and growth in a graduate school; but what interested us most was evidence that individuals benefited from the experience. They gained new awareness of themselves, their situation, and their goals. As a group they acquired a lively sense of community, of shared purposes, dissatisfactions, and ideas about what needed to be done. A process of change was set in motion. The student body as a whole proceeded to organize itself and to institute a new system for governing the school.

Through this experience, I was made aware of how unsatisfactory and dehumanizing it is to study students without a "by-their-leave" and with no feedback, on the assumption that the results might some day be used by kindly educators in the interests of students. I predicted student protests against such studies. And I argued that the way to study students was to help students study themselves. This, it seemed, would lower the barrier between researcher and subject and create a situation in which all
hands learned and developed. I reported in that 1969 paper that my theological students were ready to start next on the faculty. It was clear to me then why Vassar College had not adopted the fine recommendations made on the basis of our research. It was not, as I was fond of saying, that one cannot be a prophet in his own village; rather, it was because we had not studied the faculty. Interestingly enough, it appeared in retrospect that they really wanted to be studied but were not disposed to make that clear at the time.

Interviewing the Faculty

So, for the past 18 months some of these same theological students, joined by some others, have been interviewing faculty members at three institutions—and the total sample is now over 120. In two of the institutions, faculty members are approached as individuals rather than as members of departments or schools. In the third institution, a private liberal arts college, the researchers have an understanding with the faculty as a group. We defined ourselves as their allies. The knowledge derived from the interviewing is to be in their interest. They are to be the first to get reports of findings, and these reports will be made to groups of faculty and presented in a way designed to encourage discussion—a procedure that will at the same time yield more information. Work at the third institution was begun only in the late spring of this year and will continue throughout the coming year. Needless to say, we expect some change to occur in the institution, and we expect to be heavily involved as consultants to those we interviewed.

A Model for Research-Action

Implicit in this program is a model of action research. I believe it contains the main features of Lewin’s model—that is, analysis, fact findings, planning, execution, and evaluation. There is, however, more emphasis here on research as the action, and less emphasis on the effort to solve particular problems or induce particular changes in behavior. The aim, rather, is to promote liberation and growth and these are considered to be favored by the processes of the research and by the model the researchers present as they carry out their work.

Let us consider some of the main features of the model in turn.

Analysis. Analysis determines what kinds of questions are to be asked. These should be practical, although somewhat general and open-ended. In the present case the questions were how to
improve teaching, how to change features of academic culture, how to promote the development of individual faculty members. Most social science questions, in my view, should be of this general kind: how to arrange the environment, institution, or the social setting in such a way as to promote the development of all the individuals concerned.

Analysis with attention to the nature of the basic questions can save the researcher a great deal of trouble. Four or five years ago we sent to a government agency a proposal of a longitudinal study of graduate students. We were kept dangling for three years and were never funded. During this waiting period we realized that we were genuinely concerned about graduate students and that we did not need to study them in order to know that many were very unhappy or were not getting what they needed for their development. So instead of undertaking a conventional program of research, we started the new Wright Institute program of graduate training in social-clinical psychology. Now we are really finding out what needs to be done and something about how to do it. Had we been funded we would probably still be bogged down in the analysis of data in which—such is the march of events—we would probably have lost interest.

Fact finding. The aim of promoting individual development has several important implications.

(a) It is necessary to have a conceptualization of the person and theory concerning how he actually develops. Here, it seems to me, we must go far beyond the highly abstract formulations of Kurt Lewin, filling in a scheme such as his, with particular kinds of needs, dispositions, values, conflicts, and so forth.

(b) An approach that is concerned with individual development must be comprehensive and field theoretical. An interest in changing one aspect of the person or of his behavior must take into account the implications of such change for the total person. And to understand the person we must see him in his total setting. This means that research-action is properly multidisciplinary—and in my view it is only a focus on problems, ultimately problems of human development or human welfare, that will bring about collaboration among the academic disciplines.

(c) The concern with human development, of which I speak, is a concern with development now rather than in the future. It means that we must behave toward our subjects in a way that is favorable to development. For example, favorable to their autonomy, privacy, and self-esteem. We go contrary to this value if we use our subjects as means to some other end, including the end of their own well-being in the remote future. Research must, in other words, serve the purposes of its subjects. This does not mean that the researcher must sacrifice his own values. It does mean that he will probably have to say what they are and work out as much agreement with his subjects as possible.

The subject is the client, and reporting to him is an action.
Lewin considered that his action research would favor the purposes of "social management as well as the self management of groups." I think it was easier during World War II, and in the years shortly thereafter, than it is now to assume that "management" would use social science knowledge in the interest of all concerned. For surely there was much more agreement about large national purposes then than there is now. I am not suggesting that we ought to deprive management (that is to say, the administration or educational policy-makers, generally) of knowledge about the functioning of faculty members. Indeed, we intend to publish the findings of our research on faculties and I am going to report some here shortly. But such findings have already been reported to our interviewees.

Planning. Planning, or "planning ahead," does not have such an important role in the present model as in the action research of the past. There is no point in planning for people who will upset all such plans as soon as they find out that they have not been permitted to take part in the planning. To arrange things in such a way that the "natives" (students, faculty, poor people, ethnic minorities, and so on) can and do take part in the planning readily becomes a goal that supersedes the model of planning from above by experts. In the case of the Wright Institute's graduate school it would have been natural perhaps to have devoted a year to planning, preferably with the aid of a "planning grant," before beginning any operations. Instead we decided to do it now and plan it later. It seemed to us that our graduate students would have to take part in the planning in any case, and that the experience of helping to plan a graduate program would be of considerable educational value. This, however, puts heavy emphasis upon what Michael Scriven (1967) calls "formative evaluation." We institute a procedure, see how it works, and make a change if this seems necessary or wise, always in a spirit of continuous experimentation. I hope that after a decent interval there will be some "summative evaluation," but I also hope that this would be formative for the next program in graduate education.

Research-Action with Faculty

Now, in order to prepare the ground for a suggestion concerning an application of this model, I must report a few results from our study of faculty members. I can report that the interviewing went well and that it is a little hard to understand our reluctance to undertake such work when we were at Vassar. Graduate students are first-rate interviewers of faculty members. Indeed, they probably provoke less defensiveness than would
seasoned investigators. Faculty members, perhaps more than most other people, like to talk about themselves, and, as we know, they rarely have a chance to discuss what most concerns them—as teachers and as careerists—with their colleagues. Too often what they say can be used against them. Kindly old deans have, of course, disappeared from the scene; and the faculty wife has probably long since become bored with the whole thing.

It is not true that college and university professors neglect their teaching. They work hard at it, care a great deal about how well they perform, and on the whole think they are pretty good. They are usually hard put to it, however, to offer any rationale for what they do or to say what is the basis of the evaluation of their work. Teaching for them is not a profession and accordingly they deny themselves the satisfactions that most professionals enjoy—such as seeing tangible effects of their efforts. The rewards of their teaching are not altogether lacking. But they are random and unsystematic when they could be regular and durable. This lack of professionalization as teachers is an aspect of what we call academic culture—which is to say, a set of shared beliefs and ways, of the sort that develops in any aggregation of people who live or work together and that strongly influences what (in this case) faculty members actually do.

Faculty Culture—The Need for Change

I will not undertake to describe the whole thing here, but only to indicate some features of the culture that might have to be changed if college teaching is to improve. For example, professors usually identify with their discipline or specialty rather than with the role of teacher. They respect norms concerning how much time one may properly spend with students or how much interest in students one may display. In most institutions the norms are pretty low; if one becomes a popular teacher, he courts the danger of being ostracized by colleagues. Similarly he must beware of "popularism" lest he give away too much of the mystery upon which support of his discipline depends. One should not in conversation with colleagues or other professionals go beyond the bounds of one's own specialty. If something outside of one's specialty comes up for discussion, he should always defer to other specialists, even though this puts an end to the conversation. Another rule holds that he should always exhibit a devotion to the highest standards in matters of appointments and promotions and admission of students; let somebody else suggest that a risk be taken in particularly interesting cases.

It all seems pretty grim—as indeed it is. One might be inclined to think that we academic men would be happy as kings
since we are fundamentally free to read and study and look into whatever we like, always have interesting colleagues to talk with, and are surrounded by eager students waiting to get the word. Instead we find in our institutions of higher learning widespread unhappiness and cynicism. Indeed the academic culture seems to decree that it ought not to be otherwise. Since the faculty member is devoted to such high purposes—the pursuit of which is constantly interfered with by people who do not understand—it would seem almost immoral to take any pleasure in what one does. It doesn’t help any to be stationed at a prestigious institution. According to James Bess (1970), satisfaction of most human needs is independent of kind or rating of institution. Or to paraphrase Groucho Marx’s “happiness won’t buy you any money,” happiness won’t get you a job at Berkeley or Stanford or Harvard.

Stages of Professional Development

It turns out that college professors develop as individuals in much the same way that other people do. Their development is progressive and is marked by distinctive stages, which are only loosely related to chronological age. A particularly important stage is the one we call the achievement of a sense of competence in one’s discipline or specialty. The way in which this developmental task is approached and accomplished depends, of course, on what has gone before in the individual life. It depends, for example, on whether the professor was, as a child, “isolated” or “social.” Perhaps we should not have been surprised to discover that the overwhelming majority of the professors in our samples were isolated children. Indeed, half of those we interviewed in one liberal arts college were only children. Whether prodigies or plodders, they learned early to enjoy being rewarded by adults for academic achievement, and they learn late, if at all, to participate in the rough and tumble of campus politics. Those mischievous and sometimes disobedient social children who were to become professors were relatively late in discovering their academic potentialities and, though they are likely to wind up in charge of the important committees, and likely to relate easily to students outside of classes, they have a hard time getting over the feeling that they may not be doing the right thing in the classroom.

Until he has achieved academic competence, the professor is not ready to pass on to the stage of self-discovery, in which he gives attention to other abilities, interests, and aspirations, and so expands his personality. Even when a professor is ready to change, however, he finds that he has made commitments and must defend what he has done, while also dealing with the expec-
tations of family and colleagues who, often at some pain, have
grown used to him as he is. Our experience is in line with the
finding of Lewin that it usually takes some "group decision" to
sustain a change.

Ideally, self-discovery is followed by discovery of others, much
as in Erikson's (1950) formulation of stages wherein identity is
followed by intimacy and generativity. Now the professor is pre-
pared to use all of his skills in genuine relationships with other
people; he may find it comfortable and enjoyable to take a father
role with some students—those who can stand it or will accept it.

My colleagues and I are convinced that academic culture and
patterns of individual growth can be influenced by the sort of
research-action I have been discussing. Like the graduate stu-
dents we studied, faculty members suffer from pluralistic ignor-
ance. They are often delighted to discover that others have been
thinking along the same lines as themselves and, when they get
together after the interviews, discussion moves to a different level
than before, toward the discovery of both self and others. They are
often delighted to find that some of their old aspirations are still
there and might yet be fulfilled. And some take pleasure in dis-
covering the legitimacy of taking an interest in students.

There is nothing to indicate that professors in psychology
or other social science departments are different, on the signif-
ificant dimensions, from professors in other departments. They are
first of all academic men—participants in academic culture. It is
not only as statesmen but as representatives and defenders of their
academic professionalism that they do their work in the higher
councils of organized psychology or social science. Would not
psychology and social science then benefit from our approach to
research-action with professors in these disciplines?

Changing the Social Science Establishment

Let us go back to a consideration of the nature of our prob-
lems as social scientists. Organized social science is an elite, an
establishment, but it is part of a larger system, the political and
economic establishment which it often verbally opposes but upon
which it depends for support. Like other industries, social science
has been polluting its environment. Not only has it been spoiling
its research subjects by treating them as means rather than ends;
not only has it been disseminating a rather monstrous image of re-
searchable man; it has been creating an enormous amount of
waste in the form of useless information. Much of what ought to
be left to decompose we now make great plans to retrieve. From
the developmental point of view it looks very much as if the sys-
tem were designed by and for professors still in the stage of achieving a sense of competence.

We have to make some changes. But how? We are confronted with the old problem: we can't change the system without changing the individuals in it, and we can't change the individuals without changing the system. Where is a start to be made? One possibility is to bring pressure to bear from outside—in the form of money, of course. The usual procedure is to build new structures on top of or alongside those we already have. The U.S. Congress may take an interest in this matter; and if by some chance they should ask me, I would say the main thing is to restore the independence of social science. We do not want to be "mere technicians" (Sherif, 1968), and we do not know enough to make major practical decisions, but we do have much to offer people who are interested in changing themselves singly or collectively. Social science needs to be free of national and state political and economic establishments so that it can work out mutually beneficial relationships with people who do not ordinarily benefit from its activities, and the individual social scientist needs to be free of the social science establishment so that he may contribute according to his own lights.

The best way to accomplish this would be for the government to give the money to students and professors. Students would then, in a free market, pass enough of it along to professors so that they, freed of the demands of grantsmanship, could with the help of students integrate teaching, research, and action in a single humanistic undertaking. But since students are not always the best judges of whom they might best study with, there ought to be a guaranteed annual minimum research fund for each professor.

The funding of project research ought to be abandoned. It has spoiled the academic community, damaged—almost beyond repair—undergraduate education, given status to trivia, created an expensive bureaucracy, and corrupted thousands of investigators. This change will have to come from above for the funding apparatus has no built-in mechanisms for self-correction. A friend of mine, an anti-establishmentarian from way back, who has said publicly many times that most of our published experimental work fails to meet minimum scientific standards, has recently been induced to serve on a NIMH study committee. She will inevitably help to perpetuate what she decries, but, as she says, "NIMH has done a lot for me."

I would not, however, put the control of social science research back into the universities if I did not have an idea as to how the academic culture might be changed. The action just rec-
ommended would, of course, be a force for change, but we could hardly rely on it exclusively. We should at the same time approach the problem from the other side, involving faculty members in the social science disciplines in the kind of research-action that has been described. There should be studies of departments, colleges, professional schools, universities, state psychological associations, the American Psychological Association.

The idea is not exactly new. Some years ago when the psychology department at Berkeley was caught in one of its perennial crises, the dean of Letters and Science appointed a committee to see what could be done. When a question about the role of institutes came up at one of the meetings of this committee, its chairman said, "What we need is an institute for the study of the psychology department!"

A Self-Survey of the Social Sciences

There has been much recent discussion of studying the APA. How might this be done? Shall a task force of the Central Office select a national sample to whom questionnaires are mailed, the data being made available to a committee who will use it to plan our lives? I would say not. This sort of thing might have been all right in past studies of students and other "natives," but surely for ourselves we can arrange something more humanistic. At least we can be clients at the same time that we are subjects.

So let a few professors and students from Hayward State study a department at San Francisco State, and a group from the latter institution study a department at Berkeley, while some professors and students from Berkeley carry out a research-action at Hayward. No master plan would be needed, for those who were studied would in each case take part in the planning, contributing questions they wanted asked of others and of themselves. All that would be needed to begin would be general agreement that all hands would be interviewed. A process of change started in this way could easily continue under its own steam. For the model that has been offered is not only designed to encourage professors to be subjects of action-research, but also to arouse interest in the carrying out of action-research. The particular example that I have offered—that is, of studying academic culture—is in my view the key to acceptance of action-research by professors who need to be rescued from one another.

I would predict that departments or schools studied in the way described would never be quite the same again. The potential for change in these structures was further revealed, it seems to me, by the Cambodian crisis, which in many institutions led to what was for many faculty and students an exhilarating change in inter-
personal relationships and role definitions. There is much doubt now as to whether such changes will persist. The model I am advocating could not only lead to the kind of opening up and the sense of community evoked by the Cambodian crisis, but since the research-action would shake up the old structures, with all concerned being fully conscious of what was happening, and would at the same time liberate the energies needed for the formation of new structures, we should not expect so much reversion to the old ways.

What happened in particular departments or schools could easily spread and start discussions of our identity crisis on a national scale. The widespread use of the present model would have good effects of a more general nature. By demonstrating our ability and our willingness to study ourselves we would go some way toward restoring trust in our competence to study others singly and collectively.

The study of a department or even of a profession may seem small and timid compared with consulting for national policy-making bodies or advocating policies before legislative assemblies, but how can social science be better on the national scene than it is in microcosm? If we are to build a better society, a good place to begin is with making our own department, institution, school, or association a truly human community. If it is our purpose to liberate women and other “minorities,” let us begin at home. If we are to promote human development generally, let us begin by showing that we know how to promote the development of our students.

Actually, I am not neglecting the impact of our work on the larger community. I agree with those writers such as Miller (1969) and Snoek (1969) who suggest that our influence on the body politic has been less through the dissemination of knowledge than through the presentation of an image of man. Of course, I take a dimmer view than do these writers of the sort of image we have been presenting. But I strongly agree that our major influence is through what we do and are. The good that we may do will derive less from the models we finally build in our own domains, than from the model we present in the building. And this, I hope, will be a model of man trying to understand and to improve himself and his society.

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