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THE PRESIDENTIAL ADDRESS: THE CHALLENGE AND OPPORTUNITIES OF APPLIED SOCIAL RESEARCH*

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Introduction

The stance of our profession toward applied work of all sorts in sociology has been one of considerable ambivalence. On the one hand, applied work is decidedly below basic or theoretical work in the hierarchy of preference, prestige, and esteem. Correspondingly, academic employment is very strongly preferred over nonacademic positions, even though salary levels for the latter are often considerably higher. When we scan the credentials of job applicants or bibliographies appended to grant applications, articles published in journals that feature applied work are assumed to be of lesser quality than ones published in mainstream professional journals. Most sociologists are embarrassed when we are confused with social work or with family therapists. Indeed, some of our colleagues gloss over their applied work as if it were a vice best kept from public view.

On the other hand, our roots in applied concerns are old and very much alive. Our ranks always have been full of ministers and ex-ministers, radicals and ex-radicals, even a few conservatives and ex-conservatives, all of whom were attracted to sociology because our discipline appeared to have some relevance to social reform or its prevention. And when it comes to facing outward toward the public—and especially toward our benefactors—we are very quick to point to the many complicated social problems that justify our existence and need for support. Our public stance is often enough that a properly supported sociology will point the way to a better society with a lowered level of social troubles.

Thus, our ambivalence consists in believing—at one and the same time—that applied work is not worthy of our best efforts and our best minds and that an important reason for our existence is that sociology will lead to important practical applications.

Some of our critics apparently understand this ambivalence sufficiently to be able to exploit it and, occasionally, will criticize us on both counts. For example, earlier this year, Congressman Ashbrook (Ohio) spoke out in support of his proposal to slash the NSF appropriation for the basic research program in the social and behavioral sciences—a sterile title that hides sociology, political science, and economics in a plain brown envelope. He accused us (on the one hand) of being off in the cloudy heights of theory and speculation and, hence, not attending to the very real needs of our society, yet (on the other hand) of seriously endangering the moral order of our society by implicitly endorsing, through research on such matters, the dreadful patterns of deviance that afflict our society.

It should be pointed out that sociology

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is not the only discipline that holds an ambivalent view of applied work: this is a viewpoint we share with the other social sciences and with the natural physical sciences as well.

There must be some important reasons why this ambivalence persists, over time, in sociology and other disciplines. I will leave it to the sociologists of science to provide an understanding of why we have such mixed feelings toward applied work. My task here is to examine more closely the relationships between applied and basic work in our field, through taking up four themes:

First, I will explore what the differences are between basic and applied work, showing that the lines between the two are difficult, if not impossible, to draw.

Second, I will attempt to show that there has been considerable commerce between the two parts of our discipline, to the considerable enrichment of both. For polemical reasons, I will emphasize the contributions that applied research has made to basic sociology, an argument that may not give appropriate attention to the fact that the commerce between basic and applied work is certainly a two-way exchange. No one knows whether, in the long- or short-run, the balance of trade favors the one or the other side of the exchange—although there are many opinions on this issue.

Third, I will belabor the obvious a bit, pointing out what many of us already know quite well—namely, that applied social research has enjoyed a boom period over the last two decades, a prosperity in which sociology as a discipline has not participated strongly.

As my final theme, I will point out some of the implications of the current boom in applied social science for our field, indicating why we should become more involved and how we can do so.

There are several varieties of applied work in sociology, including clinical practice, policy analysis, consultation to businesses and government agencies on substantive issues, and applied social research. My main concern will be with applied social research, although many of the points made are, most likely, relevant to other kinds of applied work in sociology. I confine myself to applied social research mainly because I know more about that subfield of applied sociology.

The general argument running through this article is that applied social research has been a firm and fruitful part of the tradition of our discipline. Practitioners and their applied social research have not been given sufficient credit for their contributions to the development of our discipline. I argue for greater recognition to be given to applied social research—in the form of more attention to that activity within our curricula, in our professional journals, and in the rewards our profession gives for scholarly accomplishments.

A Modest Contribution toward Conceptual Clarity

It is easy to distinguish between applied and basic work in sociology if you consider only extreme cases. Using examples from my own work, it appears to be quite obvious that *After the Clean-Up . . .* (Wright et al., 1979) is applied social research, being an attempt to estimate the long-term effects of natural disasters (tornadoes, floods and hurricanes) on the population and housing stocks of small areas. It is, clearly, applied research because its main purpose was to assess whether there was any need for changes in federal policy to provide aid to communities and neighborhoods ameliorating the impacts of long-term negative effects of such natural-hazard happenings. In contrast, an apparently clear example of basic research is my recent article (Alves and Rossi, 1978) on uncovering the normative bases for distributive justice judgments concerning household earnings. What is it that appears to differentiate between the two examples? First of all, the research on the long-term effects of natural disasters was designed to serve an existing need for information for social policy decision making while the distributive justice study is far from current policy concerns. In contrast, income redistribution issues involving wage rates are not explicit issues on current political agendas of legislatures. Nor is there any consideration given to regulating earnings in the interest of increasing popular feelings about how just income discrepancies are among households. The fact that we found no discernible long-term community effects, stemming out of a decade's experience with natural disasters, meant that no relief measures designed to meet the needs of small areas were necessary. In contrast, there likely are no policy makers who are currently concerned with altering wages in order to increase the average sense of justice.

Note, however, that implied in the foregoing is the argument that for a given research to be clearly applied requires the existence of policy makers who would be interested in the outcome as well as in the possibility or existence of some policy to which the research was immediately addressed. So far, this does not appear to be a very satisfactory definition as its main provisions are not linked to the research activities per se but, rather, to the purposes to which the research might be put by some segment of our society. Indeed, this consideration points to the historical specificity of just what is often called applied social research. Because the interests and concerns of policy makers and other political partisans change, over time, research that is applied in one historical period may be basic research in another period—and vice versa. Legislatures might not be concerned *currently* with establishing new (and presumably more equitable) principles for setting wages and salaries, but it is certainly conceivable that they *might* do so. In that event, Alves and Rossi's (1979) study would become applied social research! (We will return again to this issue of historical specificity.)

A second point of difference between the two research examples lies, in the first instance, in the existence of a "client" who underwrote the costs of the research and who also had a direct interest in the outcomes of the research. The NSF-RANN program financed the research that went into the *After the Clean-Up* study because it showed promise (indeed, we actually *did* promise) to be relevant to policy issues. But the social science panel of NIMH that approved the distributive justice study apparently "couldn't have cared less" whether the research was needed for social policy or not. Nor did that grant application promise policy-relevant findings.

Having a client who is concerned about the outcome of a research project that he or she has sponsored often means a continuing connection between the researcher and client throughout the conduct of the research project. While the connection may take a variety of forms, all the way from "oppressive" supervision at the one extreme and benign neglect at the other extreme, applied research always means that somewhere there is less autonomy for the applied (as compared with the basic) researcher.

In applied work, there is more time-consciousness, deadlines to be met, interim and final reports to clients to be written; and, often, there is some specific person or group that is intensely enough interested in your work to attempt constantly to find out what you are up to, and doing, in quite specific terms. In contrast, even though basic researchers have to report to someone and be responsible in a general way for doing a job, the detail and constancy of supervision is simply not as great. I am sure that this is one of the reasons that applied research is valued less than basic research in which the autonomy of the researcher is always greater. Although the NSF-RANN program was far from the most intrusive clients I have had dealings with, my colleagues and I did have to suffer, albeit mildly, through a succession of advisory committee meetings, conferences with RANN staff members, and consultations with federal agencies that were concerned with disaster relief.

Basic research presents quite a contrast. I never again heard from NIMH, once the distributive justice grant was awarded. As requested, I sent copies of articles published as a result of work done under the support grant. If anyone on the NIMH staff actually read any of them, he/she certainly did not feel impelled to communicate either pleasure or displeasure or, for that matter, any other reaction. From almost any researcher's point of view, it is better to operate either without a client or with a very unintrusive client. Certainly, in that sense, basic research is to be preferred over applied research.

The major implication that flows from this discussion, so far, is that whether or not a given research or scholarly work is regarded as basic or applied is, at least partially, historically bound. Thus, subsequent history may treat very differently research conducted in an earlier period. For example, the now-classic 1947 NORC (Reiss, 1961) study of occupational prestige was originally applied social research conducted for a federal agency concerned at the time with the recruitment of natural scientists to federal employment. The agency was convinced that the prestige of federal employment had suffered a severe blow in the years immediately following World War II and had sponsored the research simply to get a better reading on how government scientists were viewed by the general public—as compared with their views of scientists who worked for private industry or for universities. What was, originally, a piece of applied work done for a particular client has become, with time, primarily basic research—at least in the way it has been viewed in the sociological literature.

Indeed, the NORC-North-Hatt occupational prestige research is rather instructive as an illustration of the interpenetration of applied and basic interests. It certainly would have been possible for NORC to have undertaken a study of occupational prestige that was focused exclusively on the occupations of interest to the client. However, the researchers in charge of the project convinced the client that the agency's interests would be better served by a research design that placed the occupations of policy interest into the context of the entire occupational structure. This enlargement of the original narrowly focused interest of the client into a much larger framework was guided largely by North and Hatt's interests in social stratification. The resulting study, of course, was considerably enhanced in value for basic sociology as a consequence.

Another example of applied research becoming basic is W. Lloyd Warner's study of Newburyport (Warner and Lunt, 1941). The immediate motivation for the Yankee City studies was to add a community context to the study of industrial work forces, an interest that had grown out of the Mayo and Roethlisberger Western Electric Hawthorne studies (Roethlisberger and Dickson, 1939). One can dispute whether or not the Yankee City studies were even at all relevant to the applied concerns they were designed to serve, but the fact remains that, initially, Warner was working for a client with quite specific interests and the Yankee City studies were initially conceived of as applied research.

The course of history may also turn a basic study into an applied one. Thus, Benjamin Bloom's work (Bloom, 1964) on the stability of IQ during early childhood became one of the intellectual pillars of President Johnson's Head Start program, as policy makers and their staffs searched through existing literature to find data on where best in the lives of children to intervene with compensatory educational programs. Bloom's work noted the apparent stability of IQ from the first grade on, suggesting that preschool intervention would be most effective. In the same way, E. Franklin Frazier's monograph (Frazier, 1939) on the Negro family had almost slipped into scholarly oblivion when revived by Moynihan (Moynihan, 1965) to provide an apparent explanation of why there was so much "pathology" among contemporary black families.

I started out this discussion of the differences between basic and applied social research with examples in which the distinction is easy to make. For many researches, particularly large-scale studies, the distinctions are much more difficult to draw and, at the margins, the lines between applied and basic research are almost impossible to discern. For example, the Income Dynamics Study (Morgan et al., 1974) was sponsored initially by the Office of Economic Opportunity to provide information on how often, and why, families apparently drifted back and forth across the poverty line. Currently, this longitudinal study is being supported as basic research by the Social Science Division of the National Science Foundation. There can be little doubt that this study has simultaneously contributed to applied concerns and basic social science, aided considerably by the wide diffusion of public use data sets. Essentially nothing about the design changed as sponsorship shifted from OEO to NSF, nor was there much shift in subject matter. The question of whether this is basic or applied social research is somewhat irrelevant—as it seems to have been both. Of course, it should be emphasized that the principal investigators played an important role in designing the study (and its continuation) so that findings would be relevant both to current social welfare policy and to a host of basic concerns in several social sciences.

Another interesting example is the Equality of Educational Opportunity report (Coleman et al., 1966) and the data sets lying behind that report. In its inception, Coleman and Associates' study was certainly applied, mandated as it was in the 1964 equal rights legislation. While there were some policy concerns expressed specifically in the federal legislation it was relatively general in character, calling as it did for a survey to ascertain whether or not educational opportunities were equal across ethnic groups. The design created—and the analysis followed—by Coleman and his associates went considerably beyond the mandate of the Congress to provide, among other things, estimates of the effects of schools on the achievement of pupils. Is this an example of basic or applied research? The answer is that it is both: it is applied research—by virtue of having a client and a set of policy concerns to which the research was at least partially directed; but it is also basic research—in that the researchers went beyond a narrow research mandate to consider some general issues in the sociology of education.

Special note should be taken of the important role played by the researchers in the design of applied social research. A particular applied-research study may be so narrowly designed that basic sociological (or other social science) concerns would find very little of interest in the study or in its findings, or it can be designed to accommodate basic concerns as well. Indeed, given the fact that policy concerns are often vaguely stated and, furthermore, often shift over short periods of time, broad-focused applied studies are likely to be more useful to policy interests, as well as being of greater disciplinary interest.

I hope that I have given sufficient examples to illustrate that the line between basic and applied research is a fuzzy one, subject to redefinition by the researchers, by clients, and by the drift of historical change. The implication of such fuzzy boundaries is that it is difficult to decide what is applied work and what is not. Good applied work tends to be transformed into basic contributions, a process that does not disturb the conventional view of applied social research being, somehow, lower in value and quality than is basic research. A proper recognition of the origins of much of the work that we value in our history would support a more positive view of the contribution of applied work to the growth of our discipline.

Of course, the counter-arguments can be made that (1) I have selectively skimmed the best of applied social research in order to bolster unfairly the argument I have presented, and (2) the bulk of applied social research is of poor quality and hardly likely to contribute even to the discipline, let alone to the solution of social problems. There is some truth in this argument: Much of applied social research is best left in the fugitive Xerox reports in which they were issued. But low quality and irrelevance are characteristic of most of the work in our discipline, whether applied or basic. For example, more than three-fourths of all articles submitted to *ASR* are turned down. Whether or not applied work has more low-quality output than does basic work is problematic.

Some General Contributions of Applied Social Research to Basic Concerns

Because the line between basic and applied work in sociology is fuzzy and indeterminate, it is correspondingly difficult to make a definitive assessment of the interchange between basic and applied work. Today's applied work may be tomorrow's basic science—and vice versa.

Furthermore, what I may classify as "basic" may be defined by another sociologist as "applied." Indeed, about the best one can do is to provide examples of notable transactions in which applied work has made obviously significant contributions to basic social science concerns, in the optimistic hope that these outstanding examples are fair indicators of what is true generally.

To begin with, it is important to recognize that many of the most prominent sociologists have devoted some significant portions of their careers to applied social research: Even an incomplete listing of such persons is quite impressive, including Durkheim, Giddings, Ogburn, Stouffer, Park, Hughes, Lazarsfeld, K. Davis, Phillip Hauser, Sewell, O. D. Duncan, and Coleman. Indeed, a rough and informal count I made of the last thirty Presidents of the American Sociological Association yielded an estimate that eighteen had been involved significantly in applied social research. Clearly, this is an underestimate, relying as it does on the fallibility of one observer's memory. Among the remaining twelve, there are a few who did some applied work—as, for example, Pitirim Sorokin, though his general work was certainly not of that vein. Indeed, there are likely more than a few among the twelve others who may have been "closet" applied social researchers.

Most interesting and revealing, in light of the earlier discussion about the lower esteem accorded to applied work, is that so many of the eighteen Presidents are not generally remembered as applied social researchers because, over time, some of their most important applied research has been redefined as basic work. For example, how many of us recall (or ever knew) that Lazarsfeld's seminal work on personal influence (Katz and Lazarsfeld, 1955) stemmed from very applied work financed by MacFadden Publications in an effort to obtain evidence that would convince would-be advertisers that placing ads in *True Story* magazine would reach opinion leaders? Or that the very influential series of researches by Sewell and his associates (Sewell et al., 1976) on status attainment had its beginnings in a state-sponsored survey of Wisconsin high school seniors, the major purpose of which was to forecast the demand for higher education in that state?

One of the more visible characteristics of prominent sociologists who have participated in applied work is the predominance of quantitative empirical researchers in that group. This is a feature of some importance, as I will develop more fully, later in this discussion. For the present, this characterization is noteworthy as evidence that one of the most important contributions of applied research has been to technical developments in research methods.

Most of us are familiar with the fact that much of the basic work in statistical methods has come out of applied concerns in other fields, experimental work in agriculture, psychological test construction, quality control in industry, and so on. The outstanding examples are quite numerous, as the following list demonstrates: Student's T, analysis of variance, factor analysis, regression, and on and on. By and large, sociologists have been net borrowers of research methods vis-à-vis other fields, especially with respect to statistical models.

Sociologists have played a more important role in the development of data collection methods. Along with other social scientists, we have made important contributions to the science and art of sample surveys. Much of the early work that went into the development of sampling methods, interview construction, and the like was undertaken within applied contexts, by sociologists in collaboration with other social scientists. Area probability sampling was developed at the Bureau of the Census out of a need to conduct valid periodic estimates of the labor force. Psychologists and sociologists—working for the most part with the advertising industry, newspapers, and political candidates—developed the attitude survey. Scaling methods, at least in part, developed out of the work in Stouffer's (Stouffer et al., 1950) Research Branch in the Information and Education Division of the War Department. Indeed, the Research Branch, a decidedly applied activity, did a great deal to train a fairly large set of young sociologists who became the postwar specialists in sample surveys and, more generally, helped to establish the use of sample surveys of all sorts as a prime research tool for our field. The most recent development in sample surveys, random digit dialing, was developed initially by persons working on commercial sample surveys; it is now gradually finding its way into use by sociological researchers.

In addition, one must recall that Hollerith was a Census employee when he came up with the idea of the punch card and mechanical tabulating equipment. And the first commercial version of the electronic computer, UNIVAC I, was at least partially speeded toward development by the demand for its use in the 1950 Census of Population and Housing.

Other frequently used techniques in social research were also either developed in pursuit of applied interests or heavily influenced by being used in applied research. For example, the intense postwar public policy interest in the issue of overpopulation gave a large impetus to the further development of demographic methods. Sociometric techniques had their start as Moreno (1934) developed a device for optimizing the residential arrangements of young women in a training school. Social field experiments had an early start with Dodd's (1934) randomized experiment in Syria on techniques for instilling appropriate drinking water treatment procedures in rural areas.

Qualitative research methods also have roots in applied work. The earliest American community study (Williams, 1906), conducted by a rural sociologist housed in an experimental station, was concerned with the impact of changing agricultural technology on a rural town. As mentioned earlier, Warner's study of Newburyport grew out of the Mayo and Roethlisberger work on worker productivity. The Lynds' first Middletown study (Lynd and Lynd, 1938) was financed by a foundation with a concern for studying the impact of social change on the moral life of Americans.

The technological and methodological contributions of applied to basic sociology loom large because new methods are easier to transfer across substantive fields. In contrast, theory and empirical knowledge are more closely tied to substantive fields and, hence, do not travel as easily. Perhaps most of the transfer from applied to basic work in substantive theory and empirical findings has occurred because much applied work has dealt with subject matters that are at the very heart of sociological concerns—stratification and inequality, organizations, collective behavior, deviance and social control, race relations and discrimination, life chances and health care, family, work and occupations, and so on. Indeed, there are few substantive areas that have not been studied with an applied focus.

Yet, it is difficult to point out clear one-way contributions of applied work to basic work—mainly because the interchange has been an interactive one. Some contributions of applied work (such as the concept of personal influence and opinion leadership) are directly and easily traceable to the applied work from which they originated. Others (such as the concept of relative deprivation) are indirect contributions arising out of commentaries upon or secondary analyses of applied work. In still other cases, concepts arising in theoretical work have been refined in applied work—and vice versa. Examples of the joint development of concepts include status attainment, occupational prestige, and anomie. Other examples, I am sure, will occur to the reader. Of course, one of the more important contributions of applied social research has been to the refinement of—and sometimes negations of—concepts derived from general sociology. The empirical testing of concepts that are incorporated into applied research has led often enough to their being disconfirmed. For examples: there is little empirical evidence for labelling theory, virtually no evidence for a "culture of poverty," meager support for differential association, and so on. Although it would certainly be an exaggeration to state that sociology would not have advanced at all over the past three decades without the help of applied work, it would be equally foolish to claim that applied work has not made a strong contribution to the progress that sociology has made in that period. Basic and applied work in our discipline are complementary endeavors.

Some Challenges and Pitfalls of Applied Social Research

The products of applied social research have made, as I have attempted to show, strong contributions to the development of our discipline. Although the hope of making such contributions might be sufficient to motivate one to engage in applied social research, there are also some intrinsic satisfactions to be derived from the inherent nature of applied work. There are also some pitfalls of which the would-be applied social researcher must at least be aware. Both the attractions and the dangers of applied social research have their roots in the politicized nature of such research, as I will now attempt to show.

Applied social research often demands greater technical skills than does basic research. Because the results of applied social research may be used in the political process, it is clearly important that it be done well. After all, an article in a major professional journal, or a monograph, has little consequence except upon the career of the writer and, perhaps, except for attracting the attention of the handful of other social scientists who have been doing work on the same topic. In contrast, the product of applied social research might be used in the formation and change of public policy; and an error in applied work might have consequences not only for the social scientists involved but also for institutions, agencies, policy makers, and the intended beneficiaries of the policies in question.

Although the discipline may be concerned with whether research reported upon in an *ASR* or *AJS* article is based on good samples, used good measurement instruments, used a research design of appropriate power and methods of analyses that are robust, such concerns are not as salient as in applied work. This is one of the main reasons why much applied social research is on a so-much-larger scale than discipline-oriented research. Of course, editors and readers do deduct points for defects in any of these respects, but the fact of the matter is that there are so few good ideas and data in sociology that Type I errors are clearly less important than Type II errors in the judging of articles. In contrast, for example, estimating the work disincentive effects of transfer payments calls for good samples, precision and validity in measurement, and virtuosity in analysis—because an error in any of these might be translated into social policy affecting the well-being of poor households across the nation.

Of course, the fact that applied social research may be used in policy formation is also one of the pitfalls into which it is easy to stumble. It is very easy to become the center of rancorous controversy; contending parties in some policy dispute may use and abuse your work. There are few major applied researches that have not sparked controversy, as, for example, in the cases of the Coleman Report, the evaluation of Head Start, or the Income Maintenance experiments. Indeed, the controversial character of applied social research has spawned part-time employment of researchers as methodological critics hired by partisans to provide devastating criticisms of some applied social research.

Conflict over the results of applied social research is not without its positive side, however. The competition among divergent points of view has led to a considerable acceleration of progress toward higher and higher quality in applied social research. For example, the heavy criticism directed at quasi-experimental estimates of program effectiveness has sparked considerable progress toward understanding the role of a priori theory in devising research designs (Heckman, 1980).

New methodologies have been invented, old ones adapted to new problems; and interdisciplinary transfer of ideas has been accelerated through the critical evaluation of applied research. Of course, so-called basic and theoretical work is also subject to criticism, but the critical process for applied work is more timely and more intense, characteristics which may be somewhat more painful to the applied social researcher but which are more productive of relatively rapid progress in technical and conceptual quality.

The political character of applied social research is the source of another of its attractions, at least to those of us who are still concerned with improving our society. There is the possibility that the results of applied social research will do some good. Thus, one might be tempted to undertake research on the child-care arrangements used by working mothers in the hope that the resulting findings might pave the way for better and more effective child-care policies. There are, however, some restrictions on what one can do as an applied social researcher. First of all, problems are not set by the researcher alone but, often, are worked out on the initiative of some policy-oriented agency and, sometimes, by negotiation between the researcher and the agency in question. In practice, this means that, as an applied social scientist, one is not entirely free to frame research problems in the form that appears most fruitful to the researcher. One is ordinarily restricted to what is called "policy space"—that is, the range of alternative remedies for a social policy that appears to be politically acceptable. Thus, for example, the income maintenance experiments defined a set of payment plans that were thought to span what would be acceptable to Congress, a space that did not include what you and I might consider to be very generous payment plans. In short, there are politically imposed limits on subject matter and policy issues that can be considered in applied social research.

Applied social research tends to be conservative, devoted mainly to the examination of policy alternatives that are not radically different from existing social policies. Fine tuning, rather than revolution, is on the political agenda. At best, applied social research is politically congenial, both to those who are liberals and to the right of liberals.

While applied social research will not bring about revolutionary changes, it is in practice neither reactionary nor completely supportive of the status quo. At least in the present historical period, applied social research has had the characteristics of being a demystifying instrument, exposing the faults and inadequacies of existing institutions. Criminologists in their research have had profound impacts on American prisons. Applied research on schools has raised fundamental questions about what schools do. Applied research on alcoholism has certainly changed our understanding of that disorder. Such examples can be easily multiplied. The fact of the matter is that the social policy of the past few decades was conceived by persons with—at best—amateur social scientist status and, hence, most of it (as well as most long-standing institutions) cannot stand up very well under the scrutiny of a thoroughgoing empirical testing.

It should be kept in mind, however, that applied social research is no occupation for would-be philosopher kings. The applied researcher ordinarily does not get very close to the seats of decision making and policy formation. Often enough, policy appears impermeable to both the results of research and the advice of the researcher. Even at its best, applied social research does not substitute for the political process. It merely provides another input into the policy-making process. Indeed, who would have it otherwise? One of the virtues of our political system is that decisions are made often enough as the outcome of the pulling and hauling among a variety of interest groups, a process that may value the input of social science work but does not place the work on a pedestal of absolute authority.

It is not at all clear to me how one may measure whether the attractions of applied social research are outweighed by the negative aspects of politicized work, or vice versa. If you are attracted both to technically challenging work involving the fine tuning of our existing society and to the opportunities presented by relatively high levels of resources, and are not put off by the possibility that your work will be seemingly ignored by decision makers and possibly come under attack from fellow social scientists, then applied social research is an attractive activity. If you cannot stand verbal and written abuse, or want to work on fundamental changes in our society, then applied social research is clearly not for you.

These foregoing remarks are addressed to the question of whether one would or would not find the politicized aspects of applied social research a deterrent to participation. The negative consequences account, in part, for the reasons that applied social research is undervalued in our discipline. But such arguments do not address themselves to the scholarly contributions such work may make to the growth of our discipline. Applied social research is harder on the participant, but that is at least partially balanced by the greater intellectual challenges such work may provide and by the positive contributions it may make to our society and to the discipline.

The TARP Experiments: Concrete Illustration

The technically challenging nature of applied social research (as well as some of its politicized aspects) is perhaps best conveyed through a concrete illustration. I will draw upon work recently completed by my colleagues and myself (Rossi, Berk, and Lenihan, 1980; Berk, Lenihan, and Rossi, 1980), mainly because some of the more interesting aspects of such work do not get much attention in the formal publications.

The policy origin of the Transitional Aid Research Project (TARP) experiments was a legislative directive to the Department of Labor to help ex-felons to attain productive employment. The Department had tried a variety of programs including skills training, special employment placement efforts, and small amounts of financial aid. Some hope resulted from a small-scale experiment (Lenihan, 1978) which appeared to show that modest payments simulating unemployment insurance benefits were effective in lowering rates of arrest for property crimes.

These promising findings were not convincing enough to call for a national program as the pilot experiment was small, it was conducted in only one jurisdiction, and the payments were not administered by an established government agency. An experiment, the Department reasoned, conducted by a dedicated set of social science researchers, was not the same thing as a program that would be run by state or federal civil servants.

The TARP experiments, conducted with persons released in 1976 from the state prisons of Georgia and Texas, were designed to test whether or not a modest extension of unemployment insurance benefits to released prisoners would aid such persons to make the transition to civilian life without reverting to crime. Although aimed at reducing recidivism, the TARP experiments were far from being either revolutionary or even a very radical departure from existing social policy. The TARP intervention did nothing about the prison systems of the two states involved, nor did it address in a very direct way the causes of crime. Furthermore, TARP was scarcely responsive to several important theoretical systems current in criminology, especially the conflict and labelling perspectives. It was concerned with the current policy space of policy makers: an extension of unemployment insurance coverage to persons released from state prisons seemed to Department of Labor officials to be a policy change likely to achieve enough support in Congress to have a good chance to be enacted into policy, were it possible to show that such an extension could help released prisoners and have substantial equity benefits.

Yet the TARP experiments were not raw empiricism. A very respectable, quite traditional theoretical strain in criminology links together poverty and crime and regards criminal activities as attractive alternative occupations to poorly skilled and inexperienced poor youths. Abundant cross-sectional data link together poverty conditions and crime rates, but such correlations are hardly proof of causal relations. While no one with any shred of morality would ever design an experiment that used poverty as a treatment, it is quite acceptable to most moral sensibilities to provide the exactly opposite treatment, to make persons better off economically. The TARP experiments, then, may be regarded as part of the crime and poverty theoretical tradition providing some experimental evidence on a significant theoretical issue.

Technically, the TARP experiments were very challenging. First of all, it was necessary to design a research project that could produce results that would have high internal validity. A randomized controlled experiment was, therefore, decided upon as the study design. Persons released over a six-month period from the Georgia and Texas prisons were randomly assigned either to: (1) one of three treatment groups that were offered eligibility for at least 13 weeks of unemployment insurance benefits at the minimum benefit levels prevalent in the two states—$63 and $70 per week, respectively, in Georgia and Texas; or (2) an experimental group that was offered employment counseling through the state employment security agency; or (3) one of two controlled groups. Approximately 2,000 released prisoners in each of the two states participated in the experiment. About half of the participating prisoners were followed for a year beyond release with periodic personal interviews asking about their post-release employment experiences and tracking other important life events. In addition, all participating ex-prisoners were tracked through the computerized state arrest criminal justice files which recorded arrests and convictions and through unemployment insurance records which contained quarterly reports on earnings that were subject to the taxes on employers that supported the unemployment benefit systems. Following this extraordinarily transient group, over time, was a considerable tour de force of field work: the high response rates of over 85% for each of three follow-up interviews is an accomplishment that is a considerable tribute to the local interviewing crews and the overall orchestration by Kenneth Lenihan.

The resulting data constitute a considerable body of information on the post-release experiences of ex-felons, better and more detailed than any other data set, a considerable mine of information about the lives of members of this segment of the American underclass. These data are now in the public domain and can be used by any social scientist for a variety of purposes.

As often happens, the policy implications of the experiments' findings are a mixture of good and bad news. First of all, when looked at in the traditional analysis of variance perspective, the treatments clearly failed as administered. Experimental groups were neither more nor less likely to be rearrested in the postrelease year. Hence, it is clear that an extension of unemployment benefits within the existing unemployment benefit system will not avert arrests and the crimes that such arrests represent. Additional bad news was that the benefits had rather strong work disincentive effects, with those treatment groups receiving payments working considerably less, a perverse outcome for a treatment that was supposed to aid the ex-prisoners into normal civilian life.

In order to obtain good news from the experimental results, it was necessary to reexamine the theoretical foundations of the experiment and to depart from the traditional analysis of variance perspective. The reexamination of theoretical foundations led us to consider the recently revived economic interpretation of crime and to draw upon sociological thinking on occupations and work. The resulting theoretical model explained both the seeming lack of an effect of payments on arrests and the work disincentive effects of the payments. Briefly, the payment lowered arrests by lowering the incentives to criminal activities but increased arrests by lowering employment. These two effects cancel out each other, leaving no differences between experimentals and controls in rearrest rates but considerable differences in employment experiences. Modelling this process through simultaneous equation procedures produced results that were consonant with the theoretical model and which were similar in both Georgia and Texas. In addition we were able to show that the payments had the positive effect of enhancing wage rates when employed for persons in the experimental groups that were eligible for payments—the financial cushion supplied by payments allowed some of the leisure to find better employment.

The extended results obtained by broadening the theoretical bases of the experiments and departing from the analysis of variance technique has already generated controversy. We believe that the results justify attempting to set up a benefit system—the "severance pay model," as we have called it—that does not have a severe work disincentive built into it as does the unemployment benefit system that provides benefits only when one is unemployed. Our critics point to the analysis of variance results and claim that the more complicated and, hence, more delicate simultaneous equation modelling is speculative at best. My colleagues and I are sure to be under attack on substantive and technical grounds as soon as the monograph comes under review.

The point of mentioning the controversy that exists now (and that will most likely escalate) is not to elicit sympathy for the "pure scientists" under attack. Rather, it is to illustrate that politics and methodology are not separable. The failure of the policy as tested led the liberal-minded investigators to doubt that the analysis of variance results were the only and best description of the outcome of the experiments and to analyze the data further with more sophisticated approaches. More conservative minds, who might believe that criminals are not to be coddled because they are incorrigible, distrust the results of a more sophisticated approach and prefer to rest—content with the more robust analysis of variance.

The interpenetration of politics and methodology has both positive and negative effects. On the negative side, it can mean that policy-related research is rarely regarded as definitive. On the positive side, it certainly sharpens one's sense of responsibility concerning method, enhancing the care with which data are treated and the level of skill employed.

Has the experiment affected social policy? So far, not very much. When I made a presentation of the findings to the Secretary of Labor last year, I found that Secretary Marshall was quite intrigued with the ideas and was inclined to start policy changes; but subsequent federal budget difficulties have placed any such changes on the shelf.

The TARP experiments have all of the characteristics I outlined earlier. First, on the positive side, the problem of the relationship between poverty and crime is theoretically relevant, with the experimental results providing important new data illuminating the relationship. Hence, we hope that it makes a contribution to basic sociology and, especially, to theories of criminality. Second, the research was technically challenging, with sufficient resources made available to employ a powerful (and expensive) research design and to afford extensive data processing and advanced analytical methods. Third, the policy being tested was a "good" one—that is, if enacted it would provide benefits to persons who were in need and deprive no one else of very much. Indeed, because potential savings through averted arrests can be considerably greater than the costs of payments, taxpayer dollars can be saved.

On the negative side, first, one might view the experiment as tinkering with a system that needs major overhauling. The TARP policy was scarcely radical or very innovative. Second, controversy exists and is bound to increase over whether or not the experiments actually showed the results that we believe they showed. Finally, the experiments did not immediately affect social policy. The changes (in the way in which we treat ex-prisoners) that may result from the experiment have been postponed to the future—how far in the future, one may only speculate, given the possible changes in our federal government that this year's election might produce.

What is the balance between positive and negative? Given the slowness of the process by which new research findings are assimilated into the body of sociology, it is clearly too early to tell. Interestingly enough, the negative is experienced immediately, but the positive outcomes will take place, if at all, over the next decade. My colleagues and I have great hopes that, eventually, the score will show a decided positive balance.

Opportunities in Applied Social Research Today

Applied social research may or may not be as attractive as I have described it, but if there are no opportunities for participation in such activities, then that message may be enlightening—but irrelevant—to our discipline. Such is decidedly not the case, however: applied social research must be described as having been, during the last two decades, one of the growth sectors in our economy. Estimates (Abt, 1980) of the amounts spent in the federal budgets of the last few years on applied social research, in one form or another, vary from a high of two billion to a low of about one billion. The amount spent on the evaluation of educational programs alone was estimated to be more than 100 million in 1976. Whatever estimate one accepts, the numbers indicate that applied social research is big, strong, and flourishing—especially when compared to the support of basic social research program funds, which amount to only several hundred million for all the social sciences combined. In addition, private foundations also provide support, although on a smaller scale.

Note that these funds count only the federal budget allocations. State and local governments also support applied social research, perhaps not to the same extent but adding at least several hundred million more.

With all these funds going for applied social research, one might predict that sociology and sociologists would be wallowing in prosperity. Such appears to be far from the current state of our discipline. While we are not on the threshold of destitution and widespread unemployment, it does not appear that sociologists generally have participated fully in the last two decades of growth in applied social research. There are a variety of reasons for our failure to participate.

First of all, applied social research is certainly not the monopoly of sociology. Economics, psychology, political science, education, geography, anthropology, and such hybrid fields as business, communication, social work, and operations research are all fields that can and do participate in applied social research. Indeed, my best estimate is that economists, psychologists, and educational researchers are more deeply involved in applied social research than any other field. Of course, the three leaders in applied social research are bigger fields than sociology and, hence, such leadership reflects in part the fact that there are simply many more psychologists, educators, and economists than sociologists. But it also reflects the fact that, within those fields, applied work is accorded higher regard than in sociology. Sociology has been primarily an academic field, with far fewer sociologists employed outside the groves of academe, compared to the profile of economists and psychologists.

Second, much of the funds available for applied social research does not go to universities but to nonacademic organizations. Within the last decade, new organizations have appeared on the scene to provide the applied social research that was demanded by the existence of research funds. Older, nonacademic research organizations have also responded by increasing their staffs. Research organizations loosely connected with universities, such as NORC and Michigan's ISR, have expanded as well. Some of the larger research firms now dwarf university social science divisions. For example, Abt Associates, Inc., located in Cambridge, employs more social science Ph.D.'s on its staff than any of the universities in the Boston area. The Rand Corporation in Santa Monica has enough social scientists on its staff to offer a Ph.D. program in the social sciences.

The rapid increase in effective demand for applied social research has not been without accompanying problems. First of all, it appears that legislators often imposed impossible research demands—yet, at the same time, offered funds to do those impossible tasks, a situation that is a structured strain toward hypocrisy, and fraud on the part of suppliers. Literally scores of "beltway bandit" firms were started to bid on the offered contracts, often to accomplish tasks that could be done or completed within the allotted time or budget or with existing research technology. Second, the demand for applied social researchers exceeded the available supply, with the result that there were many persons who became social researchers by fiat. A serious negative consequence of the rapid growth in applied social research was that the quality of much of such research was exceedingly poor—especially in the earliest part of the growth period and especially on the state and local level. Fortunately, there is an apparent secular trend toward increased quality accompanied by a high failure rate among the "beltway bandits." The low quality of some of this research, however, has not helped to raise the status of applied social research among the academic disciplines.

Not all of the quality problems of applied social research have been solved. There are still many applied research projects that are ill-conceived, sloppily executed, and presented in a misleading fashion. Yet the best of applied social research is as good as the best social science going. Furthermore, the average quality is rising as quality standards diffuse more widely and as the critical process described earlier takes hold.

It is especially striking that the research-oriented universities and social science departments have not participated very much in the burgeoning market for applied social research. One of the major reasons has just been alluded to: often, the research tasks were impossible to accomplish and inadequately funded. But there are also additional reasons. First, applied social research is not very high on the academic totem pole of prestige. Second, even when the research tasks are attractive and of intrinsic interest, academicians find it difficult to respond quickly to the research procurement process and to mount the often-extensive, large-scale research projects that are called for. Whatever the reasons, the concentration of applied social research activities outside the university walls has meant that sociology as primarily an academic discipline has not been reached by this new prosperity.

Third, applied social research demands skills that not all sociologists have received through training or through practice. By and large, applied social research is quantitative research and I would venture that the level of relevant skills achieved on the average by sociologists is considerably below that of the social science fields that have been more successful in applied work. In addition, applied work requires an attentiveness to policy-related structural features of our society. For example, the TARP experiments are concerned not with altering the etiological conditions of crime in our society but with slight, inexpensive, and politically expedient alterations in our social services system that might avert a very small proportion of all crimes.

Whatever the reasons for our discipline's lack of participation in applied social research, it is clear that we are missing out on some important opportunities. Primary among these missed opportunities are those that relate to the intellectual health of our discipline. Applied social research, as I have tried to argue earlier, here, provides important opportunities to learn more about how society works for building theory and for strengthening our base of empirical knowledge.

There are also organizational reasons for building a stronger level of participation in applied social research. Academic employment for sociologists at present—and even more so in the future—is drying up. We can no longer afford to run undergraduate curricula that lead to no specific vocational opportunities for graduates. Few departments in the country are experiencing any increases in undergraduate enrollment and in majors. We appear to be losing out to fields in which there are clear employment opportunities beyond graduation. I suggest that junior-level applied research opportunity is one such vocational goal that we could emphasize in our teaching and training programs.

Applied social research is even more important for graduate-level training in our discipline. There are jobs out there in the applied social research "industry" that properly trained new sociology Ph.D.'s could enter.

In order to take greater advantage of these occupational opportunities for our undergraduate majors and those who earn graduate degrees, it is necessary for the discipline to build linkages to applied social research as an institutionalized activity. This means that academic sociologists will have to participate more in applied work. We have to learn how to compete successfully for the funds that are available, learn how to undertake the more complicated and extensive research tasks that are involved. This last point may mean that we have to build research organizations within universities or loosely connected with them that can efficiently and skillfully carry out the research tasks involved. Building such organizations will mean that academicians will have the experience to understand properly the research tasks involved and will be able to provide the apprenticeship-like hands-on experiences for undergraduate majors and graduate students that can make academicians attractive as researchers. We will also have to revamp our undergraduate and graduate curricula to reflect a greater emphasis on quantitative research and on policy-oriented research. Note that I am not suggesting that sociology become a monolithic discipline as far as research methods are concerned. All I am advocating is greater emphasis, especially at the undergraduate level.

Of course, all the changes suggested above imply a greater recognition to be given to applied work as crucial to the health of our discipline. That means that we should not lift our eyebrows when our better graduate students take nonacademic research positions, conveying the notion that by so doing they have left the company of the elect. It means that we evaluate applied social research as research and not automatically deduct points because it is applied. It also means that those of us who do both basic and applied work should be as proud of the latter as of the former. Applied social researchers should come out of the closet!

There are other institutional changes that would also be helpful. Our professional association currently has an academic bias that manifests itself in a variety of ways: our employment bulletin lists few nonacademic positions, and the editors of our professional journals are reluctant to consider articles that are clearly applied research. The American Sociological Association should be taking the lead in helping to bridge the distance between our discipline and the intellectual and employment opportunities in applied social research.

Sociology has deep roots in humanity's desire to understand and control the turmoil of social change and in its striving for ways to lessen the toll of humankind. It would be to deny our heritage to become more and more turned inward to an increasingly precious academic discipline. For the sake of our intellectual growth and our disciplinary strength, we need to provide a more respectable place for applied work, a move that will surely redound to our benefit in excitement, theoretical progress, and a renewed sense of the relevance of our work to the world around us.

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