PROGRAMMING AND FINANCING SOCIAL RESEARCH

By: Paul Webbink, Social Science Research Council

The programmers of this session asked that I "provide some grand perspectives and overview on strengths and weaknesses of present planning of social statistical research ... with special reference ... to ways in which present planning and support fall short of producing a maximum additive contribution to knowledge." The assignment implies a value judgment upon the current state of our research with which I find myself compelled to agree. The reasons why we are where we are, and not somewhere else, will probably not be defined definitively this afternoon, but our general topic raises issues which should actively worry all who are concerned with the future of research in the social sciences. Perhaps we can at least air some of the contributing factors in the hope that this will stimulate reflection after we leave.

I suggest that we begin by ruling out one topic of conversation. This concerns the illusion that somehow someone ought to be able to put together a global program that would tell us who should do what research over, say, the next ten or twenty years. Presumably none of you regard this as feasible or desirable. Aspirations toward programming of this kind, however, do recur periodically. They are usually advanced by persons who reason by analogy from an imperfect understanding of how advances have taken place in the natural sciences, or who when faced with allocating newly available large funds assume that someone should be able to tell them how this can be invested quickly and simply so as to yield gratifying results within a comfortably short stretch of time. I am sure that most of us are in agreement that this sort of programming results either in a set of judgments that were superannuated before they were formulated or that represent the lowest common denominator of the thinking of those induced to participate. The reasons why research in the social sciences, and for that matter in any other important field of knowledge, does not lend itself successfully to programming on this level are undoubtedly evident to all of you.

While I know of no effort at all-encompassing programming that has produced appreciable additions to knowledge, a host of more limited attempts have had significant impacts. You are all aware of the many successes that have been gained within particular government agencies or cooperatively through the mediation of the Office of Statistical Standards. Further major contributions will surely come from its new Price Statistics Review Committee. Many of you know, too, of the planning activities of private organizations such as the National Bureau's Conference on Research in Income and Wealth, the committees on the decennial census of the Population Association of America, and the recently organized Committee on Vital and Health Statistics Monographs of the American Public Health Association. The Social Science Research Council has had research planning as a central concern since its very early years. It may be sufficient to remind you of its recent Committee on Historical Statistics, which advised the Bureau of the Census on the revision of _Historical Statistics of the United States_, or of the Committee on Population Census Monographs, which is planning analyses of recent changes in several major social phenomena, or of the planning activities of the former Committee on Labor Market Research, or of the still earlier work of the Committee on Migration Differentials that significantly affected certain phases of the 1940 census.

Apart from such relatively specific examples, a few much broader appraisals of the state of research have been valuable when done competently and with sufficient thought. Thus the labors of the President's Committee on Recent Social Trends nearly three decades ago had a marked influence on research for many years. The review of statistical programs in the federal government made by the Committee on Government Statistics and Information Services aided in reshaping many of these programs and led ultimately to the establishment of the Office of Statistical Standards. It is cheering that today an increasing number of journals in the social sciences are seriously concerning themselves with the publication of substantial review or appraisal articles.

Research programming, or planning as I prefer to call it, has gone forward under a great diversity of auspices and in considerable amount. Yet it is by nature largely transitory, especially if it is successful, and there are grounds for thinking that research planning is now lagging farther and farther behind the needs of the times. The factors producing this lag seem to me to array themselves in the following way.

First, the increasing prosperity and widening public acceptance of the social sciences are unquestionably a major cause and perhaps the most important. Research planning requires time and may have tangible results only long afterwards, if ever. Today time can often be found only by neglecting other obligations such as deadlines under sponsored projects, by limiting one's participation in exciting and remunerative consulting and public service opportunities, and by foregoing intriguing opportunities for foreign travel. Those of you who have been involved in reviewing research proposals or applications for funds know how often these allocate the time of the supposed senior investigator in bewilderingly small fractions. It would be amazing if some of our most competent friends did not occasionally find that the number of tenths of their time that has been committed far outruns ten times ten. The commitments thus amassed, and the desires of universities that their faculties give some attention to teaching (or comparable expectations of other employers) leave little time and energy for planning anything but additional project applications. Planning of a broader kind confers little recognition and less monetary reward, and may lead to no discernible results appreciated by either the donors of research grants or those responsible for promotions.

These are the realities in which most able research men are caught. Yet if basic research planning is neglected in order to deal with immediate problems, the significance of the research that is undertaken is bound to have less and less relation to long-range intellectual objectives. It is clear that determined efforts must be made to obtain more budgetary provisions both for the planning of individual projects and for longer-range research planning. It is also clear that resolute efforts should be made to enhance the pride taken in planning and the prestige accorded to those who do it well. Progress in these directions, however, will be slow and in the meantime one must rely on the consciences of those competent to plan.

The appeal to the consciences of the competent must be pursued earnestly. Research planning can be done well only by those who have competence, independence, imagination, and rigor of mind. They must be able to rise above the orthodoxies of cliques, the distress that hard reasoning may create for existing institutional or personal programs, and the too widely prevailing evil of kindness that leads one to keep silent about the softness of the work of others.

If those who have active intellectual curiosity and who are dissatisfied with the limitations of existing knowledge are content to press forward only with their own research projects, there is little likelihood that the results of their work will be cumulated in a way that will really extend the boundaries of knowledge. Some planning will be done but if it is left to those who have lesser qualifications it will have little significance or influence.

Research planning will not be adequate either if it is undertaken mainly by those who derive their greatest satisfaction out of thinking on behalf of others. Planning must be done principally by those who intend to devote themselves to work related to that being planned, or who at least are willing to mortgage their own credit and reputations to make sure that the research planned will be carried forward competently.

Efforts have too often been made to "plan" research with the aid of lay "experts" from social action or other interest groups whose knowledge and concern about current urgent problems is not accompanied by knowledge of what the social sciences can and cannot do. Their hopes that one more research project will resolve some immediate problem have rarely been fulfilled, but the effort to plan in terms of their interests or convictions has often consumed time and energy that could have been more usefully employed.

The various preoccupations of those most competent to program research in social fields too often induce them to delegate responsibility for planning to others. The years immediately after World War II brought forth hopes that the planning task could be assigned to research organizations, a large number of which had been recently established at universities and especially at universities that had little settled research tradition. Some of these, like some of the research committees with a longer history, have on occasion done excellent planning. On close examination it turns out, however, that the resulting plans were the work of one man or of a small group of dedicated individuals, rather than of an organization as such. It is significant that few such research organizations have effectively survived the outmigration of their key figures. The organizations have survived but principally as funnels--sometimes as very efficient funnels--for funds to be spent with little regard to any specific ideas or purposes.

A few other types of especially transitory planning should perhaps be mentioned. One carries out the notion that bringing together the most respected scholars in some field and giving them opportunity for a day or two of unstructured talk will lead to something important that has not previously suggested itself to any member of the assemblage. The participants often find these occasions most agreeable but they have little other significance. Then there is the sort of planning undertaken to provide a rationale for fund raising. This may produce excellent summary statements but, unless it reflects a much longer and more purely intellectual process, it too is unlikely to raise new questions or improve the formulation of continuing ones.

Research planning of a productive kind, of which we have currently too little, depends ultimately on an attitude of mind--on the willingness of competent individuals to devote time and effort and reputation to asking for what purpose research is to be undertaken; whether with present knowledge, insight and techniques research yielding more than a new verbalization of opinions is feasible; and if it is feasible, how it can be done most directly and efficiently. Since research has become a respectable, moderately well supported, and highly pleasurable activity, it is sometimes difficult to force oneself to make clear and harshly objective choices. This is all the more difficult because of the great variety of potential customers for research results. Within a relatively few years we have seen not only a substantial expansion of governmental and industrial interest in research but also an avid acceptance of research by hospital trustees, school boards, and representatives of nearly every other major social institution. Their interests have spread, furthermore, from a one-time concern with obvious administrative problems to a yearning for enlightenment on a wide range of human relations problems in both their own operations and their relation to the larger community. These believers in social science have hopefully turned to research for solutions, or at least for information and rationalizations, regarding every conceivable sort of social worry or distress, and their hopes have been effectively exploited by a growing number of self-designated expert problem-solvers.

Among these conflicting purposes genuine courage is required if the social scientist planner is to raise doubts about the validity of some of the activity that is going on about him. Our friends tend to be sensitive, and many of them have payrolls to meet. Though the raising of questions does not imply an adverse judgment, it is too often considered just that. But constructive planning of future research cannot be done if it is assumed that no questions can be raised about what is now being done. It is difficult to offer illustrations that will not immediately suggest to you that your work is under attack but I am sure that several will occur quickly to each of you.

To return to the point with which we began, the present planning of social research falls short of producing maximum contributions primarily because so few choose to labor at this with a seriousness comparable to that with which they undertake their other tasks. I am sure that when someone objectively reviews the history of social research over the past three or four decades he will find a remarkable decline in the recent years in the proportion of time given to critical appraisal of research objectives, of what research in given fields and on given problems is really adding to knowledge, and of the validity of the procedures and techniques that are being used.

Any reversal of this tendency will be caused only by a shift in the preoccupations of those who by their example set criteria of what is regarded as more important or less important among all the activities that we have come to lump together under the label of research. It will not be easy to overcome the disposition in some circles to view with equal approval all expenditures of time and money on activities that resemble research. Nor will it be easy to persuade colleagues that at some point nothing more can be learned, or that nothing is being learned. This is not to argue that there should be interference with anyone's judgment that he wishes to spend his days in pursuits that he finds intriguing and satisfying. Those, however, who wish to be assured that they are participating in a process of intellectual progress do need to reserve time to decide whether progress is indeed being achieved.

What has just been said is not an appeal for a campaign to organize a better apparatus for programming. I have tried to emphasize that planning is largely a highly personal matter. Most of us already spend too much of our limited days in instituting new organizations and mechanisms for coordinating those already in being. Rather than entrust the planning of research to some remote authoritarian body, it is well that we insist upon a continuing multiplicity of planning efforts under a multiplicity of auspices. In these circumstances the arrangements for planning may seem untidy, but the prospect that something significant will result here and there, from time to time, is bound to be greater than if the responsibility for planning is delegated to a single bureaucracy.

My assignment called for some discussion, also, of the effect of problems of financial support on the extent to which contributions to knowledge are currently being made. Here, too, I suspect that implicit in the thinking of some who are concerned with this question is a yearning for a tidier world in which the choice of sources to which to turn would be simpler, funds would be more easily available, and everyone would be able to get the financing that he needs at the moment when he could best use it. This would indeed be nice but it isn't going to happen, and might not in the end accelerate real intellectual progress.

This is not to say that improvements in arrangements for the financing of research are not desirable. But I would urge that this is not the most grievous problem that now confronts us and that if more competent attention were given to the programming of research, some of the current financial problems might be much more easily solved. Such attention might help materially in stimulating and maintaining the interest of sources such as the private foundations, whose lay boards of trustees are bound to wish for a sense of progress in some comprehensible directions. These directions will not always coincide with those that we consider most important, and vexatious difficulties in communicating the importance of certain types of research will certainly remain with us. It may be not irrelevant to note that communication is itself a problem with which one must deal in the planning of research. It does happen, regrettably, that the results of planning are sometimes verbalized in ways well characterized in a review that recently came to my attention. It commented: "The contributors, with a few notable exceptions, show the occupational characteristics of academic persons who are maintaining their dignity before their colleagues; their prose is turgid and elaborate, they conceal quite simple ideas in a vast apparatus of long words inconveniently arranged, and they sometimes establish their superiority over their readers and listeners by the unnecessary use of rare words." It is worth keeping in mind that few, if any, private foundations have ever been chartered solely for the support of research, especially for the support of social research. Here, too, a competitive situation exists in which other forms of activity than the financing of a random collection of research projects are bound occasionally to seem more appealing. Many acts of faith have been performed by foundation trustees, but their faith, like that of everyone else, needs fortification from time to time. I suspect that some of the financial problems of social science research would most easily be overcome by nothing more than a few more good pieces of work whose findings are presented in prose that is relatively generally understandable.