

PROGRAMMING AND FINANCING QUANTITATIVE RESEARCH IN THE SOCIAL SCIENCES

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When I was invited to take part in this program, I was asked to represent the point of view of a government foundation on the subject of programming quantitative research in the social sciences. The National Science Foundation has consistently stated its mission and its program to be the assistance and promotion of the development of basic research in the sciences. This view is just as true in the social sciences as elsewhere, and applies equally well to quantitative research as to any other kind. While our program is to assist those well-conceived projects that promised to add to scientific knowledge and competence, we feel that the Foundation staff should leave the choice of topics, as well as methods of research to those best qualified to judge--namely, to the scientists who are actively engaged in research work. In other words, the program of the National Science Foundation in the area of quantitative research in social science is to give as much assistance as possible to the best research projects that social scientists propose to us.

Obviously there are some criteria by which one project is judged more meritorious than another, and one of the most important of these criteria is the extent to which a given project promises to enlarge the amount of verified knowledge, and/or to advance the state of the art. Now the time is not yet at hand when social scientists can dismiss good qualitative, descriptive accounts of social phenomena. There is still plenty of need for non-quantitative research and there is plenty of it being done--more in some social science fields than in others but still a good deal of sound, respectable work that does not involve any quantification beyond simple statements of relative magnitude or relative frequency, i.e., "more than", "fewer than".

The case for the importance of non-quantitative research can be made most vividly, I believe, in ethnological and social anthropological research where there are scores of small, pre-literate societies that are as yet unstudied or inadequately described. Many of these have apparently unusual forms of social organization, special features that seem to be responses to particular environmental conditions or past history, and accordingly are informative of the plasticity of human nature and the ingenuity of man. Furthermore, many of these tiny societies are disappearing under the expansion of Western society and the "development" of underdeveloped countries. The case for non-quantitative studies can also be made in other social sciences, including sociology and political science where systematic and accurate descriptions of the structure and behavior of a variety of organizations are needed and are indeed being done. The so-called "case-study method" of collecting and arranging data of predominantly non-quantitative sort is extremely useful in these fields as a source of sheer information about social phenomena, of insights into normal processes of development, maintenance and change, and, ultimately as sources of hypotheses about

human behavior in society and general theories of social processes. Even in so highly quantified a subject as economics, the case study is still a standby--for example, in the investigation of the effect of patented inventions upon technological change, economic development and productivity.

All this is by way of saying that the social sciences, like the biological sciences, still have much to learn about the objects of study and still need sound, systematic verbal descriptions which incorporate little or no quantitative material. For this reason, any program of support for social science research cannot be soundly based on a one-sided policy of giving funds for quantitative research and withholding them from non-quantitative inquiries.

At the same time, it would be just as unwise not to recognize that some of the most exciting work in social science, and some of the most promising developments are taking place in connection with quantification of one sort or another. The growing edge of the social sciences, as I see it, is in the development of quantitative methods for collecting and analyzing data. The course of development is very uneven, with various fields in the social sciences taking part at rather diverse levels of complexity, because some fields have solved or have confronted particular kinds of problems of quantification earlier than others.

I should like to try to illustrate some of the current trends and immediate concerns that various social sciences exhibit in each of three topics: measurement, data collection, and data processing.

Measurement. Perhaps the most persistent and irritating problem in many areas of social science has been how to assign numerical values to various manifestations of phenomena. A good many things that interest social scientists are extremely difficult to measure, such as sensations, opinions, utilities, convictions, aggressiveness, and group cohesiveness. Another way of saying the same thing is that social scientists have chosen to interest themselves in matters that are extremely difficult to measure or unmeasurable, depending on how easily one gives up. The obstacles to measurement have been handled, variously, by substituting the counting of frequencies, by trying to invent mensurational techniques, or by simply ignoring the problem as the bumble-bee has often been said to ignore the laws of aerodynamics.

A good many very serious and able people have not given up trying to measure, however, and their work on basic problems now will, hopefully, provide the basis for new kinds of quantitative research in the future. A major line of development here has been in scaling, where the attempt is to develop mathematical models of the properties and behavior of attributes that are of interest to social scientists--especially such attributes as attitudes, traits and other individual characteristics, as well as attributes of groups and

collectivities. Without trying to go into the details of any of the scaling models, I will simply say that the general procedure is to look for an underlying order, usually a multi-dimensional order, in terms of which the relative magnitudes of observable phenomena can be estimated. The mathematical model is usually derived in part from masses of observations and must, of course, be evaluated or tested against further data--both phases requiring a heavy investment in quantitative data.

Economists have sometimes excited jealousy among other kinds of social scientists because economists seemed to have easily defined and readily observable units for measurement or counting, such as dollars, tons, miles, employees, and other neatly quantifiable observables. To be sure, economists have shown a good deal of ingenuity in constructing combinations of quanta, such as index numbers, that have served them well. But the measurement of preferences and utility has been a persistent problem, attracting a good deal of somewhat baffled attention. Lately, some fundamental mathematical work has been done (partly under NSF support) that relates the measurement of such diverse phenomena as sensation and utility. It promises to lead to further developments in quantitative research in economics.

I will not try to discuss the measurement problems encountered in anthropology, sociology and political science except to note that a great deal of the quantitative work in these fields is still necessarily based on simple frequency counts, a fact that may account partly for the popularity of non-parametric methods of analysis among researchers in these disciplines.

Collection of data. The problems of social science in collecting data are of a vastly different order than those of measurement, being both more and less formidable--less formidable because the difficulties today are not as intellectually taxing as measurement problems are, although still requiring considerable ingenuity and imagination, and demanding rigor of procedure as well as clear-headedness. Nevertheless, I think it is fair to say that the collection of data has been reduced to fairly mechanical procedures owing largely to the installation of agencies whose task is to keep track of the occurrences of a vast range of social, economic and political phenomena; and owing also to the enormous refinement of sampling methods and survey techniques. The precision of modern statistical surveys is no longer in doubt (provided that what is being surveyed is measurable!), and this tool is perhaps the greatest single methodological asset in the social sciences.

The formidable problems of data collection are, rather, in the realm of financing and manpower. I shall do no more than allude to the present level of expenditure by the federal government and private agencies for the collection of statistics nor will I try to estimate the number of man-hours given to planning and executing surveys. Both figures are large and I am sure many people feel they could usefully be larger. It does seem, however, that no matter what data are gathered by the

recurring surveys, there is always some potential consumer who wants an answer to a question about which data have not been collected. Many of these guilty parties are research social scientists who are interested specifically in new questions--questions that may not have ever been entertained before. Such new questions are likely to be annoying because the existing data don't quite tell what is needed; and also, in the long run, to be expensive because the persistent researcher is going to want a special survey of some sort. It may involve specially trained interviewers, a specially weighted sample, repeated interviews over time with the same respondents, or inquiries from people such as: hard-to-find at homes, bank presidents, blacksmiths, ex-convicts, or other folks with unusual stigmata. In any event, the survey is bound to be troublesome and costly. Yet the results, if obtained, might throw some light on a significant methodological or theoretical problem. On the other hand, they might not and we might just have to write off the cost as part of the risk in doing basic research. In any given case, we cannot be sure a priori, how it will come out (and that may be the best reason for doing it) but if we are serious about supporting basic research in the social sciences, we are going to have to take some risks of this sort.

There may, however, be some ways of reducing the cost of these risks, so we can take more of them within the limits of the research budget. It is not entirely clear whether the existing facilities for collecting data, either routinely or by special surveys, can be more fully and efficiently used for research purposes or how this increased efficiency could be brought about. It is possible that social science research needs new and specialized facilities for conducting surveys. It is conceivable that some research questions can "hitch-hike" or "piggy-back" on existing surveys and samples. Perhaps some collaborative arrangements that now exist can be extended to a wider group of social scientists. These matters need further discussion and exploration.

Data processing. One outstanding characteristic of social science data is the very great quantity that can be (and usually is) collected. A graduate student on his first research project typically comes home staggering under a burden of questionnaires and, for the next weeks or months, struggles to avoid drowning in the details. It is perfectly amazing how many different items or observations appear, beforehand, to be essential, important or just "interesting" to know. (It is this last adjective that seduces the inexperienced researcher). Perhaps this deplorable tendency on the part of some social scientists results from the inadequacy of the theoretical frame of reference that is supposed to be guiding their inquiry and, hence, from insufficiently sharp or pointed problems. Perhaps too it is due to the fact that the social researcher usually gets only one shot at the subjects of his inquiry. Unlike the biologist, physicist or chemist, the social scientist does not have very good control over the experimental material and cannot easily order up a duplicate batch of it if he has failed to notice something important the first time around. In this respect the social

scientist is in somewhat the same position as the astronomer, only, unfortunately, he is nearer to his material and can see its details more clearly. Perhaps we ought not to criticize the social scientist for taking his one shot with a shotgun.

Of course, there is presently on hand a machine that takes some of the tedium out of processing these vast quantities of data. High speed computers seem to have captured the attention of most quantitatively-inclined social scientists and there is no doubt that they present many advantages. Besides being useful for fast calculation, for storage and retrieval of information, computers extend the problem-solving range. They permit exponentially larger numbers of permutations of variables, so that the full range of relationships can be explored, and they readily assimilate enormous numbers of observations, when these can be obtained. This latter advantage seems to be especially noteworthy in economic research where complete series rather than samples are employed in analysis of fluctuations or cycles of economic phenomena. Finally, computers can be usefully put to work in simulating elaborate and lengthy sequential processes that, heretofore, either could not be handled directly because of their size and complexity or had to be treated by averages or other approximations.

All of these advantages computers undeniably possess and there is much promise in them for the development of quantitative research. But their very advantages are also threats, for the computer's speed and capacity seem to be a substitute for thought. In this sense, they encourage the investigator to use a shotgun of bigger bore, to dismiss forethought, planning, and the development of a sharply focussed set of questions in favor of collecting all the information he can and letting the machine sort it out. As yet, I think, this threat has not made itself felt to any great degree, but it seems to me too serious and near a possibility that social scientists (among other kinds perhaps) may be tempted to let machines do their thinking for them. It is entirely possible that the development of the social sciences may be impaired if we adopt the easy faith that we can discover truth by running through all the combinations of observations that happened to be made.

Let me turn briefly to another aspect of my

assignment and take this opportunity to report some data recently released by the Office of Special Studies of NSF ("Federal Funds for Science", covering fiscal years 1958, 1959 and 1960). In general, these figures speak for themselves and they speak a simple message.

In 1958 the total outlay for research in all fields of science by the agencies of the Federal government was almost one billion thirty-four million dollars. Obligations for research in social sciences in 1958 amounted to nearly forty million or about four percent of the total. The corresponding figures for 1959 were one billion, four hundred and forty-three millions total, forty-eight millions for social sciences; and for 1960 the estimates are one billion six hundred and two millions total, fifty-eight millions for social sciences. This estimate covers all topics in social science, includes both quantitative and non-quantitative work (but excludes routine data-collecting operations, testing, surveying or the collection of general purpose statistics). In the last three years, then, social sciences have had a slow but steady growth in absolute amount of funds available, but have not advanced in terms of proportion of total research funds. One further fact is of considerable significance: namely, that only about 23 percent of the funds available for social science research are being devoted to basic research (defined as: "where the primary arm of the investigator is a fuller knowledge or understanding of the subject rather than a practical application thereof"). This contrasts with 47 of the funds in biological sciences and 57 percent of funds in the physical sciences being devoted to basic research. This state of affairs probably reflects the great demand for practical advice and guidance in the solution of social problems--a demand that, in the view of some of us, is not always a help to the sound development of sciences of human behavior. It would seem reasonable, in such enterprises as economics, sociology, social psychology, anthropology, to devote a larger rather than a smaller share of resources to "a fuller knowledge or understanding of the subject" than is the case in the more advanced disciplines of the physical sciences. Whatever may be the extent of our efforts in this direction, I feel sure that the NSF program will include the support of both quantitative and non-quantitative work, where each type is relevant to the development of a sounder more precise and more powerful set of social sciences.