Policy and Program Research in a University Setting: A Case Study (1971)

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Advisory Committee for Assessment of University-Based Institutes for Research on Poverty; Division of Behavioral Sciences; National Research Council

Report of the
ADVISORY COMMITTEE FOR
ASSESSMENT OF UNIVERSITY
BASED INSTITUTES FOR
RESEARCH ON POVERTY
Division of Behavioral Sciences
National Research Council

Policy and Program Research in a University Setting

A CASE STUDY

NATIONAL ACADEMY OF SCIENCES
Washington, D.C. 1971

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The members of the study committee were selected for their individual scholarly competence and judgment with due consideration for the balance and breadth of disciplines. Responsibility for all aspects of this report rests with the study committee, to whom sincere appreciation is expressed.

Although the reports of our study committees are not submitted for approval to the Academy membership nor to the Council, each report is reviewed by a second group of scientists according to procedures established and monitored by the Academy's Report Review Committee. Such reviews are intended to determine, inter alia, whether the major questions and relevant points of view have been addressed and whether the reported findings, conclusions and recommendations arose from the available data and information. Distribution of the report is permitted only after satisfactory completion of this review process.

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Preface

This report is based on an intensive study of the functioning of a university-based policy research institute—the Institute for Research on Poverty at the University of Wisconsin—and the relationship between the Institute and the mission-oriented agency that is the major source of funding for its activities—the Office of Economic Opportunity (OEO). During its deliberations, the Committee has been particularly sensitive to the difficulties of drawing general conclusions and recommendations from a single case study. However, the Committee believes that a number of its findings regarding the particular institution and its relationship to the mission agency do apply more generally.

The report is responsive to a request from the OEO to the National Academy of Sciences to establish a committee to provide a broad-gauge review and evaluation of the Institute for Research on Poverty at the University of Wisconsin, its general function, the quality and quantity of its output, and its usefulness as a component of the OEO's research program. The Committee was also requested to develop the means for assessing the value of university-based institutes as behavioral science research resources for mission agencies.

In carrying out its initial task, the evaluation of the Institute for Research on Poverty, the Committee examined the management and operation of the Institute by means of briefings by staffs of the Insti-

tute and the OEO, critical evaluation of the research output of the Institute by Committee members and ad hoc consultants, and a 2-day visit to the Institute at Madison. An interim report was prepared and transmitted to the OEO in May 1970. The major thrust of the interim report has been incorporated in the present document.

The second stage of the Committee's operation involved planning and conducting a study conference at Abiquiu, New Mexico, in August 1970. Papers on topics relating to the final report were prepared by members of the Committee and by invited guests. It remained for the Committee and staff to piece together the final report. I am most indebted to the members of the Committee for their patient criticisms and comments on the various stages of this, the final report.

I am pleased to acknowledge the Committee's gratitude for the valuable contributions made to its work by its Executive Secretary, Stephen S. Baratz, and its Research Assistant, Margaret E. B. Boeckmann. Special thanks are also extended to Ann T. Garrigan of the Committee's staff who labored with this report with skill and understanding patience, and to Lynda Bell, my secretary, who carried various additional secretarial burdens. Special thanks are also due to a number of individuals who aided us during our deliberations. We wish to acknowledge the help and assistance offered to the Committee during its initial activities by Harold W. Watts, Director of the Institute for Research on Poverty; Felicity Skidmore, Assistant to the Director of the Wisconsin Institute; and to the many members of the staff of the Institute who gave freely of their time during this period. We are particularly indebted to Donald T. Campbell, David Caplovitz, John W. Evans, David N. Kershaw, Joseph A. Kershaw, Robert Lampman, Robert A. Levine, James Lyday, Martin Rein, Heather Ross, Peter H. Rossi, Lester C. Thurow, and Walter Williams for their assistance during the various stages of the Committee's operation. So, too, are we indebted to members of the staff of the Division of Behavioral Sciences of the National Research Council: Alexander L. Clark and Henry David, the Executive Secretary of the Division, who took much valuable time in commenting on the report. I must acknowledge also the contributions made to the Advisory Committee's work through the constructive review of both the interim and final report undertaken by the Executive Committee of the Division, chaired by James N. Morgan (1970–1971) and Herbert A. Simon (1969–1970). Finally, I should note that we were fortunate in having Thomas K. Glennan, Jr., Director, Office of Research and Evaluation at the OEO, serve as staff liaison to the Advisory Committee from the Office of Economic Opportunity. His knowledge and experience were invaluable to the Committee.

The research reported herein was performed pursuant to a con-

tract with the Office of Economic Opportunity, Executive Office of the President, Washington, D.C. 20506. The opinions expressed herein are those of the author and should not be construed as representing the opinions or policy of any agency of the United States Government.

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Introduction: Research and the Policy Process

Poverty in the United States and concern about poverty have long been with us. The social legislation of the 1960's was not the first conscious attempt to eliminate poverty. The Freedman Schools during Reconstruction and later legislation with regard to working conditions, wages, farm prices, social security, and unemployment typify earlier policies and programs aimed at remedying the presumed causes and consequences of being poor. Much of the social policy debate during the depression years was concerned with how to do away with poverty in America.[1]

What was new in the 1960's was the widespread perception of poverty as a national disgrace coupled with renewed confidence in the nation's ability to eliminate poverty through government intervention. That renascent confidence rested to no small extent on a belief in the power of research as a guide to effective policy-making. Two important institutions to carry out research related to government policies with respect to the poor came into being during the decade: the office concerned with policy planning, analysis, and research within the Office of Economic Opportunity; and the Institute for Research on Poverty at the University of Wisconsin.

[1]See, for example, Roy Lubove, The Struggle for Social Security, 1930–1935, Cambridge: Harvard University Press, 1968; Arthur J. Altmeyer, The Formative Years of Social Security, Madison: The University of Wisconsin Press, 1966; Broadas Mitchell, Depression Decade: From New Era through New Deal, 1929–1941, New York: Rhinehart & Company, Inc., 1947.

A principal purpose of this report is to assess the contribution that has been made by the Wisconsin Institute to the policy-making process at the OEO and the desirability of establishing other university-based policy institutions similar in function and design. Another purpose is to examine the ways in which research influences policy, for these are poorly understood and have been the subject of little disciplined inquiry. Many of the empirical and conceptual studies of policy-making processes ignore the role of research, presumably on the ground that it is not important. Even where it can be presumed that research is significant in policy development, the complex and varied ways by which its influence is exercised are poorly understood.

Research undertaken to illuminate or influence a particular policy issue has an honorable and long tradition in the social sciences, going back at least as far as the economists' research on the Corn Laws. Much research that impinges on policy is not undertaken for the sake of learning more about a particular problem. The vast data collected by agencies such as the Bureau of the Census find use in incredibly numerous and varied ways. Models and theories often find use in analysis of a wide variety of policy problems. Research specifically focused on particular policy issues is frequently done without the encouragement, or even the awareness, of a relevant government agency.

There is a view that research affects policy by providing information that enables policy-makers to estimate various parameters of a problem, or to help them choose among competing hypotheses. This certainly is part of what research does, but it is a gross mischaracterization to think of research as merely providing data or information. Perhaps the most important influence of research is through its effect on the way policy-makers look at the world. It influences what they regard as fact or fiction; the problems they see and do not see; the interpretations they regard as plausible or nonsensical; the judgments they make as to whether a policy is potentially effective or irrelevant or worse. As Keynes[2] remarked, "Practical men, who believe themselves quite exempt from any intellectual influence, are usually the slaves of some defunct economist." Much of this influence may occur before a specific policy issue arises. The conceptions that a policy-maker brings to a problem may loom large in importance relative to his efforts to learn about it by consciously surveying the state of relevant knowledge.

Some influence takes place as knowledge is marshalled in the course of the policy formation process. There has been a striking rise over the past few years in the attempts by government agencies

[2]John M. Keynes, The General Theory of Employment Interest, and Money, New York: Harcourt, Brace and Company, 1936, p. 283.

to exploit actively past research that bears on their policy problems and to undertake or finance research deemed likely to enhance their ability to design programs. The establishment of the Institute for Research on Poverty at the University of Wisconsin, under the auspices of the Office of Economic Opportunity, is an important example of this trend. In some ways, the relationship between the Institute and the Office is a special case. In recent years, the OEO has been stripped of many of its action programs and has become an agency largely concerned with research, analysis, and policy experimentation. However, at least until recently, the OEO had enough in common with other government agencies concerned with formulating and implementing government social policy to justify treating its relationship with the Wisconsin Institute as a general model for an investigation of the linkages among research institutions, policy development, and program design.

I The Office of Economic Opportunity–Wisconsin Institute Relationship

The creation of the Office of Economic Opportunity and its early days of operation have been well described.[3] Here, the concern is with the manner in which an office responsible for research and analysis was built into the structure of the agency. From the beginning, the OEO has had a special office with major responsibility for conducting analytic work and supporting external research. Although its name, as well as its organization and some of its functions have changed significantly over the years, it will be referred to in this report by its present designation—the Office of Planning, Research, and Evaluation (PR&E).[4]

[3]See Peter Marris and Martin Rein, Dilemmas of Social Reform, New York: Atherton Press, 1967; Robert A. Levine, The Poor Ye Need Not Have With You, Cambridge: MIT Press, 1970; Sar A. Levitan, The Great Society's Poor Law, Baltimore: Johns Hopkins Press, 1969; James L. Sundquist, Politics and Policy, Washington, D.C.: The Brookings Institution, 1968; Daniel P. Moynihan, Maximum Feasible Misunderstanding, New York: Free Press, 1969.

[4]For a thorough discussion of the role of the pre-1969 Planning, Research, and Evaluation mechanism at the OEO, which was then called the Research, Plans, Program, and Evaluation Office (RPP&E), see Walter Williams, Social Policy Research and Analysis: The Experience of the Federal Social Agencies, New York: American Elsevier, 1971, p. 172–179.

THE OEO OFFICE OF PLANNING, RESEARCH, AND EVALUATION (PR&E)

Neither the research nor the analysis function within the agency were given exclusively to PR&E. The various offices of the OEO concerned with major programs, the largest being Community Action, also had research and analysis functions and the funds to support them. Indeed, in the early days of the OEO the program offices had more research money than PR&E. But the PR&E office was clearly considered, and increasingly acted, as the intellectual center for analysis and advanced planning. The PR&E office, a part of the OEO director's staff, was very early presumed to be both more objective and more competent to analyze programs than the program offices themselves.

The organization of the agency's decision-making machinery then reflected current thinking about the elements of good program management. Stress was placed on (a) systematic policy analysis, including exploration, articulation, and, if possible, quantification of goals; (b) identification of alternative action strategies; and (c) cost-benefit comparisons among different possible ways of proceeding. Analysis was viewed as being based on "research." The analyst was expected to be familiar with the literature of the relevant social sciences and to possess mastery of the technique and comprehension of what past research had to say about policy problems. The term "analysis" usually implied work performed under tight time constraints on a quite circumscribed problem directly related to a specific policy question. However, the distinction between analysis and research was recognized to be imprecise. Analysis was viewed as most effective if it served as the rudder of "planning" that would later be reflected in a "program budget."[5]

This management philosophy—popularly known as a "program, planning, and budgeting system (PPBS)"[6]—had been developed and advocated during the 1950's, most notably by the Rand Corporation. In the early 1960's, it became institutionalized in the Department of Defense. By the end of the decade, offices to perform the kinds of policy analysis required by a PPBS had been established or, where they already existed, had been strengthened or reorganized in most

[5]Charles J. Hitch and Roland N. McKean, Economics of Defense in a Nuclear Age, New York: Atheneum Press, 1965.

[6]See Bureau of the Budget Bulletin 66-3, Planning-Programming-Budgeting, Washington, D.C.: U.S. Bureau of the Budget, October 12, 1965; Charles L. Schultze, The Politics and Economics of Public Spending (Gaither Memorial Lectures, 1968), Washington, D.C.: The Brookings Institution, 1968; and David Novick (ed.), Program Budgeting: Program Analysis and the Federal Budget (2nd ed.), Cambridge: Harvard University Press, 1967.

major government departments—Health, Education, and Welfare, Housing and Urban Development, Labor, Agriculture, and Justice—concerned with the design or implementation of domestic programs.[7] The Office of Economic Opportunity, however, was one of the first agencies outside the national security area to attempt to adopt the new management philosophy, and its PR&E office has since enjoyed the reputation not only of being one of the earliest agency offices of that type but also one of the best.[8]

A PR&E office has two roles: scientific and political. In the first role its staff of social scientists draw on the tools of science, the bodies of scientific knowledge, and the insights of other members of the scientific community to illuminate problems, policy alternatives, and the likely consequences of different actions. This role pushes the office in the direction of supporting external basic research and acting as a communication link with the scientific community. Some academic researchers even seem to think of the PR&E office as a little National Science Foundation, or as analagous to the President's Science Advisory Committee, but this is only part of the story.

The political role of PR&E is that of an active participant in the policy process with his own set of values and often with some strong views about what should be done, how, and by whom. This observation is not pejorative. Policy questions involve both facts and values, and the lines between research and advocacy are often unclear. Moreover, each discipline or subdiscipline in the social sciences tends to have a distinctive perspective on social problems that suggests therapy as well as diagnosis. Social scientists interested in serving the government also quite naturally believe that they have something to say about what policy ought to be, and the agencies are likely to expect them to have a normative point of view.

Unlike a research support office free from policy-making responsibility, PR&E has from the beginning been pressed to handle the OEO's analysis and planning responsibilities. In the early days, when the OEO operated a number of action programs, the PR&E office had the responsibility of helping the director of the agency prune out or strengthen weak programs, identify programs meriting expansion, and select new ones to be launched. This had to be done under the time limitations, constraints, pressures, and feuds that characterize any operating agency. Recently, as the OEO has become more concerned with formulating, testing, and evaluating policies, than with

[7]For a discussion of analysis at HEW see Alice Rivlin, Systematic Thinking for Social Action, Washington, D.C.: The Brookings Institution, 1971.

[8]Joseph S. Wholey, John W. Scanlon, Hugh G. Duffy, James S. Fukumoto, and Leona M. Vogt, Federal Evaluation Policy: Analyzing the Effects of Public Programs, Washington, D.C.: The Urban Institute, 1970, p. 61.

implementing them, the PR&E office has been less involved in monitoring OEO action programs and program budgets. However, the pressures and constraints upon the office remain. There is still need for finding or developing data on a host of questions. Digesting and critically evaluating such data is a major task. In addition, PR&E is called upon to perform such tasks as answering questions posed by the director, the Office of Management and Budget, and the Congress; putting together or monitoring analyses and evaluations; and preparing for conferences. All these activities enter into the political role of a PR&E office and lead to a quite different perception on the part of PR&E staff of the appropriate functions of "research" and of the extramural social scientist.

This different perception leads to a demand for outsiders as extensions of or sources of assistance to staff. No analysis group—PR&E being no exception—has been able to employ as many talented analysts as seems required. While tight time requirements, the importance of inside knowledge, and the need for coordination of much of the work set limits on the degree to which outside social scientists can be utilized, there are still important tasks that they can do well.

In the early days of the OEO, it was considered important not only to pull together existing knowledge but also to extend data bases; given the pressures on staff time, this was an important part of the research that was contracted out by PR&E. In addition, while current or proposed programs had to receive the lion's share of attention, long-run planning and the development of new programs and policies led naturally to a list of attractive research areas in which outsiders could effectively be employed on work that seemed of high value. For example, questions relating to migration, while not of direct concern to early OEO programs, seemed worth examining. Also, and of special importance, the exploration of a negative income tax—a policy initially rejected by other agencies—seemed particularly attractive to economists at the top of PR&E staff.

As has been mentioned, the roles and operations of both the OEO and PR&E have changed significantly over the years. With the OEO's reorganization by the President in the summer of 1969, the role of the OEO as an agency of social experimentation was accentuated and its programmatic responsibilities reduced. Consequently, PR&E was able to dedicate more of its efforts to poverty policies of concern to the administration regardless of departmental and agency boundaries, giving less attention to particular OEO action programs. At that time a separate Office of Research and Evaluation was established within PR&E.

Concurrently, there has been a dramatic increase in funds spent externally by PR&E for research, experimentation, and evaluation. The budget of PR&E has gone from a total of $6.4 million in fiscal

1968 to $23 million in fiscal 1971. The most striking increase was for social experiments, which are expensive because of the costs involved in paying subjects, employing staff to disburse the payments and to keep accounts, and so forth. In January 1969, the New Jersey negative income tax (NIT) experiment was PR&E's only major experimental program. Since that time, a second negative income tax experiment, planned but not implemented earlier, has been added. There is also a performance contracting experiment in education under way, an educational vouchers experiment that is being planned, and a variety of recently initiated child development experiments.

These several experiments account for almost 60 percent of the PR&E research budget or approximately $14.8 million for FY 1971. Evaluations of ongoing programs account for another 18 percent ($2.4 million), representing a significant expansion of such work since the days of the Head Start evaluation. The emphasis of present evaluations is on manpower, health, and legal services programs. Research for purposes other than experiments and evaluations accounts for 23 percent of the budget ($5.4 million). This includes the approximately $1 million budget of the Institute for Research on Poverty (exclusive of the social experiments undertaken there), a longitudinal study of income dynamics at the University of Michigan, research on early childhood development, and a variety of small projects.

At the same time that the external research program has been increasing, the internal staff of the research and evaluation office of PR&E expanded significantly from 18 professionals in 1969 to 41 in 1971. Moreover, as inhouse competence has grown, a change has come about in the relationship between external researchers and internal staff. The present objective of the director of the research and evaluation office is to create a staff whose interests and expertise cover the range of social and policy problems that are the concern of the OEO. Undertaking or supporting research on problems defined as relevant by staff members is, therefore, viewed as a means of augmenting the knowledgeability of the staff. This appears no less important an aim than the specific contributions of research to a particular policy issue.

It is not clear to what extent the expansion and changing character of external research and internal staff reflect the changed mission of the OEO. The PR&E office now deals with a wider range of problems and delves more deeply into them than in the past. It has ceased to be largely the research and analysis arm of an agency with its own programs and has become more a part of the Executive Office of the President. Research and analyses offices in other government departments or agencies—the Department of Health, Education, and Welfare, for example—are more closely tied to agency programs and

objectives. On the other hand, there is an observable tendency to free research and analysis from the budgetary cycle in other agencies, as well as in the OEO. There also appears to be a general rise in interest in experimentation and evaluation. This represents a departure from the earlier faith in analysis of programs before they actually are tried, partly because the data required for confident analysis of untried programs were so limited, and partly because an experimental approach could be adopted in many domestic policy areas. The interest in building an inhouse staff with strength in experimentation and evaluation also seems to be manifested in several other agencies, in addition to the OEO.

THE GENESIS OF THE INSTITUTE FOR RESEARCH ON POVERTY

The PR&E office of the Office of Economic Opportunity is one side of the interactive relationship under consideration; the Institute for Research on Poverty at the University of Wisconsin is the other. The Institute was established on March 23, 1966, a little more than a year after the inception of the OEO.[9] Until the creation of the Wisconsin Institute, the OEO used only conventional methods of procuring outside research. It wrote closely defined contracts with commercial and nonprofit institutions and universities and gave a small number of research grants, also with specified objectives. The individual contracts or grants generally did not carry any presumption of a continuing research relationship.

The establishment of the Institute for Research on Poverty reflected PR&E interest in complementing the existing program of contract and grant research by developing a continuing relationship with a research institution concerned with a broad range of poverty–research problems. Several factors influenced this decision. They included a willingness to delegate responsibility for basic research decisions to an external institution, the hope and expectation that significant benefits would flow from the interaction of a group of scholars doing basic research on poverty at a single institution, and the desire to promote not only the development of the poverty–research field but also a pool of new research talent and competence. Further, the OEO felt a need to augment its own policy-analysis capabilities by creating an outside research organization with a strong commitment to serve

[9]For an account of the early history of the Wisconsin Institute, see Robert Lampman, "The Institute for Research on Poverty," in Charles E. Higbie (ed.), Conference on Poverty Research, Communications, and the Public, Madison: University of Wisconsin Press, 1966.

the agency. In the Rand Corporation[10] there was a model for such a research capability, and, since several of the top staff of PR&E had had long experience at Rand, it was not surprising that they saw these functions being fulfilled by a "Rand-like" organization.

Rand is the best known of a class of nonprofit institutions created shortly after World War II, the earliest being established by the military services: RAND for the Air Force; Research Analysis Corporation for the Army; and Center for Naval Analyses for the Navy. At the same time other government agencies, notably the Atomic Energy Commission and the National Institutes of Health, were also establishing research laboratories or capabilities managed by universities, business, or nonprofit corporations to undertake research of interest to them. These, like the much older university-based research centers supported by the Department of Agriculture, however, were not primarily designed to do research to improve the policy-making capacities of the agencies. Rand and organizations like it also differ from such nonprofit research organizations as the Brookings Institution and the National Bureau of Economic Research, which have a long tradition of engaging in policy-oriented social research, but do not have close links with a particular government agency.

The features common to Rand and the Institute for Research on Poverty that make them representative of a distinct class of research institutions seem to be the following: (a) They are not part of the government, but they are financed by a government agency, or a group of agencies, to study policy issues. There are, consequently, close lines of interaction between the government agency and the research center; (b) although a principal criterion for evaluating their work is the contribution they make to better policy, these institutions have a considerable degree of freedom in determining their research programs; (c) the work done by some of these institutions is done in large measure by social scientists and involves a blend of research, policy analysis, quick responses to queries coming from agency heads, consultation, and advocacy. Even those institutions with primary strength in the physical sciences and engineering are also staffed by social scientists with a stress on economists. The Rand-like institutions, however, differ significantly in their closeness to government, their autonomy and independence, the kind of work they do, and their organization.

The domestic policy work of Rand—recently developed at Santa Monica—is diverse in content and style, ranging from long-run conceptual and empirical research, usually thought of as the province of

[10]For a discussion of Rand as it has operated in national security policy, see Bruce Smith, The Rand Corporation, Cambridge: Harvard University Press, 1966.

academics, to applied research, aimed at resolving or improving specific policy choices. This includes a certain amount of briefing and consultation with the contracting agency. Also, requests from agencies for information are answered, and there is some policy advocacy, even though Rand, Santa Monica, staff prefer not to emphasize this function.

Rand has traditionally fought hard to maintain a "broad mandate," with freedom to pick the problems to be studied, and to keep an "arm's length" distance from actual policy-making. Although some of the project leaders have had experience in government, a large share of the personnel consider themselves policy-relevant academics. While some members of Rand view policy-makers as their primary audience and the scholarly community as secondary, a large number see the relationship the other way around. There are some serious questions as to whether the Rand view of itself can be sustained. The domestic agencies for which Rand is now working keep the organization and the researchers under a much tighter rein than was usual in the past relationship with the Air Force. While Rand continues its efforts to maintain its original research style, funding, which permits its researchers considerable room for maneuver, is now very hard to obtain.

The New York City Rand Institute, which is an offshoot of Rand, Santa Monica, has a strikingly different cast from that of Rand, Santa Monica, even though it is zealous in insisting upon the principles of a "broad mandate" and the right to reject work that it does not want to perform.[11] Projects are much more likely to be chosen in the course of detailed discussions with clients. Little of the research is of the sort academics would do. Memoranda, short reports, briefings, and consultations are the central output, and responding to requests for information and policy advocacy are a normal part of its activities. The primary audience is the city government. The academic audience is secondary, if it is considered at all. Only a small portion of the Rand, New York, staff seem to think of themselves as policy-concerned academics, most of whom see themselves as thoughtful, research-oriented policy analysts, exerting influence on the form and content of policy decisions.

The Institute for Research on Poverty, of course, is part of a university and not a separate nonprofit institution. It resembles the basic research, academically oriented side of Rand, Santa Monica. The OEO–Wisconsin Institute agreement stipulated that the purpose of the Institute would be to conduct research on the nature, causes, and

[11]See Henry S. Rowan, "The Rand Corporation," Twentieth Year Conferences, New York City Special Session, March 11, 1969, New York: Rand Corporation, 1969.

cures of poverty; that the Institute would marshal talents from many disciplines ranging broadly over economics, sociology, psychology, law, and related fields; that the Institute staff would itself decide what kinds of problems would be researched, while being responsive to suggestions and requests for research from the OEO; and that with the aims of the War on Poverty in mind, the Institute would, on the basis of research, recommend new ways to attack poverty or propose changes in the emphasis of existing policies.

The agreement also declared that the OEO expected the director and staff of the Institute to provide services in the form of consultation, but that this type of activity would not be regarded as undermining the Institute's principal function. It was understood that the OEO would fund the Institute at approximately $1 million dollars annually for a period of at least 5 years—an arrangement that has since been shifted to a year-to-year basis.

In addition to the basic support provided by the OEO, assuring the viability of the Institute as a distinctive research institution, the OEO has entered into supplementary funding arrangements with the Institute for specific projects. The principal project of this kind, supported by the OEO for a 3-year period, is the Graduated Work Incentive (generally known as the negative income tax) experiment in New Jersey.[12] In addition, the Institute has received a grant from the Ford Foundation to design and plan a rural negative income tax experiment and support from the OEO to implement it. Funds also have been received from the Organisation for Economic Co-operation and Development (OECD) to support research on the relationships between educational processes and the distribution of socioeconomic characteristics; the Institute has had a contract with the Department of Health, Education, and Welfare under which it both reviewed the information needs for policy decisions in the area of income maintenance and outlined future research needs.

THE PERFORMANCE OF THE INSTITUTE FOR RESEARCH ON POVERTY: AN ASSESSMENT

With an appreciation of what the Wisconsin Institute is, the Committee felt that its performance would be best appraised by asking a series of questions, including the following: How does it operate? How does the research output rate as a scholarly, scientific contri-

[12]For a description of the New Jersey experiment, see Harold W. Watts, "Graduated Work Incentives: An Experiment in Negative Taxation," American Economic Review 54(2):463–472, 1969.

bution to knowledge? Has OEO financing meant that people, who would otherwise have worked on other things, have been drawn to poverty-related questions? Are there any consequences of bringing a number of people doing research on poverty into an "institute"? Has there been the development of fruitful interaction among disciplines? How have the operations and findings been fed into the OEO, and what difference have they made? What have been the key factors that have determined its performance and usefulness over the past years? How has the OEO–Institute relationship evolved?[13]

The Institute for Research on Poverty, as has been said, was formally established in March 1966. In the early days, the research program funded by the Institute consisted both of work being done by the Institute's new members, recruited from the University of Wisconsin and elsewhere, and of projects funded by the Institute but undertaken by researchers not in it. The story of funding and staffing reveals the Institute's evolution. In the fall of 1966, there were on the regular staff three economists, two sociologists, two political scientists, one psychologist, one geographer, one lawyer, one educator, and one specialist in social work. In addition, on project support there were six economists, six sociologists, three political scientists, three home economists, two social workers, and one researcher each in law, education, psychology, anthropology, geography, and speech. By September 1967, project support had been entirely phased out. By the midpoint of the academic year 1969–1970, the regular staff had grown and changed to comprise ten economists, nine sociologists, four political scientists, three lawyers, two social workers, and one psychologist.

The Institute has worked largely according to an articulated philosophy of trying to secure staff of high caliber having a high interest in the problems of low-income people and in policies aimed at the elimination of poverty. It had always been intended that the research program should evolve from the interests of the staff. Generally speaking, individual staff members select their own research topics and projects although they must consult with and gain agreement from the director, particularly if substantial outlays will be involved for support personnel, computing, survey work, and other purposes.

The Institute's fundamental reliance on the caliber and the initiative of its senior staff, rather than on overall planning and assigning of research projects, is made quite explicit in various written statements by the Institute management.

[13]The steps taken to answer these questions are described in the Appendix to an Interim Report that the Committee submitted to the Office of Economic Opportunity in June 1970.

Faced with the inevitable choice between, say, an excellent candidate who is likely to either duplicate someone else or serve a lower-priority interest and another candidate whose talents seem rather pedestrian, but whose research promises to be more on a high-priority target, then the former would be chosen.[14]

This most certainly has not meant that the Institute has been unresponsive to the OEO's needs. A prime example of responsiveness is the Institute's participation in the two negative income tax experiments, explicitly taken on as an additional, special project. Given the staff selection process it was natural that staff members would welcome the opportunity to work on the experiments. For the OEO, the existence of the Institute provided an exceptionally convenient and qualified vehicle for running the negative income tax experiment in New Jersey.

It is the policy of the Institute to appoint staff members on a long-term basis only if they are also appointed in one of the regular departments of the University. The Institute per se does not offer tenure to its staff members. The Institute does recruit staff on a short-term basis for either the summer months or an academic year, and such staff may or may not receive joint appointments with one of the regular departments. At the junior level, research assistants and associates are generally students pursuing a graduate degree in one of the departments at the University.

The staff has been recruited in a variety of ways. The most natural was to identify faculty members at Wisconsin in various departments who had demonstrated a commitment to the poverty area in their research, and who were desirous of having a part of their teaching responsibilities relieved so that they could devote substantial time to research within the framework of the Institute. A second was to find new faculty members who could serve the Institute as well as university departments. In the words of the director,[15]

The process by which candidates come to be considered is quite eclectic. A great deal of it has come by way of regular departmental recruitment, having one eye out at all times for possible appointments that would fill both their needs and ours. Early in the departments' recruiting efforts such possibilities are mutually discussed. Promising credentials usually lead to interviews and the appointment is ultimately made by the department in question, with the knowledge of the availability—or absence—of Institute participation in the salary and the summer support of the candidate.

[14]"Statement on the Operational Management of the Institute for Research on Poverty," prepared for the Advisory Committee for Assessment of University Based Institutes for Research on Poverty, February 1970, p. 1.

[15]Ibid., p. 1–2.

It is apparent that the vast majority of Institute staff members consider themselves to be academics, and their research output largely takes the form of articles for scientific journals and books. However, the Institute spends more time and energy than the typical basic research organization in communicating its findings to its sponsoring agency and to the interested lay public. The regular reports filed with the OEO every 6 months contain detailed information on the work of each member of the staff and other aspects of the Institute's research program. The Institute has produced a number of special documents for the OEO and articles in lay journals designed to inform the nontechnical reader about its work.

For many members of the Institute's staff, the consultative part of the relationship with the OEO is minimal. For a small fraction, it is an important channel of influence, information, and assistance. Some direct consultation is provided to the OEO through specific tasks, visits to Washington by a few Institute staff members, and visits to the Institute at Madison by OEO staff members.

The research program of the Institute has yielded important results, particularly in the field of economics. Here the research has been extensive and excellent in quality and long-term policy relevance. A large fraction of the Institute staff and research are in fields other than economics. Some important papers have been produced in these other fields and some of the ongoing work carries promise of distinction. On the whole it appears that this body of work does not exhibit a comparable strength in economics.

It is difficult to reach a judgment on what difference the existence of the Institute for Research on Poverty has made with respect to the research activities of its staff members. Moreover, recent developments in the Institute's relationship with the OEO have increased the extent of the OEO overview of the research program in consultation with the Institute director. The key question here, of course, is what the highly talented Institute economists would have been doing if the Wisconsin Institute had not existed. To answer, one would have to rewrite history. The Committee's impression, based both on discussions with the people involved and consideration of their pre-Institute interests, is that a significant share of the economists would have worked on poverty problems anyway. However, some of them would have worked in other fields and some would have allocated a smaller fraction of their research time to poverty-related problems. Further, while in the absence of the Institute many would have obtained other kinds of research support, it seems evident that it has enabled its people to spend more of their time doing research than would have otherwise been the case. In sum, while the net gain is less than the gross, there undoubtedly have been some significant additions to our understanding of poverty as a result of the funding of the poverty Institute.

Support by OEO has clearly encouraged a concentration at Wisconsin of talented people interested in poverty policy. While most of the senior economists were at Wisconsin before the Institute was established, some were not. The Committee suspects that the faculty-loss rate from the University has been reduced by the Institute's presence. Several of the excellent younger economists attest that it was the Institute that made them decide to come to Wisconsin. A number of articles produced by the Institute are jointly authored, and most carry footnotes acknowledging the assistance of ideas of other people on the Institute staff.

With regard to the sociologists, political scientists, psychologists, and lawyers, there is reason to believe that the presence of the Institute and Institute funding may have had a significant impact both in terms of the kind of work done and on having it done at Wisconsin. The political science department, for example, feels that it was able to bring two people into the department in part because of the Institute's help. In discussions with the sociologists and political scientists of the Institute, several remarked that the presence of the poverty Institute definitely had influenced what they were doing. It has provided a home for certain noneconomists interested in poverty research who found their traditional departments less congenial. To date, the Institute appears to have had little or no effect on the number of graduate students drawn into research on poverty. As a research institute, it has not had this objective.

What seem to be the effects of interaction across disciplines? The economists at the Institute attest that they have learned something from the noneconomists, the noneconomists from the economists. This undoubtedly is so. However, with the exception of negative income tax work, it is hard to find much evidence that interaction has affected anyone's research. Economists continue to look at the economic variables, sociologists at sociological variables, and both continue to work within the conceptual frames of their own disciplines. While there have been instances where social scientists have undertaken interdisciplinary research that transcends the individual discipline to look at its own piece of a problem and then somehow interlock the pieces, examples of success stories are hard to find. Thus it might well be a mistake to try to do interdisciplinary work, and the Institute has thought of its research approach as the latter and not the former.

The negative income tax case is as close to being "interdisciplinary" as anything the Institute has done. The hypotheses and questions to be probed have been developed by a multidisciplinary group dominated by economists. It is difficult to determine whether what has happened thus far transcends the simple addition of sociological questions to economic questions. However, the collection of data in common format is itself rather uncommon (although there are other examples).

When all the data are in an available there will be opportunities to ask questions and test hypotheses that truly cut across disciplinary boundaries.

THE INSTITUTE'S USEFULNESS TO THE OEO

What has the existence of the Wisconsin Institute meant to the OEO? First, the research of the Institute has made a significant contribution to a better understanding of poverty in the United States, to its measurable dimensions and causes, and to the policy instruments that could be used for its diminution or elimination. Thus, research at the Institute has illuminated the difference to the poverty count of different definitions of poverty, factors behind black and white income differentials, the impact of inflation on the poor, the relationship of migration to poverty, the role of health and education, and many other facets of the poverty problem. The research output of the Institute for Research on Poverty constitutes much of the best analysis on the economics of poverty, as well as on certain other aspects, evaluated by accepted scientific or scholarly standards. The very strength of the Institute in economics has almost defined the mainline of research on the economics of poverty. The mainline clearly has been fruitful both intellectually and in its impact on policy. The Institute's research output is strongly represented in the education and training of candidates for advanced degrees whose work revolves around the range of domestic problems linked with poverty. Not only the OEO but also other governmental agencies and Congressional committees concerned with poverty rely heavily on Institute staff members as consultants and as participants in conferences. The Institute staff has been extensively utilized in the development of various aspects of the welfare legislation currently before Congress. In short, the research of the Institute has influenced strongly the way policy-makers look at poverty problems.

The consultative role of the Institute has been important to the OEO. While only impressionistic judgments can be formed on the basis of the available evidence, it appears that in the early years of the relationship much of what the agency gained from the Institute involved direct personal assistance and consultation in such forms as memoranda, brief documents, and conferences, rather than full-blown research reports. There is strong reason to believe that most of the formal reports of the Institute were read very hastily, if at all, at the OEO. However, this appears to be changing with the change in size and character of the staff at PR&E. Institute people were and still are available to help judge research proposals, discuss and give advice on specific questions, and brief the OEO staff members on develop-

ments in poverty research. This kind of assistance can be acquired in other ways, i.e., through independent individual consultants, that would be more cumbersome.

Because the negative income tax experiment has been such a major concern of the Institute, it is sometimes difficult to disentangle those elements specifically related to the project from the general flow of information, ideas, and advice from the Institute to the OEO. The working out of the experiment called for many skills—theoretical, empirical, and administrative. It would not have been easy to put together an effective team had not one already been partially assembled. MATHEMATICA, Inc., Princeton, New Jersey, had initially been assigned the task of actually carrying out the experiment. For the OEO, the existence of the Institute provided an exceptionally convenient and qualified vehicle for design, overview, and analysis.[16]

Involvement in the negative income tax experiment marked a significant change in research emphasis and operating style at the Institute. Individual researchers joined together to work on a problem of direct concern to the OEO. The NIT experiment also changed the mode by which the Institute's research became related to the policy concerns for the agency. Once the Family Assistance Plan became a live policy issue in Washington, research that might have been described as basic became highly relevant to the development of income distribution policy. Discussion and consultation between the OEO and the Institute thus became an essential part of the OEO's policy-development machinery.

It is suggestive to speculate about what the OEO–Wisconsin Institute relationship might have been had the negative income tax experiment not been instituted. At the beginning, the OEO clearly looked to the Institute as its Rand. The advent of the negative income tax experiment meant that the expectation of the Institute as a wide-ranging research institute was never tested. It is reasonable to suggest that the OEO's earlier expectation that the Institute for Research on Poverty would act like a Rand would not have been realized. By virtue of its structure and staffing the Institute for Research on Poverty could not function as a Rand.

Moreover, as has been noted, the OEO has been attempting to build internally the equivalent of a Rand capability. Consequently, it now has less need than before to look outside the agency for the intellectual planning and monitoring work associated with major experiments. The kind of planning and overview function played by the Institute in relationship to MATHEMATICA, Inc. in the New Jersey negative in-

[16]For a discussion of how the Institute became involved in the New Jersey experiment, see Williams, op. cit., p. 155–156.

come tax experiment is likely to be played increasingly by PR&E itself. Thus, in the Head Start evaluation, a good deal of the planning and design was done inhouse, while Westinghouse–Ohio University, the group that conducted the evaluation, was responsible for carrying out the agreed upon design and analysis. There is some reason to believe that this type of relationship will increasingly characterize PR&E's relationship with its contractors.[17]

As the research budget has grown at the OEO, the funds allotted to the Institute have become a smaller and smaller share of the total. In 1968 the Institute's annual budget of approximately $1 million accounted for roughly one third of PR&E's research budget, but in 1971 it accounts for only about 5 percent. In effect, the earlier conception of the Institute as a general purpose Rand has been abandoned. The Institute is increasingly viewed as a capability for basic and long-run research and as a source of expertise.

[17]However, it is unlikely that PR&E staff would participate in carrying out the research as the Institute has done in the New Jersey experiment.

II The Role of Research in Program Design and Policy Formulation

The evolution of PR&E at the OEO and the character of research financed by that office reflect changes in beliefs regarding the most fruitful way to apply the social sciences to solve social problems. The very attempt at such problem solving involves a belief that a given condition can be changed in a desired direction through governmental programs. The objectives and broad designs for social problem solving are expressed, with varying degrees of ambiguity, as policies.

Declared policies such as eliminating poverty carry with them some rough specifications of the objectives to be attained, e.g., increased income, more jobs, more retraining of the unemployed, or improved education, and some broadly defined ways of reaching those objectives, e.g., income transfers, employment and retraining services, and education reform. However, the declared policy of eliminating poverty is only the first step in social problem solving. It is the task of government agencies to develop in more detail and to implement the particular programs by which that declared policy becomes operational.

In all the stages of translation of policies into programs, issues of knowledge, both theoretical or empirical, are involved. In addition, other nonquantifiable elements, such as national resolve, plays a determining role. However, even if the necessary national resolve could be taken for granted, the problems of designing programs that actually will eliminate poverty would remain formidable.

Social program design is a complex and difficult task for at least

three basic reasons. First, it often is hard for interested parties to agree to a common definition of program success, much less measure it. Second, where partial or proximate objectives for policies can be agreed upon, simple, efficient strategies for their achievement often do not exist. Third, successes are difficult to replicate. In order to understand these difficulties, it is useful to compare the War on Poverty with Project Apollo. Many people literally have asked, "If we can get to the moon, why can we not eliminate poverty?" It seems important to clarify why the problems are essentially different and why the analogy is highly misleading.

Project Apollo was characterized by a well-defined objective, which served as a clear target, and against which it was possible to assess progress. In contrast, it has been difficult to set precise directions and ascertain progress in eliminating poverty. This is not to say that there are no widely agreed upon objectives. Increasing the incomes of the poor is one. The objective can be attacked directly, judgments made as to the promise of different programs for achieving it, and assessments of degree of success can be made. One of the striking characteristics of the dialogue on poverty policy in the early 1960's was the debate between the economists and the noneconomists, the economists insisting on the feasibility of helping the poor by simply giving them more money and the noneconomists insisting that this formulation ignored other dimensions of the problem.

To most people, solving the poverty problem means enhancing the capability of the presently poor to earn an income, not just sending them income supplements. It is apparent that this requires, among other things, basic changes in the educational system. Many people see the poverty problem as inextricably intertwined with social and cultural discrimination. Dealing with the problem of social and cultural discrimination will require a change in values, perceptions, and behavior for the nonpoor, perhaps even more than for the poor. Further, it has been argued by some that poverty is but one manifestation of social ills resulting from the basic structure of the political economy, and that resolution of the problem requires radical reform of that structure. It is difficult to establish precise operational goals for dealing with these aspects of the poverty problem, let alone to develop programs to implement them.

Further, the term poverty has different meanings and implied causal models for different people. One's definition of poverty influences both one's assessment of progress and one's judgment regarding the kinds of programs that are important. Such diversity is the rule for major social problems. The current debate regarding educational reform, reform of the system of criminal justice, and the organization and delivery of health services in the United States, are examples. The rather broadly defined and diffuse nature of some

of the particular objectives of different policies would themselves make it hard to set directions and assess progress. Differences in viewpoints compound the problem.

Consider, for example, the evaluation of Project Head Start, a program established for a variety of reasons but with an emphasis on cognitive development. The evidence that the program significantly enhances cognitive ability is unimpressive. However, those who like the program argue that the cognitive measurements used are misleading. In addition, supporters of the program argue that in other dimensions, e.g., provision of health care and enlisting of community interest, the project has been successful. But "success" in these areas is hard to measure.[18]

This is not to say that the objectives of a policy designed to eliminate poverty should be more precise. It is often not possible to define policies simply enough so that clear operational objectives are apparent. Attempts to force clear quantitative specification of objectives run the danger of focusing the resulting programs and their evaluation on that which is easily measured. This would be like forcing marriage partners to establish quantitative indices for the success of their marriage and agree to stay together or divorce depending on those indices regardless of how they felt about it. Both guidance as to direction and feedback on progress toward declared objectives in major social programs will necessarily remain ambiguous. And this makes sustained progress difficult.

Success in designing programs to deal with the poverty problem is also hindered by the limited ability to design reliable strategies for achieving proximate objectives, where these can be defined. Achievement of the objectives of Apollo could be defined in terms of the achievement of a set of engineering tasks. The underlying physical science and engineering data permitted good bets to be made

[18]For a thorough discussion of the positive and negative aspects of the Head Start study, see Donald T. Campbell and Albert Erlebacher, "How Regression Artifacts in Quasi-Experimental Evaluations Can Mistakenly Make Compensatory Education Look Harmful," in Jerome Hellmuth (ed.), Disadvantaged Child, New York: Brunner/Mazel, 1970, Vol. 3, p. 185–210; Victor G. Cicirelli, "The Relevance of the Regression Artifact Problem to the Westinghouse–Ohio Evaluation of Head Start: A Reply to Campbell and Erlebacher," in Hellmuth (ed.), ibid., p. 211–215; John W. Evans and Jeffry Schiller, "How Preoccupation with Possible Regression Artifacts Can Lead to a Faulty Strategy for the Evaluation of Social Action Programs: A Reply to Campbell and Erlebacher," in Hellmuth (ed.), ibid., p. 216–220; Donald T. Campbell and Albert Erlebacher, "Reply to the Replies," in Hellmuth (ed.), ibid., p. 221–225; E. L. McDill, M. S. McDill, and J. Spreche, Strategies for Success in an Appraisal of Evaluation Research, Baltimore: Johns Hopkins Press, 1969.

on how to accomplish those tasks. Certain subobjectives or subtasks can be stated for poverty policy, such as increasing incomes, or enhancing the reading capabilities of young children, or training unemployed adults for new jobs. While we know how to increase incomes, it is harder to design programs to attain these other objectives. Knowledge of social technology is not strong enough to suggest high confidence in the means for implementing them. The weakness of basic theory in the social and behavioral sciences makes it hazardous to move to ideas for program design from the bits and pieces of empirical knowledge held by social scientists. Although correlations among well-defined classes of behavior may be established, it is often difficult to assume that one behavior results from the other. This peculiarity of our social science knowledge hinders attempts to judge what will and what will not work in the field. The possibility exists that the two observed classes of behavior are but the effect of a third, unmeasured variable, which may be the cause of observed relationships. For example, suppose it is known from past studies of existing schools that the size of a school's library is positively correlated with children's learning performance.[19] In the absence of knowledge about the mechanism, one cannot infer from this observation that expansion of school libraries will enhance learning. Schools with large libraries may already be at a point where the returns on expansion are low or zero. Further, library size is only one factor associated with school quality and pupil performance. Expansion of libraries could be the result, not the cause, of students being highly motivated to learn.[20]

In the design of social programs, as opposed to hardware design, we are less able to exploit piecemeal and incomplete knowledge. Much of the strategy of hardware development involves breaking down the overall design problem into a set of separate subproblems. Thus, the design of the guidance system and the engines for space flight can be established as subproblems. Potential interactions among subsystems can be kept to a minimum by suitable "shielding" of different subsystems. It is often possible to "shield" the components of a system from variables that, because they are poorly understood, would complicate prediction. The designer of hardware is rarely forced to design for the complexity of the total environment. Guidance systems,

[19]James S. Coleman, Ernest Q. Campbell, Carol J. Hobson, James McPartland, Alexander M. Mood, Frederick D. Weinfeld, and Robert L. York, "Equality of Educational Opportunity," American Sociological Review 32(3): 475–483, June 1967.

[20]For a discussion of this issue along similar lines, see Glen Cain and Harold W. Watts, "Problems in Making Policy Inferences from the Coleman Report," American Sociological Review 35:228–242, April 1970.

which can be designed for moderate temperatures, can be shielded both from the heat of the engines and from the wide range of temperatures of interplanetary space.

In contrast, new social programs that might work reasonably well in the simple environment of theory or laboratory experience often have failed because the nonlaboratory environment is more complex and the policy could not be "shielded" from the complicating elements. Early childhood educational programs are an example of this problem. Programs such as this cannot be designed so as to be insensitive to the attitudes and reactions of parents or teachers. Moving such programs from laboratory schools to nationwide implementation brings the program manager face to face with this type of difficulty. Regional welfare policies that would work well in a closed community often have been defeated by immigration. The metal parts and shielding of hardware not only make performance more uniform and predictable, but also isolate performance from a variety of complexities that, therefore, need not be understood or foreseen in advance. Such shielding is not possible for most social programs, which must meet and cope with the complexities of the world head on.

Further, hardware development is facilitated by the ability to run reasonably reliable experiments quickly and inexpensively. The strength of theory often permits predictions about complex systems to be made from experience with small and simple models. One can learn much about the proper design of a large aircraft from wind-tunnel testing of a small model. It is often possible to simulate or model environmental stresses, which, in operation, would appear only over a long period of time; thus, strain and durability testing of components and materials can be conducted under artificially accelerated conditions, with performance under such conditions usually being a good predictor of performance in the operational environment.

In the design of social programs, experiments are more difficult to perform. There is the natural reluctance of many people to be experimented with. Qualms of a similar nature are often expressed by experimenters. When experiments are possible, the weakness of social theory means that small-scale experiments are insufficient for testing hypotheses implicit or explicit in a program in full operation. It usually is impossible to accelerate testing beyond the rate at which events would normally unfold under the conditions in which a system will operate. If early childhood intervention programs are to be judged by children's performance several years beyond intervention, one simply has to wait for those years to elapse before he will know about the program. There is no way to accelerate or compress the aging process.

These factors shed light on another difference between the design of hardware and the design of programs to eliminate poverty—namely, the difficulty of replicating experimental successes in the latter. Usu-

ally one wants many copies of a new hardware design. Even in Apollo, several models of the engine were needed. In engineering, if a few good copies have been produced, it generally is a safe bet that more can be produced without serious difficulty. Program design, however, is plagued by the inability to infer from pilot programs (for example, laboratory schools) what will happen when attempts are made to reproduce the model for widespread use. Part of the difficulty here stems from the "shielding" problem referred to above. The conditions within which the experimental version operates are unlikely to carry over identically to other situations. For instance, the personality and style of an individual teacher in a successful program cannot be replicated. Part of the trouble stems from the weakness of theory in identifying cause and effect and in determining which factors are "essential" and which "incidental." It is hard to judge whether a particular set of teachers is unique in any important sense.

For all these reasons, social program design is more difficult than hardware design. The design of programs to eliminate poverty, or attempts to reform the health services system, or efforts to deal with pollution, cannot be expected to be as well defined in advance as an engineering project. While it is possible to develop a well-thought-through social program, there is no assurance that the plan can be implemented or, if implemented, that it will work. This predictive capacity is beyond existing social science knowledge and the state-of-the-art of program design. However, there are better and poorer ways to go about designing social programs, and, over the long run, the nature of the links between social science and program design may have a significant influence on how well we deal with social problems.

THE CONTRIBUTION OF SOCIAL SCIENCE

A wide variety of research methods and styles have characterized the various social science disciplines of psychology, sociology, economics, and political science, and there are a variety of mechanisms by which social scientists can bring their knowledge to bear on social problem solving: by serving as consultants and advisors in analysis; by conducting field experiments and evaluations and other research tied closely to policy needs and priorities; and by performing research done in appreciation of policy problems, but not closely constrained by governmental perceptions and priorities.

Expert Advice and Policy Analysis

Social scientists often are called on to provide this kind of service on various matters; often they will simply volunteer it. This advice and

opinion rests on past research on related questions, both their own and that of other social scientists, the conceptual frame of their discipline, and sometimes particular current research in which they are engaged that makes their personal knowledge particularly relevant. From the perspective of the policy-maker or the policy-analyst in government, simply obtaining the advice and judgment of social scientists is a form of research.[21] While no new knowledge is added to social science, new knowledge is fed into the policy process. In many cases, the advice of social scientists, based on research and professional thinking on related problems and not on explicit detailed analysis of the particular problem, clearly has value. A good macroeconomist can say some sensible things about how to reduce unemployment on the basis of his knowledge of reasonably well-tested theories, even if he is not intimate with the details of a current situation. A child psychologist or linguist may be able to make some good suggestions about how and how not to teach ghetto children to read, that apply to many locales and population groups different from those with which he has worked personally. Usually, however, knowledge of the details of a situation and of the proposed remedies is essential if the advice is to be specific and relevant. Policy analysis aims to provide the consideration of details and specifics.

The term "policy analysis" can be used in a number of ways. It may involve simply finding and bringing together available data and hypotheses that bear on a problem and, on that basis, attempting to evaluate alternative diagnoses and prescriptions. Considerable sophisticated statistical manipulation may or may not be involved in what the analyst does. Sometimes analysis may require the attempt to attain new data, or to formulate new models or interpretations, sometimes not. Systems analysis and cost-benefit analysis are terms often used to describe research that tries to specify policy alternatives and to work out the consequences in a quite formal way. Analysis can mean anything from sensible marshalling of what is known to attempting to increase knowledge.

Field Experiments and Evaluations

In the early 1960's, there was considerable faith in the power of *ex ante* policy analysis to illuminate policy problems, policy options,

[21] In this discussion the terms data gathering, policy analysis, and research are not clearly delineated. For some purposes it is useful to make a distinction. For a discussion, see Williams, *op. cit.*, p. 12–14.

and their consequences.[22] As we have seen in the case of the OEO, the late 1960's brought a growing appreciation of the difficulties in predicting how an untried program will work, or which of two or more programs will work better. Also, many of the new analysts in Washington developed strong beliefs that certain ongoing programs were not accomplishing a great deal. Hence the increase in evaluations. At the same time, others began to think more intensively about trying programs out on a small scale, and about how variations or uncertainties could be probed by actual field experiments.

Adoption of a more experimental approach to program development would make the process more similar to that of hardware development. The view is widely held that hardware development is closely guided by basic science, and that the path of development is clearly laid out in advance through planning based on analysis. In fact, design and development of hardware involves a considerable amount of error, error observation, and feedback, rather than the simple following of a path well illuminated by scientific understanding.[23]

There is a variety of different kinds of social experiments and evaluations designed to gather information regarding different kinds of questions. The negative income tax experiment in New Jersey is designed to get answers to a particular question important in assessing the merit of various possible income maintenance schemes—namely, the effects on labor supply of various guaranteed income levels and marginal tax rates. Historical data were not adequate to shed much light on this question. This effort is as close as one can come to a real social experiment. But it was not intended to be an experimental version of a government program. This is reflected in the fact that all households in the experiment include at least one working-age, able-bodied male; a large share of the families under the proposed Family Assistance Plan will not meet this requirement. However, information about the labor supply of working-age males is important in assessing the desirability and guiding the design of a family assistance plan.

The Westinghouse–Ohio University evaluation of Project Head Start differed from the NIT experiment in several respects. It was not an experiment but rather an ex-poste type of evaluation of cognitive and affective performance of children in the Head Start pro-

[22]Glen G. Cain and Robinson G. Hollister, "Evaluating Manpower Programs for the Disadvantaged," in G. G. Sommers and W. D. Wood (eds.), Cost-Benefit Analysis of Manpower Policies, Kingston, Ontario: Industrial Centre, Queens University, 1969.

[23]Richard R. Nelson (ed.), The Rate and Direction of Inventive Activity, Part V, Princeton: Princeton University Press, 1962.

gram. The research design was not originally built into Head Start. This aspect of the program evaluation has attracted criticism,[24] but post hoc statistical design is not necessarily inherent in evaluations. The essential difference is that the Westinghouse Head Start evaluation was designed to assess the overall performance of an existing program. The central question asked in the Head Start study was whether the particular program, on average, had or had not lifted educational performance. In contrast, the NIT experiment aimed to probe the promise of a possible future policy departure.

The negative income tax experiment and the Head Start evaluation demonstrate the conceptual distinction between experiments and evaluations. In practice experimentation and evaluation may be less sharply distinguishable and quite similar in methodology. In the future, research designs will increasingly be built into new programs. It also will be possible to build in greater program variation. Much of the discussion about evaluation and experimentation includes the desirability of being able to determine what versions or facets of a program worked or did not work, not simply whether a total program was successful or not. The questions here are finer than in gross program evaluation. In gross program evaluations like the Westinghouse Head Start study, it is never quite clear which parts of the program worked or did not work. A program may not show great impact in the gross evaluation, yet still contain effective aspects even though the latter remain unexplored. The key in designing a program with planned and controlled variation is understanding the kind of variation that might make a difference and the factors that somehow must be held constant. This kind of thinking has gone into the design of several of the more recent programs at the OEO.[25]

In the recently organized field experiments with performance contracting in education and the proposed educational voucher experiments, particular policy proposals will be tested on a small scale. Ostensibly, the negative income tax experiment was not intended to test a particular policy proposal (although this certainly was in some people's minds), but rather to gain knowledge about the possible disincentive effects of income transfers, to help guide possible new programs. The new experiments, however, are explicit tests of program ideas. After a period of time the performances of schools and school

[24]Donald T. Campbell and Albert Erlebacher, "How Regression Artifacts in Quasi-Experimental Evaluations Can Mistakenly Make Compensatory Education Look Harmful," in Hellmuth (ed.), op. cit.

[25]For a full discussion of this point, see Richard J. Light and Paul V. Smith, "Choosing a Future: Strategies for Designing and Evaluating New Programs," Harvard Educational Review 40(1):1–28, February 1970.

children who have participated in variants of the programs will be compared with each other and with schools and school children who have not participated in the experiment.[26]

But while many of the difficulties with the Head Start evaluation might have been mitigated had the program been set up initially to facilitate later evaluation, many of the difficulties inherent in evaluation of social programs will remain because they are innate. Where it is difficult or impossible to define and measure success, evaluations focused on particular indices will be at best partial, and the implication of the outcomes of experiments ambiguous. How, for example, will the voucher and performance contracting experiments be evaluated? What yardstick should be used? It may be possible to measure success in terms of reading scores. Perhaps costs can be compared. But what weight is to be given to how the parents and teachers like the system? Aside from changes in reading scores, what is the effect on children? The assessment of these elements of impact will be extremely ambiguous. How then will one judge whether the experiment has shown a promising policy approach or not? And to whom does "one" refer? The OEO? The local School superintendent? The voters of the community?

Even when the criteria are clear, the problems of judging what the tests actually show will be formidable. It is not possible to fully meet the random-selection criterion in assigning people to different types of programs. Geographical factors alone will make this impossible. Nor is it possible to control fully for external variables. How will the results be affected if malnutrition impinges unevenly over the experimental group? This cannot really be predicted, nor is it possible to shield the experiment from such a development. If the analyst thinks about the possible effects of different malnutrition rates, various statistical procedures can be used to compensate for the effect. But what about the host of variables that affect reading scores and that about which little is known? Is it safe to assume that their effects are distributed randomly across program versions?

Similarly, there is the replication problem. What confidence is there that a successful experimental program can serve as a model for a successful national program? To our knowledge very little em-

[26]For a description of the performance contracting experiment, see Ed Willingham, "Education Report/Performance Contracting in Schools Tests Administration's Accountability Idea," National Journal, 2(43):2324–2332, October 1970; also, the reader interested in the plans for the education vouchers experiment should see Center for the Study of Public Policy, Cambridge, Massachusetts, Education Vouchers: A Preliminary Report on Financing Education by Payments to Parents, Cambridge: Center for the Study of Public Policy, March 1970.

pirical research has been done on what happens when government tries to move from experiment to full-scale program.

These are serious difficulties. They are pointed out not to denigrate but to ward off oversell of these new policy instruments. The current emphasis on evaluation almost certainly is a move in the right direction. Gradually social scientists will learn better ways to design experiments and evaluations and what we can and cannot learn from them.[27]

INDEPENDENT RESEARCH IN APPRECIATION OF POLICY ISSUES

The contributions of research to policy and programs that have been discussed above are made in close contact with the policy process. Moreover, they are closely guided by important issues as seen from the government point of view. It is certain that a considerable portion of the knowledge needed to guide policy formulation and the design of programs is not going to be secured unless research is aimed directly at achieving that knowledge. The importance of research integral to design always has been clear in hardware development. Based on our observations at the OEO and on those of other observers of the scene, we think the 1970's will see enlarged government research and analysis staffs, a growing governmental demand for specific tasks done by outside researchers, and increased emphasis on large-scale experiments and evaluations.[28]

There is some reason to fear that the governmental capability to run large inhouse research operations will lag behind the pressures for them to do so. There is also some reason to stress that government control of policy research can have negative effects on the development of alternative formulations of policy. For, if closeness to policy sharpens focus, it also shortens and narrows perspective. There is no evidence that closeness to policy issues enhances perception of important long-run questions. Commitment to a policy strengthens commitment to the values and perceptions that generated it. It is unreasonable to expect that those committed to a point of view would eagerly support research that suggests the possibility that they may be mistaken. Furthermore, the inherent pressure in a govern-

[27]For further discussion on the state-of-the-art of evaluation, the reader is referred to Thomas K. Glennan, Jr., Evaluating Federal Manpower Programs: Notes and Observations, Memorandum RM-5743-OEO, Santa Monica: The Rand Corporation, September 1969; Wholey et al., op. cit.; Glen G. Cain and Robinson Hollister, op. cit.

[28]Wholey et al., op. cit.

ment office for timeliness and relevance causes built-in resistance to questions that may take time to answer and that, if probed, may call for considerable rethinking of policy. From the point of view of a policy-maker with a tight budget and time constraints, research within a particular framework is relevant; research outside that framework is not. This might well be the policy-maker's definition of policy-relevant research.

Yet, the greater the reliance on experiments and evaluations as policy tools, the greater is the need to strengthen the basic knowledge on which these instruments must rest. Thus, lack of relationship between educational expenditures and educational performance revealed by the Coleman Report and other studies[29] is a puzzle to be resolved; it is not an invariant fact of life. If social scientists knew more about the processes involved in this observation, resources could be applied more effectively to the improvement of the educational process. The intellectual challenge, posed by Coleman's findings and directed to a wide range of social scientists, would be of great value to society if met.

The complaint, which permeates the debate on the Head Start evaluation, about lack of adequate measures of cognitive, affective, and linguistic change, must be translated into better measurements of a wide range of attributes of human development. In case after case what social scientists will learn from experiment and evaluation is the deficiencies of their knowledge base, rather than clear-cut answers to particular problems. Yet, in view of pressures to deal with short-term policy questions, it is not likely that many directors of research offices in mission-oriented agencies will give much attention to theoretical questions involving long-term investigation. While PR&E at the OEO has been exemplary in recognizing the need for basic research, it appears that it is not typical of policy-analysis offices in other government agencies. Typically, the agency's research program is buried deeply within an operating section, while policy analysis is carried on in the office of the secretary of the department.

The danger associated with "research to order" extends to research on closely defined problems of immediate concern. The plausible argument of the government research director, that he knows what he needs to know, should not go unchallenged. The history of operations research contains many examples of how the policy-maker wrongly identified his own problems, how the analyst respec-

[29]Coleman et al., op. cit.; H. Gideonse, "Relative Impact of Instructional Variables: The Policy Implications of Research," Teachers College Record 69:625–640, April 1968.

ified them for him, and of the struggle to get the alternative interpretations accepted.[30] Textbooks on operations research usually make the point that the client's specifications of the problem should not be accepted uncritically. While there are many instances in which the operations researcher wrongly specified the problem, this point is important. The National Academy of Sciences report on the management of behavioral and social science research in the Department of Defense remarks that every government agency needs an assistant secretary for questioning assumptions.[31] For practical reasons, most questioning of assumptions and proposing of new alternatives must come from sources outside the government.

At the present time, great emphasis is placed upon experiments and evaluations. A few years ago the fad was systems analysis. The social science community is not immune to changing fashions, but research heads of mission-oriented agencies, if only because they comprise a smaller group, are particularly prone to methodological narrowness or inertia.

The lines between "basic" and "applied" research are unclear. Even in the natural sciences, while the notion of a sharp split between basic and applied work is often convenient for expositional and political purposes, the distinction is difficult to make in practice. The locus of the work—the university as opposed to other places, for example—is not a clear indicator of the type of research undertaken. Nor does the question being examined in the research provide a better index. For example, consideration of the effect of higher income on work behavior was of intellectual interest to economists long before it became an important policy question. A better criterion is the motivation of the researcher, but this is far from foolproof. Much "basic" research is motivated by what the researcher perceives as a social problem; and conversely "applied" research often involves the research in general modeling and broad speculation, based on the need for a better understanding of what is going on in the process he is observing. The Committee believes that the most important distinctions are based on whether the researcher sets his own directions, is free to diverge from the original project specifications, the existence or nonexistence of externally imposed deadlines, and the extent to which the researcher aims for an understanding of a more general problem, as well as solution of a specific problem. Thus,

[30]Smith, op. cit.

[31]Advisory Committee on the Management of Behavioral Science Research in the Department of Defense, National Research Council, Behavioral and Social Science Research in the Department of Defense: A Framework for Management, Washington, D.C.: National Academy of Sciences, 1971.

rather than using the misleading terms, applied and basic research, we shall hereafter distinguish between task-order research and independent research.

The Committee believes that the new thrust toward increased research and analysis guided by the perceptions and priorities of the mission agency needs to be complemented. We feel this can be done by means of research undertaken in an environment in which the investigators define their own problems and areas of study in appreciation of the public policy questions involved. However, the studies performed must be on the basis of the researchers' own priorities. This will not be easy to achieve. There are likely to be obstacles on the public funding side, as well as difficulties in establishing an environment in which research is independent of, but still relevant to, policy.

Perhaps in the social sciences even more than in the natural sciences, the necessity for political independence is an argument for sources of financing not tied to government agencies concerned with policy formation. However, a considerable part of fundamental research in the natural sciences is supported by mission-oriented agencies in fields of interest to them. It is desirable that similar arrangements be made by government agencies to support a share of the research in the social sciences. To a much greater extent than in most of the natural sciences, social science research questions have been defined in terms of long-run policy problems. Often the kind of inquiry most needed in the social and behavioral sciences is research that, while not constrained by present social problems, is influenced by appreciation and understanding of such problems. Such appreciation and understanding are likely to be encouraged if the researcher's funds come from a mission-oriented agency, discouraged if research funding is strictly separated from the policy process.

The best arrangement may be for mission-oriented agencies to spend a certain fraction of their total research budgets on broadly defined fields and general topics of their choice, to be conducted in an independent research environment. Such a policy occasionally has been written into legislation. However, while there is no firm evidence, there is reason to suspect that when a budget squeeze is on, task-order research is emphasized more than independent research. It is to be expected, therefore, that agencies with a principal mandate to support research will have to carry a larger share of the burden for independent research in the social and behavioral sciences than they ought to ideally. This means that these agencies must weigh practical importance as a criterion for funding support. It is not clear how the supporters of independent research on policy issues can acquire the policy expertise necessary for such decisions. Review panels involving the mission agencies in some way would be of help. How-

ever, even in this case something is still lost at both ends of the policy research communications channel. The mission agencies lose the immediate contact with the findings and thinking of the researchers that comes with research support. The research community loses the familiarity with actual policy and policy issues as seen by the policy-maker. While offering no precise judgments on the percentages of independent research that the action agencies should finance, the Committee strongly urges that the mission agencies not abdicate responsibility for financing independent research.

Some kind of funding mechanism that assures freedom of choice to the researcher, yet provides him with an appreciation and understanding of the policy issues, is only half of what is needed. The other half is a research environment that attracts first-rate social scientists who are willing to define their research interests in terms of public-policy problems rather than in terms of problems related to their disciplines. Two recent reports related to the topic have sharply criticized the traditional academic research environment as inadequate by this criterion.[32] It has been argued that in the traditional academic department structure, incentive structure and intellectual stimulation pull the researcher toward advancing his own discipline. To the extent that policy problems are perceived at all as guides to research, they are defined in the discipline's own terms, and the solutions to problems defined in those terms usually are not solutions at all from the point of view of the policy-maker. Now, however, it appears that appreciation of policy issues is a stronger influence on academic social sciences than it was earlier. However, this does not deflect the basic point made about the limitations of the traditional department structure as an environment for policy research.

One of the more common arguments for institutions like Rand as centers for independent research, as well as for work closely keyed to particular policy issues, is that they are more successful in establishing an environment in which policy issues define research problems. The two reports mentioned above have recommended the establishment of more Rand-like structures, perhaps associated with a university but removed from the basic departmental structure.[33]

[32]Behavioral and Social Sciences Survey Committee, National Academy of Sciences and Social Science Research Council, The Behavioral and Social Sciences: Outlook and Needs, Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1969; Special Commission on the Social Sciences of the National Science Board, National Science Foundation, Knowledge Into Action: Improving the Nation's Use of the Social Sciences, Washington, D.C.: U.S. Government Printing Office, 1969.

[33]Ibid.

The Institute for Research on Poverty has some elements in common with these proposed institutions but differs in other important ways. The Institute's function seems increasingly to be that of conducting independent policy-oriented research, which it has done very well. In the following chapter we will consider the Institute for Research on Poverty as a general model for an independent university-affiliated policy research institute.

III The Role of University Policy Research Institutes

What will be the role of institutes similar to the Institute for Research on Poverty in the evolution of capabilities for policy research? This will depend on how such institutes look from the perspective of government and the perspective of the university.

GOVERNMENT DEMANDS

In mounting a policy research and analysis operation, a government agency faces several basic questions: How large should its inhouse staff be, what work should be done inhouse, what extramurally, and which external capabilities are most appropriate for which kinds of tasks?

A good deal of recent literature about the relationships between the social sciences and policy-making has recommended expansion and improvement of government research and analysis offices.[34]

[34]Advisory Committee on Government Programs in the Behavioral Sciences, National Research Council, op. cit., p. 43–49; Advisory Committee on the Management of Behavioral Science Research in the Department of Defense, National Research Council, op. cit., p. 22–29; Committee on Social and Behavioral Urban Research, National Research Council, A Strategic Approach to Urban Research and Development, Washington, D.C.: National Academy of of Sciences, 1969, p. 60–63; Special Commission on the Social Sciences of the National Science Board, National Science Foundation, op. cit., p. 55–56; Williams, op cit., p. 150–188.

Many government agencies, however, still do not have more than a rudimentary inhouse policy research and analysis staff. The Committee feels that the capabilities of some agencies appear to have declined over the past few years. In others the internal capabilities have been strengthened, and it is likely that the future will see a significant, if perhaps erratic, increase in the policy research and analysis staff capabilities in a large number of government agencies. In part, this expansion will permit more research and analysis work to be done; in part, an inhouse staff will do intramurally work that used to be contracted out; and, in part, the quality of the work contracted out will be improved because of better inhouse formulation of projects. The evolution of PR&E illustrates all of these tendencies.

Government agencies have a variety of sources to tap for outside research. The traditional mode of obtaining outside talent by giving contracts and grants to individual social scientists will continue to be important. However, more contracts will also be awarded to organized groups—profit-making as well as nonprofit institutions, various university centers, and other types of organizations.

Much of the research and analytical work done for the government must be done under relatively tight deadlines and guidelines. There are disadvantages in the use of university staff, individually or in research centers, for mission-oriented work. Sometimes individual faculty members may be willing to do a closely prescribed task. Often they may not, although sometimes the quality or experience of an academic man may outweigh, in the eyes of the agency, his unwillingness to be closely prescribed and monitored. The existence of university policy institutes, however, would certainly make universities more attractive to the government as sources for manpower for mission-oriented research. Indeed, some university institutes, staffed by nonfaculty members and a few senior faculty, are not distinguishable from the nonprofits or profits in their willingness or ability to meet tightly defined research constraints and deadlines. However, these institutes are quite different from organizations like the University of Wisconsin's Institute for Research on Poverty, which operates largely in an academic mode. Given the existence of competent profit and nonprofit organizations, institutions like the Wisconsin Institute are unlikely to be preferred sources of task-order research.

Without an institute structure, universities clearly are not capable of organizing and carrying out large-scale evaluations and experiments. Policy institutes may give universities that capability. The OEO's history indicates that government agencies will turn to academic institutes for such work. The Committee believes, however, that profit and nonprofit research institutes and nonprofit-like university institutes, perhaps augmented by academic consultants, probably will be the preferred resources of agencies that conduct large-

scale evaluations and experiments. This is likely to be particularly true when the tasks involved are well specified and have tight deadlines. For the more broadly defined and fundamental experiments, like the original negative income tax experiment, university institutes like the Wisconsin model may well be the preferred choice.

University-based institutes like the Wisconsin Institute are more attractive capabilities for research and independent policy analysis. Although there is today some disenchantment with the idea of using academic social scientists even for this function, it is likely that most first-rate social scientists will be found in academic institutions for some time to come. Furthermore, meeting the growing demands of governmental agencies and nonuniversity policy research centers for trained people will require that the universities train growing numbers of social scientists versed in the skills of policy research. One way to enhance the supply is for government agencies to support the development of policy research and teaching institutes at the universities.

The report of the Special Commission on the Social Sciences of the National Science Board, Knowledge Into Action (the Brim Report), envisioned institutes that could do both task-order and independent policy research.[35] The report considers universities as a possible location for such institutes, suggesting, however, that if institutes are established at universities, they be made financially and administratively independent of departmental structures. The report sponsored by the National Academy of Sciences and Social Science Research Council, The Behavioral and Social Sciences: Outlook and Needs (the BASS report), is concerned with both research and teaching. It recommends the establishment of graduate schools of applied behavioral sciences, which, like the institutes suggested in the Brim Report, would be separate from the traditional academic departments.

The Committee is not persuaded by the case for independence from the departments. The fact that the senior staff of the Institute for Research on Poverty all have departmental appointments, does not appear to have diminished the influence of public policy concerns on the Institute's research program. It is difficult to imagine the Institute having such a top-flight staff unless its personnel also are given university departmental appointments. We are not arguing against the position taken by the Brim and BASS Reports. We are suggesting that the issue may be much less clear-cut than those reports suggest. Probably the critical issue is whether the policy institute can have a staff that works closely with members of disciplinary de-

[35]Special Commission on the Social Sciences of the National Science Board, National Science Foundation, op. cit. p. 87–95.

partments, being their peers both intellectually and in status terms, which probably will involve joint appointments, and yet have financial and administrative autonomy.[36]

POLICY INSTITUTES FROM THE UNIVERSITY PERSPECTIVE

The question of the future role of policy-oriented university institutes cannot be answered solely in terms of their possible usefulness in government policy-making. The university itself has its values, goals, and problems and must consider the benefits and the costs of establishing new institutes. Universities and institutes are not strangers. University institutes comprise a large and heterogeneous group.[37] Many are little more than titles plus a list of names on a letterhead. They are utilized chiefly for the purpose of obtaining grants. Others are organized around some reasonably coherent program or purpose. Of the latter, some are concerned with advancing a particular academic field or subfield of study and serve as subdepartments. Others cut across disciplinary lines. Child study centers and area study programs are prominent examples in the behavioral and social sciences of this type. Others are concerned with activities that are more applied or policy-oriented than is normal in the academic university structure. Of these, many but not all, employ a considerable number of nonfaculty people. Some institutes, such as survey research centers, specialize in certain kinds of data collection activity. Some administer or provide special courses or training programs; others do not. Some have separate faculties or offer special degrees; others do not.[38]

From the university perspective, the key attributes of the Institute for Research on Poverty follow: First, the Institute is defined in terms of a set of social or political problems rather than in terms of an academic discipline or collections of disciplines. The Institute has

[36]Walter Williams, The Capacity of Social Science Organizations to Perform Large-Scale Evaluative Research, Seattle, Washington: Institute of Governmental Research, University of Washington, April 1971, p. 28–29.

[37]The BASS Report surveyed a wide range of universities and discovered 406 separately budgeted institutes involved in behavioral and social science research. The full-time equivalent professional employees of these institutes accounted for approximately 10 percent of the social and behavioral scientists at the surveyed institutions with institutes.

[38]For further discussion of research organization in the social sciences, see Paul F. Lazarsfeld and Sydney S. Spivack, Observations on Organized Social Research in the United States, a report to the International Social Science

a formal contractual relationship through the university with the government agency concerned with policy in the domain of the Institute's research interests. Second, a large share of Institute personnel are faculty members. The Institute for Research on Poverty has some attributes in common with the characteristics of the graduate schools of applied behavioral science recommended by the BASS Report. The key differences are that the Wisconsin Institute does not offer courses or degrees and the large majority of its staff have joint appointments in the disciplinary departments.

THE RESEARCH ROLE OF UNIVERSITY INSTITUTES

Policy-oriented institutes are likely to become important centers of academic research. Policy-makers will increasingly employ social science, and as a result policy issues will be more important in setting the problems and challenges for academic social science. Funds are going to be tied to research that purports to be policy relevant, even though the pull transcends money.

The increased emphasis on social experimentation and evaluation cannot help but have a noticeable impact on academia. The attraction of experimental programs to some academic social scientists will go beyond the opportunity to "be more relevant." For many academics, the data from social experiments will be the best data with which to work. The data collected by experiments and evaluations will provide obvious testing material and challenges for theory. The reports of experiments and evaluations, including criticism and postmortem theorizing about the reasons for the results, will likely become a growing part of the scientific literature. The Head Start evaluation already has generated a significant amount of heated academic debate and, perhaps, even some light. Policy experimentation will provide a check for social theory that has been lacking in many fields. Engineering has long served both as a check on, and a stimulus to, research in the physical sciences. When theory seemed to suggest

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Council, New York, August 1961; Harold Orlans, The Nonprofit Research Institute: Its Origin, Operation, Problems, and Prospects (in preparation); Peter H. Rossi, "Observations on the Organization of Social Research," Symposium on the Organization, Management and Tactics of Social Research, Vocational Guidance and Rehabilitation Services, Cleveland, Ohio, February 1969; Peter H. Rossi, Researchers, Scholars and Policy Makers: The Politics of Large Scale Research, National Opinion Research Center, University of Chicago, 1964; Walter Williams, The Capacity of Social Science Organizations to Perform Large-Scale Evaluative Research, Seattle, Washington: Institute of Governmental Research, University of Washington, April 1971.

that something practical would work, which in fact did not (there are many early examples in electricity), doubt was cast on theory. When successful devices worked in a way science did not comprehend, as in thermodynamics, the challenge to science was clear. In macroeconomics the failure of policies suggested by theory has sharply conditioned the evolution of that field. Similar developments will probably occur in other areas of the social and behavioral sciences.

Influences of this kind will affect many traditional disciplinary departments. They also will increase the attractiveness to many academic social scientists of policy research institutes. The resources and organizational capabilities of institutes will be especially needed if academic social scientists are to play an active role in experiments and evaluations. But even in the absence of such activities, the orientation toward policy issues and contact with government policy makers that such institutes can provide are likely to be increasingly in demand by academic social scientists.

Although the Committee is hesitant to accept some of the enthusiastic forecasts that are made about interdisciplinary research, we are convinced that the separate social sciences tend to define their problems and specify their paradigms too narrowly. A broadening out and cutting across disciplinary limits by social scientists well trained in their particular disciplines seems to be one of the more important and promising routes toward improving the social sciences individually and collectively. Junior faculty at universities seem to be the most likely candidates to achieve this end. Policy problems are not easily delineated by disciplines, nor should those who work on them be so divided.

EDUCATIONAL ROLE OF INSTITUTES

The same considerations may be even more salient with regard to graduate training. The market for social scientists with advanced training is changing. During the heydays of the 1950's and 1960's, the rapid growth of college-age population and the rising fraction of people attending college required a rapid expansion of university faculties. Today, both the rate of population growth and the rise in the fraction of college-age people attending college has slowed down. This means that, in the absence of a cutback in graduate student enrollments, a significantly higher fraction of new PhD's will have to find their place of employment outside the universities.[39] At the

[39]Allan M. Cartter, "Scientific Manpower for 1970-1985," Science 172(3979): 132–140, April 9, 1971.

same time, the demand for social scientists trained in policy research and analysis will be likely to grow in government and in contract research organizations. This clearly implies a greater spread of social science talents and less concentration in academia.

The intellectual broadening of a graduate student after he has become acquainted with his discipline seems to be particularly desirable and a function well suited to policy-oriented institutes. In most universities the graduate student in one social science discipline has almost no contact with the substance of others during his graduate training. In view of the changing job market for new social science PhD's, training in dealing with applied problems with a wide range of tools and concepts may be better for a large fraction of graduate students than training in the specialized tools of a particular discipline. Cross-disciplinary experience can give the student some appreciation of the contribution of other disciplines to policy problems. The requirement of working together and communicating automatically places students in a climate of the give and take of competing viewpoints that is essential in policy making. Institute seminars for graduate students from a variety of departments can serve an extremely important educational function. In some cases, institutes might well develop formal course curricula that go beyond a collection of seminars and workshops. It is possible that graduate degrees in applied social science (not in a particular traditional academic discipline) may be useful, a step that has been recommended by a committee of the National Academy of Sciences.[40]

To the Committee, it appears that most university institutes, if they play a direct educational role at all, perform it almost exclusively with respect to graduate students. The Committee has not had a chance to investigate seriously the possible role of university institutes in undergraduate education. However, it seems, as the authors of the BASS report conclude, that institutes could well play a useful and constructive role in undergraduate education.[41] Undergraduates now are demanding curricula that they see as relevant to the understanding of social problems. These demands will not soon abate; nor should they. Many universities have begun to design and develop new majors defined in terms of broad problem areas rather than in terms of academic disciplines. Institutes can provide a suitable organization for some types of majors and, in fact, already have at some universities. The kinds of courses and seminars, defined in terms of policy problems, that institutes seem particularly well de-

[40]Behavioral and Social Sciences Survey Committee, National Academy of Sciences and Social Science Research Council, op. cit., p. 200–210.

[41]Ibid., p. 206–207.

signed to conduct are likely to be in growing demand by this and the coming generation of undergraduates.

THE INSTITUTE AND THE DEPARTMENTS

The BASS and other reports on policy institutes at universities recommend that the institute have its own faculty and grant its own graduate degrees.[42] The Institute for Research on Poverty does neither. While the separate degree may be a good idea, there is a strong case to be made to open institute courses and seminars to graduate students, aiming for traditional departmental degrees, and to give departmental credit for these courses. This is the policy of the Institute of Social Sciences at Yale. It is not yet clear whether the kinds of courses taught at a policy institute are more useful as a supplement to, or a substitute for, traditional instruction. It is likely that both departmental and institute training will prove valuable. An institute degree program may be the most efficient way to train future policymakers and middle-level policy analysts who will work outside the universities. But, if the kinds of thinking and research that go on in an institute are to feed back into the disciplines, this will be facilitated by an institute program that supplements the training of graduate students pursuing departmental degrees.

While a separate institute faculty may ease the problem of developing a separate degree program, it may hinder both a supplementary education program and the influence on the traditional disciplines. The fears expressed in the BASS report—departmental veto on the institute faculty—have some basis in fact. However, there are considerable advantages of faculty with joint appointments in university departments. Joint appointments serve to open links, which can be mutually valuable, between the institute and the traditional departments. The departments provide a standard-setting mechanism that, while sometimes narrowing, is difficult to establish outside the traditions of disciplines. Further, until graduate schools of applied behavioral science have been in operation for a long time, most of the first-rate social scientists will have a disciplinary affiliation. To recruit them to an institute often may require a joint appointment. Perhaps the appropriate organization would involve a mix of special and joint appointments. Clearly this will depend on the specific purposes of the institute, and the flexibility and quality of the departments at the university. There does not appear to be a unique formula that can be guaranteed to work for all universities and all institutes.

[42] Ibid., p. 203.

A KEY QUESTION: HOW CLOSE THE TIES TO GOVERNMENT?

The Committee believes that the institutes that define themselves in terms of a broad policy field may be quite valuable for a university, as well as of use to a government agency. But, as was stressed earlier, a key attribute of the kind of institute examined is interaction with the government mission agency. This kind of linkage is, from the vantage point of an academic institution, a mixed blessing. The advantages to the institute researcher are access to knowledge and to the policy-maker; the disadvantages center around the possibilities of erosion of independence. Universities clearly would prefer access and influence with total independence. Some government agencies are willing to grant access and some influence but only in return for some control. Familiarity with policy issues and contact with people in government is, we believe, essential to good policy research. At the same time, we think it imperative that policy-oriented university institutes have considerable autonomy. There is a real problem with trade-off. Different universities must make their own judgments on the benefits and costs associated with different degrees and forms of intimacy with government agencies concerned with major political problems of a sensitive nature. Some will opt for minimal governmental links; others for considerable interaction.

There is the related question of the extent and manner of involvement of university institutes in field experiments of social programs. Important social experiments and evaluations will necessarily be scientifically ambiguous, value laden, and often politically hot. The Head Start evaluation and the negative income tax experiments are cases in point. In both, political pressures forced the release of a preliminary report before it was appropriate from a scientific point of view. The data were important for public policy and were seen by some as supporting a political position.[43] These pressures can and should be fought, but cannot be resisted fully. The environment for those involved in policy research often will be quite unacademic.

It seems to the Committee that, at the least, institutes of the kind under consideration should follow closely the ongoing experiments and evaluations conducted by the agencies, do secondary analysis of their data, and criticize the conclusions. Beyond this, there are various degrees of involvement. As noted earlier, the Wisconsin Institute's participation in the negative income tax experiment was essen-

[43]Walter Williams and John W. Evans, "The Politics of Evaluation: The Case of Head Start," The Annals of the American Academy of Political and Social Science 385:118–132, September 1969; Walter Williams, Social Policy Research and Analysis: The Experience of the Federal Social Agencies, New York: American Elsevier, 1971.

tially in design, consultation, and overview. The detailed operations were left to another organization—MATHEMATICA, Inc. at Princeton. In contrast, the Institute's role in the recent rural negative income tax experiment has resulted in the growth of a nonacademic staff and administrative apparatus for the purpose of payment administration and management. The greater the extent to which the institute administers large programs for government, the more it is pulled away from the university's protection and the more vulnerable it becomes politically. The question of the extent to which a university institute modeled after the Wisconsin Institute will engage in doing tasks on request and hiring special staff clearly will, and should, differ from university to university and circumstance to circumstance.

What an institute does is not independent of the sources of finance. Universities clearly would prefer endowment or at least dependable long-run financing. As the Committee understands the drift of thinking in Washington, it is highly unlikely that many new institutes will be financed under terms similar to those that until recently have characterized the OEO's financing of the Institute for Research on Poverty. The Wisconsin Institute now is financed on a year-to-year basis. Universities traditionally have not been accustomed to hiring people when the financial situation is favorable and dropping them when finances tighten. Unless governmental funding for institutes can be put on a long-term basis, the soft funds that institutes are likely to obtain, in addition to creating political vulnerability, are likely to mean that institute personnel are differentiated, in terms of job security, from faculty members not in institutes. In addition, soft funding makes the junior faculty most vulnerable, particularly when it comes to joint appointments in departments. This poses some severe internal dilemmas of institute status within the university. The Committee wishes to differentiate the issue of long-term financing from that of unconstrained research monies. Financial security and stability can even be achieved by university research institutes operating on task-order programs of research, so long as those programs have long-time horizons. One critical element is the stability offered to the staff, regardless of the nature of research to be undertaken. At present, university institutes are operating in, or close to, financial crisis. If they are to become useful institutions for policy research, long-term funding is absolutely essential to assure continuity in function and quality of output.

These issues, relating to the degree and kind of connection with government, are complex. The institute problem reflects in microcosm the conflicting pressures on the university to be more relevant and influential, on the one hand, and to be free of influence by government, on the other. The latent pressures for the social and behavioral sciences would appear particularly strong. These are the disciplines,

after all, that purport to have things to say about social policy problems. The increasing use by government of social science concepts and techniques is at once an opportunity and a threat to the social sciences. The opportunity is that of potential relevance and power. The threat is that the social sciences will be accused of being useless and/or pernicious. University institutes carry the promise of meeting demands for relevant research, providing more meaningful training for students who increasingly will be finding their work outside the university (perhaps in the government itself), and attracting more funds to the university. On the other hand, they carry the danger of visibly linking the university to the centers of political power and funds. The Committee feels that generally the benefits of such a relationship exceed the costs to the universities.

IV Issues and Findings

The Committee has had a unique opportunity to consider a number of issues related to policy research. Our findings, many of which have already been presented, deserve restatement and further elaboration. Two are of particular importance because of their broad implications: the danger of overselling the fruits of even well-conducted evaluations and field experiments; and the strong partisan political currents that swirl around several of the recent studies supported by the Office of Economic Opportunity. Both these findings point to the need for considering the kinds of ground rules that might be established to ensure the effective management of policy research.

THE PROBLEMS OF OVERSELL AND BIAS

Many policy-makers, policy-analysts, and academic social scientists are becoming increasingly skeptical about both the power of social science theory to illuminate policy issues and the usefulness of the more traditional modes of social science research. Many of these same people appear to the Committee to be unrealistically optimistic about the usefulness of experiments and evaluations for solving major policy problems.

It is important to recognize that the number of people and research institutions presently capable of doing first-rate policy research and, more particularly, field evaluations and experiments is limited. These projects are hard to design, execute, and interpret. They

are worthless if done by researchers who are not highly competent.

Even with first-rate people there are reasons to be skeptical about the potential power of evaluations and social experiments as policy instruments. The rising demand within government for experiment and evaluation is understandable, as is impatience with the limited ability of social science theory to deal with policy questions in a manner of immediate utility to policy-makers. However, there is also less awareness than there should be among the proponents of evaluation and experimentation that the state of existing social science theory imposes severe limits on what one can conclude from these instruments for assessing or improving policy-making. This is particularly so when the evaluations are of programs whose effects may be multidimensional and hence not easily measured, or where the mechanisms of impact are poorly understood. If good and relatively complete measures of performance are lacking, the relationship of partial and proximate output measures to policy variables provides little guidance to policy. If the processes going on are poorly understood, correlations with factors possibly affecting performance provide some clues into causal links, but not more than that.

There are several real dangers of bias, and a failure to be aware of them may be costly. One is a proclivity to focus experiments or evaluations on what can be measured readily, to use these easily measured effects as indicators of overall program merit and to discount arguments relating to hard-to-measure dimensions that pull the other way. Numbers must be our servants, not our masters.

Another serious possible bias arises in studies concerned with our understanding of racial or ethnic groups. The issue has been raised by some that policy-makers, analysts, and researchers of different ethnic backgrounds may interpret ethnically derived behaviors in terms of their own cultural experiences. For example, only recently have social scientists become aware that the observed deficiencies of ghetto black children in the use of standard English is attributable to the fact that they were not speaking that language but a dialect of the standard language.[44] The Committee is not in a position to judge the depth of this problem, but it wishes to call attention to it. The possibility of ethnocentric bias entering into the design and interpretation of research related to the problems of minority groups has led some social scientists and the public directly involved to question the validity of such evaluative work.

It is important that policy-makers comprehend these and other

[44]For a discussion of this view, see Stephen S. Baratz and Joan C. Baratz, "Early Childhood Education: The Social Science Base of Institutional Racism," Harvard Educational Review 40(1):29–50. February 1970.

problems connected with evaluative research better than they seem to. It is essential that the social science community understand them. The employment of the language and the style of "science" in policy studies means that the academic social science community has the deepest responsibility for assuring open and critical discussion of both methods and conclusions. While open dialogue cannot solve the above problems, they are inherent in policy research. However, an atmosphere of criticism would appear to provide some safeguard against both oversell and bias.

Since field experiments and evaluations are becoming important determinants of social policy, attempts should be made to assure that an active, intelligent, and critical environment is built into them from the beginning. There are several suggestions that the Committee feels merit exploration: replication of experiments either by having a number of studies conducted in series or, when time presses, concurrently; and division of responsibility for the conduct of an evaluation or experiment between two or more teams so that the knowledge of each would serve as a check against the oversights or misjudgments of the others. At the very least, open access to the data for subsequent recalculation, criticism, and evaluation by independent groups is required. The Office of Economic Opportunity seems to be proceeding according to this ground rule. It is the Committee's belief that the latter practice should become general governmental policy.

POLITICAL PRESSURES ON SOCIAL RESEARCH

At least as important an issue as those of oversell and bias is the danger that policy research will be overwhelmed by political infighting. That public policy research takes place in a political setting is self-evident.[45] The Committee is aware of the intense political climate that surrounded several of the recent OEO-financed studies well before their publication. In both the Head Start evaluation and the negative income tax experiment, political pressures forced release of preliminary reports before the research was actually ready for public scrutiny, and the reports were subjected to acrimonious debate after their release.

The early release of a preliminary version of the Westinghouse–Ohio University evaluation of Project Head Start was related to the preparation of a message by the President to the Congress concerning Head Start and other OEO programs. White House staff, when it

[45]Carol H. Weiss, "The Politicization of Evaluation Research," Journal of Social Issues 26(4):57–68. 1970.

learned of the ongoing evaluation, requested and received from the Office of Economic Opportunity preliminary information on the study as background material for the message. In the President's message praise was given to Head Start for its many side effects, such as increases in medical care. However, the message also indicated that the results of the national evaluation suggested the cognitive effects of Head Start were extremely weak.

Naturally, the Presidential message was denounced by advocates of the program. When its protagonists in Congress demanded and were denied access to the study prior to its publication, charges were made that the OEO was doctoring the report or, worse still, was not going to release it at all. The agency, on the other hand, insisted that the major reason for withholding the report was to assure that all necessary statistical tests were run and that the report was methodologically sound. However, the political pressures were so great that the OEO was required to release a preliminary report. The report was made available to the public a month and a half prior to the intended release date, with many analyses of the data still to be run. It is interesting to note that this report had stamped on each of its pages the words "preliminary draft." A series of newspaper denunciations of the report followed, ranging from statistical critiques to political claims that the report was aimed at doing away with Head Start altogether.[46]

In the negative income tax study, the contract stipulated that the results of the experiment and the data sustaining them would be released only with the consent of the Institute for Research on Poverty. The early release of the NIT data was again in response to a White House request for additional support for the administration's Family Assistance Plan then being debated before the House Ways and Means Committee. The Wisconsin Institute complied, with misgivings and under pressure, on the ground that it would be in the public interest to release data that might have some impact on an issue of great national concern. The first report, drafted by the OEO, was based on the data accumulated during the early phases of the study.[47] The data reported by the OEO were subjected to reanalysis by staff of the General Accounting Office (GAO), which demanded and attained access to aggregate data held in the offices of MATHEMATICA, Inc.

[46]Walter Williams, Social Policy Research and Analysis: The Experience of the Federal Social Agencies, New York: American Elsevier, 1971, p. 111–122.

[47]Office of Economic Opportunity, Preliminary Results of the New Jersey Graduated Work Incentive Experiment, Washington, D.C.: Office of Economic Opportunity, February 18, 1970.

The GAO report criticized the OEO report on the ground that the data from the experiment could not sustain the conclusions drawn from it, a position later rebutted by the Institute in a longer document published under its own auspices.[48] The basic accuracy of the initial analysis contained in the OEO version was, in the main, borne out during testimony before the Senate Finance Committee, but a quick reading of the OEO document might lead to conclusions about the impact of the experiment unwarranted by the data.[49]

These two cases warn of a general problem to be anticipated. Both the NIT and the Head Start evaluation were quite visible, conducted on a large scale, and had objectives that created expectations that their findings might influence decisions on a major policy issue on which there were divided interests and opinions. Two other social experiments currently planned or in process within PR&E—the educational vouchers experiment and the performance contracting experiment—have already been drawn into political controversy. The performance contracting experiment has been attacked because it involves industrial concerns in public education; the vouchers experiment, because it implies public support of private education, among other reasons. The opposition generated by the initial proposal for an educational vouchers experiment indicates that many recognize that such experiments may have a significant role in the development of future public policy.

People interested in a piece of social policy that might be supported or threatened by experiment or evaluation are justified in being concerned with their initiation and conduct. The claim that undue weight may be placed on the evidence they produce is also justified. In part the diminution of these concerns requires the kind of environment of replication and criticism we discussed earlier in this chapter.

[48]Harold Watts, "Adjusted and Extended Preliminary Results from the Urban Graduated Work Incentive Experiment," Discussion Papers, University of Wisconsin, Madison: The Institute for Research on Poverty, June 1970.

[49]That the Wisconsin Institute did not become politically vulnerable because of its rural experiment, but did because of its work on NIT, does not change this point. The NIT data were available when needed, the rural data were not. Further, had difficulties arisen with the rural experiment, the direct relations of the university with its clients (the subjects) might have been seriously jeopardized. The obligations assumed by the university in social experiments are quite important and cannot be dismissed casually. See statements of Keith E. Marvin and John O. Wilson, U.S. Senate, Committee on Finance, Hearings on Family Assistance Act of 1970, Washington, D.C.: U.S. Government Printing Office, 1970, p. 905–975.

The appearance of the General Accounting Office in the monitoring of social experiments and evaluations may confound rather than resolve the problem. The concern of Congress for increased sophistication and competence in this area was made apparent in the course of testimony before the Senate Finance Committee on the GAO's evaluation of the New Jersey experiment. Public Law 91-510, passed in November 1970, authorizes the Office of the Comptroller General to "review, analyze, and evaluate ongoing programs of the government, including the making of cost benefit . . . and other analytical studies" and "to make analyses and reviews of legislative proposals and alternatives to such proposals, including . . . the long-term costs and benefits thereof."[50]

The significance of this law should be plain to social scientists operating under a contract with a government agency. The researcher must envisage the possibility that, at an early stage in the development of his study, employees of the GAO, or its consultants, will be looking over his shoulder, developing the bases for a continuing critique of the study. This kind of oversight by Congress through the Office of the Comptroller General may be detrimental to the development of high-quality work in this field. The threat of investigations of this sort, the Committee believes, might discourage investigators of high caliber from involving themselves in such policy-related work. The Committee also believes that the pressure by agencies or the Executive Office to provide results of evaluative work early in the process of research may have the same effect.[51]

THE KEY FINDINGS

While this report has focused on a particular organizational relationship (between the Institute for Research on Poverty and the OEO's Office of Planning, Research, and Evaluation), much of what has been

[50]Public Law 91-510, October 26, 1970, 84 Stat. 1168, Sec. 204.

[51]Legislation introduced in the 91st Congress, 2nd Session, but not passed, would give the Comptroller General certain subpoena powers regarding materials and records of government contractors or grantees. For evaluative and experimental activities, this could mean that not only aggregate but also individual data could be subpoenaed, and concern has been expressed that, if this were to occur, the confidential relationship between the researcher and his subject might be threatened; the researcher would be unable to guarantee anonymity. See report of the Committee on Government Operations on S.4432, "Subpoena Power," Report No. 91-1264, Calendar No. 1282, October 2, 1970, p. 21.

said has wider implications for the organization and funding of policy research and for the future role of social scientists in that field. In what follows, we have attempted to summarize those broad implications, restating them in a way that gives structure to the rest of our major findings.

First, even though research by social scientists has long been an important influence on certain kinds of government policies, the relationships between research and policy-making remain complex and diffuse. In recent years, attempts have been made to neaten and sharpen those relationships. Government agencies have established special offices to do policy analysis and to support policy research. Policy analysis has been made a formal component of the budget-making process. Various nongovernmental organizations have been funded to do research aimed at particular policy issues for particular government agencies. The relationship between research and policy-making probably is not only more important now than it was before but also more direct. However, the role of research relative to other factors impinging on policy should not be overestimated, nor should one adopt an overblown view of the value of research in guiding policy. In cases in which policy judgments must be made, social science research is able to provide only very limited illumination.

Second, significant changes in agency perspective, regarding policy research, have occurred over the past decade. As mission agencies have become increasingly disenchanted with the ability of research to map alternatives and to evaluate them before programs are actually tried out, increased emphasis has been placed on program evaluation and, where possible, on social experimentation. Faith in the value of fundamental research and in the power of social science theory has been eroded. Government agencies financing external research are now specifying the details of that research to a greater extent than before. As task-order research has increased, government willingness to provide relatively unconstrained funding to research institutions has diminished.

Third, while there are good reasons for this changing funding emphasis, a great deal should not be expected to flow from it. There is a proclivity to think that the solution to social problems, such as poverty, would be facilitated by careful, clear projection of policy objectives, program alternatives, and underlying economic and social forces, proceeding toward a solution through experimentation and feedback guided by theory and analysis. This ideal image of policy research is both useful and highly misleading. The design of good policies is very different from systems engineering. Most social problems are hard partly because it is virtually impossible to analyze them neatly; they are inherently complex. Policy objectives tend

to be diffuse and difficult to quantify. Program mechanisms are intricate and not easily controllable. Success is difficult to evaluate. While social science can help in the design and improvement of programs, good policy-making requires art, judgment, and weighing of conflicting values. It can never be reduced to engineering in the familiar sense of the term.

Fourth, social experiments and evaluations in many cases can help improve public policy. But in many cases the results will be ambiguous and subject to a variety of interpretations. Often what we will learn from experiment is that the problem is poorly understood. It is highly important to have multiple and independent sources of judgment and ideas. To achieve this, the new emphasis of government agencies in financing evaluations, experiments, and specific applied research projects must be complemented by considerable support of independent research conducted in an environment in which policy issues and governmental perspective are clearly understood, but the researchers have considerable freedom regarding what they work on and how, and what they say. Independent research is needed across the spectrum from very fundamental work to work quite closely focused on current policy issues. The Committee believes it extremely important that government mission agencies not abandon funding of such research. Funding by the traditional research support agencies is not a complete substitute from the point of view of either the government agency or the researcher. The mission agency loses the contact with the independent researcher that such funding provides. The researcher loses the contact with the thinking of the policy agency. The result is a sharp split between independent and task-order research, which is a hindrance to the development of both theory and practice.

Finally, university-based policy institutes would appear a good locus for this kind of work and can serve important educational, as well as research, functions. There are many forms that such institutes might take and many trade-offs to be considered. A balance needs to be struck between autonomy in hiring and teaching and useful linkages with the traditional academic departments. Similarly, there must be some balance between independence and interaction with government mission agencies. Perhaps the sharpest constraint on the evolution of policy institutes at universities will be the availability of long-term, relatively unconstrained funding. University institutes, or any institution with the capability of doing first-rate independent research, cannot live on hand-to-mouth financing. Much more thought needs to be given to the nature of the financing of policy research institutes, the nonprofits as well as the universities, than appears to have been given up to now. At present, most of these institutions are operating in, or close to, financial crisis.

Policy research and university-based institutions to perform it are relatively new phenomena. Increased government demand for this type of social science contribution promises to influence many aspects of the research scene. The Committee has entered into this rapidly evolving field for a brief moment. Its findings may very well be outdated by the time this report is published, but we do not think so. A continuing assessment and monitoring of the developments we have considered is surely needed, and we hope it will be provided.