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# 8 Social Science Analysis and the Formulation of Public Policy: Illustrations of What the President “Knows” and How He Comes to “Know” It

Ernst W. Stromsdorfer

All projects and programs are of course evaluated, with more or less accuracy and effectiveness, as decisions are made to continue, terminate, or redirect various activities. . . . How best to do the evaluation, what skills are needed by the evaluators, and what specific questions need to be answered for local, State and Federal purposes—these are problems that have not been fully resolved, and to which differing views and experience combine to give quite different answers.

—*Education and Training: Opportunity Through Learning*. Ninth Annual Report of the Secretary of Health, Education, and Welfare to the U.S. Congress on the Manpower Development and Training Act.

## 8.1 Introduction

This study analyzes the manner in which social science analysis is developed and utilized by legislators and policy makers to formulate social policy. The initial charge for this study was to determine whether information about social and economic behavior was more likely to be used in policy development if it was developed from a classical experimental as distinct from a quasi-experimental method of analysis. Based on the way policy makers behave, this charge proved to be too

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narrow a focus. While it is certainly true that data on behavior derived from properly designed social experiments are more believable and do allow unambiguous assertions of cause and effect, it requires more than random assignment to a treatment and control group to make experimental data *usable* and *reliable*. This is borne out by the discussion of Dennis Aigner on residential electricity time-of-use pricing experiments and by experiments such as the National Supported Work Demonstration which, while yielding positive employment results for welfare women, still has a variety of methodological problems that reduce the reliability of the results and constrain its application (Masters 1980).

After reading commentary on the policy-development process and inspecting policy and budgetary documents and evaluations of actual social programs, it has become obvious that policy makers, while not totally subjective and nonrational, will use whatever data are at hand to support their case, regardless of the methodological purity by which it has been developed. Canons of scientific evidence are not ignored but are applied selectively. Taste or preferences for certain methods, such as the case-study approach, are as much determinants of what data are used as is any perceived methodological purity or rigor. Furthermore, the same person or agency, when evaluating two programs whose, say, economic effect on state and local governments is the same, is capable of using evidence quite selectively to support one program and reject another. As pointed out below, the cases of Public Service Employment (PSE) and educational block grants to state and local governments are an example of this interesting bit of policy rationalization. The phenomena of substitution and displacement are used to reject the former program while they are not mentioned for the latter where they also operate fully.

As a result, the initial question posed for this paper is probably not as interesting as is the general question of analyzing how information is generated and used. Thus, I shall instead discuss the following types of questions:

- To what extent does information lead to policy formulation or change?
- What is the context of the application of research to policy?
- Does analysis precede or follow development of policy?
- Does analysis ever account for the variance or change in policy formulation and application?
- How are research methods and the resulting information constrained by the political process?
- Under what conditions do research and analysis appear to have no impact?

The analysis of social policies and programs will always receive a weight of less than unity in policy development, given the complex nature of the political development of a program. Also, the results of analysis will sometimes be used in ways and for purposes that are not entirely

consistent with the original objectives of the analysis. Furthermore, the political process will often dictate that some kinds of analysis simply cannot be performed, or if performed, the research must be carried out in a fashion inconsistent with the most appropriate scientific method. In fact, I would argue that the above phenomena are the rule rather than the exception in the conduct and application of social research. And finally, research based on a range of methods varying in their appropriateness to the specific problem at hand will be found to exist side by side in a given agency charged to provide information to policy makers. I do not really intend to paint a pessimistic and cynical picture of this process. Some information, if founded on an understanding of the policy in question, is better than none at all. And if one route of analysis, such as a benefit-cost analysis, is cut off, it is often possible to use alternative routes, such as an analytic treatment of program process and service delivery. Indeed, although we might prefer to think otherwise, a complex or expensive analysis of a program or policy is not always needed, especially if there is little or no conceptual development to guide the execution of such work. It has been the case that simple cross-tabulations of salient program data have been sufficient to effect major changes in programs. The Public Service Employment component of the Comprehensive Employment and Training Act (CETA) was significantly revised using such simple data. At the same time, it is depressing to recognize that occasionally the U.S. Congress will expressly prohibit the use of public funds to carry out certain types of analysis. For example, section III(b) of CETA prohibits use of CETA funds to conduct research on the subminimum wage. Classical experiments are also illegal with respect to analyzing the Employment Service and the Unemployment Insurance programs—at least as of 1977.

However, this same adversary relationship, given the existence of our open government and the canons of scientific evidence our system subscribes to, forces or induces the execution of research that is politically unpopular. The case discussed below of substitution and displacement within the PSE program is instructive of this. Highlighted by the work of George Johnson, this issue generated an incredibly heated and sometimes acrimonious debate within the Department of Labor throughout the late 1970s (Johnson and Tomola 1977). Nevertheless, the Department of Labor did fund a demand-side study in the Employment Opportunity Pilot Projects to study this phenomenon in the private, private nonprofit, and public sectors.

### 8.1.1 Plan of the Chapter

Before moving into the main body of the discussion, it is useful first to discuss the production of knowledge as it is generated by and for government and the general ways it is utilized by government.

Following this, the study will discuss examples of social research that

have had an impact on public policy and other examples that have not. For both types I will describe cases where the research was on the cutting edge, initiating debate, and other cases where the research followed debate.

## 8.2 The Production and Use of Information

Analysis of this issue requires that the process of translation of analytical results into social policy be broken down into two broad components: 1) How is knowledge produced? 2) How is it utilized? Each of these two questions requires further breakdown.

### 8.2.1 Knowledge Production

The production of knowledge occurs through at least three processes:

1. *The production of management information-system (MIS) data.*

Such MIS data can be classified into three general types:

- a. *MIS data developed within a behavioral context*; data on program inputs which can be related to program output. Such MIS data are generally rare, mainly because it appears to be beyond the capacity of bureaucracies to produce it.
- b. *MIS data not developed within a behavioral context*; situations where measures of input or output only are collected and cannot be related to their respective output or input. Most commonly, measures of input only are collected. When such data are used in policy development, output is assumed to be equivalent to input. Much educational-policy data are of this type, e.g., more teachers imply better education.
- c. *MIS data that measure neither input nor output directly*, for instance, simple counts of people receiving a broad, undifferentiated program treatment, such as the number of people covered by a new law. Program data on the federal minimum wage comes directly to mind. Depending on whether one is an opponent or proponent of minimum-wage legislation, legislation that implies wider coverage is worse or better.

The conditions under b and c above tend to arise out of two general contexts. First, the most common, most programs are not designed with the purpose of discovering their effectiveness. They are generally passed by Congress on the consensual assumption that they work. A major example is Title I of the Elementary and Secondary Education Act of 1965 (ESEA) or the (extinct) Neighborhood Youth Corps (NYC) (Rivlin 1971, 80). Second, it is intended that programs not be analyzed in terms of their behavioral impact. A good example is the Unemployment Insurance program, which wasn't analyzed behaviorally until the early 1970s; over the parallel time period, the Manpower Development and Training

Act (MDTA) and CETA both had an evaluation component built directly into them. In fact, the MDTA was the major catalyst for modern evaluation of social programs in the U.S. Department of Labor.

In general, the more political support there is for a program, the more limited will be the available systematic information on that program. The old National Alliance of Businessmen JOBS program is a case in point. In contrast, a relatively unpopular program, such as Job Corps during the Nixon administration, was required regularly to report very detailed *cost* data, though the first relatively valid study of Job Corps *benefits* did not occur until 1977 (Thornton, Long, and Mallar 1980).

2. *The production of knowledge through natural or quasi experiments.* Here, two general approaches are discernable:

- a. *Natural or quasi experiments that attempt to model an existing program's process and estimate its effect through econometric or other means.* All evaluations of the federal minimum-wage program and the Unemployment Insurance System are of this class.
- b. *Natural or quasi experiments in the form of (more or less) carefully designed demonstration projects.* Dozens of examples exist here, such as the Youth Incentive Entitlement Pilot Projects (YIEPP) or the Minnesota Work Equity project, both of which are subsidized employment and training/education programs, the former for disadvantaged youth and the latter for welfare clients. Following Rivlin, this form of analysis can be through either systematic development or through more or less unfocused innovation. The Youth Act of 1977, with a \$1.1 billion combined research and program component, had both systematic development—the Youth Incentive Entitlement Pilot Projects—and unfocused innovation—the bulk of the act and its resources.

3. *The production of knowledge through classical experiments* wherein there is random assignment to treatment and nontreatment groups, thus allowing assertions of cause and effect to be made. Examples are the National Supported Work Demonstration, the Seattle and Denver Income Maintenance Experiments (SIME-DIME), and the Housing Allowance Demand Experiment.

Finally, a fourth form of knowledge production is possible: knowledge that arises in the form of untested but testable hypotheses through independent theoretical development. The human-capital revolution, and its extensive application in the War on Poverty, is in part an expression of this phenomenon.

### 8.2.2. Knowledge Utilization

Not only are there diverse forms and qualities of information production, but decisions based on such knowledge are also diverse. Given the

political drive to develop a given policy, it is the general case that any data at hand and supportive of the case will be used. Policy decisions are made on the basis of:

1. *No data or information at all but rather faith, bias, or political desire.* Someone wants something and has enough votes to get it. The decentralization of CETA is a straightforward example. This is discussed further below.
2. *Hypotheses suggested by impressions of regularities in data associated with a social problem.* Title I of the ESEA had this characteristic. Consider the following:
  - a. Educated people are less likely to be poor—a datum.
  - b. Children from poor families tend to perform badly in school—a datum.
  - c. Therefore, provide poor children with compensatory education and you will break the cycle of poverty—a hypothesis to be tested; only, as Alice Rivlin points out, ESEA wasn't set up to test the hypothesis (Rivlin 1971).

The Neighborhood Youth Corps was developed at about the same time and in a similar way. Consider the following:

- a. Rich kids are less likely to drop out of school than poor kids—a datum.
  - b. The drop-out rate of poor kids is directly related to the business cycle—a datum.
  - c. Therefore, give poor kids a job while they are in school and they will be less likely to drop out due to the opportunity costs of staying in school—a hypothesis to be tested. However, the program designers assumed the hypothesis was not rejected and proceeded to set up the NYC. Subsequent tests showed no effect on schooling retention (Somers and Stromsdorfer 1970).
3. *Hypotheses tested by data from natural experiments.* The Public Service Employment components, titles II-D and VI of CETA, have been under attack for several years in Congress and are currently scheduled for elimination, largely due to the econometric analysis performed to measure substitution and displacement. The argument that substitution reduces PSE effectiveness is directly employed both by the Congressional Budget Office (U.S. Congress, Congressional Budget Office 1981) and the Office of Management and Budget (U.S. Office of Management and Budget 1981) in suggesting budget cuts for the FY82 federal budget. For instance, the OMB argues that: "Charges that PSE is primarily a subsidy supporting State and local services are based on experience in the rapid 1977-78 build-up of Title VI and may not be as true today as then. . . . Independent estimates of the proportion of PSE jobs that

are substituted for regular State and local jobs range from a high of 90% (after three years) to a low of 20%.”

Two observations are in order here. First although substitution undoubtedly exists and is probably high—consensus seems to fall in a range from 40 percent to 60 percent at this time—neither of the studies referred to from which the above numbers are taken are reliable. (Johnson and Tomola 1977, for 90 percent and Nathan et al. 1979, for 20 percent; see Borus and Hamermesh 1978 for a critique of these results.) Next, the Reagan administration, while objectively rejecting PSE due to the substitution phenomenon, does not mention the fact that the noncategorical block grants proposed for education are almost pure revenue sharing and, therefore, should result in 100 percent substitution (U.S. Office of Management and Budget 1981). Thus, one begins to wonder about the basis upon which research and analysis results are applied. The persons in question apparently also engage in this selective use of analysis in good faith and apparently are unaware of (or uninterested in) the inconsistency in their thinking and practice.

4. *Hypotheses tested by data from classical experiments.* Classical experiments are relatively new to the scene as well as rare in the area of social and economic behavior. With one or two exceptions, there are no classical experiments of existing Department of Labor (DOL) programs. In fact, as noted above, for the “old” DOL programs, such experiments are illegal. Thus, all of the policy development in the DOL has been based on quasi experiments as well as intuitive or impressionistic methods.

Where experiments have been conducted, we have had to undergo a considerable learning process. Experiments are not simple to design. Aigner rejects five of the fifteen residential electricity time-of-use pricing experiments as not useful. (Aigner, this volume). With respect to the negative-income-tax (NIT) experiments, only SIME-DIME appears to be highly reliable and usable. The Rural NIT Experiment appears to be useless. The National Supported Work Experiment’s (Masters 1980) positive results for subsidized employment for welfare women are encouraging but are qualified because, in particular, substitution and displacement are not netted out of final program impact. The National Supported Work Demonstration employs a random assignment to an experimental/control group. Through closely supervised work experience in a supportive peer-group environment, the program seeks to improve job skills per se and change personal behavior such as work habits and motivation. AFDC women, drug addicts, ex-offenders, and disadvantaged youth represent the four treatment groups. The treatment period is constrained variously to twelve to eighteen months. The AFDC women had to volunteer for the program. Thus, this study group repre-



sented less than 18 percent of the AFDC population. Masters reports positive results on employment, though unsubsidized private-sector effects were not as large as hoped. The study is strongly indicative but not conclusive that such a program can reduce welfare dependency. The positive results are concentrated in just a few sites; there is apparent interaction between treatment and site effects; and, as noted above, there is an undertermined amount of displacement and substitution (Masters 1980). National Supported Work Demonstration represents our only experiment of subsidized public-sector employment. It is not clear that the Reagan administration even knew of the results before it moved against PSE. And, had the administration known, such knowledge may not have made any difference in its desire to eliminate PSE, since the evidence, while positive, is limited in scope.

Finally, the Housing Allowance Demand Experiments show a minor impact of housing allowance on increased consumption of housing, yet there is no attack on this program in the OMB budget document (U.S. Office of Management and Budget 1981) nor does the CBO budget document discuss this program (U.S. Congress, Congressional Budget Office 1981).

In short, a variety of experimental data exist, but they are of uneven quality and, in addition, they are not used consistently.

We turn now to a more detailed discussion of the use of research with respect to specific social programs.

### **8.3 Research with an Impact: Initiating Debate**

Within the past few years policy research has initiated debate and forced the consideration of issues that would otherwise have received less attention by Congress and the several administrations. Examples of such issues are welfare reform, unemployment insurance, and Social Security.

#### **8.3.1 Welfare Reform**

The reform of welfare is a perennial policy issue. The basic concern with welfare services has been the presence of too many and too complex categorical aid programs whose eligibility requirements and cumulative tax rates have led to considerable horizontal inequity (unequal treatment of equals) and reduction in the work incentive. Milton Friedman, James Tobin, and others proposed substituting a negative income tax for the existing set of categorical aid programs. This idea was timely and struck a responsive chord among academics, government administrators, and policy formulators. As a result of this interest, a set of classical experiments was developed to test the impact on labor-market and other social and psychological behavior of differently structured income-maintenance programs. Experiments were conducted in New Jersey and

Pennsylvania; Gary, Indiana; Seattle, Washington; Denver, Colorado; and rural counties in Iowa and North Carolina.

Since the concern over the work-disincentive effects of welfare was high, initial analysis of the data from these experiments centered on estimating individual and family labor-supply behavior. Results based on three years of experimental treatment indicate that the labor supply of married white males with wives present in the household may be reduced by as much as 5 percent in response to a modest income-maintenance program. For white wives with husbands present in the household, labor supply may be reduced by over 20 percent (Robins and West 1980). The income and substitution elasticities of this study have been used in developing recent welfare-reform proposals to estimate the demand for low-wage PSE jobs and structured job search.

The five-year treatment shows larger labor-supply reductions and will most certainly solidify resistance to the NIT among the new administration (as well as others), though at present the administration's rejection of an NIT seems to be less due to the work-disincentive effects as due to the dilemma posed by the interdependence of the welfare tax rate, the minimum-income guarantee, and the income cutoff and the resulting budgetary cost implications (Anderson 1978, 1980; Stockman: Proposals for welfare reform 1981).

However, a propos the way in which experimental (and other) data are interpreted, we should note that, based on the results from the New Jersey experiment, the following policy implications were initially proffered:

First, public opposition to coverage of all intact families by an income-related case transfer program—to the extent that such opposition is based on fear of large reductions in work effort—should decrease. Second, the concern of policy makers about the disincentive effects of particular tax rates and guarantee levels should diminish. . . . Third, the case for a work test in an income-related case transfer program covering intact families is weakened. (Barth, Orr, and Palmer 1975)

For a time, these attitudes probably did prevail. However, with the hearings held by Senator Moynihan in 1978, the previously “low” rates of labor-supply reduction were now seen to be “high” (Lynn and Whitman 1981). And the focus on a work test is paramount in the Reagan administration, whether one is discussing food stamps, AFDC, or unemployment insurance.

Workfare, in several forms and emphases, has been the polar alternative to the NIT. Versions of a strong work test and provision of PSE jobs were incorporated in the Family Assistance Program under President Nixon as well as proposals in discussion during the Ford and Carter administrations. Rejecting the subsidized-employment component of

past workfare proposals, the Reagan administration is strongly fostering work tests for AFDC, food stamps, and the unemployment insurance program, (U.S. Office of Management and Budget 1981; "Donovan; Jobless Need New Careers," *Boston Globe*, 26 February 1981), without being fully aware that this work-test requirement is observed in the breach and significant enforcement of it has nontrivial budgetary costs. (The OMB document does note that projected cost savings do not net out enforcement costs.)

Currently, the Department of Labor is testing workfare or work-conditioned welfare reform in Minnesota, the first direct test of the guaranteed-jobs component of the Carter welfare-reform package, though the Supported Work results are strongly suggestive of the probable direction of effect (Rodgers et al. 1979). New Community Work Experience Program demonstrations are now being proposed by the Reagan administration to test out a decentralized local state-oriented workfare and training program for AFDC recipients.

The Employment Opportunity Pilot Projects (EOPP) currently underway is also a variety of workfare. It has an even stronger test treatment of structured job search. At this time some sites are placing almost 50 percent of their eligible clients in jobs, though there appears to be wide variation in these placement rates among pilot sites (ASPER/DOL officials, discussion with the author, December 1980).

### 8.3.2 Unemployment Insurance

Historically, the analytic focus of the administrators of the unemployment insurance (UI) system has been on the establishment of the optimum benefit level, mainly from the standpoint of the adequacy of wage replacement. A work test, often casually or indifferently administered, is the principal technique whereby the government reflects its concern over the potential work disincentive effects of this income-transfer program. In the wake of the 1975 recession, with benefit payments approaching \$19 billion in fiscal year 1976, concern was expressed in some areas of the administration and in Congress over the effect on the aggregate unemployment rate of the weekly benefit level and the extension of benefits to a maximum of sixty-five weeks for eligible individuals. Economists in several branches of the Department of Labor funded a variety of studies to measure this impact; independent researchers conducted their own studies (Katz 1977). While the data in these analyses were faulty and the econometric techniques used to overcome these data problems were often inadequate, the analyses did indicate that the behavior of insured workers had several effects on the measured unemployment rate. For example, the duration of unemployment among UI recipients appeared to increase as benefits and their duration increased. Daniel Hamermesh argues that the best estimate is that a 10 percent increase in weekly

benefit amount will increase an individual's unemployment by about one week. The overall effect of the current program on the civilian unemployment rate is to increase it by about half a percentage point (Hamermesh 1977). Overall, however, it was probably not the particular point estimate that was as significant in influencing policy on the UI system as was the breakthrough in this new way of looking at the UI program and the determination of the direction of effect.

Concern over the employment-disincentive effects of the program, particularly problems of financing the state trust funds and determining the adequacy of UI tax rates, led to a revision of the UI legislation in the fall of 1976. However, the only efforts made to reduce disincentives to work in this legislative revision were denial of summer benefits for school employees with contracts for the forthcoming term and for school employees with a reasonable assurance of postvacation employment, and reduction of unemployment-compensation benefits for retired individuals by the amount of any public or private pension based on a claimant's previous employment.

Interesting enough, the current OMB recommendations, while attacking the Unemployment Insurance Extended Benefits Program, do not focus on the possible disincentive effects of the program except to argue for a more stringent work test for extended benefit claimants (U.S. Office of Management and Budget 1981). Recent announcements by the Secretary of Labor suggest a major refocus of UI, however ("Donovan: Jobless Need New Careers," *Boston Globe*, 26 February 1981), and current legislative proposals for UI can be viewed as implicit recognitions of the disincentive effect.

### 8.3.3 Social Security

Evaluation research has had an interesting impact on the interpretation of the effects on labor supply of increased Social Security payments, marginal payroll tax rates, and changes in eligibility for retirement. From 1947 to 1974 the labor-force participation rate of men aged sixty-five and over dropped from 48 to 22 percent. Beneficiaries within the old-age and survivors' component of the Social Security Administration (SSA) rose from 15 to 67 percent of the total number of people aged sixty-five and over during the same time period. Studies conducted by the SSA staff as benefits were increased or extended concluded that almost all persons retired involuntarily as a result of bad health, difficulties in finding a job, or compulsory retirement age. The basic method used in these studies was a direct questionnaire—people were asked why they retired. Since American society places considerable emphasis on the value of work as well as the social and personal obligation to work—the secularized Protestant ethic—it is not surprising that people, when asked why they retired, cited illness or business downturn (i.e., socially acceptable

reasons) rather than the financial and leisure opportunity associated with retirement (Munnell 1975).

In contrast to this method of analysis, the approach of the economist is to look for evidence of revealed behavior; that is, the focus is on measurement of actual behavior and the interaction among variables affecting this behavior, rather than on direct query concerning motives and actions. Studies using economic models that include variables representing the benefit and eligibility characteristics of the SSA system find that these variables have a large economic effect on retirement decisions. Indeed, as might be expected, there is an interaction between health and financial factors (Munnell 1975). This recognition of the voluntary nature of the retirement decision in response to increasing benefits has brought into sharper focus the financial problems of the Social Security system. The current administration is discussing changes in the retirement age, and the disability insurance (DI) component of SSA is under OMB revision and challenge (U.S. Office of Management and Budget 1981), perhaps due to OMB awareness of the above. Finally, Congress has mandated that an experiment be conducted of the disability insurance component to determine ways to reduce the drain on the DI funds. (*U.S. Code Congressional and Administrative News* 1980).

#### **8.4 Research with an Impact: Following Debate**

The best example of evaluation research that followed the lead of policy development is in the area of training and retraining programs for both prime-age workers and youth. These programs reflect the strong belief in American society that education is the key to economic growth and the assumption, though basically unproved and unmeasured, that there are large external benefits from any type of education. Also, training programs appealed (although for different reasons) to both conservative and liberal elements in Congress and the administration. Such programs were passed with minimal political and intellectual controversy, and as a result the major evaluations of such programs came after the fact of policy development and revision. Almost every one of the evaluations of training and retraining programs have been case studies impaired by selectivity bias, lack of proper control groups, nonresponse bias, and insufficient follow-up period. Over time, however, these studies suggested that there were positive and statistically significant effects on earnings large enough, under reasonable assumptions for a social discount rate and earnings projections, to cover program costs. Nevertheless, one cannot generalize from these case studies. The inconsistency in earnings benefits across studies, linked with the growing realization that training cannot create jobs in a period of cyclical unemployment, led to a disenchantment with training as a panacea for unemployment; thus,

beginning in the early 1970s the view that retraining “did not work” became prevalent in government circles and among some academics, even though the basic evaluation results did not change (Aaron 1978a, 65). Indeed, it was in the mid-1970s that fairly accurate data on national samples of prime-age trainees and nontrainees were generated from SSA earnings records. Sophisticated econometric analysis of these records indicated that for males benefits averaged from \$150 to \$500 per year in the period immediately following retraining. The “decay rate” in those earning benefits was estimated to be about 15 percent per year. For women the benefits were between \$300 and \$600, with no apparent decline over the five-year period following retraining (Ashenfelter 1978). Given costs, the impact appeared to be marginally efficient.

However, the presumption that training did not work led to a major shift from a categorical-program approach to the provision of a unified set of services, under a decentralized program, that prime sponsors would employ in various combinations that would be more efficient and equitable. But this program change was made in the absence of reliable analysis.

To verify that the categorical training program did not work would have required a complex set of cost-benefit analyses for different types of program treatments, as well as training courses administered to different sociodemographic target groups. Such evidence simply did not then and does not now exist. Nevertheless, the decision to decentralize the programs prevailed. The conditions whereby this decentralization would have improved the effectiveness of training were not met, however. Namely, the detailed labor-market information necessary to specify the appropriate training programs was no more available under CETA than it was under MDTA. Decentralization reduced federal control, a positive gain given the mood of the period, but it surely did not increase program efficiency. At this time an elaborate longitudinal study of a national sample of trainees and other program participants is underway in the DOL. However, it is not designed such that one can test if decentralization has made any difference in program effectiveness. There is only one possibility for a crude test—to replicate the Ashenfelter analysis with SSA earnings data for the CETA period and compare the MDTA and CETA period results.

### **8.5 Research without an Impact: Initiating Debate**

Retraining programs, job counseling, and other services for the cyclically and structurally unemployed gradually fell into relative disfavor during the early 1970s, though, as noted above, in the absence of reliable supporting evidence. The new hope for alleviating unemployment became public-service employment, first authorized under the Public Em-

ployment Program of the Emergency Employment Act of 1971 and made permanent in 1974 as the Public Sector Employment Program under CETA.

Two conditions have to be satisfied in order for PSE to be a socially effective method for alleviating unemployment. The more important of the two is that unemployed workers who are put to work on subsidized public-sector jobs must not displace other (similar) employed workers in the public or private sector or be used to perform work that would have been performed in the absence of the PSE program. Second, the output produced by PSE workers should have some positive social value.

George Johnson, who was then Director of Evaluation in the Office of the Assistant Secretary for Policy, Evaluation, and Research, U.S. Department of Labor, pointed out that the first condition was not likely to be fulfilled. In short, displacement would not only occur, it would occur very quickly; even when state and local governments were not in fiscal straits, they had every incentive to substitute federal funds and PSE workers for their own fiscal effort. If they were in fiscal straits, their regular employees, after a period of lay-off, would be rehired as PSE workers. Such behavior was endemic in New York City, for instance. Congress was not ignorant of the fact that such displacement might occur. Indeed, "maintenance-of-effort" clauses are consistently written into laws like CETA that have a revenue-sharing component. The error of Congress lay in believing that such legalisms could be enforced. To detect the extent of fiscal substitution requires a complex data set beyond the capability of local governments to provide or the federal government to finance and monitor. Both the General Accounting Office and the program auditors of the Department of Labor became cognizant of the great difficulty in measuring the degree of displacement or substitution so that maintenance of effort could be legally enforced. Ultimately, the Department of Labor auditors simply ceased to concern themselves with the problem.

Paradoxically, during the Ford Administration displacement was used as an argument *against* the expansion of PSE and *for* the expansion of conventional training within CETA. The advocates at that time did not recognize or discuss the fact that under a revenue-sharing program such as CETA, displacement or failure to maintain effort applied to any program so funded—training as well as public-service employment. And, of course, the fascinating behavior of the Reagan administration with regard to this substitution issue has been mentioned above.

With the rebirth of interest in welfare reform under Carter, PSE gained a new lease on life as an integral part of a workfare program. However, the displacement issue was still a nagging concern. Widespread displacement could eliminate the effectiveness of any long-term effort to combat the structural unemployment implicit in the welfare problem. With displacement, no net reduction in overall economic dependency need occur;

churning in the unskilled sector of the labor market may be the only result. This phenomenon strikes at the heart of section II-D of CETA which is aimed at alleviating long-term structural unemployment.

### **8.6 Research without an Impact: Following Debate**

The most unfortunate recent example of social-program research and data collection that has had no meaningful impact on policy debate is the analysis of the Occupational Safety and Health Act of 1970 (OSHA). At present, the Bureau of Labor Statistics expends 4.7 million dollars annually to collect industrial accident statistics for OSHA. These data are used to help target safety inspections more efficiently. However, they are deficient in helping evaluate the net effect of OSHA safety inspections, and little of this expensive effort has helped clarify the policy cacophony that embattles this potentially useful program. Indeed, while there is a variety of data sets on industrial accidents and injuries in existence, such as state workers' compensation data, these data could not provide systematic information to guide congressional debate during the formulation of the act, and such systematic information still does not exist. The proposed Schweiker Amendment to OSHA (*Congressional Record* 1979), for instance, referred only to aggregate time-series data on nationwide accident rates as suggestive of the "failure" of the OSHA program. With a few notable exceptions (Smith 1979), only limited analysis of OSHA impacts exists. The Occupational Safety and Health Administration did not have a coherent research and evaluation plan between 1974 and 1977, even though this was a requirement in the annual budgeting process. During that time the OSHA administration was making no concerted effort to discover the net social impact of its policies. It was only ineffectively pressing forward selected health standards supported by ill-designed and ill-executed economic impact statements. Even so, by 1979 a total of approximately one billion dollars had been spent on the administration of OSHA (Bolle 1980). Even allowing for the extreme politicization of the agency, given the above costs plus the social costs imposed by the program on industry, it is difficult to understand that a more coherent and determined effort was not achieved to measure the effects of this program and collect more meaningful management-information data.

### **8.7 Concluding Remarks**

It is clear that social research has a considerable impact on policy formulation and execution, but not exactly in the way a fastidious scientist would prefer. It is also true that political factors define or inhibit what will be analyzed with public resources and what will be applied in any



practical political context. In some cases constraints are such that it is not possible to collect appropriate descriptive data, much less to perform a critical analysis of the program in question.

Lest I overemphasize the negative side, however, let me assert that program analysis and evaluation can be an extremely valuable policy tool. In particular, I have come to believe that analysis designed to reveal the administrative and operational process of a social program can be extremely valuable. As most social scientists who become members of government discover, program processes are not clearly thought out or understood by the people who define and operate them. The Trade Adjustment Assistance Act, for instance, which is predicated on the reasonable assumption that those workers ought to be compensated who lose their jobs due to structural economic change, e.g., auto workers, has been found to be very difficult to administer since the true losers and how much they lose cannot be properly identified. Providing understanding ahead of time of how a program might work through process analysis can render an invaluable practical, as well as social service when done correctly. A basically sound or deficient program can often be discovered by an effective process analysis. The installation of an effective work test is a prime example where such a process analysis can be helpful, and, the avoidance of the economic and political costs of TAA, discussed above, is another example.

And, finally, as the level of expertise increases within the bureaucracy and Congress, more attention and resources can be paid to formulating efficient MIS systems and systematic experiments and less on shot-gun approaches to program analysis as characterized by the ESEA of 1965 and the Youth Act of 1977. Gradually, experimental analysis just may gain an ascendancy which it most certainly does not now have.

## Comment      Henry Aaron

Holding a conference on social experiments in today's budgetary climate is rather like holding a conference on dirigible flight a month after the Hindenburg disaster. One cannot escape a feeling of unreality.

I shall divide my comments on David Mundel's and Ernst Stromsdorfer's papers into three parts. The first is a summary of each of the papers. The second consists of specific comments on the two papers. The third part is a collection of comments inspired as much by the subject the papers address as by the papers themselves and in fair degree by the desire to provoke. I fear you may find them provoking rather than provocative.

Mundel poses a series of questions about social experiments: Can experiments answer questions important to policy makers? Can the answers be understood? Can the answers alter the beliefs of policy makers?

The first question is divided into four subquestions. Can social experiments identify problems? Can social experiments pinpoint who has a problem and whether it is important? Can social experiments assist in the implementation of policy? Can social experiments identify the behavioral consequences of policy? Mundel answers no to the first three of these subquestions and yes to the fourth, qualified by the observation that other possibly more effective and certainly cheaper instruments are available.

The second major question concerns whether the results of social experiments can be understood. Mundel concludes that experiments can be designed to yield comprehensible results, but often they are not because the experiment is too complex or the experimenter too unclear.

The final question—can results alter the beliefs of policy makers?—also receives a qualified yes, if the experiment is simply designed and undertaken early enough in the process of policy development.

Mundel does not state whether any experiments undertaken to date have met his standards and fulfilled the rather limited potential he adumbrates, but he implies that the prospects are poor for social experiments in the future.

Ernst Stromsdorfer has attempted to draw specific lessons from a variety of social experiments, research projects, and evaluations. His theme is not so much social experimentation as it is the manner in which information, in general, has influenced policy and the generalizations that this experience supports. He stresses the fact that, willy-nilly, most policy research, experimentation, and evaluation comes sooner or later to smack of what Alice Rivlin in her review of Christopher Jenck's *Inequality* called "forensic social science." In the policy forum, social science research is "evidence" to be introduced as part of a frequently multifaceted adversary process in which each side builds its case to make it as persuasive as possible.

Stromsdorfer begins by placing knowledge production relevant to policy into a variety of categories: data produced in the course of program administration, the results of natural or quasi experiments such as a new program that changes the environment palpably, and findings of social experiments. Research based on census data or surveys does not fit comfortably in any of these categories, but one of them can be broadened to include it without affecting anything of substance.

Stromsdorfer then comes around from the other side and suggests that decisions may be based on faith without data, hypotheses suggested by regularities in data but not formally tested, hypotheses tested by data

from natural experiments, or hypotheses tested against data from classical social experiments. Once again, other research can be appended to this list. He also mentions hypotheses untested or only weakly tested by data, such as the belief in human capital underlying Johnson's Great Society initiatives. In common parlance, we call such hypotheses faiths.

The succeeding four sections review a variety of research, categorized according to whether or not it had an impact in initiating debate on policy issues, influenced a debate already under way, or followed a decision already made. Because the issues treated—welfare reform, social security, employment and training programs—are continuing issues, one can quibble with the categorization, but the essential point is that most of Stromsdorfer's comments seem balanced and well informed. I shall return to a few with which I do not fully agree below.

Stromsdorfer concludes with a brief section in which he singles out analysis of administrative and operational process as likely to be particularly useful, a point flatly at odds with one of Mundel's observations, but demonstrably correct in my opinion. He also expresses the rather wistful hope that the accretion of expertise within the bureaucracy and Congress may promote experimental analysis to a position of influence from which it is now clearly absent.

### Specific Comments

Mundel's paper is hard to discuss because it is a collection of generalizations not buttressed by specific examples. I found myself feeling uncomfortable with the generalizations and thinking "Yes, but . . ." He warns that experiments are costly in terms of people and institutions. Yes, but they produce analysts expert in a program whose know-how did not exist before the experiment and probably would not have existed without it. He states that experiments can do little to inform policy makers about problems of implementation, but how should one treat the findings of the NIT experiments on monthly reporting or of the housing-allowance experiments on the appropriate fees for agents under section 8? He points out that policy makers frequently do not understand what analysts tell them. Well, yes, but they can be made to understand and sometimes do. The answer may lie as much in the attitude of the policy maker as in the attributes of the analyst.

In general, Mundel asks whether experiments directly and clearly influence decisions. He finds that they seldom do. For reasons to which I shall return, I think that he has correctly answered the question posed, but that he has posed the question incorrectly.

The most striking aspect of Stromsdorfer's paper is that it does not confine itself to social experimentation. Rather he recognizes that social experiments can only be evaluated in comparison with other methods by which information finds its way into the process of political decision. In

this connection, he emphasizes the selective application and disregard of scientific canons. He does not clearly say who is guilty of this dereliction, but I trust that he would level this charge not only at politicians and bureaucrats but also at analysts.

### General Comments

I believe the lurking in the back of many of our heads is a view of how the evidence of social experiments ought to be used. This view is consistent with a number of statements in some of the papers. Inspired by genuine uncertainty about how to deal with a problem or by the belief that research evidence will persuade opponents, supporters of a policy initiate a social experiment. (In passing, I would suggest that social experiments have never preceded debate but have always followed its beginning. Results of other research may initiate debate.) Economists and other social scientists step in and design a test to estimate the key parameter. On the basis of the experiments, the parameter is found. It resolves doubts about which line of policy to pursue, and policy makers act on it.

Such is the Platonic image of the social experiment, and indeed of research in general, that many of us hold. Consciously, we recognize that it is hard to design experiments and that the results are often needlessly obscure. And we understand that many forces other than the rational application of science are involved in setting policy. But when things do not work out quite as we hoped, we go back to the drawing board to do things better. (It is hard to accept the metaphor of Hilton Head as drawing board, but there it is.) And that is precisely what we should do.

But as we sharpen our instruments, we should recognize that the process is quite unlike the naïve model I have sketched. As Stromsdorfer correctly puts it, the social experiment or the research paper with policy implications is part of an adversary process. What Stromsdorfer did not say, but what I know he recognizes and what needs saying, particularly in a group such as this, is the fact that the adversaries are contending for power, not truth. And deep down, that is what many of us are contending for too—how else can one explain the appalling lapses from analytical evenhandedness that Stromsdorfer mentions and documents. Through such lapses analysts have undermined their own credibility and persuasiveness and debased their own currency.

If the model of the social experiment on a straight line that passes through the truth on the way to action and power is so wide of the mark, what analogy fits better? Oddly enough, the model I would choose is that of efficient markets, although I make no claim of efficiency in any sense for the political process. A large number of actors, with divergent and conflicting interests and information, try to buy cheap and sell dear. The market efficiently digests all information and the result is a more or less

determinant price. A rumor of some change in market conditions will alter expectations of some people in some degree, but the trend in prices is based on more basic market conditions.

Most research in the political market place is a rumor believed by one person, the analyst. Other analysts may accept his findings; more often they will try to better them or to alter them. The half life of rumors is short. The difference between results of work by the solitary investigator and the social experiment is a matter of scale. Experiments involve politicians who must vote large sums to support them after approving the idea of experimentation itself. They involve state and local governments and voluntary organizations, lots of administrators to run them, and analysts to plan them and to study their results. They must be expertly designed and implemented because if opponents can show errors in their execution, either the experiments will not be completed or the persuasive value of the effort will be destroyed. But the chief difference between a study by a solitary research worker and a social experiment is political and social, not intellectual. It is a mistake to seek important intellectual differences, as some of the papers presented here have pointed out.

The question then is whether social experiments serve the rumor-generating function more effectively than does solitary research. The fact that social experiments are political events would appear to give them a great advantage. Furthermore, their scale enables them to look at issues such as new administrative processes that small-scale research finds it harder to investigate. As I have pointed out elsewhere, serendipitous findings of the income-maintenance and housing-allowance experiments will more than repay the U.S. Treasury the cost of the experiments in very short order.

In the end, however, social experiments, like most research on social programs, have been a force for slowing the adoption of new policies. The evidence cited by Stromsdorfer points clearly in that direction. My point is not that such a result is good or bad, merely that it has occurred. Social experiments, like other analysis, show problems to be more complicated and subtle than we had thought, and results are harder to achieve than we had hoped.

I sometimes wonder what would have happened if Franklin Roosevelt had had the good fortune to be president when economics was further advanced than it was in the 1930s. Instead of appointing the Committee for Economic Security in 1935, he might have fielded a social experiment to find out how people would respond to retirement benefits, welfare for the aged, and cash payments to the unemployed. By 1937 we might have had the research design completed instead of the legislation Congress actually enacted. By 1940 we might have seen the completion of payments to the first wave of experimental subjects instead of the initiation of benefits under a new program. The Second World War, alas, would have

interrupted the analysis of results and perhaps payments to the cohorts who were still receiving treatments to test duration effects. But we could have resumed treatments in, say, 1946 and completed analysis by the mid-1950s. We could have spent some time debating whether the decline in saving that we observed was attributable to the experiment or magnified by the expectation of the end of the Great Depression or by the postwar consumption boom, whether the increase in duration of unemployment was a steady-state reaction or was magnified by the change of postwar labor-market conditions and hence transitory. Oh, what a missed opportunity!

Speculations such as these prompt the observation that Mr. Stockman is making a grave mistake in trying to put us all out of work. He has not realized that we are the instrumentality for inaction. By diverting us to teaching rather than research or even to still more reputable ways of earning a living, he will make easier the growth of ideas for activist social change undisturbed by critical analyses when the mood of the country shifts.

## Comment      Laurence E. Lynn, Jr.

I must begin by confession that I find this subject—the use of social science in policy making—boring. One reason is that I have written on the subject and discovered that the number of interesting things one can say is limited. Another is that there is an extensive literature on the subject: the interesting things have already been said quite well by others. Additional papers constructed on no more than casual observations encased in the authors' unique taxonomies seem superfluous. To attract bored readers, an author must bring new data or insights to the subject, or else restate the received wisdom in an original and provocative way. Regretably, our two authors come up short.

David Mundel argues that social science experiments can answer only some of the questions of interest to policy makers: those of the form, If I implement policy X, what will be the behavioral consequences? The answers will be understood, and thus influential, only if (a) the experiments are limited in scope and rigorous in design, thus increasing the likelihood of unambiguous results, and (b) the results are available early in the policy-making game, before everyone's positions have hardened.

In keeping with a much-honored tradition, Stromsdorfer abandons the question he was assigned—Are findings from classical experiments more or less useful than findings from quasi experiments?—and addresses one

he finds more interesting: How is “information” generated and “used”? In his view, “knowledge” is produced through three processes: generation of data for management information systems (MIS), natural or quasi experiments, and classical experiments. Policy decisions are based on no data or information, hypotheses suggested by regularities in MIS data, or hypotheses tested by data from natural or classical experiments. He discusses the actual impact of several sets of data according to whether experimental findings initiated debate or followed debate.

Neither author has added to our store of insight and wisdom. Both appear to use criteria appropriate to the standard “rational actor” model of choice, though Stromsdorfer notes (ruefully) that “politics” has something to do with the initiation and use of research. That is, both appear to define “use” as an identifiable official reacting rationally and directly to the findings of a study, presumably embracing them. Neither has bothered to refer to the much richer formulations of the issues available in the literature. (For the best single essay, see Weiss 1978). Both suggest that it is better to introduce social science “early” rather than “late” in policy formulation, “before” debate rather than “after” it. Unlike many of their brethren in the academic community, both would manipulate the research product to enhance its usefulness or impact. Neither offers any recommendations concerning changes in research administration which would improve matters.

They reach opposite conclusions, however, concerning the future use of social experiments in policy making. Stromsdorfer believes that “gradually experimental analyses just may gain an ascendancy which it most assuredly does not now have.” Says Mundel, “the likelihood of further experimentation is probably declining.” Perhaps this difference reflects the authors’ differing faiths in the social-research enterprise. Mundel is clearly circumspect, a bit skeptical. Stromsdorfer is an enthusiast: research should be used, and it would be if there weren’t so many political “constraints.” The authors of these views are capable researchers with substantial experience in government. Take your pick.

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