

91st Congress }
1st Session }

JOINT COMMITTEE PRINT

THE ANALYSIS AND EVALUATION OF
PUBLIC EXPENDITURES:
THE PPB SYSTEM

A COMPENDIUM OF PAPERS

SUBMITTED TO THE
SUBCOMMITTEE ON ECONOMY IN GOVERNMENT
OF THE
JOINT ECONOMIC COMMITTEE
CONGRESS OF THE UNITED STATES

VOLUME 3

Part V. The Performance of Program Budgeting and
Analysis in the Federal Government

Part VI. Analysis and Evaluation in Major Policy Areas:
Unresolved Issues and Next Steps



Printed for the use of the Joint Economic Committee

U.S. GOVERNMENT PRINTING OFFICE

WASHINGTON : 1969

27-877

JOINT ECONOMIC COMMITTEE

[Created pursuant to sec. 5 (a) of Public Law 304, 79th Cong.]

WRIGHT PATMAN, Texas, *Chairman*

WILLIAM PROXMIRE, Wisconsin, *Vice Chairman*

HOUSE OF REPRESENTATIVES

RICHARD BOLLING, Missouri
HALE BOGGS, Louisiana
HENRY S. REUSS, Wisconsin
MARTHA W. GRIFFITHS, Michigan
WILLIAM S. MOORHEAD, Pennsylvania
WILLIAM B. WIDNALL, New Jersey
W. E. BROCK III, Tennessee
BARBER B. CONABLE, Jr., New York

SENATE

JOHN SPARKMAN, Alabama
J. W. FULBRIGHT, Arkansas
HERMAN E. TALMADGE, Georgia
STUART SYMINGTON, Missouri
ABRAHAM RIBICOFF, Connecticut
JACOB K. JAVITS, New York
JACK MILLER, Iowa
LEN B. JORDAN, Idaho
CHARLES H. PERCY, Illinois

JOHN R. STARK, *Executive Director*

JAMES W. KNOWLES, *Director of Research*

ECONOMISTS

RICHARD F. KAUFMAN ROBERT H. HAYEMAN JOHN R. KARLIK
FRAZIER KELLOGG DOUGLAS C. FRECHTLING (Minority)

SUBCOMMITTEE ON ECONOMY IN GOVERNMENT

WILLIAM PROXMIRE, Wisconsin, *Chairman*

SENATE

JOHN SPARKMAN, Alabama
STUART SYMINGTON, Missouri
LEN B. JORDAN, Idaho
CHARLES H. PERCY, Illinois

HOUSE OF REPRESENTATIVES

WRIGHT PATMAN, Texas
MARTHA W. GRIFFITHS, Michigan
WILLIAM S. MOORHEAD, Pennsylvania
BARBER B. CONABLE, Jr., New York

CONTENTS

VOLUME 3

	Page
Alphabetical listing of authors appearing in compendium.....	v
PART V: THE PERFORMANCE OF PROGRAM BUDGETING AND ANALYSIS IN THE FEDERAL GOVERNMENT	
SECTION A: THE OPERATION OF THE PPBS IN THE EXECUTIVE BRANCH	
The Status of PPB in Federal Agencies: A Comparative Perspective, by Keith E. Marvin and Andrew M. Rouse.....	801
SECTION B: A CRITIQUE OF THE PPB SYSTEM AND SOME PROPOSALS	
Systems for Analysis: PPB and its Alternatives, by Allen Schick.....	817
Rescuing Policy Analysis from PPBS, by Aaron Wildavsky.....	835
Prescription for an Effective Government: Ethics, Economics, and PPBS, by Paul Feldman.....	865
Today's PPBS: The Fatal Triumph of Financial Management Over Economics, by Samuel M. Greenhouse.....	886
SECTION C: PPB IN TWO PROMINENT AGENCIES: SOME LESSONS FROM EXPERIENCE	
The Planning, Programing, and Budgeting System in the Department of Defense: Some Lessons from Experience, by Alain C. Enthoven.....	901
The Planning, Programing, and Budgeting System in the Department of Health, Education, and Welfare: Some Lessons from Experience, by Alice M. Rivlin.....	909
SECTION D: THE PPB SYSTEM AND THE INSTITUTIONS OF GOVERNMENT	
Public Expenditure Analysis and the Institutions of the Executive Branch, by Fred S. Hoffman.....	925
Policy Analysis and Congress, by Nelson W. Polsby.....	943
PART VI: ANALYSIS AND EVALUATION IN MAJOR POLICY AREAS: UNRESOLVED ISSUES AND NEXT STEPS	
SECTION A: NATIONAL SECURITY AND FOREIGN AFFAIRS	
The Planning, Programing, and Budgeting System in the Department of Defense: Current Status and Next Steps, by Alain C. Enthoven and K. Wayne Smith.....	955
Policy Analysis in International Affairs, by Henry S. Rowen and Albert P. Williams, Jr.....	970
SECTION B: THE "NEW TECHNOLOGY" BUDGETS	
Prospects for PPB at AEC, by Milton F. Searl.....	1005
Policy Analysis in the National Space Program, by Bruno W. Augenstein..	1020
Some Policy Issues in the Analysis of Research and Development Programs, by Wade P. Sewell.....	1069

IV

SECTION C: SOCIAL OVERHEAD EXPENDITURES

	Page
Economic Analysis in Natural Resource Programs, by Jack L. Knetsch..	1087
Policy Analysis in Transportation Programs, by James R. Nelson.....	1102
Program Analysis and Agricultural Policy, by Vernon W. Ruttan.....	1128
Policy Analysis in the Post Office, by John Haldi.....	1151

SECTION D: SOCIAL DEVELOPMENT AND HUMAN RESOURCE PROGRAMS

Determining the Results of Manpower and Antipoverty Programs, by Garth L. Mangum.....	1171
Policy Analysis and Economic Opportunity Programs, by Robert A. Levine.....	1181
Problems of Resource Allocation in Health, by Robert N. Grosse.....	1197
Education Program Analysis at HEW, by John E. Brandl.....	1224
Policy Analysis and Housing and Urban Development Programs, by William B. Ross.....	1233

AUTHORS LISTED ALPHABETICALLY

	Vol. 1 Page	Vol. 2 Page	Vol. 3 Page
ACHINSTEIN, Asher	369		
ARROW, Kenneth J.	47		
AUGENSTEIN, Bruno W.			1020
BAUMOL, William J.	489		
BONNEN, James	419		
BRANDL, John E.			1224
CARLSON, Jack W.		613	
COTTON, John	332		
d'ARGE, Ralph C.	87		
DAVIS, Otto A.	67		
DEMSETZ, Harold	167		
DORFMAN, Robert	226		
ENTHOVEN, Alain C.			901, 955
FELDMAN, Paul			865
FREEMAN, A. Myrick, III	565		
GREENHOUSE, Samuel M.			886
GROSSE, Robert N.			1197
HALDI, John			1151
HAVEMAN, Robert H.	1, 547		
HIRSHLEIFER, Jack	505		
HOFFMAN, Fred S.			925
JACOBY, Henry D.	226		
KAMIEN, Morton I.	67		
KNEESE, Allen V.	87		
KNETSCH, Jack L.			1087
KRUTILLA, John V.	277		
LEVINE, Robert			1181
MCGUIRE, Martin C.	592		
MANGUM, Garth L.			1171
MARGOLIS, Julius	533		
MARVIN, Keith E.			801
MILLIMAN, Jerome W.	291		
MUSHKIN, Selma J.	332		
NELSON, James R.			1102
OLSON, Mancur L.	321		
POLSBY, Nelson W.			943
RIVLIN, Alice M.			909
ROSS, William B.			1233
ROUSE, Andrew M.			801
ROWEN, Henry S.			970
RUTTAN, Vernon			1128
SAWHILL, Isabel V.	473		
SCHICK, Allen			817
SCHMID, A. Allan	579		
SCHULTZE, Charles L.	201		
SEARL, Milton F.			1005
SEWELL, Wade			1069
SHAPIRO, David L.	505		
SMITH, K. Wayne			955
STEINER, Peter O.	13		
VICKREY, William S.	119		
WEIDENBAUM, Murray L.	357		
WEISBROD, Burton A.	177		
WHOLEY, Joseph S.	451		
WILDAVSKY, Aaron			835
WILLIAMS, Albert P, Jr.			970
ZECKHAUSER, Richard	149		

NOTE

The copyright material reproduced in this volume was printed with the kind consent of the copyright holders.

Section 8, title 17, United States Code, provides:

“The publication or republication by the Government, either separately or in a public document, of any material in which copyright is subsisting shall not be taken to cause any abridgement or annulment of the copyright or to authorize any use or appropriation of such copyright material without the consent of the copyright proprietor.”

The subcommittee requests that no material in this volume be republished or reprinted without the permission of the author.

Part V

THE PERFORMANCE OF PROGRAM BUDGETING AND ANALYSIS
IN THE FEDERAL GOVERNMENT

SECTION A

THE OPERATION OF THE PPBS IN THE EXECUTIVE BRANCH

THE STATUS OF PPB IN FEDERAL AGENCIES: A COMPARATIVE PERSPECTIVE*

BY KEITH E. MARVIN and ANDREW M. ROUSE

Keith E. Marvin is Associate Director of the Office of Policy and Special Studies at the General Accounting Office.

Andrew M. Rouse is Director of the Resources Planning Staff at the U.S. Bureau of the Budget.

In recent months, both the General Accounting Office and the Bureau of the Budget have conducted surveys relating to the institution and progress of the PPB System in Federal agencies and the organizational process by which policy analysis is documented and reviewed. This paper presents some of the results of these surveys. After reviewing the expectations for the PPB System, Mr. Rouse and Mr. Marvin discuss the actual form of the system in the agencies. They present thirteen factors which have influenced the form of PPB development in the various Federal agencies. These factors include the attitude of the relevant Congressional committees toward PPB, the attitude and interest of the agency head, and the size and qualifications of analytic staffs. They conclude by isolating those five factors which "appear in each agency that has made substantial progress toward the development of PPB Systems for policy decisionmaking."

Introduction

Much has been written on the purposes of planning-programing-budgeting (PPB), its uses in analyzing areas of public expenditures, the applicability of various analytic techniques, the effect of the PPB process on various public functions and on the Federal political and administrative environment. While there has been considerable discussion, little factual knowledge of the organizational impact of PPB on the civil agencies particularly is available.

The PPB system has been used formally in the Defense Department for 8 years and in many civil agencies for 3 years. Enough time has elapsed to examine the systems created by the Federal agencies. Such an examination is useful if we are to discern ways of making the system more effective.

The purpose of this paper is to describe the current status of PPB in the Federal agencies and the facts which appear to account for this condition. The paper is based on recent surveys of PPB systems conducted independently by the General Accounting Office and by the Bureau of the Budget. These surveys looked at the systems which had been developed from the point of view of agency management. They assumed that management would want analysis of agency policy issues and would want these analyses carried out in a systematic way.

*The authors wish to acknowledge their debt to Herman Galvin of the GAO and Edwin Harper of the Bureau for their help in writing this paper. We would also like to thank Lucy Harper for editing several versions of this paper. The patience of all three, in interspersing work on this paper among their other duties, is much appreciated.

Major aims of the surveys were to ascertain if and how managers used analysis in decisionmaking and the organizational process for documenting and reviewing it.

The paper first describes what the expectations for the PPB system were. It then looks at what was actually created in the agencies. Discussed are the patterns of development of agency PPB systems and 13 of the factors which appear to have affected the development of these patterns.

PBB IN CONCEPT

Just what PPB was intended to be has been a source of some misunderstanding. There are those who think that PPB is simply the injection of the analytic techniques associated with modern operations research and systems analysis into the decisionmaking process; they point out that analysis of sorts existed in many bureaus and agencies long before the PPB innovation was ordered. Others say that PPB is a system for decisionmaking; they point out that the men who introduced and supported PPB through the years intended "to develop a coherent and comprehensive system through the imposition of certain formal elements of procedure and requirement for documentation."¹

While the introduction of PPB has undoubtedly led to the currency of certain tools of analysis common to the field of operations research and systems analysis, there seems to be little doubt that it was the intention of both President Johnson and the supporters of PPB to develop a systematized approach to decisionmaking and not merely to introduce analytic tools.² Early in the literature of PPB, Arthur Smithies suggested that such systems serve as the focus of a *process* of comparison and coordination. He argued that it involved:

- (1) Appraisals and comparisons of various Government activities in terms of their contributions to objectives;
- (2) Determination of how given objectives can be obtained with minimum expenditure of resources;
- (3) Projection of Government activities over an adequate time horizon;
- (4) Comparison of the relative contribution of private and public activities to national objectives; and
- (5) Revision of objectives, programs, and budgets in the light of experience and changing circumstances.

"These operations are inherent in any planning, programing and budgetary *process*. Program budgeting involves more explicit recognition of the need to perform them than has been traditional. It also involves the application of new analytical techniques as an aid to the exercise of human judgment on which choices must ultimately rest."³

¹ *A Progress Report on PPB in the Federal Government*, a paper prepared for the Committee for Economic Development by Fred S. Hoffman, former Assistant Director of the Bureau of the Budget, Washington, D.C., Oct. 10, 1968.

² While President Johnson launched PPB and gave it strong initial support, his interest in more recent years appeared casual. For example, subsequent to the Presidential memo of Aug. 25, 1965, which announced PPB to the civil agencies, only one further official Presidential paper, other than several paragraphs in the budget messages for fiscal years 1968, 1969, and 1970, evidence Presidential concern for the status of the implementation of PPB in the civil agencies. (This was a Presidential memorandum of Nov. 17, 1966, on Government-wide PPBS. While this memo required quarterly reports by the Budget Director on the implementation of PPB in agencies, such reports, to the best of the authors' knowledge, were neither submitted nor asked for.)

³ Smithies, Arthur, "Conceptual Framework for the Program Budget" in *Program Budgeting*, David Novick, ed., Washington, D.C., GPO, 1965, p. 5.

The early proponents of PPB saw planning, programing, and budgeting as interrelated activities, each an aspect of a circular process of decisionmaking; i.e., a new process, not simply the injection of different or new analytic techniques into an already established decision-making process.⁴

Three early decisions should be noted since they were aspects of Presidential expectation and have indeed affected the development of the system in the Government. These, cited by the former Assistant Director of the Bureau of the Budget, Fred S. Hoffman, were:

- (1) Assigning to the Bureau of the Budget the role of leading and monitoring the introduction of PPB in the civil agencies of the Government;
- (2) Applying the Defense PPB model as a prototype on which to base expectations for the domestic agencies; and
- (3) Introducing across-the-board PPB in most large civil agencies.⁵

The role of the Bureau of the Budget was expected to be primarily a managerial one. However, the Bureau, because of its need for the outputs of analysis, soon became as much a user as a monitor of the PPB system. The ambiguity of the Bureau's role tended to emphasize PPB as a Bureau tool rather than a tool of agency policymaking. Some agencies which looked to the Bureau for help and guidance found that there was little to be had. The Bureau appeared to be, contrary to expectation, a demander of their output. The decision to assign the Bureau a central role in developing the PPB system left as an unresolved problem the ambiguity inherent in the dual and sometimes conflicting roles of the Bureau in dealing with the PPB implementation in agencies.

The institution of PPB in the civil agencies was expected to produce some of the benefits which had been produced in the Defense Department. There is, however, question about the applicability of the Department of Defense's PPB process as a prototype on which to base expectations for civil agencies. Because the Defense Department's goal is primarily national security, the objectives to be achieved by Defense programs have been reasonably well defined. On the other hand, most civil agencies have more undefined, varied and multi-dimensional objectives than the Defense Department. Further, the Defense establishment had used policy analysis extensively prior to the formal introduction of PPB and so the technical requirements were not unfamiliar to people in the Department. However, in the civil agencies, with few exceptions, systematic policy analysis was rare.

Finally, the across-the-board introduction of PPB in the civil agencies had at least two important consequences which tended to defeat what was expected of PPB. One was that hundreds of analysts were needed, of which there were almost none, either in the Government outside Defense, or the private sector. The result was to spread then existing talent and to literally reclassify as "policy analysts" large numbers of men without the requisite training. Result: analytic studies were extremely variable in quality; almost non-existent in some agen-

⁴ Hitch, Charles, *Decision Making for Defense*, H. Rowan Gaither Lectures in Systems Science, University of California, 1965. Novick, David, *A New Approach to the Military Budget*, RM-1759, Rand Corp., June 1956. Smithies, Arthur, *op. cit.*, note 3.

⁵ *Op. cit.*, footnote 1.

cies. Across-the-board implementation created other problems such as the need to articulate rapidly program structures to be used in analysis and output measures.

This, in effect, brought to the surface problems which had been dealt with for years in an intuitive manner. The lack of quantitative measures which are related to program achievement and objectives, for example, became apparent in the process of developing program structures and output values. The fact that many structure and output definition problems remain unsolved creates an impression that PPB has been unsuccessful. In retrospect, it is clear that the expectations of Federal agencies exceeded their ability to satisfy.

A complicating environmental aspect is the fact that the executive decisionmaking process has never operated independently of the legislative bodies. Various subcommittees have become deeply involved in monitoring the execution of the programs authorized or funded by legislation which they have formulated. The formal approach of PPB impinged upon the highly variable legislative liaison process. Thus, each agency considered how it could make use of PPB analysis in support of its particular position within its particular legislative environment.

As a result of these factors, agencies, left to their own devices, created a wide variety of systems—none of which has fully satisfied the expectations of its proponents.

PATTERNS OF DEVELOPMENT

The formal elements of the PPB process are well known and are described in detail elsewhere in this collection.* For our purposes, note that the definition of program structure, analytic study, program memoranda and program and financial plans have undergone much change from the days of their Defense Department incarnation. Our focus is not on the quality of the elements of PPBS but rather on the way their development has been incorporated into the policy decision-making apparatus of the agency.

The systems which are emerging in the Federal agencies are not uniform.⁶ However, agencies can be grouped according to the degree to which they have developed a decisionmaking process which incorporates the elements of PPB as these elements seem to fit the environment, programs, and organizational realities of their agencies.

In placing agencies within these groups, a number of criteria were used. These criteria were:

- (1) The use of and attitude toward policy analysis;
- (2) The use of planning as an aid in achieving stated goals;
- (3) The development of a process by which planning, the analysis of program alternatives, and budgeting is integrated; and
- (4) The adequacy of analytic staffs both in number and qualifications.

Each of these may be looked upon as a continuum on which each agency can be placed. Some agencies which conform to one criterion

⁶ Sixteen agencies in particular were covered. They are: USDA, HEW, OEO, Corps of Engineers, AEC, GSA, Interior, VA, Treasury, Labor, Commerce, Justice, NASA, DOT, POD, and HUD.

* Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

may not conform to another. The groupings of the agencies, therefore, represent the congeries of agency ratings along the various continua.

Five agency groupings are apparent. In one group, analytic activities have evolved toward integration with the PPB process and their outputs have been used by decisionmakers; for example, HEW. Agencies in this group also utilize a planning document which displays future year figures for agency programs geared to agency goals in many areas. The agencies have either formal or informal processes by which the outputs of the PPB staffs are fed into the budget process. That the success of such integration has been sporadic at best reflects the difficulties of adaptation rather than deep seated resistance to the injection of the products of policy analysis into the decisionmaking process.⁷

In a second group, well-developed analytic activities have contributed to decisionmaking and did so long before the advent of PPB. The AEC is an example of an agency in this group. The result is that PPB's contribution in restructuring the decisionmaking process in these agencies has been marginal.

Detailed PPB processes have been developed in the third group of agencies; for example, Interior. With the exception of the work in an occasional bureau, the materials produced through these processes have not been used extensively by decisionmakers. Such agencies have developed one of the aspects of PPB, the process, to a high level, but have not yet succeeded in developing the program analysis and planning aspects which are the heart of the PPB process.⁸

Analysis, most often generated outside the PPB process, has contributed to decisionmaking intermittently in the fourth group; for example, the Post Office Department. In these agencies, the planning and analytic effort has been fragmented by relatively strong bureaus, by separation of the functions in the formal structure, and other disintegrative factors.

In the last group, rudimentary analysis generally has been integrated with the PPB process and used at the program or bureau level, but top level management has used these analyses very irregularly. GSA is an example in this group.

FACTORS CONTRIBUTING TO PATTERNS OF DEVELOPMENT

Many factors have contributed to the emergence of the different agency patterns. Thirteen, in particular, were commonly perceived to have had some impact upon the form which the development of PPB took in Federal agencies.⁹ These are:

(1) Confusion among analysts and program managers as to whom PPB is intended to serve, the agency or the Bureau of the Budget;

(2) Duplication of effort seemingly required by PPB as distinct from the traditional budgeting process;

⁷ The authors conducted over 400 interviews. In only a small number of these was any objection raised to the purposes or value of analysis in policymaking. More often expressed, particularly by nonanalysts, was resentment at being uninformed and unconsulted. Also expressed were misgivings about too hasty use of "theoretical" analytic results unsupported by existing data.

⁸ Process as used in this paper refers to the procedures, the system for producing PM's and PFP's primarily. This is distinguished from analysis; i.e., substantive studies of programs and issues.

⁹ There are undoubtedly others. Some of these have been suggested by other commentators on PPB in the Federal Government. However, the 13 covered here were those most often mentioned in the GAO and BOB surveys upon which this paper is based.

- (3) The attitude of the relevant congressional committees toward PPB;
- (4) Bureau of the Budget attitude and guidance;
- (5) The attitude and interest of the agency head;
- (6) The qualifications of the man selected to head the central analytic staff;
- (7) The qualifications of both the central and bureau level analytic staffs;
- (8) The size of the agency analytic staffs;
- (9) The age of the agency or its programs;
- (10) Formal organization, including both comprehensive procedures for the PPB system and the reporting and role relationships of the analytic functions to policymaking executives;
- (11) The susceptibility of the agency's mission to analytic effort, notably the difficulty in designing benefit measures;
- (12) The degree of congruity between the PPB program structure and the agency's organization structure; and
- (13) The difficulties of developing appropriate data and accounting systems.

CONFUSION AS TO WHOM PPB SERVES

One factor which has had a most important effect upon the pattern of PPB development in the agencies has been diversity in perception as to whom PPB serves. In some agencies, personnel in general and the agency head in particular have seen PPB as a tool in agency decision making. In other agencies, frequently reflecting the indifference of the agency head, most agency personnel have seen PPB largely as a requirement of the Bureau of the Budget.

In those agencies where PPB is perceived as serving a Bureau need, the PPB innovation has been viewed largely as a budget justification requirement. In other agencies, PPB has increasingly been seen as a system for improving decisions within the agency, but even here there are variations in perspectives. There are some agencies where the tool has been regarded largely as a mechanism serving the agency head, developing information upon which he may make his decisions. In others, the process has been viewed as useful to program and bureau chiefs as well.

These perceptions of agency personnel have had an important impact upon the organization and the working relationships of the PPB function in the agency. In those agencies which see it largely as a BOB tool, more often than not the PPB responsibility has been divided between analytic and process activities; each of these reporting to a different agency official. In many of these agencies, program memoranda and program and financial plans are prepared by units which are responsible for preparing more traditional budget justification materials. Analytic activity, if any, is separately staffed. These activities frequently serve as a staff resource to the agency head working, in most cases, outside the context of the agency's PPB process. On the other hand, in those agencies seeing PPB as an agency tool, it has been integrated under a single official, more often than not the agency head.

A key force in the development of the agency's perception of PPB appears to have been the agency head. In those agencies in

which the agency head has been indifferent to the development of PPB and has used the results of analysis intermittently, for advocacy of programs or for organizational control, PPB has usually been seen as a Bureau of the Budget tool. Where the agency head has been either strong in his support or passive in his support, but has recruited strong leadership for the PPB unit, the PPB system has more often been regarded as an agency decisionmaking process.

DUPLICATION OF EFFORT—PPB AND TRADITIONAL BUDGETING

Budget decisionmaking in most agencies is described as a "two-track system," the traditional budget process being one "track" and the PPB system, the other. Associated with each "track" are different documentation requirements, classifications, and data systems.

The pattern of linkage between the two "tracks" in the advanced PPB agencies has been different from that in the slower agencies. In the latter, it was hard to find evidence that the two "tracks" have been functionally relevant to each other; there has existed little interest in or use of the crosswalks which would relate appropriation budget categories to PPB program categories. Budget and PPB units carried on their affairs almost without reference to the work of the other. However, in those agencies where substantial progress has been made, there was general agreement that PPB had made some impact upon budget decisionmaking. In one of these agencies, OEO, the budget function has been absorbed by the PPB unit. In another, HEW, the program and financial plan has been used as a source for internal budget targets at the beginning of each budget season.

In the slower agencies, relationships between the PPB and the budget activities were often strained. Budget units frequently claim difficulty in satisfying the requirements for data of the PPB activity; and that the requirement for PPB documentation represented little more than "make work." It is not surprising that these sentiments still exist if the DOD experience is taken as a model of the manner in which PPB and traditional budgeting interact. After all, the Defense Department started to integrate the program and traditional budgets in the 1950's and this integration has only recently been accomplished.

CONGRESSIONAL ATTITUDE

The attitude of the Congress toward PPB is not entirely clear but whatever it is, it is not uniform. Individual members and committees have expressed a variety of attitudes varying from a desire to obtain direct outputs of PPB, to advocacy, to curiosity, to skepticism. Interest in PPB has been expressed infrequently, suggesting that the Congress, in general, has not considered this innovation in the executive decision process as very important to the legislative bodies. In one case, for example, the chairman of an appropriations subcommittee has expressly prohibited any salary expenditures for PPB personnel in one agency. The agency, not surprisingly, has made little progress toward developing a PPB system or integrating it into agency decisionmaking. Even here a cause/effect relationship is difficult to establish because many of the agency professionals express an anti-PPB bias. Some Members of Congress, however, are now asking for the analytic studies and agency plans which the PPB system has produced. This interest,

unlike the past indifference, may have an important effect on agency PPB development.

Agencies, regardless of how they have progressed in implementing PPB, claim to have used analytic studies often to generate and support legislative changes. However, there is little evidence of this use to be found in the legislative hearings.

BUREAU OF THE BUDGET ATTITUDE AND GUIDANCE

While officially the Bureau supported PPB, among the Bureau's examining units the attitude has been more ambivalent. In general, even negative Bureau examiner attitudes, in the cases where they were apparent, have not led to noncompliance with requirements, but rather have affected the time and emphasis placed on submission of program memoranda and program and financial plans. Consequently, examiner attitude had little effect on the patterns which developed.

The Bureau's clearest impact upon the development of the PPB system in the agencies has been through its issuance of formal guidance. Agencies, to varying degrees, have relied upon it as the mechanism to force their bureaus to submit PPB materials and to inject the analytic staffs into agency information flows. In a few agencies which have made relatively little progress toward the injection of systematic analysis into the agency decision process, the Bureau of the Budget's general guidance has been only slightly recast and promulgated as agency procedures. Not unexpectedly, these agencies are also among those that have regarded the PPB system as a Bureau tool.

AGENCY HEAD ATTITUDES

The attitude of the agency head has been the single most important factor in the development of a PPB system and its integration with the agency decisionmaking system.

Where agency heads have supported analytic effort, agencies have made substantial progress toward the integration of the PPB system and the decisionmaking process. Agency head support is most clearly evidenced by requests for and use of analytic studies. In all of the advanced agencies, the agency head has used the PPB outputs for policy decisionmaking as well as subsequent advocacy of his position. Several of the agency heads also have used PPB outputs to assist them in coping with agency biases and occasionally in reviewing program performance.

Where agency heads have been indifferent toward the development of systematic analysis and planning processes, agencies have made less substantial progress. In most of these agencies, there are instances of the sporadic use of policy analysis. More often than not, however, the examples reflect *ad hoc* requests for analysis to be used for advocating new programs. These studies are generally produced outside the framework of the PPB process.

Three reasons often cited for agency head indifference were: wide experience in the agency's program area; professional background which leaned toward bargaining or argument as issue resolving techniques; and finally, strong agency constituencies whose interests would not be served by the kind of policy analysis contemplated by the PPB system.

QUALIFICATIONS OF THE HEADS OF CENTRAL ANALYTIC STAFF

The successes of those agencies which have made progress toward the development of PPB have been attributed to the quality of leadership of the central analytic staff. There appear to be some attributes which all of these men have in common. First, they were generally acknowledged to have strong analytic skills. While, in some cases, there was little evidence of strong managerial competence in their backgrounds, they were all known as aggressive men, interested in developing systematic analysis as part of the decisionmaking process. Finally, each of them, early in their tenure, undertook one or more studies which were intended to demonstrate the value of systematic analysis to their agencies.

There are similarly qualified analytic staff heads in agencies which have made relatively little progress. The difference appears to lie in the attitude of the agency head. Where a strong staff chief has been indifferently supported but not hampered, he seems to have been able to operate effectively. On the other hand, where a strong staff head has been unsupported, where the agency head either has not reviewed analytic results, and/or has not had the analytic shop reporting directly to him, the staff chief has had little impact. Similarly, in agencies having interested agency heads, but comparatively unqualified central analytic staff heads, the staff heads have had little impact. Consequently, a qualified central analytic staff head appears to be necessary to the development of a useful PPB system, although such a staff head is not sufficient to guarantee progress in the face of a nonsupportive agency head.

QUALIFICATION OF STAFFS

Table 1 summarizes personnel data on approximately 800 analysts in 16 domestic agencies. Included are data on the education, training, and work experience of members of staff identified as spending most of their time on special analytic studies, writing program memoranda and program and financial plans.

The advanced agencies have staffs which have had more formal education, more recently acquired training, have spent fewer years in the agency, and have had broader experience than the staffs in the less advanced agencies. The central staffs of the agencies which have made progress differ from their bureau staffs in the same ways.

It is interesting to note that in the less advanced agencies, the differences in the characteristics of the central and bureau staffs vary in much the same way as for the agencies which have made progress. While there are differences between the central and bureau staffs in the recency of degrees, for example, in general the qualifications of central staffs of the slower agencies appear to bear a similar relationship to bureau staffs as do the central and bureau staffs in the more advanced agencies.

In short, the advanced agencies have better qualified staffs, but central staffs are, in general, better qualified than bureau staffs in these agencies. Central staffs in both advanced and slower agencies bear similar relationships to their respective bureau staffs.

TABLE 1.—BACKGROUND OF ANALYTIC STAFFS,¹ BUREAU COMPARED TO CENTRAL ANALYTIC STAFFS,² 16 SELECTED DOMESTIC AGENCIES

	Education and training						Work experience							
	Years of formal education		Recency highest degree (years)		Percent staff quantitative major		Percent staff PPB training		Percent staff quantitative experience		Percent staff broad experience		Average years present agency	
	Central ³	Bureau ⁴	Central ³	Bureau ⁴	Central ³	Bureau ⁴	Central ³	Bureau ⁴	Central ³	Bureau ⁴	Central ³	Bureau ⁴	Central ³	Bureau ⁴
Average for 3 agencies making progress toward use of analysis and planning in decisionmaking.....	18.4	17.5	8.6	16.1	47.9	53.6	18.6	24.4	28.1	23.8	56.8	21.4	2.8	7.9
Average for 16 agencies.....	17.7	16.8	13.4	13.9	47.9	39.5	25.4	27.7	37.8	23.9	35.9	19.3	6.5	8.3

¹ These data are derived from analysis of personnel data supplied by agencies on professionals identified as analysts meeting the following definition: professional involved most of the time (more than 50 percent) in carrying out special analytic studies associated with issues generated within the PPB system and/or writing program memoranda and program and financial plans.

² OEO, USDA, HEW, VA, GSA, AEC, Corps of Engineers, Labor, Treasury, NASA, Justice, POD, HUD, DOT, Commerce.

³ Central analytic staff.

⁴ Agency bureau staffs as a group.

STAFF SIZE

PPB staff size is difficult to ascertain. First, the PPB staffs cannot, in all cases, be considered incremental, required solely for PPB. Some of the analytic and planning responsibilities of the PPB staffs were performed prior to the implementation of the formal PPB system and would continue in its absence.

Second, identifying the PPB analysts was another kind of problem encountered in assessing the personnel resources involved in PPB. Different definitions were used in the Bureau and GAO surveys. These generated different responses by the agencies. Given the differences in definition, however, the responses were compatible. Third, part of the difficulty also lies in the fact that the agencies themselves are not clear on who is and who is not a PPB analyst.

With these caveats, there are about 1,600 full-time PPB employees in the 21 agencies surveyed by GAO.¹⁰ Another 2,100 employees spend part time on PPB for an additional full-time equivalent of about 900 full-time PPB employees. The grand totals are 2,500 full-time equivalent employees allocated to the planning, programing, and budgeting functions.

The impact of the sufficiency or insufficiency of staff size upon the development of the agency's PPB system is also difficult to assess. Several agencies which have made very little progress—HUD, for example—are understaffed, but so is HEW which has made significant progress.¹¹ AEC is understaffed but is one of the agencies which has a well-developed analytic process. These inferences suggest that the *total* agency staff size has not had a great impact upon the agency's development of PPB unless the staffs are so small as to be a mere nod toward a policy analysis function.

The size of the central staff, however, seems to have been of some significance. In all of the agencies which have made substantial progress, the central staff has at least sufficient staff to provide an important analytic capability.

AGE OF AGENCY OR AGENCY PROGRAMS

New programs and old bureaucrats are often mentioned as major roadblocks to the development of PPB.

Pressures for the establishment of program apparatus for new programs have made the deliberate approach of the PPB process appear an undesirable burden to top program officials. Consequently, agencies with new programs, such as EDA or HUD, frequently have not developed a PPB process with the same speed as they have developed their program apparatus.

The argument is often made that in older bureaus well-developed procedures and entrenched bureaucracies make it very difficult to change the decisionmaking process, a fact which accounts for the slow development of PPB in such organizations.

There are good examples which support both the "new program" and "old bureaucrat" arguments and equally good examples which

¹⁰ The agencies surveyed by the GAO include the following: Post Office, Commerce, Treasury, GSA, VA, NASA, DOD, AEC, DOT, Corps of Engineers, HEW, DOL, HUD, OEO, NSF, USDA, Interior, USIA, State, AID, and Peace Corps.

¹¹ The sufficiency of staff size was estimated by using the DOD Office of Systems Analysis as a standard for central staffs, and a generous workload capacity estimate to establish a range within which the total agency staff size should fall.

demonstrate the opposite of each proposition. However, it does appear that in older bureaus, the pattern developed for the implementation of PPB tended to emphasize process, and the few analyses prepared have relied heavily upon less complex techniques. Agencies with very new programs did not develop analytic processes at the same pace as they geared up the new programs.

FORMAL ORGANIZATION

The Bureau's PPB instructions to the agencies have provided very general guidance, allowing for agency adaptation of the PPB system to its own needs and peculiarities.

In almost all agencies, even those with well-established analytic and planning activities, the day-to-day responsibility for the PPB function has been assigned to newly created units at both the central and bureau levels. The structure of these units and the formality and detail of the PPB procedures established vary widely from agency to agency. Twelve formal organization patterns in fact have emerged among the 16 domestic agencies studied.¹²

Those agencies making substantial progress have a very similar formal organization pattern. They each have:

- (1) Integrated reporting relationships for both the analytic and process elements of PPB;
- (2) The responsible unit reports to the agency head;
- (3) The agency head formally reviews the analytic studies and the program memoranda, injecting the results of analytic effort into agency policymaking; and
- (4) The agency has promulgated detailed procedures, often down to field units, dealing with required inputs for the planning and analytic process.¹³

The formal organization of agencies which have made less progress may have some of the above characteristics but they tend also to have one or more of the following:

- (1) Split reporting relationships for analytic and process activities;
- (2) Responsible units report to assistant secretaries;
- (3) Absence of detailed procedures; and
- (4) Intermittent or no review of analytic output by agency head.

While much has been said about reducing conflict between budget and PPB units in agencies by the combination of the two, this has occurred in only two agencies—DOT (very recently) and OEO. The formal arrangement differs in each and in both cases, results are still completely speculative.

SUSCEPTIBILITY TO ANALYTIC EFFORT

Some argue that wide variation in mission makes for varying degrees of difficulty in applying PPB. Nothing inherent in the subject

¹² Agencies with PPB systems not included are DOD, USIA, AID, Peace Corps, SBA, and NSF.

¹³ It should be pointed out that the extent to which procedures are formalized and detailed should correspond to the extent to which PPB responsibilities have been decentralized within the agency. Where staffs are small and centralized, as in many regulatory agencies, the need does not appear pressing.

content of any agency's program mix should impede PPB analysis, although organizational lines may do so.

Frequently heard is the complaint that output/benefit definition is not possible for many programs. While such measurements are difficult to define in many program areas, some of both advanced and slower agencies have yielded to the temptation of using intermediate outputs in place of program benefits. This avoided wrestling with benefit definition, the role of secondary benefits, and like problems, but it does so at substantial cost to program evaluation capability. The factor, in short, does not appear to have affected the *pattern* of development of PPB in the agencies, although it has been used as justification for the slow rate of progress made by many agencies.

PROGRAM STRUCTURE

Whether existing program structures aid or impede analytic effort is often discussed. One reason for this is the importance attached to structure in Bureau guidance. Another is the implication for Government-wide planning and analysis, a subject which elicits strong feelings.

A good structure should ease the analysis of agency activities directed to the same or similar objectives. However, analytically sound structures will not insure progress in integrating analysis into policy decisionmaking. Similarly, analytically inept structures do not make success impossible. There does appear to be a relationship between the progress of PPB and structure. The slower developing systems are frequently characterized by program structures which resemble agency organization structure. What seems to follow from this is that the PPB analyses and displays become collections of supporting information for the particular means employed by the organization, without due consideration of alternatives whose adoption might require changes in entrenched activities or even more shattering, involve administration outside of the organization.

APPROPRIATE DATA AND ACCOUNTING SYSTEMS

Fundamental to analysis is credible cost and output data. However, few agencies have systems which produce the timely, routinized cost and output information appropriately classified for use in analysis.

The least difficult to get are costs expended in the aggregate for various time periods. More difficult is obtaining reliable information about the achievements of the programs, that is, quantitative outputs for given periods of time. Therefore, it is also difficult to determine unit costs of the services or other achievements of the programs.

The problem of finding usable quantitative measures of achievement for many programs has been discussed briefly above. The special requirements for data for this purpose have frequently been misunderstood. These needs have not been integrated into the routine accounting procedures, although there are currently some significant projects underway, for example, in the Department of Labor, which have this as one of their objectives.

Some of the impediments to developing appropriate data systems stem from the fact that organizational lines and program structures do not coincide. The requirement for accounting on a program basis is superimposed across organizational accounting requirements. The

practical difficulties in accounting for costs of all kinds under such circumstances has led to the use of estimation and statistical allocation methods which are seen as an imposition but have been prepared on an "as required basis" in almost all agencies.

Generally, agencies have depended on *ad hoc* data collection for their analytic studies. Some analysts in both advanced and less advanced agencies have become inventive, creating personal data files and ingenious data constructs to substitute for regular reports on costs, intermediate outputs and the benefits resulting from agency expenditures.

This creativity has been required particularly in agencies having programs in the form of formula grants to States and localities. In these agencies, neither cost nor output data is identified by the formal systems in the detail required to relate it to specific objectives within the broad statutory categories. Various studies are underway which may ameliorate these data systems problems.¹⁴

CONCLUSIONS

None of the factors discussed, taken alone, cause the patterns of development of PPB in Federal agencies. Many of them in combination make up the fact of an agency's adaptation to PPB. Which seem to have had the greatest impact upon PPB development? The authors find that five factors appear in each agency that has made substantial progress toward the development of PPB systems for policy decision making. These are:

(1) The active support, both formal and informal, of the agency head. He evidences support by asking for, using, and talking about analytic studies; and by encouraging the systematic production of these studies and the action documents based on them;

(2) Leadership of the central analytic unit by an executive with recognized analytic experience, managerial skills, and an aggressive interest in developing a systematic process for the production and use of analysis in his agency;

(3) A general perception in the agency that PPB is essentially an agency decisionmaking tool rather than a Bureau of the Budget requirement;

(4) Qualified agency staffs; and

(5) Sufficient agency staffs, particularly in the central analytic unit.

In addition, the more successful agencies have evidenced a tolerance of the differences between the analytic and budget processes, permitting effective interaction of the two "tracks" at appropriate points during the year. That these are frequently not formal interactions only emphasizes the fact that policymaking and systematic planning and analysis are still loosely connected.

¹⁴ Examples of efforts to improve the information systems of agencies are:

<i>Agency</i>	<i>Study or experimental system</i>
Post office-----	Postal Source Data System.
DOT-----	Transportation Information Program.
Labor-----	Touche, Ross, Bailey & Smart study on accounting and management information systems.
Commerce-----	Study with Technical Analysis Division of Bureau of Standards to improve Department information system.
HEW-----	Lindsay, Noble & Associates study on accounting systems.

SECTION B

A. CRITIQUE OF THE PPB SYSTEM AND SOME PROPOSALS

SYSTEMS FOR ANALYSIS: PPB AND ITS ALTERNATIVES

BY ALLEN SCHICK

Allen Schick is Research Associate at The Brookings Institution.

PPB is not the only methodology for the systematic analysis of public choice. Dr. Schick here examines the strength and weakness of the conception of the PPB system—"not its operation"—relative to alternative approaches to public expenditure analysis. The major drawback he finds in the current PPB system is its excessively close ties to the budgeting process. There are many factors which make our budgeting process "antianalytic", and an inhospitable environment for analysis: rigid traditions and time schedules, the insularity of budgeting from outside happenings, and the reluctance of budget officials to depart from traditional programs or areas of concern.

Several considerations are essential in appraising alternative approaches to analysis. Should we emphasize the structural or analytic aspects of "systems analysis"? Dr. Schick provides evidence that both are important. Should the system for analysis be oriented toward Presidential perspectives or agency purposes? Should it be applied across the board or only to selected agencies? Dr. Schick suggests that although an across the board Presidential system may be desirable in the long-run, in the short-run the most effective approach would be to encourage economic policy analysis in those agencies which are most receptive or at least not hostile to it.

Dr. Schick outlines four alternative systems for analysis—crosswalk systems, two-track systems, analytic budgeting, and a policy planning system. The adoption of the crosswalk system is criticized for linking policy analysis too closely to budgeting and, hence, restricting it. "Unless analysis is somewhat insulated from the budget function, there is a danger that it will be preempted by the control and management routines of budgeting. . . . If policy analysis is to flourish, it will have to be rescued from budgeting."

Introduction

PPB is only one of several possible systems for the analysis of public expenditure policies. It was the system pioneered in the Defense Department and later extended to other Federal agencies, and it is the system that is now being applied in many State and local governments. But PPB is not the only methodology for the systematic analysis of public choice; there is no *a priori* justification for the premise that it is the best of all possible systems. Moreover, the type of PPB used is not the only possible form; within the general PPB framework a number of alternatives are available. Three budget cycles have run their courses since PPB was promulgated in 1965; yet there is little to show for all the effort. There have been too many costs and too few benefits. It behooves public officials to examine their handiwork to determine whether it is the most appropriate and effective instrument for bringing policy analysis to bear on public policymaking.

I do not intend to compile a list of PPB defects and problems or to evaluate the application of PPB in Federal agencies.* I propose to appraise the conception of PPB (not its operation) and to compare it with alternative systems. First, however, I will consider several

*Further discussion of this issue is found in the paper by Marvin & Rouse in this volume.

questions relating to the purpose and scope of policy analysis: (1) Why is the budget process nonanalytic or antianalytic? If budgeting were inherently analytic, or at least encouraged the use of analysis, there would be no necessity for a new analytic system. (2) Is a systems approach the most effective way to enhance the analytic caliber of public choices? Perhaps it would be more efficacious to promote analysis without any regard for its systems character. (3) If a systems approach is justified, what should be its mission and scope—presidential or agency, selective or across the board? (4) What should be the scope and focus of analysis—public benefits or program effectiveness? The answers to these questions affect the kind of analytic system that is appropriate for Federal operations.

THE NONANALYTIC BASIS OF BUDGETING

Some decades ago Walter Lippmann noted that the world outside does not correspond to the pictures in our heads.¹ Man's view of the world is stereotyped, formed by media and other secondary influences, not via first-hand experiences and observations. Lippmann was writing about mass man who has little primary cognizance of the world as it is, but his generalization often applies to the public official who has command of the decisional institutions and the media of influence. Bureaucratic man perceives the world through the routines and roles that govern the daily affairs of public agencies; budgetary man's view is shaped and bounded by the accounts and forms that supply the bits and pieces from which the budget is aggregated. What is the connection between the figures inside the budget and the world outside? The predominant liberal stereotype seems to be that higher public spending yields higher social returns. The usual conservative view is that greater public spending produces waste and a net decrease in social welfare. Suppose one wanted to test these contradictory images against reality in the case of the Safe Streets Act: What is the relation of more Federal money for law enforcement to my ability to walk home or through the park safely at night? This is not the kind of question that ordinarily is raised or answered in the course of budget making.

The answer cannot be derived from the figures in the budget or from the data collected during the budget cycle. The budget view is insular, riveted to the figures inside, not to what happens outside government as a consequence of public choices. The budgeter's points of reference are what was spent last year, what is mandated by existing legislation or by price and workload increases, and what is the revenue outlook. Although he may be dedicated to the improvement of health, the budgeter can give very little formal and precise attention to the impact of public expenditures on health.

At the very minimum, an analytic disposition in budgeting demands that the figures in the budget be explicitly and reliably related to the world outside. Of course, that is not all that is required, but one cannot make analytic budget decisions without linking the expenditures to real world outcomes. Accordingly, before any system for analysis is prescribed or tried, one ought to be informed of the forces that have made contemporary budgeting nonanalytic (or antianalytic). After all, there were almost 50 years of budget experience before PPB arrived

¹ Walter Lippmann, *Public Opinion* (The Macmillan Co., 1922).

on the scene. Formative practices have hardened into traditions that cannot be dislodged easily. Here are some of the factors that have contributed to the current state of affairs in budgeting.²

The routinization of budget choice.—Budgeting has become one of the triumphs of bureaucratic order and regularity. The books are opened and closed for each fiscal year, the accounts maintained, and the forms filed, all with fidelity to the deadlines and the rules. This routine invites insulation from the winds of change that blow outside. Budgeting comes to esteem and rely on that which can be routinized; the things that can be routinized often are the matters pertaining to the operations of public agencies rather than to outside events. A Gresham's law is at work: routine drives out analysis.³ The forms and routines force one's attention to the worksheets and the ledgers, away from the ghettos, hospitals, or schoolrooms. The massive paperwork spawned by Circular A-11⁴ deals with the business of public agencies, and only inferentially with the quality of education or the inventory of housing. It is commonplace to speak of the potency and political implications of the budget process, but the facts often are otherwise. Of the incalculable number of manyears spent on the budget, only a few are spent on making decisions. For the most part, budgeting is the costing out of decisions already made rather than making the decisions themselves. If one wishes to alter the course of events (usually this means new program starts rather than terminations), often one must use the less routinized channels of legislation making rather than the formal apparatus of the budget. While the nonanalytic tendency exists at all levels of the budget process, it is especially pronounced within the agencies. At the central levels, in the Bureau of the Budget, there is considerably more freedom from routine and concentration on program issues. (See "Budgeting from the bottom-up," below)

The control of conflict.—Budgeting is a tribute to the art of conflict management. Billions of dollars are at stake, but the competition is played according to rules that limit the scope and intensity of conflict. The insularity of budgeting from outside happenings is one of the means of limiting conflict. If budget choices were made explicitly in terms of external events and objectives, the participants probably would be divided over the conditions outside and the proper governmental role. Moreover, the disputes would spill into public arenas and not be confined to the privacy of executive discussion and negotiation. The insular perspective of budget making allows the claimants to bargain according to conflict-limiting guidelines such as last-year's level of spending. These are nonanalytic rules which reduce conflict by providing each budget claimant a measure of security and status. If budget choices are to be made in terms of input-output relations, the bureaucracy will have to tolerate the higher level of sustained conflict that accompanies the questioning of existing courses of action and the analysis of alternative opportunities.

² Aaron Wildavsky's *The Politics of the Budgetary Process* (Little, Brown & Co., 1964) is an excellent and interesting source for studying why the strategies and roles of the participants leave little opportunity for analytic budgeting.

³ This use of the term is adapted from James G. March and Herbert A. Simon, *Organizations* (John Wiley & Sons, 1958), p. 185, who explain: " * * * when an individual is faced both with highly programed and highly unprogramed tasks, the former tend to take precedence over the latter even in the absence of strong overall time pressure."

⁴ Circular A-11 is the set of instructions issued by the Bureau of the Budget for the preparation of budget estimates by the agencies. It contains a large number of forms and is the basic document used in the preparation and review of agency requests.

Budgeting from the bottom up.—The lower one descends in a bureaucracy, the more provincial and confined are its perspectives. In the bowels of an organization, the view is almost entirely insular, shaped by the flow of paperwork from one desk to another. Lower downs commune with other bureaucrats, usually their peers and immediate superiors. They have few transactions with outsiders, or only with outsiders who are their direct clients (for example, hospital patients and social security recipients). Higher officials move in broader circles where outside ferments are closely related to their work. They also handle a more diverse range of assignments and are not as constrained by the administrative routines of the organization. It is consequential, therefore, that the budget is built from the bottom up rather than from the top down. In most instances, lower levels formulate their estimates with minimal policy guidance from above. Their mode of operation, inevitably, is to consult the accounting records of previous spending and to adhere to the formal rules. They have little incentive or capability to explore possible departures from established programs, for they lack both analytic insight and central perspectives. Accordingly, the budget presented by the President is largely the nonanalytic aggregation of bits and pieces appropriate for lower level choice. It is whole or systemic only in the sense that the parts are totaled into larger categories for presentation as a unified document.

The limits of budgetary intelligence.—Nowadays, public officials are faced with a revolution of rising ignorance. As the variety and scope of Federal programs have expanded, it has become increasingly difficult to relate what is decided in the bureaucracy to what happens outside. The rise in public entanglements (intergovernmental, interdepartmental, and public-private) has outpaced our ability to supply sure and accurate answers. No one can speak with certainty of the impact of Federal aid to education on ghetto children via title I of the Elementary and Secondary Education Act. Where there are no answers, soon there will be no questions, for there always is a deadline around the corner and a job to be done. Paralysis by analysis (the not-so-kind epitaph applied by some prominent critics to the McNamara approach) is not a welcome pastime among officials who would rather have settled decisions than better data. When a budgeter maintains his insular view, he can sharply cut down the difficulties of deciding, for he eliminates from the picture most of the variables that cannot be accounted within the organization. The budgeting and administrative reporting processes are tooled up to produce masses of information on what is being spent, but only the most scattered information on the effects of public spending. To put together an analysis that is adequately informed is at best a difficult task. Often the final product is flawed by telltale signs of guesswork and patchwork. Budgeting's repertoire of nonanalytic data is keyed to its cycle of routines and deadlines, not to top-level program choices. Even when special reports are made by contractors or task forces, they usually are outside the stream of budgeting. To obtain estimates on objectives or effectiveness, one must track down forgotten reports and neglected pilot studies. Sometimes no data is available, regardless of the inventiveness or industry expended in the search. It is no wonder that many of the early PPB analyses have been statements of what is not known or specifications of what ought to be known.

An organization is encouraged to analyze if it knows the relation between what it does and what happens. It might be argued that analysis is more essential when the link between programs and results is unknown or uncertain. But often the opposite will occur; the organization will be deterred from analysis if it lacks satisfactory knowledge of the impact of its decisions. After all, if you are ignorant about the connection between action and results, why analyze; it's a lot better to try many things at once in the hope that a few might work even if the rest don't. This is particularly true in the crisis atmosphere of current urban and antipoverty programs.

The net effect of the budget traditions described above is to foster an environment that is hostile to analysis. The incremental routines vividly described by Wildavsky are the very antithesis of analytic budgeting. Given this nonanalytic condition, it is necessary to ask whether the analytic enterprise should be as closely tied to budgeting as it is under PPB. Perhaps the prescribed linkage of analysis (and planning) to budgeting will inhibit rather than encourage usable policy analysis. This is a question to which I will return in subsequent sections.

SYSTEMS OR ANALYSIS?

Two popular terms have been brought together in the systems analysis concept. This alliance is predicated on faith in the compatibility of systems and analysis. Yet these terms represent divergent conceptions of the appropriate means for enhancing the quality of public choice.

The analytic position has been presented by Aaron Wildavsky in his paper published in this collection.* Wildavsky argues that the best way to improve the supply and use of analysis is to drop the systems framework and to pursue analytic opportunities wherever they are available. Although his arguments are directed against the PPB system, they are applicable to all systems approaches. At the core of the analytic view is the fear that systems inevitably detract from analysis, that they impose considerable costs of their own, and that policymakers lose sight of their analytic goals and get bogged down in the routines and requirements that are mandated by the system. In an analytic approach, there would be no overarching information or decisional system (such as is imposed by PPB's program categories). Nor would there be any formal procedure for commissioning analytic studies and for feeding the studies into decisional channels. Rather the analytic enterprise would be sparked by the native interests of top officials and by spasmodic opportunities for analysis.

The systems approach is grounded on the conviction that analysis will wither unless it is sponsored and done within an established decisional structure. Those who favor the systems tactic are mindful that analysis is the main event and that a system is no better than the analytic choices it produces. It cannot be denied that the prevailing system's prodigious amount of paperwork requirement has retarded analysis. Nevertheless, the case for systems remains valid, though systems people have become somewhat alert to the need for a system that does not impede analysis. But it is not easy to routinize analysis with-

*Further discussion of this issue is found in the paper by Wildavsky in this volume.

out making the analysis routine. In order to ensure favorable conditions for analysis, systems should be designed with a minimum of formal specifications. And all systems requirements should be tested in terms of the analytic ends they are intended to further.

If analysis is the objective, why not discard the systems framework altogether? Budgeting's antianalytic posture makes it essential that some structure for analysis be provided. To advocate analysis without providing a framework within which it can be done and used is an empty gesture. The utter impoverishment of the budget process from an analytic standpoint attests to the need for some new spur for analysis. Before PPB there was no bar against analysis, but the incremental rules and routines effectively preempted public expenditure analysis. If budgeting were analytic or receptive to analysis, the case for a systems approach would be weak. But one cannot divorce the systems versus analysis issue from the established budgetary context and traditions. While he has forcefully argued against systems budgeting, Wildavsky has compiled the evidence which justifies a systems approach.

The contrasting experiences of two municipalities in the forefront of local PPB applications suggests the necessary relationship of systems and analysis. Philadelphia began its PPB efforts on a systems basis. The first order of business was the construction of a citywide program structure that cut across departments and brought together program elements that were dispersed in a number of agencies. Concomitant adjustments were made in the accounting structure and the basic information systems. But all this systems apparatus did not produce a substantial analytic payoff until the city established analytic teams to handle specific issues. New York City began on an analytic track. In testimony before the Joint Economic Committee, New York City's budget director explained:

Our overall approach has, deliberately, been opportunistic, rather than systematic and comprehensive. We have concentrated our efforts on analysis, rather than on program structure and accounts, and we have focused on sectors of high apparent yield.⁵ Despite its analytic start, New York City recently found it desirable to establish a PPB system that far exceeds any other government's in its specification of forms and procedures.⁶ The lesson of these experiences is that regardless of where you begin, sooner or later you will have both systems and analysis. If there is no system for analysis, there will be a lack of analytic data and the demand for analysis will wane.

In plotting the implementation of PPB, the Bureau of the Budget tried a middle course.* It prescribed a minimum of procedure and documentation, but even that minimum was perceived in the agencies as a call for technique rather than analysis. The standard BOB-agency relationship and the forceful manner in which PPB was introduced, not the systems characteristics, have been responsible for the overformalization of PPB. Recent PPB guidelines (bulletins 68-2 and

⁵ Subcommittee on Economy in Government, Joint Economic Committee, *The Planning-Programming-Budgeting System: Progress and Potentials*, p. 95.

⁶ See: The City of New York, *Fiscal Year 1969-70 Program/Budget Instruction*.

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection, and Hoffman in this volume.

68-9) have tried to shift the system to a clearer analytic focus, but the identifications formed during PPB's initial years have not been eradicated.

PRESIDENTIAL OR AGENCY SYSTEMS

A system for analysis can be oriented either to presidential perspectives and objectives or to agency purposes. If it tries to serve both masters, a system will break down under conflicts of interest and design. In conception, the Bureau opted for an agency system, but the agencies (outside the Defense Department) felt that the system was intended for a presidential mission. The source of this "understanding gap" was BOB's role in the introduction of PPB. The agencies saw PPB as the Bureau's brain child, and they expected BOB to play the pied piper of the Federal bureaucracy, forcing recalcitrant and indifferent agencies from their tradition ways and using its analytic system to impose the Presidential will on department programs and budgets. From an agency point of view, BOB was the client demanding program memoranda, issue analyses, future year estimates. The Bureau had a different view of its role and of the use of PPB.⁷ It regarded PPB as a tool of departmental policy leadership, to be used by the department head to gain command over subordinate bureaus and to shape his agency's programs according to his wishes. It is probable that the agency orientation is due to the McNamara influence. The Bureau saw PPB as the means of bolstering each Secretary to enable him to gain command over subordinates. Accordingly, BOB has gone along with a program structure that is a composite of individual agency categories. There has been no attempt to apply an interdepartmental program structure or to formalize procedures for the analysis of programs that transcend department lines. Moreover, the Bureau added only minimally to its own staff to handle the PPB effort, but it directed agencies to establish separate staffs to manage the new system and to conduct analysis. Yet the signals from BOB have been ambiguous. For example, the issue analysis process, beginning with the commissioning of special studies by the Bureau and ending with Bureau review of agency submissions, stamps PPB as a tool of central authority. In effect the agencies are given the message: "Examine these issues because we suspect that the returns do not justify the costs." If PPB were truly agency-oriented, each agency would determine its analytic work.

The prospects for agency systems vary with the conditions in each agency. Undoubtedly, the devolution of systems initiative and responsibility to departments would mean that certain departments would abandon their fragile analytic operations and revert to non-analytic budgeting and ad hoc program making. But some agencies possess the leadership interest and analytic capability to maintain their own analytic systems even if BOB requirements are terminated. Enough analytic interest has been seeded in some departments to insure that the clock will not be turned back to 1965 and earlier.

⁷ It may be misleading to imply that the Bureau viewed PPB in Presidential versus agency terms. Top Bureau officials believed that as a department head becomes stronger he also is more likely to be a President's man rather than beholden to agency interests.

Nevertheless the main orientation of the PPB system was agency. Bureau policy was that the best way to enhance the Presidential position is by building up departmental PPB capability, not by using the power and resources of the Executive Office to do extra-departmental planning and analysis.

(Contrary to some expectations, the new Secretary of Defense, Melvin Laird, has decided to retain the systems analysis group established by McNamara and Hitch.)

An agency oriented system would diverge from the PPB pattern in several ways: (1) There would be greater variety in agency systems for analysis. Each agency system would be contoured to its own circumstances, subject only to some general guidance from BOB. (2) Agencies would decide how to invest their analytic resources. They would probably be more interested in analysis for program development than evaluation of ongoing programs. (3) BOB would have an advisory role, and perhaps some direct responsibility for activities that are not clearly within the jurisdiction of a single agency. (4) In some agencies, the analytic system would be separated from the budget process. Analytic effort would be targeted to the legislative process and to the other channels of program making.

An agency oriented analytic system can be a steppingstone to a presidential system. Indeed, departmental success and confidence may be a prerequisite for a presidential system. Nevertheless, an agency system must labor with several critical limitations. First, it does not provide for representation of the presidential interest in department policymaking. Second, policy analysis probably would be confined to new programs while existing programs continue to escape scrutiny. Third, the basic nonanalytic budget process would be preserved. Finally, an agency orientation would be of little value for the growing number of key programs that involve the resources and interests of several departments.

A presidential system for analysis would overturn many budget and political relationships. The bottom-up budgeting procedure described earlier would be supplanted by greater presidential and central policy involvement before the estimates are formulated. Department heads would have to become presidential men in fact as well as in name before some central authority (whether Bureau of the Budget or some new unit in the Executive Office) could attain preeminence in program policymaking. The early evidence suggests that President Nixon will not want PPB to have an expanded policy role; perhaps he will want the Bureau to revert to its caretaker, economizing role of the 1950's.

In sum, a presidential system is premature and an agency system is precarious. A presidential system carries the risk of more innovation than political interests will tolerate; an agency system carries the risk of more status quo than these troubled times can afford. Regardless of the system that is installed, its mission and focus should be clear; there should be no inconsistency between the intent of the system and the way it is perceived. We should not continue with the error of a PPB system which was intended for the agencies, half-heartedly designed for the President, and operated to serve neither interest properly.

SELECTIVE OR ACROSS THE BOARD

Of subsidiary concern is the issue of whether a system for analysis should be limited to selective agencies or applied across the board. The Bureau of the Budget wrestled with this problem in 1965 and it

leaned toward a selective approach. But certain circumstances induced the President and the Bureau to opt for a governmentwide institutionalization of PPB.

Actually, the issue does not have to be confronted in either/or terms. Clearly, analysis cannot be comprehensive. To try to analyze everything is to end up analyzing nothing. It is not possible to fit all types of analysis into some procrustean mold. The methodology of analysis necessarily is governed by the subject and the analyst. Accordingly, the analysis itself must be individualistic. Yet the system for feeding analysis to decisionmakers can be relatively uniform for all agencies and programs. In its latest PPB bulletins, the Bureau of the Budget correctly has moved toward selectivity in analysis even though the systems features are standard for all agencies. Thus, program memoranda now need be submitted only where there are major program issues.

Analysis would be enhanced if the Bureau of the Budget made the across-the-board system subservient to selective analysis. Where agencies are incapable of or unwilling to undertake program analysis, the cause of policy analysis is not served by insisting that they ritualistically adhere to systemswide requirements. There is no gain in going through the motions without producing any analytic output. HUD's failure to submit its required program memoranda was not more injurious to the fiscal year 1970 budget than were the successes of other agencies in meeting submission deadlines and giving lip service to the PPB routines.

Without abandoning an overall systems strategy, the Bureau of the Budget might authorize analytically competent agencies to adjust the formal requirements to their analytic operations. For example, HEW might be allowed to transmit its program analyses in lieu of some of the prescribed documentation. Judging from the current state of analysis, little would be lost if the Bureau gave less attention to the program categories and instead bolstered its procedures for identifying major program issues and reviewing the analytic studies.

TYPES OF ANALYSIS

If systems are for analysis, they ought to be tailored to the types of analysis that are being done. One could design a hypothetical system that confronted V. O. Key's classic question: "On what basis shall it be decided to allocate x dollars to activity A instead of activity B?"⁸

But such a system would not correspond to the problems that are being handled by budget and program makers. The welfare economist might be concerned about the last dollar's worth of battleships versus poor relief (in Pigou's formulation), but the working analyst traffics in much more modest concerns. Despite all the talk about cost-benefit analysis, there are too many conceptual and operational difficulties to the implementation of useful benefit analysis at this time. Economists who have joined the analytic staffs have had to trim their sails and put a good deal of their methodological equipment into storage. It is not that the problems confronting Government are simple; they are too difficult to solve with the high powered methods now at hand. Before benefits can be measured, they have to be identified. Some scale of

⁸ V. O. Key, "The Lack of a Budgetary Theory," *The American Political Science Review*, vol. XXXIV (December 1940), pp. 1137-44.

values must be set. The question of values is especially troublesome, for each discipline and interest has its own way of seeing and evaluating things.

If policy analysis were focused on public benefits, it would be appropriate to have a system structured according to the purposes of Government. An end-product program structure would facilitate the comparison of alternative program opportunities on some homogeneous value scale. Such is not the case, however. Most policy analysis deals not with benefits, but with program effectiveness.⁹ Only implicitly does the analyst put a value of the program he is studying. For example, a billion dollar health care program might be adjudged the most cost effective if it yields a lower infant mortality rate than any alternative billion dollar program. Unlike benefit analysis which begins with some social value, effectiveness analysis begins with a concrete set of objectives that are embodied in specific programs or with a problem that concerns policymakers. In appraising a health care program, one need not place some value on the life of an infant. One need only assume that more lives saved is preferable (i.e., more effective) to fewer lives saved.

The conception of effectiveness analysis was expressed by William Gorham in his statement for the Joint Economic Committee's PPB hearings in September 1967:

* * * we have not attempted any grandiose cost-benefit analyses designed to reveal whether the total benefits from an additional million spent on health programs would be higher or lower than that from an additional million spent on education or welfare. * * * The benefits of health, education, and welfare programs are diverse and often intangible. * * * No amount of analysis is going to tell us whether the Nation benefits more from sending a slum child to preschool, providing medical care to an old man or enabling a disabled housewife to resume her normal activities. * * *

The less grand decisions, those among alternative programs with the same or similar objectives within health—can be substantially illuminated by good analysis. It is this type of analysis which we have undertaken at the Department of Health, Education, and Welfare.¹⁰

For purposes of effectiveness analysis, much of the systems machinery associated with PPB is irrelevant. The starting point for an analysis of effectiveness is a specific problem or objective, not a set of program categories. The President is interested in programs, not in program categories. Unless the categories are designed with sensitive attention to problems as they are perceived by top officials and unless they are revised frequently to reflect changing perspectives, the program categories will hinder rather than abet useful policy analysis. It is very doubtful that this kind of categorization can be devised. The analyses undertaken in HEW ignored the boundaries imposed by the program categories. Problems don't come packaged according to some grand formulation of governmental ends. The analyst must pursue his problem in whatever format is appropriate, regardless of the constraints of the data system. Sometimes he will want to look at health from the viewpoint of target groups—expectant mothers,

⁹ This matter is elaborated in Allen Schick, "PPB's First Years: Premature and Maturing," (Mimeo: U.S. Bureau of the Budget: September 1968), pp. 14-24.

¹⁰ Joint Economic Committee, Hearings, *op. cit.*, p. 5.

the needy, the elderly. Other times, he will want to study health in terms of diseases—heart, kidney, cancer, and so on.

The machinery of PPB was determined not by analytic purposes but by the characteristics of the budget process. Program categories were established as a counterpart to the conventional budget and appropriation categories, not because they would be helpful for analysis. Much the same applies to the program memoranda (PM's) and the program and financial plans (PFP's). These documents were conceived as means of linking analysis to budgeting, not as means of spurring the supply of and demand for analysis. Unfortunately, there has been little analysis to report via the PM's and PFP's, for little analysis has been done. As a matter of fact, the cumbersome systems for reporting analysis have discouraged policy analysis by forcing agencies to invest more effort on keeping the system going than on producing analysis.

If the system is to fit the analysis, several changes will have to be made in the system currently in operation. First, the system itself should be designed to abet analysis, not for reporting analysis to budgeters. Second, the most formal and carefully developed aspect of the system should be that pertaining to the commissioning of analytic studies and the use of these studies. Third, the system should be geared to the analysis of effectiveness not to generalized conceptions of governmental purposes and benefits. Can the PPB system meet these criteria, or is a different system for analysis required? To answer this question, it is necessary to compare PPB with alternative systems.

ALTERNATIVE SYSTEMS FOR ANALYSIS

Although the PPB system was designed for budgetary purposes, there are a number of different ways of relating a new analytic enterprise to the ongoing budget process. In this section, I will describe four alternative configurations, three of which are keyed to budgeting and a final one which is geared to other decisional arenas. These four systems are: (1) A *crosswalk* system in which budgeting and analysis are closely and formally linked; (2) a *two-track* system in which analysis is relatively independent of budgeting, but the analytic products are targeted to budget decisions; (3) *analytic budgeting* in which the budget process is revamped and made analytic; (4) a *policy planning* system in which analysis is divorced from budget operations and is channeled to other instruments of public choice such as the procedures for determining legislative proposals and program policies.

*Crosswalk systems.**—The distinctive feature of crosswalk systems is that analysis is commissioned, produced and reported expressly for purposes of budget making. The procedures for selecting analytic studies and for reporting their findings are governed by the requirements and priorities of budgeting. The calendar for analysis is phased to the existing budget cycle; budgetary deadlines, not the shape of the analytic problem determine how and when analyses are reported. Similarly, budgeting and analysis are connected; at the top by a central budget agency that directs both efforts, and within the

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

agencies by giving a single official (usually the undersecretary or an assistant secretary for administration) responsibility for budgeting and analysis. In a crosswalk system, the job of the central authorities is to manage the analytic apparatus and to monitor analytic activity in the agencies, but not to do the analytic work itself. For this purpose, a small staff (along the size of the program evaluation staff in the Bureau of the Budget) is established. The traditional budget documents (embodied in Circular A-11) are not modified significantly; rather new reporting documents feed the analytic data into the budget stream. The program memoranda and program and financial plans were intended for this crosswalk function. They are not analytic documents, but only the means of conveying analytic findings to budgetmakers. A crosswalk arrangement requires a precise and formal reconciliation of the financial and organizational accounts with the program categories. For this reason, considerable attention has been given to the program structure and to the accounting system. The analytic enterprise itself is only a small aspect of the system, for the main problem is the delivery of analysis to the right (budget) client. Accordingly, the apparatus for selecting and reviewing analytic studies is not elaborated in the PPB guidelines.

When the Bureau of the Budget decided to establish a system for policy analysis, it opted for the crosswalk system. Planning and programming are linked to and serve the system's end product—budgeting. Let us suggest several explanations of the Bureau's course of action. For one thing, the new system was conceived and operated by budgeters, policymakers who tend to regard the budget outcome as the critical decision. Had a different set of officials—say planners—been in control, they probably would have built a system that reflected their specialized perspectives. Moreover, the economists who had command of the Budget Bureau in the mid-1960's understandably viewed the budget as Government's most effective rationing and economizing device. From their view point, it is the only formal instrument for trading-off among competing claims for public funds. This attitude is not confined to economists. It accounts for the tendency of governments to attach many management and control chores to the budget process. Third, the crosswalk system seemed to be the best way of insuring that analytic data would be used. Finally, the crosswalk system involved fewer upheavals in budget and administrative practice than did any of its alternatives.

Two-track systems.—A two-track system also is aimed at budget decisions, but it allows much more independence of analysis from budgeting. The linkage is not tightly forged for pumping analysis into budget choice. Rather analysis is allowed "to do its thing" according to its own timetable and procedures. The objective remains an analytic product that can influence budget choice, but the impact of analysis depends on its quality and strength as well as on organizational and political circumstances, not on some standard formula for relating it to the budget. Consequently, the use of analysis will be spasmodic, rising or falling as top level support is granted or withheld.

In the two-track system, the budget process is not explicitly revamped to accommodate the new analytic enterprise. The routines and procedures continue as they were, and the roles and relationships of the budget authority are shaped by traditional influences. But the

existence of a large analytic operation (unlike the small program evaluation nucleus provided under the crosswalk arrangement) lends status and potency to policy analysis and enables it to compete with the budget office when critical program decisions are made. The mission of the central analytic group is to do and use analysis, not to maintain the system. The procedures for selecting and reviewing analytic studies are more formalized and elaborated than under crosswalking, while the methods for reporting the analysis (such as the PM's and PFP's) are more casual.

The two-track system recognizes that budgeting and analysis have different time requirements, and that budgeters and analysts have different perspectives and operate with different constraints. Analysis is not bent to the routines of budgeting. The selection of analytic studies is more likely to be influenced by program than by fiscal considerations. Therefore, the studies probably will be more concerned with program development than with the evaluation of ongoing programs.

Although it is generally assumed that the Government-wide analytic system was modeled after the Pentagon's, Defense employed some characteristics of a two-track system, especially after the bifurcation of systems analysis and budgeting. The system worked in Defense because two crucial criteria were met: top support from Secretary McNamara and his aides, and a critical mass of analytic talent and influence. Without these conditions, a two-track system can be endangered by irrelevant and unused analysis. If there is no strong, sustained demand for analysis, the lack of a formal, close linkup of analysis to budgeting will be fatal. Analysis will be irrelevant and outside the channels of policymaking.

Analytic budgeting.—Both of the systems outlined above retain the traditional budget process but subject it to new analytic influences. Analytic budgeting means the revamping of the budget process so that it becomes analytic. There is no separation of analysis from budgeting; hence, no need for a formal (crosswalk) or informal (two-track) connection. But in order to achieve analytic budgeting, radical changes must be made in many facets of the budget process. The central budget agency will have to spin off its control and management routines to some new unit or to the operating departments. This divestiture will enable it to take on a program development and policy leadership role. The budget staff's skills and perspectives must be altered in accord with the new orientation. There may be a separate analytic group in BOB or the Executive Office, but it handles matters that do not fit into the analytic routines of budgeting. The bulk of analysis is done by regular units in the central Budget Bureau and the departments. The budget process is purged of a good deal of its detail, and the existing budget forms and documents are replaced by new informational and decisional procedures that pertain to policy outcomes rather than to the internal affairs of Government agencies. The bottom-up budget tradition is reversed as top levels furnish policy direction to subordinates prior to preparation of the detailed budget estimates.

In a full-blown analytic budget system, there would be routine trading off among alternatives. The decisional channels are reserved for matters warranting top action which are subjected to full analytic scrutiny within a framework that allows policymakers to compare

prospective outcomes. The budget horizon is lengthened to an appropriate multiyear span. Decisions regularly are made in terms of multi-year rather than next-year impacts. Adjustments have to be made in the authorization and appropriation process to accommodate this longer time frame. Instead of pretending to review every item in the budget annually, central executive and legislative authorities cycle the budget process to examine some segment of the budget each year, with the remaining portions being funded under continuing appropriations until they are scheduled for review. Of course, allowances would have to be made for program changes that cannot be deferred until the program's turn is reached in the multiyear cycle. In addition, methods would have to be provided for unanticipated contingencies: a fiscal crisis, changing political positions, a rise in unemployment, or in welfare costs. Probably there would be a need to equip the President with authority to adjust a program's budget within the range and according to rules set by Congress.

Obviously, a radical analytic budgeting scheme is not going to be approved by Congress or the President unless there are enormous, unforeseeable changes in political attitudes and relationships. In 1965, analytic budgeting was not an operational alternative to PPB. But more forceful and effective steps toward analytic budgeting can be taken than are provided under either the crosswalk or two-track method. The first steps might include the reorientation and reorganization of the formal budget process, the importation of new men and methods into budgeting, changes in the utilization of the budget process by the President, and in the Budget Bureau's relationships with the departments. None of these will be easy to attain, but they are the implications that must be faced if a commitment is made to use budgeting for analytic purposes.

RELATING ANALYSIS TO BUDGETING

Before examining the final systems approach (planning), let us compare the three systems that are (more or less) tied to budgeting. Analytic budgeting has one great virtue. More than any alternative system, it brings analysis into the established centers of policymaking. But the price is too high. Practical men in Congress and the executive cannot adopt it.

The two-track system has the advantage of allowing analysis to determine its own pace and place in policy formulation. It is not unduly constrained by the necessities and habits of budgeting. But it, too, goes further than budgeters are able to move, and it constantly is threatened by irrelevance. The Bureau of the Budget opted for crosswalking because it was the easiest, least disruptive course of action. The entrenched budget apparatus was left alone. No major analytic organization rivaling BOB was established. All that was necessary was to couple some rudimentary analytic work to the budget cycle. This was accomplished via the program categories, program memorandums, and program and financial plans—the constituent routines of PPB. Analysis would bloom slowly, in the relevant and safe context of budgeting. There would not have to be big upheavals to accommodate the newcomer. The trumpets would announce a major new system, but the insiders would know that the core had remained

intact and changes at the periphery would not challenge the existing order. The risks to the budget process were held to a minimum. The time-proven operation under Circular A-11 was not traded away in favor of a new system. Not a single beat was missed in the cycle of budget events. The figures in the big document had their old meaning and reliability.

In terms of minimizing risk and difficulties of installation, the cross-walk alternative was the clear and necessary choice. But this is the view from the budget process. Viewed in terms of the quality of public choice, the other alternatives might be superior, precisely because they take greater risks in attempting to inject policy analysis into public decisions. Unless analysis is somewhat insulated from the budget function, there is a danger that it will be preempted by the control and management routines of budgeting. The considerable divergence between the roles and skills required for analysis and those currently used in budgeting suggests that some slack is necessary if analysis is going to be able to stand on its own feet and make a creditable input into policy decisions. This is particularly true during the early years of systems analysis when the nascent analytic enterprise has not yet established roots and linkages.

Because it projects a tight integration of budgeting and analysis, the crosswalk system is the most vulnerable to the disparate tendencies and necessities of the budget and analytic processes. And because budgeting is the entrenched of the two processes, it will dominate whatever alliance is formed. The result will be the failure of analysis. I believe this is precisely what has happened in the case of PPB. Apart from any errors in design or implementation, PPB could not have succeeded because it forced analysis into a mold that was antithetic to analysis.

Consider the time frames of budgeting and analysis.¹¹ The lengthy and detailed preparation of estimates demands a strict sequence of repetitive steps and techniques. All this is appropriate for the accuracy in details that is sought for purposes of budgetary control. Real analysis is destroyed by fixed routines and omnipresent deadlines. Analysis is opportunistic and episodic, taking advantage of circumstance and need. It cannot be programed in advance for the full sweep of Government activities and expenditures. Although short-term analysis can be—and has been—crowded into the budget calendar, fundamental policy analysis leading to possible changes in major programs must have some independence from the budget routine. While the management and control functions of budgeting invite a retrospective focus, analysis has a prospective bias. For budget purposes, one is likely to be interested in how this year's spending compares to last year's. Accordingly, the information system for budgeting will have to provide detailed data on the spending history of each agency. Information systems for analysis, however, have a different focus. The emphasis shifts from past spending to future goals. Budgeting inevitably is bounded by the fiscal year calendar. It is imperative to know exactly how much was or is to be spent during some standard unit of time. Analysis has a variable time frame; it is not coterminous with the fiscal year nor does it have uniform beginnings and endings. Viewed from an analytic per-

¹¹ This section is adapted from Allen Schick, "Multipurpose Budget Systems" (mimeo: U.S. Bureau of the Budget: March 1968), p. 23ff.

spective, the fiscal year is an artificial and possibly obstructive boundary.

As yet no reliable way has been found to synchronize the analytic timetable with the budget clock. PPB has met the chronological dilemma in a number of unproductive, yet understandable, ways. The prevalent practice has been to let budgeters and budgeting retain their paramountcy at the expense of planning and analysis. In the absence of market constraints, the budget is Government's most effective rationing apparatus. There is no substitute for budgeting's ability to force a balance between resources and demands. Recently, the dominance of budgeting has been strengthened by the Vietnam-induced stringency and the virtual halt on program development. When the budget becomes tighter, the budgeters become stronger. A second response, discussed above, had been to make analysts work within the budget calendar. Thus the PM's and the PFP's have been cycled to the usual budget deadlines with the predictable result that many of the plans and analyses have been nonthink pieces. A third response has been to convert the analysts into budgeters; that is, to divert them from analytic efforts to pending budget assignments.

The timing dilemma is due to a faulty conception of the analytic input into policy. When PPB was launched, there was a determination to build up its record of successes; it was never allowed to move at a pace appropriate for analysis. I am convinced that if the new analysts had been instructed at the start—"Don't be concerned about this year's budget. Here are a few issues that the Secretary really is interested in. Show us what you can produce." PPB would have a much more impressive record. But the hectic attempt to produce immediate results did not afford much opportunity for patience and farsightedness in the installation of the new system.

As the operational form of a crosswalk system, PPB is easy to install and involves few risks. But it is not likely to introduce significant changes into the methodology of public choice. Analysis will succumb to the nonanalytic routines of budgeting, and decisions will continue to be made under the old rules. If the goal is to make a difference in the quality of policy outcomes, it would be sensible to consider a shift from the crosswalk to a two-track system. Although the risks are higher, so, too, are the prospects for meaningful improvements in the institutions of public choice.

POLICY PLANNING SYSTEMS

The attempt to link analysis to budgeting is a logical recognition of the place and potency of the budget process in public policymaking. All of the analytic systems outlined above depend on the budget process and anticipate that analysis will bear fruit through the outcomes in the budget. Yet it is appropriate to question the connection to budgeting and to raise the possibility of some alternative outlet for analysis. I have argued that budgeting is nonanalytic and that a rigid integration of analysis and budgeting will not be successful. Now I want to carry the argument one step further by suggesting that the cause of analysis would be better served if analytic work were addressed to the processes of program determination and legislative recommendation. These processes are not well formalized, but they are the processes which deal

with the big issues, which mark departures from the status quo and changes in direction. The overwhelming weight of the budget process favors the continuation of what is already on the books. When a President wishes to launch new programs, he is impelled to rely on task forces, advisory staff, and ad hoc arrangement. All these are lacking sustained analytic focus, but perhaps they are more useful than the budget process. In the crowded months of the budget cycle, there just isn't enough time or inclination to consider the bigger issues, to look beyond the present and the certain to the future and the speculative.

While analysis can be channeled to both planning and budgeting, I would urge that attention be given to the neglected opportunities for planning. We tend to rely too heavily on an overburdened budget process and not enough on other decisional institutions. I am not able in this paper to spell out the possible planning configurations. Clearly, there will be a variety of possibilities, including the institutionalization of planning in the Executive Office and the formalization within executive agencies of some program change procedure such as exists in the Defense Department for weapons systems decisions. At the very least a strong planning apparatus will open up another center in Washington for the application of intelligence and creativity to the solution of hard-core social and economic problems. It might even come to rival the budget process and provide an antidote to budgeting's status quo biases.

Recent moves by the new administration seem to portend a shift from budgeting to policy planning centered in the White House. The expansion of the President's staff beyond its previous size and scope suggests that the President will want to rely more on policy advisers than on a Bureau of the Budget-centered operation. The enlargement of the National Security Council staff, the Kissinger apparatus, and the establishment of an urban affairs council under Daniel Moynihan must be clues to the President's thinking. The "eviction" of Bureau of the Budget units from the prestigious Executive Office Building is not just a change in locale. It represents a loss of status and presence. It is ironic that the Bureau of the Budget reached its policymaking apogee in the 1960's when its inadequacies as a policy planning institution became evident. PPB could not change the gap between analytic need and performance because it was dependent on a budget process suited to nonanalytic functions.

CONCLUSION: RESCUE POLICY ANALYSIS FROM BUDGETING

The theme of this paper is that the poverty of analysis stems from its forced linkage to budgeting. If policy analysis is to flourish, it will have to be rescued from budgeting. This can be accomplished via the two-track and policy-analysis systems. The only other productive alternative is to reshape the budget process into an analytic instrument. To continue with PPB's crosswalk relationship is to invite certain failure and disappointment.

This conclusion has nothing to do with the competence of budgeters; it pertains to their roles and to the historical use of the budget process. The control and management functions are predominant and preemptive. PPB has not and cannot change that situation. Only a venturesome and major investment in analysis can overturn decades of tradition making. It is worth the effort and the risk, for the costs of ignorance and the opportunities for intelligent public choice man-

date a full commitment to analytic decisionmaking. There is some evidence that the effort will be forthcoming. The Bureau of the Budget is now preparing for the fiscal year 1971 budget cycle. As one of the first steps, a memorandum has been circulated clarifying the process for selecting analytic studies. It is the intent of the Bureau that only major policy issues be examined (a \$50 million threshold is suggested) and that the number of analyses be reduced so that agencies and the Bureau can concentrate on truly significant issues.* In addition, renewed use will be made of the spring preview as the appropriate occasion for reviewing policy analyses and programs. Although these moves do not abandon the crosswalk system, they show a recognition that analysis is the objective of the system and that it is imperative to improve the system's capability to produce quality analysis in a form suitable for policy choice.

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

RESCUING POLICY ANALYSIS FROM PPBS

by AARON WILDAVSKY*

Aaron Wildavsky is a Professor of Political Science at the University of California at Berkeley and is associated with the Center for Planning and Development Research there.

The growing complexity of our national problems, and the inability of many old policies to deal with them, have made policy analysis of crucial current importance. Yet, asserts Professor Wildavsky, policy analysis in the form of PPBS is so inappropriate to our current needs that "there is a danger that policy analysis will be rejected along with its particular manifestation in PPBS."

Professor Wildavsky discusses the flaws in the PPB system which have caused it to fall short of the optimistic expectations voiced upon its inauguration as a governmentwide system. He points out that many of the factors which contributed to the success of PPBS in the Department of Defense are absent in the civilian agencies. He suggests that both bureaucratic inertia and a lack of trained personnel have added to the difficulties of introducing the PPB system into non-defense agencies.

Professor Wildavsky stresses that "the fixation on program structure is the most pernicious aspect of PPBS." He feels that the emphasis on program structure, and the formal connection of policy analysis with the budget cycle, sacrifice sound analysis, initiative, and imagination for *pro forma* structure and schedules. He advocates releasing policy analysis from these artificial constraints. If analysis is encouraged to concentrate on major issues rather than detailed budget items, it will become more relevant to both Executive and congressional decisionmakers. Professor Wildavsky claims that only when this relevance becomes apparent, as it has not under PPBS, will effective use be made of policy analysis. He notes that "if strategically located Congressmen demanded more policy analysis there is little doubt that we would get it."

In two appendices, Professor Wildavsky examines the nature of systems analysis and advances "radical incrementalism" as a proposal to improve upon the current budgetary process.

Introduction

Everyone knows that the Nation needs better policy analysis. Each area one investigates shows how little is known compared to what is necessary in order to devise adequate policies. In some organizations

*This paper supplements my recent studies. It is meant to be read in conjunction with these other works. Thus I have felt no need to describe the traditional budgetary practices covered in *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964) or modern modes of "efficiency" analysis beyond the account in "The Political Economy of Efficiency" (*Public Administration Review*, Vol. XXVI, No. 4, December 1966, pp. 292-310). Nor have I sought to set forth fully my ideas on desirable budgetary reform as found in "Toward a Radical Incrementalism" (Washington, D.C.: American Enterprise Institute for Public Policy Research, December 1965), also, in *Congress: The First Branch of Government* (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1966), pp. 115-165. See also Appendixes 1 and 2 of this paper.

I wish to thank Arnold Meltsner, a graduate student in the Department of Political Science, for his critical comments and for giving me the benefit of his experience with Defense budgets. I also wish to thank Robert Biller, Yehezkel Dror, Todd LaPorte, Frederick C. Mosher, and Nelson Polsby for helpful comments. Peter Dahl made useful stylistic suggestions. No one who reads this paper will doubt that I mean to take all the blame.

there are no ways at all of determining the effectiveness of existing programs; organizational survival must be the sole criterion of merit. It is often not possible to determine whether the simplest objectives have been met. If there is a demand for information the cry goes out that what the organization does cannot be measured. Should anyone attempt to tie the organization down to any measure of productivity, the claim is made that there is no truth in numbers. Oftentimes this is another way of saying, "Mind your own business." Sometimes the line taken is that the work is so subtle that it resists any tests. On other occasions the point is made that only those learned in esoteric arts can properly understand what the organization does, and they can barely communicate to the uninitiated. There are men so convinced of the ultimate righteousness of their cause that they cannot imagine why anyone would wish to know how well they are doing in handling our common difficulties. Their activities are literally priceless; vulgar notions of cost and benefit do not apply to them.

Anyone who has weathered this routine comes to value policy analysis. The very idea that there should be some identifiable objectives and that attention should be paid to whether these are achieved seems a great step forward. Devising alternative ways of handling problems and considering the future costs of each solution appear creative in comparison to more haphazard approaches. Yet policy analysis with its emphasis upon originality, imagination, and foresight, cannot be simply described. It is equivalent to what Robert N. Anthony has called strategic planning: "* * * the process of deciding on objectives of the organization, on changes in these objectives, on the resources used to attain these objectives. * * * It connotes big plans, important plans, plans with major consequences."¹ Policy analysis is similar to a broadly conceived version of systems analysis²; interested readers may consult the first appendix for a statement of what systems analysis is about. Yehezkel Dror has pointed up the boundaries that separate a narrow study from one with larger policy concerns. In policy analysis—

1. Much attention would be paid to the political aspects of public decision-making and public policy-making (instead of ignoring or condescendingly regarding political aspects). * * *
2. A broad conception of decision-making and policy-making would be involved (instead of viewing all decision-making as mainly a resources allocation). * * *
3. A main emphasis would be an creativity and search for new policy alternatives, with explicit attention to encouragement of innovative thinking. * * *
4. There would be extensive reliance on * * * qualitative methods. * * *
5. There would be much more emphasis on futuristic thinking. * * *
6. The approach would be looser and less rigid, but nevertheless systematic, one which would recognize the complexity of

¹ Robert N. Anthony, *Planning and Control Systems: A Framework for Analysis*, (Boston: Harvard University Press, 1965), p. 16.

² Aaron Wildavsky, "The Political Economy of Efficiency," *Public Administration Review*, Vol. XXVI, No. 4, December 1966, pp. 298-302.

means-ends interdependence, the multiplicity of relevant criteria of decision, and the partial and tentative nature of every analysis. * * *³

Policy analysis aims at providing information that contributes to making an agency politically and socially relevant. Policies are goals, objectives, and missions that guide the agency. Analysis evaluates and sifts alternative means and ends in the elusive pursuit of policy recommendations. By getting out of the fire-house environment of day-to-day administration, policy analysis seeks knowledge and opportunities for coping with an uncertain future. Because policy analysis is not concerned with projecting the *status quo*, but with tracing out the consequences of innovative ideas, it is a variant of planning. Complementing the agency's decision process, policy analysis is a tool of social change.

In view of its concern with creativity, it is not surprising that policy analysis is still largely an art form; there are no precise rules about how to do it. The policy analyst seeks to reduce obscurantism by being explicit about problems and solutions, resources and results. The purpose of policy analysis is not to eliminate advocacy but to raise the level of argument among contending interests. If poor people want greater benefits from the government, the answer to their problems may not lie initially in policy analysis but in political organization. Once they have organized themselves, they may want to undertake policy analysis in order to crystallize their own objectives or merely to compete with the analyses put forth by others. The end result, hopefully, would be a higher quality debate and perhaps eventually public choice among better known alternatives.

A belief in the desirability of policy analysis—the sustained application of intelligence and knowledge to social problems—is not enough to insure its success, no more than to want to do good is sufficient to accomplish noble purposes. If grandiose claims are made, if heavy burdens are placed on officials without adequate compensation, if the needs of agency heads are given scant consideration, they will not desire policy analysis. It is clear that those who introduced the PPB system into the federal government in one fell swoop did not undertake a policy analysis on how to introduce policy analysis into the federal government.*

In a paper called "The Political Economy of Efficiency,"⁴ written just as PPBS was begun in national government, I argued that it would run up against serious difficulties. There is still no reason to change a single word of what I said then. Indeed, its difficulties have been so overwhelming that there is grave danger that policy analysis will be rejected along with its particular manifestation in PPBS. In this essay I shall assess the damage that the planning-programming-budgeting system has done to the prospects of encouraging policy analysis in American national government. Then I would like to suggest some ways of enabling policy analysis to thrive and prosper.

³Yehezkel Dror, "Policy Analysts: A New Professional Role In Government Service," *Public Administration Review*, Vol. XXVII, No. 3, September 1967, pp. 200-201. See also Dror's major work, *Public Policy-Making Reexamined* (San Francisco: Chandler, 1968).

*Further discussion of this issue is found in the papers by Carlson in vol. 2 of this collection, and Hoffman, Rivlin, and Marvin & Rouse in this volume.

⁴Aaron Wildavsky, *op. cit.*

WHY DEFENSE WAS A BAD MODEL

A quick way of seeing what went wrong with PPBS is to examine the preconditions for the use of this approach in the Defense Department, from which it was exported throughout the Federal Government.* The immediate origins of PPBS are to be found in the RAND Corporation,⁵ where, after the Second World War, a talented group of analysts devoted years of effort to understanding problems of defense policy. It took 5 years to come up with the first useful ideas. Thus the first requisite of program budgeting in defense was a small group of talented people who had spent years developing insights into the special problems of defense strategy and logistics. The second requisite was a common terminology, an accepted collection of analytical approaches, and the beginnings of theoretical statements to guide policy analysis. When Secretary of Defense Robert McNamara came into office, he did not have to search for men of talent nor did he have to wait for a body of knowledge to be created. These requisites already existed in some degree. What was further necessary was his ability to understand and to use analytical studies. Thus the third requisite of program budgeting is top leadership that understands policy analysis and is determined to get it and make use of it.

The fourth requisite was the existence of planning and planners. Planning was well accepted at the various levels of the Defense Department with the variety of joint service plans, long-range requirement plans, logistical plans, and more. Military and civilian decision-makers believed in planning, in coping with uncertainty and in specifying some consequences of policy decisions. The problem as the originators of PPBS saw it was to introduce cost considerations into planning; they wanted to stop blue-sky planning and to integrate planning and budgeting. They wanted to use the program budget to bridge the gap between military planners, who cared about requirements but not about resources, and budget people, who were narrowly concerned with financial costs but not necessarily with effective policies.

Policy analysis is expensive in terms of time, talent, and money. It requires a high degree of creativity in order to imagine new policies and to test them out without requiring actual experience. Policy analysis calls for the creation of systems in which elements are linked to one another and to operational indicators so that costs and effectiveness of alternatives may be systematically compared. There is no way of knowing in advance whether the analysis will prove intellectually satisfying and politically feasible. Policy analysis is facilitated when: (a) goals are easily specified, (b) a large margin of error is allowable, and (c) the cost of the contemplated policy makes large expenditures on analysis worthwhile. That part of defense policy dealing with choices among alternative weapons systems was ideally suited for policy analysis. Since the cost of intercontinental missiles or other weapons systems ran into the billions of dollars, it was easy

⁵ See David Novick, "Origin and History of Program Budgeting," The RAND Corporation, October 1966, p. 3427.

*Further discussion of this issue is found in the papers by Enthoven, Enthoven & Smith, and Hoffman in this volume.

to justify spending millions on analysis.⁶ The potential effectiveness of weapons like intercontinental missiles could be contemplated so long as one was willing to accept large margins of error. It is not unusual for analysts to assume extreme cases of damage and vulnerability in a context in which the desire for reducing risk is very great. Hence a goal like assuring sufficient destructive power such that no enemy strike could prevent devastation of one's country may be fuzzy without being unusable. If one accepts a procedure of imagining that possible enemies were to throw three times as much megatonnage as intelligence estimates suggest they have, he need not be overly troubled by doubts about the underlying theory. If one is willing to pay the cost of compensating against the worst, lack of knowledge will not matter so much. The point is not that this is an undesirable analytic procedure, quite the contrary, but the extreme cases were allowed to determine the outcomes.

INERTIA

The introduction of new procedures that result in new policies is not easy.* Inertia is always a problem. Members of the organization and its clientele groups have vested interests in the policies of the past. Efforts at persuasion must be huge and persistent. But there are conditions that facilitate change. One of these is a rising level of appropriations. If change means that things must be taken away from people in the organization without giving them anything in return, greater resistance may be expected. The ability to replace old rewards with larger new ones helps reduce resistance to change. The fact that defense appropriations were increasing at a fast rate made life much easier for Mr. McNamara. The expected objections of clientele groups, for example, were muted by the fact that defense contractors had lots of work, even if it was not exactly what they expected. Rapid organizational growth may also improve the possibilities for change. The sheer increase in organizational size means that many new people can be hired who are not tied to the old ways. And speedy promotion may help convince members that the recommended changes are desirable.

The deeper change goes into the bowels of the organization, the more difficult it is to achieve. The more change can be limited to central management, the greater the possibility for carrying it out. The changes introduced in the Defense Department did not, for the most part, require acceptance at the lower levels. Consider a proposed change in the organization of fighting units that would drastically reduce the traditional heavy support facilities for ground forces. Such a change is not easily manipulated from Washington. But the choice of one weapons system over another is much more amenable to central control. The kinds of problems for which program budgeting was most useful also turned out to be problems that could be dealt with largely at the top of the organization. The program budget group that McNamara established had to fight with generals in Washington but not with master sergeants in supply. Anyone who knows the Army knows what battle they would rather be engaged in fighting.

⁶ I once tried to interest a graduate student who had experience with defense problems in doing research in the City of Oakland. He asked the size of Oakland's budget. "\$50 million," I said. "Why, in the Air Force we used to round to that figure," was his reply.

*Further discussion of this issue is found in the paper by Schick in this volume.

The ability of an organization to secure rapid change depends, of course, on the degree of its autonomy from the environment. I have argued elsewhere⁷ that the President of the United States has much more control over America's foreign policy than over its domestic policy. In almost any area of domestic policy there is a well-entrenched structure of interests. In foreign and defense policy, excluding such essentially internal concerns as the National Guard, the territory within the American political system is not nearly so well defended; there are far fewer political fortifications, mines, and boobytraps.

PERSONNEL

Experienced personnel may be a barrier to change. They know something about the consequences of what they are doing. They may have tried a variety of alternatives and can point to reasons why each one will not work. If I may recall my low-level Army experience (I entered as a private first class and was never once demoted), the usual reply to a question about the efficacy of present practice was, "Have you ever been in combat, son?" But the most dramatic changes introduced in the Pentagon had to do with questions of avoiding or limiting nuclear war, in which no one had a claim to experience and in which the basic purpose of analysis is to make certain that we do not have to learn from experience. If the system fails, the game is over. And since McNamara's men possessed a body of doctrines on defense policy, they had an enormous advantage over regular military who were for a long time unable to defend themselves properly in the new field.⁸

The new policy analysts did not accept the currency of military experience. In their view, naked judgment was not a satisfactory answer to why a policy should be adopted. The Army might know the firepower of an infantry division, but firepower was not "effectiveness." Competition among the services for appropriations, however, was favorable to PPBS. There was a defense budget that covered virtually all of the Department's subject matter. There were defense missions in which trade-offs could be made between the services. Resources could actually be diverted if the analysis "proved" a particular service was right. Programs could easily be developed because of the facile identification of program with weapons systems and force units. Once the military learned the jargon, they were willing to play the game for an extra division or carrier. So long as dollar losses in one program were more than made up by gains in another, the pain of policy analysis was considerably eased.

The favorable conditions for the limited use of program budgeting in the Department of Defense do not exist in most domestic agencies. There are no large groups of talented policy analysts expert in agency problems outside the Federal Government. These nonexistent men cannot, therefore, be made available to the agencies. (The time has passed when eighth-rate systems engineers in aerospace industries are expected to solve basic social problems overnight.) Most agencies had few planners and even less experience in planning. There is no body of knowl-

⁷ Aaron Wildavsky, "The Two Presidencies," *Trans-action*, vol. IV, No. 2, December 1966, pp. 7-14.

⁸ For further argument along these lines see my article, "The Practical Consequences of the Theoretical Study of Defense Policy," *Public Administrative Review*, vol. XXV, No. 1, March 1965, pp. 90-103.

edge waiting to be applied to policy areas such as welfare and crime. A basic reason for wanting more policy analysis is to help create knowledge where little now exists. There are only a few agencies in which top managers want systematic policy analysis and are able to understand quantitative studies. Goals are not easily specified for most domestic agencies. Nor do they usually have handy equivalents for programs like expensive weapons systems. What Thomas Schelling has so pungently observed about the Department of State—it does not control a large part of the budget devoted to foreign policy—is true for the domestic departments and their lack of coverage as well.⁹

Except for a few individual programs like the proposals for income supplements or assessing the desirability of a supersonic transport, the cost of most domestic policies does not rise into the billions of dollars. Congress and interested publics are not disposed to allow large margins of error. Instead of increasing, the availability of Federal funds began declining soon after the introduction of program budgeting. A higher level of conflict was inevitable, especially since the acceptance of proposed changes required the acquiescence of all sorts of people and institutions in the far-flung reaches of the agencies. Social workers, city officials, police chiefs, welfare mothers, field officers, and numerous others were involved in the policies. Program budgeting on the domestic side takes place in a context in which there is both less autonomy from the environment and a great deal more firsthand experience by subordinates. On these grounds alone no one should have been surprised that program budgeting in the domestic agencies did not proceed as rapidly or with as much ostensible success as in the Defense Department.¹⁰

NO ONE CAN DO PPBS

In past writings I argued that program budgeting would run up against severe political difficulties. While most of these arguments have been conceded, I have been told that in a better world, without the vulgar intrusion of political factors (such as the consent of the governed), PPBS would perform its wonders as advertised. Now it is clear that for the narrow purpose of predicting why program budgeting would not work there was no need to mention political problems at all. It would have been sufficient to say that the wholesale introduction of PPBS presented insuperable difficulties of calculation. All the obstacles previously mentioned, such as lack of talent, theory, and data, may be summed up in a single statement: *no one knows how to do program budgeting*. Another way of putting it would be to say that many know what program budgeting should be like in general, but no one knows what it should be in any particular case. Program budgeting

⁹ Thomas C. Schelling, "PPBS and Foreign Affairs," memorandum prepared at the request of the Subcommittee on National Security and International Operations of the Committee on Government Operations, U.S. Senate, 90th Cong., first sess., 1968.

¹⁰ Dr. Alain Enthoven, who played a leading role in introducing systems analysis to the Defense Department, has observed that: "The major changes in strategy, the step-up in production of Minutemen and Polaris and the build-up in our non-nuclear forces including the increase in the Army, the tactical air forces, and the air lift * * * were being phased in at the same time that PPBS was being phased in. * * * We speeded up the Polaris and Minuteman programs because we believed that it was terribly important to have an invulnerable retaliatory force. We built up the Army Land Force because we believed it was necessary to have more land forces for limited non-nuclear wars. We speeded up the development of anti-guerrilla forces or special forces because we believed that was necessary for counter-insurgency. Those things would have happened with or without PPBS. PPBS does not make the strategy. Subcommittee on National Security and International Operations of the Committee on Government Operations, U.S. Senate, *Hearings, Planning-Programming-Budgeting*, 90th Cong., first sess., pt. 2, Sept. 27 and Oct. 18, 1967, p. 141.

cannot be stated in operational terms. There is no agreement on what the words mean, let alone an ability to show another person what should be done. The reason for the difficulty is that telling an agency to adopt program budgeting means telling it to find better policies and there is no formula for doing that. One can (and should) talk about measuring effectiveness, estimating costs, and comparing alternatives, but that is a far cry from being able to take the creative leap of formulating a better policy.

PATTERN OF EVENTS

On the basis of numerous discussions with would-be practitioners of program budgeting at the Federal level, I think I can describe the usual pattern of events.* The instructions come down from the Bureau of the Budget. You must have a program budget. Agency personnel hit the panic button. They just do not know how to do what they have been asked to do. They turn, if they can, to the pitifully small band of refugees from the Pentagon who have come to light the way. But these Defense intellectuals do not know much about the policy area in which they are working. That takes time. Yet something must quickly come out of all this. So they produce a vast amount of inchoate information characterized by premature quantification of irrelevant items. Neither the agency head nor the examiners in the Bureau of the Budget can comprehend the material submitted to them. Its very bulk inhibits understanding. It is useless to the Director of the Budget in making his decisions. In an effort to be helpful, the program analysis unit at the Budget Bureau says something like, "Nice try, fellows; we appreciate all that effort. But you have not quite got the idea of program budgeting yet. Remember, you must clarify goals, define objectives, relate these to quantitative indicators, project costs into the future. Please send a new submission based on this understanding."

Another furious effort takes place. They do it in Defense, so it must be possible. Incredible amounts of overtime are put in. Ultimately, under severe time pressure, even more data is accumulated. No one will be able to say that agency personnel did not try hard. The new presentation makes a little more sense to some people and a little less to others. It just does not hang together as a presentation of agency policies. There are more encouraging words from the Budget Bureau and another sermon about specifying alternative ways of meeting agency objectives, though not, of course, taking the old objectives for granted. By this time agency personnel are desperate. "We would love to do it," they say, "but we cannot figure out the right way. You experts in the Budget Bureau should show us how to do it." Silence. The word from on high is that the Bureau of the Budget does not interfere with agency operations; it is the agency's task to set up its own budget. After a while, cynicism reigns supreme.

PPBS must be tremendously inefficient. It resembles nothing so much as a Rube Goldberg apparatus in which the operations performed bear little relation to the output achieved. The data inputs into PPBS are huge and its policy output is tiny. All over the Federal Government the story is the same: if you ask what good has PPBS done, those who have something favorable to say invariably cite the same one or two policy analyses. At one time I began to wonder if the oil

*Further discussion of this issue is found in the paper by Schick in this volume.

shale study¹¹ in the Interior Department and the maternal and child health care program¹² in Health, Education, and Welfare were all that had ever come out of the programing effort.*

The orders to expand PPBS did not say, "Let us do more policy analysis than we have in the past." What it said was, "Let us make believe we can do policy analysis on everything." Instead of focusing attention on areas of policy amenable to study, the PPBS apparatus requires information on *all* agency policies.

PROGRAM STRUCTURE

The fixation on program structure is the most pernicious aspect of PPBS.** Once PPBS is adopted, it becomes necessary to have a program structure that provides a complete list of organization objectives and supplies information on the attainment of each one. In the absence of analytic studies for all or even a large part of an agency's operations, the structure turns out to be a sham that piles up meaningless data under vague categories.¹³ It hides rather than clarifies. It suggests comparisons among categories for which there is no factual or analytical basis. Examination of a department's program structure convinces everyone acquainted with it that policy analysis is just another bad way of masquerading behind old confusions. A mere recitation of some program categories from the Department of Agriculture—"Communities of Tomorrow," "Science in the Service of Man," "Expanding Dimensions for Living"—makes the point better than any comment.***

Even if the agency head does understand a data reduction summarization of the program budget, he still cannot use the structure to make decisions, because it is too hard to adjust the elaborate apparatus. Although the system dredges up information under numerous headings, it says next to nothing about the impact of one program on another. There is data but no causal analysis. Hence the agency head is at once oversupplied with masses of numbers and undersupplied with propositions about the impact of any action he might undertake. He cannot tell, because no one knows, what the marginal change he is considering would mean for the rest of his operation. Incremental

¹¹ *Prospects For Oil Shale Development* (Washington, D.C.: Department of the Interior, May 1968).

¹² The study is presented in Committee on Government Operations, *op. cit.*, pp. 10-45.

¹³ Similar difficulties under similar conditions evidently occur in the business world. It is worth citing Anthony's comments: "Strategic planning [that is, policy analysis] is essentially *irregular*. Problems, opportunities, and 'bright ideas' do not arise according to some set timetable: they have to be dealt with whenever they happen to be perceived. * * * Failure to appreciate the distinction between regular and irregular processes can result in trouble of the following type. A company with a well-developed budgeting process decides to formalize its strategic planning. It prepares a set of forms and accompanying procedures, and has the operating units submit their long-range plans on these forms on one certain date each year. The plans are then supposed to be reviewed and approved in a meeting similar to a budget review meeting. Such a procedure does not work. * * * There simply is not time enough in an annual review meeting for a careful consideration of a whole batch of strategic proposals. * * * It is important that next year's operating budget be examined and approved as an entity so as to insure that the several pieces are consonant with one another. * * * Except for very general checklists of essential considerations, the strategic planning process follows no prescribed format or timetable. Each problem is sufficiently different from other problems so that each must be approached differently." *Planning and Control Systems, op. cit.*, pp. 38-39.

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection, and Feldman in this volume.

**Further discussion of this issue is found in the paper by Greenhouse in this volume.

***Further discussion of this issue is found in the paper by Ruttan in this volume.

changes at the Bureau of the Budget at the agency level are made in terms of the old budget categories. Since the program structure is meant to be part of the budget, however, it must be taken as a statement of current policy and it necessarily emerges as a product of organizational compromise. The program structure, therefore, does not embody a focus on central policy concerns. More likely, it is a haphazard arrangement that reflects the desire to manipulate external support and to pursue internal power aspirations. Being neither program nor budget, program structure is useless. It is the Potemkin Village of modern administration. The fact that generating bits of random data for the program structure takes valuable time away from more constructive concerns also harms policy analysis. The whole point of policy analysis is to show that what had been done intuitively in the past may be done better through sustained application of intelligence. The adoption of meaningless program structures, and their perversion into slogans for supporting existing policies, does not—say the least—advance the cause of policy analysis.

GORHAM TESTIMONY

I do not mean to suggest that the introduction of PPBS has not led to some accomplishments. Before we consider the significance of these accomplishments, however, it is essential that we understand what PPBS has manifestly *not* done. One could hardly have a better witness on this subject than William Gorham, formerly Assistant Secretary (Program Coordination), Department of Health, Education, and Welfare, and now head of the Urban Institute, who is widely acknowledged to be an outstanding practitioner of program budgeting.

At the highest level of generality, it is clear that PPBS does not help in making choices between vast national goals such as health and defense, nor is PPBS useful in making tradeoffs between more closely related areas of policy such as health, education, and welfare. In his testimony before the Joint Economic Committee, Gorham put the matter bluntly:

Let me hasten to point out that we have not attempted any grandiose cost-benefit analysis designed to reveal whether the total benefits from an additional million dollars spent on health programs would be higher or lower than that from an additional million spent on education or welfare. If I was ever naive enough to think this sort of analysis possible, I no longer am. The benefits of health, education, and welfare programs are diverse and often intangible. They affect different age groups and different regions of the population over different periods of time. No amount of analysis is going to tell us whether the Nation benefits more from sending a slum child to preschool, providing medical care to an old man, or enabling a disabled housewife to resume her normal activities. The “grand decisions”—how much health, how much education, how much welfare, and which groups in the population shall benefit—are questions of value judgments and politics. The analyst cannot make much contribution to their resolution.¹⁴

¹⁴ Joint Economic Committee, Congress of the United States, Hearings, *The Planning, Programming-Budgeting System: Progress and Potentials*, 90th Cong., first sess., September 1967, p. 5.

It turns out that it is extremely difficult to get consensus on goals within a single area of policy. As a result, the policy analysts attempt to find objectives that are more clearly operational and more widely acceptable. Gorham speaks with the voice of experience when he says:

Let me give you an example. Education. What we want our kids to be as a result of going to school is the level of objective which is the proper and the broadest one. But we want our children to be different sorts of people. We want them to be capable of different sorts of things. We have, in other words, a plurality of opinions about what we want our schools to turn out. So you drop down a level and you talk about objectives in terms of educational attainment—years of school completed and certain objective measures of quality. Here you move in education from sort of fuzzy, but very important, about what it is that you want the schools to be doing, to the more concrete, less controversial, more easily to get agreed upon objectives having to do with such things as educational attainment, percentage of children going to college, etc.

I think the same thing is true in health and in social services, that at the very highest level objective, where in theory you would really like to say something, the difficulty of getting and finding a national consensus is so great that you drop down to something which is more easily and readily accepted as objectives.¹⁵

What can actually be done, according to Gorham, are analytic studies of narrowly defined areas of policy. "The less grand decisions," Gorham testified, "those among alternative programs with the same or similar objectives within health—can be substantially illuminated by good analysis. It is this type of analysis which we have undertaken at the Department of Health, Education, and Welfare."¹⁶ Gorham gives as examples disease control programs and improvements in the health of children. If this type of project analysis is what can be done under PPBS, a serious question is raised: Why go through all the rigamarole in order to accomplish a few discrete studies of important problems?

A 5-year budget conceived in the hodgepodge terms of the program structure serves no purpose.¹⁷ Since actual budget decisions are made in terms of the old categories and policy analysis may take place outside of the program structure, there is no need to institutionalize empty labels. If a policy analysis has been completed, there is no reason why it cannot be submitted as part of the justification of estimates to the Bureau of the Budget and to Congress. For the few program memorandums that an agency might submit, changes could be detailed in terms of traditional budget categories. Problems of program structure

¹⁵ *Ibid.*, pp. 80–81. One might think that a way out of the dilemma could be had by adopting a number of goals for an area of policy. When Committee Chairman William Proxmire suggested that more goals should be specified, Gorham replied, "I would like to be the one to give the first goal. The first one in is always in the best shape. The more goals you have, essentially the less useful any one is, because the conflict among them becomes so sharp" (p. 83).

¹⁶ *Ibid.*, p. 6.

¹⁷ Anthony again supplies a useful comparison from private firms that makes a similar point: "An increasing number of businesses make profit and balance sheet projections for several years ahead, a process which has come to be known by the name 'long-range planning.' * * * A 5-year plan usually is a projection of the costs and revenues that are anticipated under policies and programs *already approved*, rather than a device for consideration of, and decision on, new policies and programs. The 5-year plan reflects strategic decisions already taken; it is not the essence of the process of making new decisions. * * * In some companies, the so-called 5-year plan is nothing more than a mechanical extrapolation of current data, with no reflection of management decisions and judgment; such an exercise is virtually worthless" (*Planning and Control Systems*, *op. cit.*, pp. 57–58).

would be turned over to the agency's policy analysts who would experiment with different ways of lending intellectual coherence to the agency's programs. There would be no need to foist the latest failure on a skeptical world. Nor would there be battles over the costs of altering a program structure that has achieved, if not a common framework, at least the virtue of familiarity. The difference is that stability of categories in the traditional budget has real value for control¹⁸ while the embodiment of contradictions in the program structure violates its essential purpose.*

INCENTIVES FOR POLICY ANALYSIS

PPBS discredits policy analysis. To collect vast amounts of random data is hardly a serious analysis of public policy. The conclusion is obvious. The shotgun marriage between policy analysis and budgeting should be annulled.** Attempts to describe the total agency program in program memorandums should be abandoned. It is hard enough to do a good job of policy analysis, as most agency people now realize, without having to meet arbitrary and fixed deadlines imposed by the budget process. I have proposed that policy analysis would be facilitated by abolishing the annual budget cycle. One of the great weaknesses of governmental policymaking is that policies are formulated a good 2 years before funds become available. Given the difficulties of devising policies in the first place, the timelag wreaks havoc with the best analysis. Since no one seems disposed to consider this alternative seriously, I mention it merely in passing as a change that would fit in with what has been suggested.¹⁹

There is no way of telling whether an analysis will be successful. There is, therefore, no point in insisting that half-baked analyses be submitted every year because of a misguided desire to cover the entire agency program. The Budget Bureau itself has recently recognized the difficulty by requiring agencies to present extensive memorandums only when major policy issues have been identified. It is easier and more honest just to take the program structure out of the budget.

The thrust of the argument thus far, however, forces us to confront a major difficulty. Policy analysis and budgeting were presumably connected in order to see that high quality analysis did not languish in limbo but was translated into action through the critical budget process. Removing policy analysis from the annual budget cycle might increase its intellectual content at the expense of its practical impact. While formal program structures should go—PPBS actually inhibits the prospects for obtaining good analysis that is worth translating into public policy—they should be replaced with a strong incentive to make policy analysis count in yearly budgetary decisions. I am therefore proposing a substitute for PPBS that maintains whatever incentive it provided for introducing the results of policy analysis into the real world without encouraging the debilitating effects.

¹⁸ An excellent discussion of different purposes of budgeting and stages of budgetary development is found in Allen Schick, "The Road to PPB: The Stages of Budget Reform," *Public Administration Review*, vol. XXVI, No. 4, December 1966, pp. 243-258. See also the paper by Schick in this volume.

¹⁹ See appendix 2, this paper.

* Further discussion of this issue is found in the paper by Rivlin in this volume.

** Further discussion of this issue is found in the paper by Schick in this volume.

The submission of program memorandums supported by policy analysis should be made a requirement for major dollar changes in an agency's budget. The Bureau of the Budget should insist that this requirement be met by every agency. Agency heads, therefore, would have to require it of subunits. The sequence could operate as follows:

1. Secretary of agency and top policy analysts review major issues and legislation and set up a study menu for several years. Additions and deletions are made periodically.

2. Policy analysts set up studies which take anywhere from 6 to 24 months.

3. As a study is completed for a major issue area, it is submitted to the Secretary of the agency for review and approval.

4. If approved, the implications of the study's recommendations are translated into budgetary terms for submission as a program memorandum in support of the agency's fiscal year budget.

No one imagines that a mechanical requirement would in and of itself compel serious consideration of policy matters. No procedure should be reified as if it had a life of its own apart from the people who must implement it. This conclusion is as true for my suggestion as for PPBS. We must therefore consider ways and means of increasing the demand for and supply of policy analysis.

INCREASING DEMAND AND SUPPLY

The first requirement of effective policy analysis is that top management want it.* No matter how trite this criterion sounds, it has often been violated, as Frederick C. Mosher's splendid study of program budgeting in foreign affairs reveals.²⁰ The inevitable difficulties of shaking loose information and breaking up old habits will prove to be insuperable obstacles without steady support from high agency officials. If they do not want it, the best thing to do is concentrate efforts in another agency. Placing the best people in a few agencies also makes it more likely that a critical mass of talent will be able to achieve a creative response to emerging policy problems.

Policy analysis should be geared to the direct requirements of top management. This means that analysis should be limited to a few major issues. Since there will only be a few studies every year, the Secretary should have time to consider and understand each one. The analytical staff should be flexible enough to work on his priority interests. Consequently, one of the arguments by which program budgeting has been oversold has to be abandoned. Policy analysis will not normally identify programs of low priority. Top management is not interested in them. They would receive no benefit from getting supporters of these programs angry at them. Instead, agency heads want to know how to deal with emergent problems. Practitioners of policy analysis understand these considerations quite well. Harry Shooshan,

²⁰ Frederick C. Mosher, "Program Budgeting in Foreign Affairs: Some Reflections," memorandum prepared at the request of the Subcommittee on National Security and International Operations of the Committee on Government Operations, U.S. Senate, 90th Cong., second sess., 1968.

*Further discussion of this issue is found in the papers by Carlson in vol. 2 of this collection, and Rivlin, Hoffman, and Marvin & Rouse in this volume.

Deputy Under Secretary for Programs, Department of the Interior, presents a perceptive analysis:

* * * We have tried to more heavily relate our PPB work and our analytical work to the new program thrusts, and major issues, not because it is easier to talk about new programs, but rather, there is a good question of judgment, on how much time one should spend on ongoing programs that are pretty well set. So you restate its mission and you put it in PPB wrapping and what have you really accomplished?

There are going to be new program proposals, new thrusts of doing something in certain areas. Let's relate our analyses to that and get the alternatives documented as well as we can for the decisionmakers. So it is a combination of on the one hand it being difficult to identify low priorities in a manner that really means something and on the other hand, it is the fact of what have we really accomplished by simply putting old programs in new wrappings when new programs really should get the emphasis right now in terms of what are the decisions now before, in my case, the Secretary of the Interior, in terms of what should he know before he makes decisions relative to where he is attempting to go. If I can relate PPB to the decision on his desk today and the near future, I can sell him and in turn, our own Department on the contribution that we can make.²¹

The implications of Shooshan's point go beyond making policy analysis more desirable by having it meet the needs of top management. The subjects for policy analysis ought to be chosen precisely for their critical-fluid-emergent character. These are the places where society is hurting. These are the areas in which there are opportunities for marginal gains. Indeed, a major role for top management is scanning the political horizon for targets of opportunity. Yet the characteristics of these new problems run counter to the criteria for selection that PPBS currently enforces, since they are identified by ambiguity concerning goals, lack of data upon which to project accurate estimates of costs and consequences, and pervasive uncertainty concerning the range of possible changes in program.

There would be a much larger demand for policy analysis if it were supplied in ways that would meet the needs of high-level officials. Let us consider the example of the President of the United States. He can certainly use policy analysis to help make better decisions. Substantial policy studies would give him and his staff leverage against the bureaucracy. Knowledge is power. Indeed, command of a particular field would enable Presidents to exert greater control over the agenda for public decision and would give them advantages in competition with all sorts of rivals. Presidents could use perhaps a dozen major policy studies per year of their most immediate concerns. If even a few of these turn out well, the President may be motivated to make use of them. Contrast this with the present inundation of the Executive Office by endless streams of program "books," summaries, and memoranda that nobody ever looks at.

What is true of the President is also true for important executives in the agencies. Policy-oriented executives will want to get better analysis.

²¹ Hearings, *The Planning-Programming-Budgeting System: Progress and Potentials*, op. cit., pp. 77-78.

Executives wishing to increase their resource base will be interested in independent sources of information and advice. Those who would exert power need objectives to fight for. It is neither fashionable nor efficient to appear to seek power for its own sake. In polite society the drive is masked and given a noble face when it can be attached to grand policy concerns that bring benefits to others as well as to power seekers. The way to gain the attention of leaders is not to flood them with trivia but to provide examples of the best kind of work that can be done. The last years of the Johnson administration witnessed a proliferation of secret commissions to recommend new policies. The department secretary often became just another special pleader. If they have any interest in curbing this development, secretaries may find that producing their own policy analyses allows them to say that outside intervention is not the only or the best way to generate new policies.

CONGRESSIONAL DEMAND

If strategically located Congressmen demanded more policy analysis, there is little doubt that we could get it.* What can be done to make them want more of it? The answer does not lie in surrounding them with large staffs so that they lose their manifestly political functions and become more like bureaucrats. Nor does the answer lie in telling Congressmen to keep away from small administrative questions in favor of larger policy concerns. For many Congressmen get into the larger questions only by feeling their way through the smaller details.²² A threat to deprive Congressmen of the traditional line-item appropriations data through which they exert their control of agency affairs also does not appear to be a good way of making Congressmen desire policy analysis.

Policy analysis must be made relevant to what Congressmen want. Some legislators desire to sponsor new policies and they are one clientele for analysis. For other Congressmen, however, policy is a bargainable product that emerges from their interactions with their fellows. These Members must be appealed to in a different way. They often have a sense of institutional loyalty and pride. They know that Congress is a rare institution in this world—a legislative body that actually has some control over public policy. They are aware that the development of new knowledge and new techniques may freeze them out of many of the more serious decisions. Policy analysis should be proposed to these men as an enhancement of the power of Congress as an institution. The purpose of analysis would be, in its simplest form, to enable Congressmen to ask good questions and to evaluate answers. Oftentimes it is hardest for a layman to recognize the significant questions implicit in an area of policy. Are there other and better questions to be asked, other and better policies to be pursued?

A Congress that takes seriously its policy role should be encouraged to contract for policy analysis that would stress different views of what the critical questions are in a particular area of policy. Each major committee or subcommittee should be encouraged to hire a man trained in policy analysis for a limited period, perhaps 2 years. His task would

²² See appendix 2, this paper.

* Further discussion of this issue is found in the paper by Polsby in this volume.

be to solicit policy studies, evaluate presentations made by government agencies, and keep Congressmen informed about what are considered the important questions. In the past, chairmen have not always paid attention to the quality of committee staffs. Following the lead of the Joint Economic Committee, seminars might be held for a couple of weeks before each session. At these seminars discussions would take place between agency personnel, committee staff, and the academics or other experts who have produced the latest policy analysis. If all went well, Congressmen would emerge with a better idea of the range of issues and of somewhat different ways of tackling the problems, and the policy analysts would emerge with a better grasp of the priorities of these legislators.

SUPPLIERS OF POLICY ANALYSIS

Thus far we have dealt solely with the incentive structure of the consumers who ought to want policy analysis—agency heads, Presidents, Congressmen. Little has been said about the incentive structure of the suppliers who ought to provide it—analysts, consultants, academics. Our premise has been that the supply of policy analysis would be a function of the demand.* Now, the relationships between supply and demand have long been troublesome in economics because it is so difficult to sort out the mutual interactions. Upon being asked whether demand created supply or supply created demand, the great economist Marshall was reported to have said that it was like asking which blade of the scissors cuts the paper. There is no doubt, however, that changes in the conditions and quality of supply would have important effects on the demand for policy analysis.

Disengaging policy analysis from PPBS would help build the supply of policy analysis by:

1. Decreasing the rewards for mindless quantification for its own sake. There would be no requests from the Bureau of the Budget for such information and no premium for supplying it.

2. Increasing the rewards for analysts who might try the risky business of tackling a major policy problem that was obviously not going to be considered because everyone was too busy playing with the program structure. Gresham's Law operates here: programed work drives out unprogramed activity, make-work drives out analysis.

One way of increasing the supply of policy analysis would be to improve the training of people who work directly in the various areas of policy. Instead of taking people trained in policy analysis and having them learn about a particular policy area, the people in that area would be capable of doing policy analysis. Three-day or 3-month courses will not do for that purpose. A year, and possibly 2 years, would be required. Since it is unlikely that the best people can be made available for so long a period, it is necessary to think in terms of education at an earlier period in their lives. There is a great need for schools of public policy in which technical training is combined with broader views of the social context of public policy. Although no one knows how to teach "creativity," it is possible to expose students to the range of subjects out of which a creative approach to public policy could come.

* Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

Another way of increasing the supply of policy analysis would be to locate it in an organizational context in which it has prestige and its practitioners are given time to do good work. Having the policy analysis unit report directly to the secretary or agency head would show that it is meant to be taken seriously.²³ But then it is bound to get involved in day-to-day concerns of the agency head, thus creating a classic dilemma.

Tactics *

The effective use of a policy analysis unit cannot be specified in advance for all agencies. There are certain tensions in its functions that may be mitigated on a case-by-case basis but cannot be resolved once and for all. Serious policy analysis requires months, if not years, of effort. A unit that spends its time solely on substantial policy analysis would soon find itself isolated from the operational concerns of the agency. There would be inordinate temptations on the part of its members to go where the action is. Before long, the policy unit might become more immediately relevant at the expense of its long term impact. The frantic nature of day-to-day emergencies drives out the necessary time and quiet for serious study and reflection. What can be done? One tactic is for the policy unit to consider itself an educational as well as an action group. Its task should be to encourage analysis on the part of other elements of the organization. It should undertake nothing it can get subunits to do. The role of the policy unit would then be one of advising subunits and evaluating their output.

A second tactic would be to contract out for studies that are expected to take the longest period of time. The third tactic is the most difficult, because it calls for a balancing act. Immediate usefulness to top management may be secured by working on problems with short lead times while attempting to retain perhaps half of the available time for genuine policy analysis. To the degree that serious policy analysis enters into the life of the organization and proves its worth, it will be easier to justify its requirements in terms of release from everyday concerns. Yet the demand for services of the analysts is certain to increase. Failures in policy analysis, on the other hand, are likely to give the personnel involved more time for reflection than they would prefer. Like headquarters-field relationships, line and staff responsibilities, and functional versus hierarchical command, the problems of the policy unit are inherent in its situation and can only be temporarily resolved.

These comments on incentives for increasing the supply and demand for policy analysis are plainly inadequate. They are meant merely to suggest that there is a problem and to indicate how one might go about resolving it. We do not really know how to make policy analysis fit in with the career requirements of Congressmen, nor can we contribute

²³ When Charles Hitch was controller of the Defense Department, the policy analysis unit reported directly to him, as did the budget unit. One reported result is that the policy unit was able to do its work without being drawn into the daily concerns of the budget men. When policy analysis (called systems analysis) was given separate status, with its own assistant secretary, there was apparently a much greater tendency for its members to insist upon control of immediate budgetary decisions. Hence the distinction between longer-run policy analysis and shorter-run budgeting tended to be obscured. It would be interesting to know whether the participants saw it in this way. Optimal placement of a policy analysis unit is bound to be a source of difficulty and a subject of controversy.

*Further discussion of this issue is found in the paper by Schick in this volume.

much beside proverbial wisdom to the structure and operation of policy analysis units. There are, however, opportunities for learning that have not yet been used. One of the benefits flowing from the experience with PPBS is that it has thrown up a small number of policy analyses that practitioners consider to be good. We need to know what makes some live in the world and others remain unused. Aside from an impressive manuscript by Clay Thomas Whitehead,²⁴ however, in which two recent policy analyses in defense are studied, there has been no effort to determine what this experience has to teach us. Despite the confident talk about policy analysis (here and elsewhere), a great deal of work remains to be done on what is considered good and why. The pioneering work by Charles E. Lindblom should not be wrongly interpreted as being antianalysis, but as a seminal effort to understand what we do when we try to grapple with social problems.

REEXAMINATION

Critical aspects of policy analysis need to be reexamined. The field cries out for a study of coordination as profound and subtle as Martin Landau's forthcoming essay on "Redundancy."²⁵ That most elemental problem of political theory—the proper role of the government versus that of the individual—should be subject to a radical critique.²⁶ The fact that cost-benefit analysis began with water resource projects in which the contribution to national income was the key question has guided thought away from other areas of policy for which this criterion would be inappropriate. There are policies for which the willingness of citizens to support the activity should help determine the outcome. There are other policies in which presently unquantifiable benefits, like pleasure in seeing others better off or reduction of anxiety following a visible decrease in social hostility, should be controlling. Although social invention is incredibly difficult, the way is open for new concepts of the role of government to liberate our thoughts and guide our actions.

In many ways the times are propitious for policy analysis. The New Deal era of legislation has ended and has not yet been replaced by a stable structure of issues. People do not know where they stand today in the same way they knew how they felt about Medicare or private versus public electric power. The old welfare state policies have disenchanted former supporters as well as further enraged their opponents. Men have worked for 20 years to get massive education bills through Congress only to discover that the results have not lived up to their expectations; it takes a lot more to improve education for the deprived than anyone had thought. There is now a receptivity to new ideas that did not exist a decade ago. There is a willingness to consider new policies and try new ways. Whether or not there is sufficient creativity in us to devise better policies remains to be seen. If we are serious about improving public policy, we will go beyond the fashionable pretense of PPBS to show others what the best policy analysis can achieve.

²⁴ Clay Thomas Whitehead, "Uses and Abuses of Systems Analysis," The RAND Corporation, September 1967.

²⁵ See Martin Landau, "Redundancy," *Public Administration Review*, scheduled for publication in Volume XXIX, No. 4, July/August 1969.

²⁶ For a fine example of original thought on this question, see Paul Feldman, "Benefits and the Role of Government in a Market Economy," Institute For Defense Analysis, Research Paper, February 1968, p. 477. See the paper by Feldman in this volume for an elaboration of this argument.

APPENDIX I

SYSTEMS ANALYSIS¹

The good systems analyst is a "chochem," a Yiddish word meaning "wise man," with overtones of "wise guy." His forte is creativity. Although he sometimes relates means to ends and fits ends to match means, he ordinarily eschews such pat processes, preferring instead to relate elements imaginatively into new systems that create their own means and ends. He plays new objectives continuously against cost elements until a creative synthesis has been achieved. He looks down upon those who say that they take objectives as given, knowing full well that the apparent solidity of the objective will dissipate during analysis and that, in any case, most people do not know what they want because they do not know what they can get.

Since no one knows how to teach creativity, daring, and nerve, it is not surprising that no one can define what systems analysis is or how it should be practiced. E. S. Quade, who compiled the RAND Corporation lectures on systems analysis, says it "is still largely a form of art" in which it is not possible to lay down "fixed rules which need only be followed with exactness."² He examined systems studies to determine ideas and principles common to the good ones, but discovered that "no universally accepted set of ideas existed. It was even difficult to decide which studies should be called good."³

Systems analysis is derived from operations research, which came into use during World War II when some scientists discovered that they could use simple quantitative analysis to get the most out of existing military equipment. A reasonably clear objective was given, and ways to cut the cost of achieving it could be developed, using essentially statistical models. Operations research today is largely identified with specific techniques: *linear programming*; *Monte Carlo (randomizing)* methods; gaming and game theory. While there is no hard and fast division between operations research and systems analysis, a rough separation may perhaps be made. The less that is known about objectives, the more they conflict, the larger the number of elements to be considered, the more uncertain the environment, the more likely it is that the work will be called a systems analysis. In systems analysis there is more judgment and intuition and less reliance on quantitative methods than in operations research.

Systems analysis builds models that abstract from reality but represent the crucial relationships. The systems analyst first decides what questions are relevant to his inquiry, selects certain quantifiable factors, cuts down the list of factors to be dealt with by aggregation and by eliminating the (hopefully) less important ones, and then gives them quantitative relationships with one another within the system he has chosen for analysis. But crucial variables may not be quantifiable. If they can be reduced to numbers, there may be no mathematical function that can express the desired relationship. More important, there may be no single criterion for judging results among conflicting objectives. Most important, the original objectives, if any, may not make sense.

¹ From "The Political Economy of Efficiency: Cost-Benefit Analysis, Systems Analysis, and Program Budgeting," *op. cit.* pp. 298-302.

² E. S. Quade, *Analysis for Military Decisions* (Chicago, 1964), p. 153.

³ *Ibid.*, p. 149.

It cannot be emphasized too strongly that a (if not the) distinguishing characteristic of systems analysis is that the objectives are either not known or are subject to change. Systems analysis, Quade tells us, "is associated with that class of problems where the difficulties lie in deciding what ought to be done—not simply how to do it—and honors go to people who * * * find out what the problem is"⁴ Charles Hitch, the former Comptroller of the Defense Department, insists that:

* * * learning about objectives is one of the chief objects of this kind of analysis. We must learn to look at objectives as critically and as professionally as we look at our models and our other inputs. We may, of course, begin with tentative objectives, but we must expect to modify or replace them as we learn about the systems we are studying—and related systems. The feedback on objectives may in some cases be the most important result of our study. We have never undertaken a major system study at Rand in which we are able to define satisfactory objectives at the beginning of the study.⁵

Systems analysts recognize many good reasons for their difficulties in defining problems or objectives. Quade reaches the core: "Objectives are not, in fact, agreed upon. The choice, while ostensibly between alternatives, is really between objectives or ends and nonanalytic methods must be used for a final reconciliation of views."⁶ It may be comforting to believe that objectives come to the analyst from on high and can be taken as given, but this easy assumption is all wrong. "For all sorts of good reasons that are not about to change," says Hitch, "official statements of national objectives (or company objectives) tend to be nonexistent or so vague and literary as to be non-operational."⁷ Objectives are not only likely to be "thin and rarified," according to Wohlstetter, but the relevant authorities "are likely to conflict. Among others there will be national differences within an alliance and within the nation, interagency, interservice, and intra-service differences. * * *⁸

Moreover, even shared objectives often conflict with one another. Deterrence of atomic attack might be best served by letting an enemy know that we would respond with an all-out indiscriminate attack on his population. Defense of our population against death and destruction might not be well served by this strategy,⁹ as the Secretary of Defense recognized when he recommended a city-avoidance strategy that might give an enemy some incentive to spare our cities as well. Not only are objectives large in number and in conflict with one another, they are likely to engender serious repercussion effects.

Many objectives, like morale and the stability of alliances, are resistant to quantification. What is worth doing depends on whether it can be done at all, how well, and at what cost. Hence, objectives really cannot be taken as given; they must be made up by the analyst. "In fact," Wohlstetter declares, "we are always in the process of choosing and modifying both means and ends."¹⁰

Future systems analysts are explicitly warned not to let clients determine objectives. A suggestive analogy is drawn with the doctor who

⁴ *Ibid.*, p. 7.

⁵ Charles J. Hitch, *On the choice of objectives in systems studies* (RAND Corporation 1960), p. 19.

⁶ E. S. Quade, *op. cit.*, p. 176.

⁷ Charles J. Hitch, *op. cit.*, pp. 4-5.

⁸ Albert Wohlstetter, "Analysis and Design of Conflict Systems," in E. S. Quade, *op. cit.*, p. 121.

⁹ See Glenn H. Snyder *Deterrence and Defense* (Princeton, 1961).

¹⁰ Wohlstetter in Quade, *op. cit.*, p. 122.

would not ignore a patient's "description of his symptoms, but * * * cannot allow the patient's self diagnosis to override his own professional judgment."¹¹ Quade argues that since systems analysis has often resulted in changing the original objectives of the policymaker, it would be "self-defeating to accept without inquiry" his "view of what the problem is."¹²

I have stressed the point that the systems analyst is advised to insist on his own formulation of the problem because it shows so closely that we are dealing with a mixed concept of efficiency.

Adjusting objectives to resources in the present or near future is difficult enough without considering future states of affairs which hold tremendous uncertainty. Constants become variables; little can be taken for granted. The rate of technological progress, an opponent's estimate of your reaction to his latest series of moves based on his reaction to yours, whether or not atomic war will occur, what it will be like, whether we shall have warning, whether the system we are working on will cost anything close to current estimates and whether it will be ready within 5 years of the due date—on most of these matters, there are no objective probabilities to be calculated.

An effective dealing with uncertainty must be a major goal of systems analysis. Systems analysis is characterized by the aids to calculation it uses, not to conquer, but to circumvent and mitigate some of the pervasive effects of uncertainty. Before a seemingly important factor may be omitted, for example, a sensitivity analysis may be run to determine whether its variation significantly affects the outcome. If there is no good basis for calculating the value of the factor, arbitrary values may be assigned to test for extreme possibilities. Contingency analysis is used to determine how the relative ranking of alternatives holds up under major changes in the environment, say, a new alliance between France and Russia, or alterations in the criteria for judging the alternatives, such as a requirement that a system work well against attacks from space as well as earth. Contingency analysis places a premium on versatility as the analyst seeks a system that will hold up well under various eventualities even though it might be quite as good for any single contingency as an alternative system. Adversary procedures may be used to combat uncertainty. Bending over backward to provide advantages for low-ranking systems and handicaps for high-ranking systems is called a *fortiori* analysis. Changing crucial assumptions in order to make the leading alternatives even, so that one can judge whether the assumptions are overly optimistic or pessimistic, is called break-even analysis.¹³ Since all these methods add greatly to the burden of calculation, they must be used with some discretion.

A variety of insurance schemes may also be used to deal with uncertainty. In appraising what an opponent can do, for instance, one can assume the worst, the best, and sheer inertia. In regard to the development of weapons, insurance requires not one flexible weapon, but a variety of alternatives pursued with vigor. As development goes on,

¹¹ E. S. Quade, *op. cit.*, p. 157. Quade attempts to soften the blow by saying that businessmen and military officers know more about their business than any one else. But the import of the analogy is clear enough.

¹² *Ibid.*, pp. 156-157.

¹³ Herman Kahn and Irwin Mann, *Techniques of Systems Analysis* (Santa Monica, The RAND Corporation, 1957), believes that "More than any single thing, the skilled use of a *fortiori* and break-even analyses separate the professionals from the amateurs." They think that convincing others that you have a good solution is as important as coming up with one.

uncertainty is reduced. Consequently, basic strategic choice involves determining how worthwhile paying for the additional information is by developing rival weapons systems to the next stage. The greater the uncertainty of the world, the greater the desirability of having the widest selection of alternative weapons to choose from to meet unexpected threats and opportunities. Alchian and Kessel are so wedded to the principle of diversified investment that they "strongly recommend this theorem as a basic part of systems analysis."¹⁴

As a form of calculation, systems analysis represents a merger of quantitative methods and rules of thumb. First, the analyst attempts to solve the problem before he knows a great deal about it. Then he continuously alters his initial solution to get closer to what he intuitively feels ought to be wanted. Means and ends are continuously played off against one another. New objectives are defined, new assumptions made, new models constructed, until a creative amalgam appears that hopefully defines a second-best solution, one that is better than others even if not optimal in any sense. In the famous study of the location of military bases conducted by Albert Wohlstetter and his associates at the RAND Corporation, widely acknowledged as a classic example of systems analysis, Wohlstetter writes:

The base study * * * proceeded by a method of successive approximations. It compared forces for their efficiency in carrying a payload between the bases and targets without opposition either by enemy interceptors or enemy bombers. Then, it introduced obstacles successively: first, enemy defenses; then enemy bombardment of our bombers and other elements needed to retaliate. In essence, then, the alternative systems were tested for their first-strike capability and then they were compared for their second-strike capacity. And the programed system performed in a drastically different way, depending on the order in which the opposing side struck. In the course of analyzing countermeasures and counter-countermeasures, the enemy bombardment turned out to be a dominant problem. This was true even for a very much improved overseas operating base system. The refueling base system was very much less sensitive to strike order. It is only the fact that strike order made such a difference among systems contemplated that gave the first-strike, second-strike distinction an interest. And it was not known in advance of the analysis that few of the programed bombers would have survived to encounter the problem of penetrating enemy defenses which had previously been taken as the main obstacle. The analysis, then, not only was affected by the objectives considered, it affected them.¹⁵

The advantage of a good systems study is that by running the analysis through in theory on paper certain disadvantages of learning from experience may be avoided.

If the complexity of the problems encountered proved difficult in cost-benefit analysis, the burdens of calculation are ordinarily much greater in systems analysis. Many aspects of a problem simply must be put aside. Only a few variables can be considered simultaneously. "Otherwise," Roland McKean tells us, "the models would become impossibly cumbersome, and * * * the number of calculations to consider would mount in the thousands."¹⁶ Formulas that include everything may appear more satisfactory but those that cannot be reduced "to a single expression are likely to convey no meaning at

¹⁴ Armen A. Alchian and Reuben A. Kessel, *A Proper Role of Systems Analysis* (Santa Monica: RAND Corporation, 1954), p. 9.

¹⁵ Albert Wohlstetter in E. S. Quade, *op. cit.*, pp. 125-126.

¹⁶ R. N. McKean, "Criteria," in E. S. Quade, *op. cit.*, p. 83.

all * * *." ¹⁷ Summing up their experience, Hitch and McKean assert that:

* * * analyses must be piecemeal, since it is impossible for a single analysis to cover all problems of choice simultaneously in a large organization. Thus, comparisons of alternative courses of action always pertain to a part of the government's (or corporation's) problem. Other parts of the overall problem are temporarily put aside, possible decisions about some matters being ignored, specific decisions about others being taken for granted. The resulting analyses are intended to provide assistance in finding optimal, or at least good, solutions to subproblems: in the jargon of systems and operations research, they are suboptimizations.¹⁸

Although admitting that much bad work is carried on and that inordinate love of numbers and machines often get in the way of creative work,¹⁹ practitioners of systems analysis believe in their art. "All of them point out how the use of analysis can provide some of the knowledge needed, how it may sometimes serve as a substitute for experience and, most importantly, how it can work to sharpen intuition."²⁰ Systems analysis can increase explicitness about the assumptions made and about exclusions from the analysis. The claim is that systems analysis can be perfected; sheer intuition or unaided judgment can never be perfect.

Yet there is also wide agreement that systems analysts "do philosophy,"²¹ that they are advocates of particular policy alternatives. What Schelling calls "the pure role of expert adviser" is not available for the analyst who "must usually formulate the questions themselves for his clients."²² Beyond that, Wohlstetter argues that systems analysts can perform the function of integrating diverse values. New systems can sometimes be found that meet diverse objectives.²³ The politician who gains his objectives by inventing policies that also satisfy others, or the leader of a coalition who searches out areas of maximum agreement, performs a kind of informal systems analysis.

APPENDIX II

RADICAL INCREMENTALISM ¹

The President, the agencies, and Congress are now compelled to give at least *pro forma* consideration to all the activities in the whole budget in a limited period of time. This results in a brief period characterized by frantic activity and the rote presentation of masses of information, most of which is not subject to change and of no special interest to anyone at that time. Why? Because of unthinking acceptance of the idea that there must be a budget containing all expenditures presented and considered at one time. As the Federal budget grows, and life and budgeting become more complex, the demand for central direction increases. Yet the overload of information is already staggering; aids to calculation are used in a desperate

¹⁷ E. S. Quade, *op. cit.*, p. 310.

¹⁸ Charles J. Hitch and Roland N. McKean, *The Economics of Defense in the Nuclear Age* (Cambridge, Harvard University Press, 1961), p. 161.

¹⁹ See Hitch on "Mechanitis—putting * * * machines to work as a substitute for hard thinking." Charles Hitch, "Economics and Operations Research: A Symposium. II," *Review of Economics and Statistics*, August 1958, p. 209.

²⁰ E. S. Quade, *op. cit.*, p. 12.

²¹ *Ibid.*, p. 5.

²² T. C. Schelling, "Economics and Operations Research: A Symposium. V. Comment," *Review of Economics and Statistics*, August 1958, p. 222.

²³ Albert Wohlstetter in E. S. Quade, *op. cit.*, p. 122.

¹ From "Toward a Radical Incrementalism," *op. cit.*, pp. 35-44.

attempt to simplify consideration of small parts of the budget. The time has come to cast aside the myth of comprehensiveness. Theory should be brought in line with experience so that there will be a chance of improving the experience. The budget needs to be further fragmented. Attention needs to be directed to matters of political interest which can be changed. Evaluation of budgetary requests must be spread out so that greater time and attention may be devoted to each of them. The development and refinement of further aids to calculation should assume a high research priority. The delays in the budgetary process should be markedly reduced by permitting the most immediate response to budgetary requests.

My proposal is that we abandon the annual budgetary process, as it is now known and substitute a continuous consideration of incremental changes to the existing base. Each agency will assume that the funds for its programs will automatically be continued. All appropriations will be continuous except for a small number designed for a limited time period. When an agency wishes to increase or decrease its funds for a program or to eliminate an old program or begin a new one it will submit a request to Congress through the Bureau of the Budget. The President may submit requests for change to Congress, and have them considered right away. The appropriations committees may call for testimony at any time on any budgetary matter and change appropriations irrespective of the fiscal year. By altering authorizations to spend, the substantive committees may also bring reconsideration on budgetary matters. I call this proposal radical incrementalism because it is based on pushing the evident incremental tendencies in budgeting to encompass the entire process.

A basic purpose of radical incrementalism is to facilitate speedy and continuous adaptation to emergent problems. While some programs may remain in a steady state, others can be reviewed as often as any participant deems it necessary. Supplemental appropriations would become a thing of the past. Demands could be dealt with as they arise. If the latest incremental move suggests a new step requiring changes in appropriations, a decision could be made right then and there. The tyranny of the annual budget—requiring formal review of programs of little immediate interest and inhibiting action on programs which need attention at the moment—would be ended.

Suppose that a subcommittee wished to look at trends in personnel or building costs. It could simply ask for these figures and act on them as it saw fit. Should a subcommittee want to view any budgetary item in relation to an agency's total appropriations, it could request both sets of figures. In order to facilitate this procedure, the appropriations committee should require agencies to develop quick and inexpensive methods of estimating expenditures. The agencies as well as the appropriations committees need to develop better aids to calculation. It may well be the case that much agency budgetary work is far too expensive and cumbersome for the results achieved. The development of rough and ready cost estimates should make it possible for agencies to provide serviceable breakdowns of their activities from a variety of conceptual viewpoints. Instead of being stuck with a rigid set of program categories, terribly expensive to maintain under proper accounting, the agencies and the subcommittees would have the advantage of being able to look at activities from diverse perspectives.

An objection that might be raised to radical incrementalism is that certain programs could escape scrutiny over a period of years. This potential problem may be solved by appointing people to review periodically those programs or activities that do not change very much from year to year, and would, therefore, tend to escape frequent scrutiny. Since they do not alter radically, a thorough going over every 5 years or so would be sufficient. Nor need any one organization do it all; the incremental approach can make use of the division of labor that is a part of the national system. Departmental budget offices, the bureaus themselves, the Bureau of the Budget, and the House and Senate Appropriation Committees and their investigating staffs might use sampling techniques so that each would review a few programs of this kind every year. The results could then be used to see if congressional scrutiny were warranted the next year. In this way, a large part of the problem may be met while adding only a little to the burden of the participants. Should the appropriations committee decide that they wish to review every activity as often as every 5 to 8 years, they could make it a rule that each appropriation lapses 5 to 8 years after the last congressional act.

Narrowing, fragmenting, and dispersing these budgetary views has considerable advantage from the viewpoint of encouraging experimentation and innovation: because no one organization is overburdened, the most thorough analysis is facilitated; more active participation by high level officials is encouraged because the material to be considered at any one time is not overwhelming; as the knowledge and interest of top officials is fed back down the line, the significance of the activity and the importance of those who engage in it is likely to be enhanced. If budgetary reviews can be liberated to some extent from the peak periods of the formal budgetary cycle, imagination and creativity can be given freer play. The absence of immediate deadlines may encourage speculation and experimentation, while the increased probability that hierarchical superiors have time to listen gives greater promise that the efforts may lead to tangible results. The variety of organizations involved should also lead to consideration of a broad range of values and perspectives.

At first glance, it might appear that problems of coordination would be made more difficult than they are today. I think not; unless, of course, one is prepared to define coordination as placing all appropriations within the cover of one huge book at one time. Nor does it make much sense to define coordination as a central review, since this begs the question of whether policies have actually been related to one another in a reasonable way. It is a lot easier to mesmerize oneself with talk about central coordination than it is to practice it. Radical incrementalism, however, can be practiced. Each increment of the budget can be considered as it comes up. Attempts can be made to adapt the new policy, through successive approximation, to major features of the environment as revealed by experience. Thus, a series of rapid adjustments can be made in a budgetary system which encourages (indeed, compels) decisionmakers to take into account the preferences of others and to mitigate the adverse consequences that policies may have for them. Under radical incrementalism, adaptation can be undertaken with greater intelligence because (1) the action is close in time to awareness of the problem; (2) changes are smaller, quicker, alterable,

and, therefore, more easily made; (3) the decisionmakers are enabled to have a better grasp of where they are in relation to where they want to be; (4) each change can be separately evaluated against a general picture of the most relevant programs then in operation instead of, an immensely more complicated task, multitudes of suggested changes being pitted against each other simultaneously; (5) every change is always important in the sense that a major participant in the system wants it.

Nothing in radical incrementalism prevents any participant in the budgetary process from using any and all analytic techniques at his disposal. Everyone is permitted to be as wise as he knows how to be. If the day should come when a simultaneous comparison of all governmental programs appeared desirable, the President or Congress could consider the budget in just that way. If it appears desirable to consider all programs dealing with water or land or any other area of policy, the President or the Appropriations Committees can call for action. Indeed, a radical incrementalism might foster such an approach by permitting scheduling when other great matters were not up for immediate decision. The endless search for "needless duplication," "sheer waste," and "irrational decisions" could go on with as much, or as little, sense as before.

CONSEQUENCES OF RADICAL INCREMENTALISM FOR MAJOR PARTICIPANTS

What would happen to the President's budget? It would represent the President's preferences on any and all budgetary items on which he cared to express an opinion. It could be as complete a document as he (through the Budget Bureau) knows how to make, or it could contain positions only on selected matters. It would go to Congress as a source of information, but it would not be the action document that it is now. Instead, action on Presidential requests would take place when he sent specific demands for specific items to the Appropriations Committees. The President's budget would be much like his State of the Union message where he presents his legislative priorities and shopping list, but where he does not necessarily comment on policies he does not wish changed. When he wants action, he follows up his address by submitting a series of concrete proposals for action. Then, as his pending requests are acted upon, the President takes these decisions into account in submitting his next wave of requests. The President would gain flexibility he does not have now because he would not have to commit himself in advance on all appropriations requests as is the case under the annual budget approach. Nor would he and his chief advisers have to engage in the chaotic activity of the fall, when tired and overburdened men work furiously to put together all appropriations. Outgoing Presidents would not have to go through the charade of developing a budget with which to stick their successor, and incoming Presidents would not have to face the immediate task of putting together another full-scale budget to counteract the one that is then operative. The new President could deal with the most vital matters first, and then take up the rest in a more leisurely way.

The President's ability to pursue economic policies would be enhanced rather than diminished by radical incrementalism. There would be no decrease in his ability to plan for a desired relationship between

revenue and expenditures. He could set out the relationship he believes desirable in his budget message or in his economic report or in any other way he deems appropriate. And he could propose action to meet his preferences through regular legislation, appropriations, or executive action. But he would not be compelled to do this at any specific time as is now the case. He could wait until he thought a change was necessary, receive the most current predictions of current revenue and expenditure, and act at once. When emergencies require increased expenditure, as in the Vietnam situation, or when long-range estimates proved to be faulty, as frequently happens, he could modify his plans. Since the possibility of substantial change in expenditures is confined to a few areas of policy, these could be restudied when necessary. While automatic stabilizers, such as unemployment compensation, work well in guarding against depressions, voluntary action by the Federal Government has not proved effective.² Perhaps the flexibility provided by radical incrementalism will permit speedier and more appropriate adaptation to contemporary needs.

A possible objection to radical incrementalism might be that Congress would suffer because agencies would not have to come before the appropriations committees every year for all the appropriations that (aside from trust funds and the like) are usually included in the annual budget. However, instead of concentrating their attention on appropriations requests only in the once-a-year period when all requests are made, agencies would be continually thinking of the prospect of making their next request. On vital matters, the agencies might be called for repeated appearances. To the extent that Congress is more often on their mind its influence should grow rather than decrease.

Opinion on radical incrementalism will probably be divided in Congress. Some members who identify with a presidential constituency might object on the grounds that welfare policies would be hurt by enhancing the power of the appropriations committees to cut in crucial places. However, this would not happen, because, while conservatives now gain somewhat by the special positions they hold on committees, this advantage is rapidly disappearing.³ There is good reason to believe that the seniority system will increasingly benefit proponents of welfare legislation. Both Presidents and the formal congressional leadership have ample means at their disposal to place members who represent preferences of the party majority on the appropriations committees, and they have already used this to good effect in the House. Deviance from the party majority is largely a southern, Democratic phenomenon and will diminish in size and importance with the growth of Negro voting, population shifts out of the Deep South, and increased Democratic Party representation elsewhere. Moreover, the best analysis we have of the appropriations committees in Congress (see forthcoming book by Prof. Richard F. Fenno, Jr. of the University of Rochester, Rochester, N.Y.) suggests that they do not markedly transgress on the preferences of the mass of other legislators. While it is true that service on the appropriations committees does tend to make members suspicious of executive advocacy, it is also true that the substantive committees are generally packed with legislators whose con-

² Wilfred Lewis, Jr., *Federal Fiscal Policy in the Postwar Recessions* (Washington, D.C.: The Brookings Institution, 1962).

³ Raymond E. Wolfinger and Joan Heifetz, "Safe Seats, Seniority and Power in Congress," *American Political Science Review*, 1965, LIX, pp. 337-49.

stituency interests suggest a more expensive view of governmental programs. A creative tension between the somewhat differing orientations of the two levels of committees does not appear to be a bad thing.

Fiscal conservatives might also oppose radical incrementalism for fear that it would result, in general, in higher governmental expenditures. Such critics might argue that, in considering programs one at a time, Congress would lose track of the implications for the total rate of expenditure. However, there would be little difficulty in arranging for a reporting service in Congress that would issue frequent statements on total approved expenditures. The solution to the problem of securing decreases, or holding down increases, in expenditures lies in the elimination of programs and not in budgetary procedures. If fiscal conservatives wish to make a drastic impact on expenditures, they will have to elect many more legislators who support their views than is now the case. Barring this unlikely development, there is no point in making the appropriations process the whipping post for developments that represent secular trends in the political system as a whole. Where appropriations subcommittees appear to stand in the way of expenditures desired by a significant majority of their colleagues, they may be outvoted on the floor, or congressional majorities may resort to backdoor spending or to other devices that take control of appropriations out of the offending subcommittees' hands. When fiscal conservatives, or liberals for that matter, are able to assert themselves in Congress, radical incrementalism should provide somewhat better opportunities for selective intervention than now exist.

In my opinion, the most serious obstacle to the acceptance of radical incrementalism is an ideological one. The proposals may not receive serious consideration because they run counter to the reigning ideologies of comprehensiveness and annual budgeting. But I still think them useful to have at hand if and when Congress gets serious about improving its capabilities as an institution.

STRATEGIC POLITICAL KNOWLEDGE

By reducing the information requirements of budgetary decisions, radical incrementalism increases the possibility of reasonable action. Whatever knowledge exists can be brought to bear on the problem by some participant in the system. Knowledge may be increased in the sense that the data are more recent and the feedback from one action can be immediately used in the next appraisal. But knowledge about how to deal with problems is only one kind of knowledge. There is a prior knowledge which often assumes greater political importance: namely, what problems should be considered? A radical incrementalism provides an important aid to calculation in that it focuses attention on those changes from the status quo which are important to some participant. But there are other ways of being alerted to matters of importance which would be especially useful to Congressmen.

The Budgeting and Accounting Act of 1921 provides for presidential submission of agency budgets to Congress through the Bureau of the Budget. The appropriations committees do not formally receive original agency requests but only those requests as amended or deleted

by the Chief Executive. We all know, to be sure, that when ties between agencies and appropriations committee chairmen are close, the original agency demands may be brought out in private or in committee hearings. But, agency officials are under restrictions in how far they can go in open advocacy. In any event, junior members of the appropriations committees may never discover this information, and the same will most certainly be true of most other Members of Congress.

As political men in a representative assembly, legislators are, above all, dealers in preferences. Since they are makers, shapers, molders, brokers, and bargainers of preferences, the most important information for them to have is information about what people want. Related to this as an aid to calculation is information on where preferences of key participants differ and why, for it alerts legislators to a conflict of preferences in which they may wish to intervene. Congress could well use Franklin D. Roosevelt's well-known practice of programing for conflict, which was designed to assure him that he would be called in on important matters, that is, matters on which preferences and policies differed. This kind of strategic political knowledge is of special importance to Congressmen because they appear to be more skilled in reconciling conflicting preferences than in evaluating complicated sets of budgetary figures.

Therefore, I propose that, along with radical incrementalism, there should be a legal requirement that the original requests of agencies be made public, together with a statement by the Budget Bureau giving its reasons for making changes. Congressmen would be immediately alerted to a conflict of preferences and would have the rationales of both the agency and the Budget Bureau presenting rival arguments. Both the agencies and the Budget Bureau would be highly motivated to make the best possible case for their demands. If they were also motivated to reach an agreement through bargaining, the very fact of their success would be one indicator that the matter was not of the highest priority for congressional attention. While some agencies might try to raise their demands inordinately for bargaining purposes, a series of attempts would soon reveal that consistently coming in too high would not serve their interests and would be abandoned.

Thus far I have deliberately used the general word "agency" to avoid complicating the argument with distinctions between bureaus and departments. My initial recommendation is that each department retain its present power to make secret recommendations to the President on behalf of the bureaus within its jurisdiction. In this way, general presidential influence on initial bureau requests could be maintained through his power to hire and fire Cabinet members and other heads of organizations. Since department heads must maintain themselves in an environment which necessarily differs from that of the President, their recommendations may sometimes be expected to differ from his on crucial matters. (If this were not the case, the President would have much less need for a Budget Bureau and an Executive Office.) The congressional purpose of unearthing significant political matters through the airing of conflicts would be served. Should this proposal prove insufficient, Congress could go further and require department heads to present in writing their reasons for disagreements with the Budget Bureau request.

Under a system of congressional programing for conflict, the President would lose his ability to maintain the fiction that agencies uni-

formly support his budget. If this means that Congressmen would learn more about where to intervene, there might be a corresponding decrease of the Presidential influence now gained by keeping Congress in the dark. Undoubtedly, the proposal will be fought for that reason. But, in fact, the President's support would still be terribly important to the agencies. Congress would still rely on the President's figures as a starting point for their consideration and as a bench mark for making cuts or (less frequently) increases. Agencies would almost always be better off with the President's support than without it; since Congress tends to cut the President's budget, an agency would have to mount a special campaign, with no certain prospect of success, in order to have a chance for victory. It would hardly be advisable, therefore, for agencies to flaunt the Chief Executive. The President might gain in another direction through his ability, under radical incrementalism, to intervene continuously in the appropriations process rather than to confine his energy largely to consideration of the annual budget.

I have no intention of proposing a system that would interfere with the confidential relationship between the President and the Bureau of the Budget. All communications from the Budget Bureau to the President would be as privileged as they are today. Nothing would prevent the Budget Bureau from presenting one kind of argument to the President and another to Congress. The only requirement would be that the President (through the Bureau of the Budget) comment on the differences between his recommendations and those of the agency involved.

A painful adjustment on the part of the Bureau of the Budget would undoubtedly be required. It has grown up in an environment which nurtures secrecy. Its confidential relationship with the President has been used to prevent public scrutiny of its action. Rationalizations of its positions on issues, which have become partly implicit in the subculture of the Executive Office, would have to be raised to the surface at some point. The Bureau's claim to a more rational mode of decision-making in the public interest (as opposed to irrational procedures in agencies surrounded by special interests) would become open to public examination. The Bureau of the Budget could no longer operate entirely as if it were guided by an informal version of the Official Secrets Act, which so effectively shields executive personnel in Great Britain from outside intervention. While Bureau personnel would gain by being liberated from the physically and mentally exhausting task of putting together an entire governmental budget in a few frantic weeks, they might not be happy with a radical incremental approach to budgeting.

By raising conflict to a more public and hence more visible level, interest groups may be stimulated to greater activity. In a democracy, where public knowledge is generally deemed good, this hardly appears to provide an objection to radical incrementalism. Recent scholarship has suggested that in many cases the power of interest groups in relation to public officials has been exaggerated. Where interest groups are already very powerful, as in the case of the Rivers and Harbors Congress, the chances are that they are privy to the additional information that would be made public under the new system. Thus, the proposals for increasing the availability of strategic information might work to strengthen groups presently weak while adding little or no additional power to the strong.

PRESCRIPTION FOR AN EFFECTIVE GOVERNMENT: ETHICS, ECONOMICS, AND PPBS

BY PAUL FELDMAN*

Paul Feldman is Staff Economist at the Institute for Defense Analyses. He was formerly Assistant Director of the Program Evaluation Staff at the U.S. Bureau of the Budget.

In the 4 years since the inception of the planning-programing-budgeting system, a number of new institutional arrangements have been established in both the Bureau of the Budget and executive agencies. Mr. Feldman asserts that while formal procedures have changed, there has been little modification in the substance of budget decisionmaking. The reason efforts to bring program evaluation to bear on the decisionmaking process have fallen short is that "generally accepted criteria for program evaluation" have not been developed. He argues that a criterion is needed which is based on an explicit ethical judgment concerning "the distribution of wealth and the rights to use that wealth."

In this paper, Mr. Feldman posits an ethical rule and traces the implications for effective program evaluation if the proposed ethic is adopted. He analyzes the implications of the ethical rule with respect to a number of market imperfections: lack of competition and knowledge, restrictions on mobility, and spillover effects. His analytical framework leads him to conclude that the government's major responsibility is to remove market imperfections because they are inconsistent with the ethical criterion; furthermore, when the Government undertakes expenditures, it should attempt to distribute the tax burden in accord with the distribution of benefits. By drawing the implications of the ethical standard and the analysis based upon it, Mr. Feldman proposes a new program structure for the PPB system. This "program structure discards spurious objectives and determines the direction in which analysis will proceed. It indicates what standards should be used in measurement, and thus tells both the analyst and the decisionmaker what is really at issue for the Government."

Introduction

The planning-programing-budgeting system (PPBS) was introduced into civil agencies of the Federal Government in August 1965, heralded by President Johnson's announcement that a revolution within government was at hand; that through the employment of modern management methods, such as benefit-cost, or systems analysis, we would be able to identify national goals and measure progress toward achievement of those goals. Despite the fanfare and the optimism with which it was announced the revolution has not materialized. What is more, it is unlikely that improved management techniques will ever produce a revolution in government, for the basic problem of government goes far deeper than simply to manage its activities efficiently. As Burke wrote in 1795:

"It is one of the finest problems of legislation * * * What the state ought to take upon itself to direct by the public wisdom,

*The author wishes to thank Dr. Robert Haveman and Miss Eloise Hally for their helpful comments.

and what it ought to leave, with as little interference as possible to individual discretion."

What Burke perceived as "the finest problem" in 1795 is still the finest problem of government. We have no standard to determine what possible actions of government can improve on the actions of individuals. But without such a standard, analysis of how efficiently government does what it now does is not likely to be helpful. To put it another way, benefit-cost analysis cannot be useful or even performed unless we can define the benefits of government activities. Introduction of the PPB system did not solve the basic problem of defining benefits, it only indicated that President Johnson was confident that the problem was tractable.

Nonetheless, the great expectations engendered by public announcements about PPB have been followed by equally great disillusionment over its failure to revolutionize government through the use of cost-benefit analysis. It may be possible for a new administration to generate new enthusiasm and really make PPB a useful tool, but it is the major argument of this paper that asking for "information" about programs, and exhorting analysts to "set goals" and "consider alternatives" will not lead to more success than has been achieved so far.

The paper has three major sections. The first discusses the reasons for the failure of PPB, and points out the necessity of defining an ethical basis for Government (and private) behavior, without which PPB can never be useful. In the second section, an ethical code is specified and conclusions drawn concerning the desirable activities of Government. Finally a specific proposal is presented which applies the conclusions of the preceding section to the operations of the PPB system.

I. THE FUTILITY OF PPB WITHOUT AN ETHIC

The PPB system is an information system designed to increase the flow of policy guidance into an agency, and the flow of information on achievements and opportunities for improvement to the agency's executives.* The system requires the establishment of central analytic staffs in departments and major executive agencies. These staffs are to categorize their agency's programs into a program structure, which reflects the national goals being sought, and to analyze the agency's effectiveness in achieving them. Each year, the Secretary or agency administrator is to present to the Budget Bureau a program memorandum showing the agency's major expenditure proposals for the year with analytic arguments supporting the proposals, and a financial plan which shows future implications of the spending on each of the proposals.

In the three and a half years since the system was instituted, analytic staffs have been created, program structures have been defined, and program memoranda and financial plans have been presented. Despite adherence to the formalities of the system, however, the predicted revolution has not taken place. Presentations purporting to be analyses are often no more than poetic rewording of old style budget submis-

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

sions. Where attempts have been made to present real calculations of costs and output, the conclusions have been rejected on the grounds of political irrelevance. Considering the often-stated desire of both the executive and legislative branches of government for more and better budget information, and that the PPB system was designed to satisfy that desire, it is vital to discover why better results have not been obtained.

That visible changes in the budget decision process and in budgets have not taken place is commonly acknowledged, and observers of government who have an interest in PPBS, either as critics or as supporters, recognize that dissatisfaction with the system is widespread in the government.* As an antidote for disaffection, some of PPB's supporters search for ways to adjust the system to make it more effective. Those critics like Aaron Wildavsky** who recognize that better information is an exceedingly important ingredient in the recipe for better decisionmaking are concerned about how to save policy analysis from the demise expected for PPBS. What both supporters and critics seem to miss in their prescriptions is that it is not PPB which is sick; the system works effectively to produce a flow of information. It is the information which flows through the system and its use which must be improved. Rather than saving analysis from the disaffection over PPB, we should try to make the analysis useful. If we succeed, we may rescue PPB from the disaffection over the useless policy analysis it has produced.

To suggest that the PPB system can be saved by improving the analysis performed is like suggesting that if only a sick man would become healthy, his disease would disappear. An orderly approach to curing a disease, however, involves identifying the disease from its symptoms, finding the remedy specific to the disease, and administering it to the patient. In the case of the PPB system, it is really the Government that is the sick patient.

The major symptom is that "analysis" is performed, some of it at a high level of technical competence, but it does not contribute to the decisions it is intended to influence. It could be that this is due to a pervasive lack of interest in improving the responsiveness and efficiency of Government in meeting its responsibilities. The competitive nature of the elective process, however, is a form of insurance against this being the case. An alternative diagnosis, which is strongly supported by experience, is that the analyses do not tell decisionmakers what they want to know. C. Lindblom makes this point repeatedly¹ in describing the administrator's reaction to the "academic" or "theoretical" approach to analysis. Wildavsky points out that the typical Bureau of the Budget response to PPB analyses is an indication that the analyses do not really hit the mark. Yet no one from the President on down into the hierarchy, seems able to tell the analyst what information he wants. Unless the analyst knows what to measure, he is in a position similar to that of a doctor whose patient will not tell him "where it hurts." When he prescribes a medication he is told the

¹ Charles Lindblom, "The Science of 'Muddling Through,'" *Public Administration Review*, vol. XIX, spring, 1959, pp. 79-88.

*Further discussion of this issue is found in the paper by Marvin & Rouse in this volume.

**Further discussion of this issue is found in the paper by Wildavsky in this volume.

pain persists and that he should prescribe something else, and something else, and so forth; but his pills never work.

Symptoms that all is not well with policy analysis have been identified as a divergence between decision criteria and analytic criteria. The next step is to identify the disease which causes the symptom. Why is it that the decisionmaker does not specify his criteria and have more relevant information presented to him? The obvious but painful answer is that generally accepted criteria for program evaluation do not exist. This comes as no news to welfare economists who have for years been trying unsuccessfully to define practical measures of the conditions under which Government can act to increase the welfare of society. They accept the philosophical position that it is possible to assert that the welfare of society has increased only if everyone in society is at least as well off and some are better off as a result of Government action. Since most, if not all, Government actions involve taxing some people (making them worse off) and providing benefits to others, economists have felt unable to give unequivocal advice on the desirability of particular programs. Typically, they have restricted themselves to judgments that programs do or do not increase national income, while leaving to the politicians the judgment of whether or not the particular groups helped and hurt are deserving of the help or the hurt. But if Government programs always do and always have helped some and hurt others, policy analysis which adds a dimension for evaluation (national income) without addressing the politician's problem (distribution of benefits and costs) simply makes his decision problems harder rather than easier. Any program change, as well as any new program, presents such a distributional problem. These distributional problems are what concern politicians, and while efficiency is a desirable attribute of any program, it cannot be the sole, or even the major criterion of judgment.

The disease affecting policy analysis is thus seen as a disease afflicting Government, i.e., there is no apparent basis for governmental judgment between people for the purpose of imposing taxes and providing benefits. A partial remedy for this disease would be for policy analysis to concentrate more on illuminating the distributional effects of programs. This is the tack which has been taken by a number of economists and political scientists in recent technical literature.² But there is considerable room for doubt that distributional studies as suggested in the articles cited really provide politicians with valuable information. Usually the beneficiaries of a program can be counted on to make their presence and their wants known to the politician, and he may know better than does the analyst, who pays and who benefits.

For policy analysis to become more useful, it must go beyond simply indicating who are gainers and who are losers and give some guidance on the ethical matters which form the bulk of the politician's decision problem. One approach involves evaluating outcomes of policy decisions by estimating efficiency losses associated with specific programs and measuring the implied weights which would have to be placed on

² For example, see articles by A. Maass in the *Quarterly Journal of Economics*, May 1966, and by R. Haveman in the same journal, November 1967, and by B. Weisbrod and J. Bonnen in S. E. Chase ed., *Problems in Public Expenditure Analysis*, Washington, D.C.: Brookings Institution, 1968. See also the papers of Weisbrod, Freeman, and Bonnen in vol. 1 of this collection.

beneficiaries and losers to make the program desirable. This is the approach followed by Maass, Haveman, Weisbrod, *et al.* in the articles cited earlier. While work along this line has not yet proceeded far enough that predictions of its eventual success or failure can be made with confidence, I suspect that success will be limited at best. The reason is that in its application this kind of analysis will either have to disregard second order distributional effects or create a "general" model which calculates *all* effects. If some effects are disregarded, the decisionmaker will have to worry about whether those which are disregarded should have been considered. With a general model, the amount of information presented will probably be more than he can encompass.

A different approach involves the statement of some basic rules embodying widely held concepts of distributional equity, and drawing from them conclusions on how the Government should act in specific cases. This approach is potentially more productive because it does not require new judgments and new weights to be made for each new analytic effort. Of course, to reach agreement on an ethical judgment is no mean feat, but since any alternative approach will require some consensus on distributional equity, the difficulty of reaching agreement is no argument in favor of one approach over another.

The next section will trace the implications of Government's acting under, and assuring that individuals in society adhere to, an explicit ethical rule concerning the distribution of wealth and the rights to use of that wealth. The ethical rule adopted in this analysis is one I think is descriptive of the basis upon which American society is organized. It is presented as a practical suggestion for the operation of Government and, hence for the guidance of analysis of Government activities. The reader should be forewarned that the conclusions reached are logically derived from the stated judgment. If the conclusions are not acceptable, it is the initial ethical judgment not the conclusions which should be discarded.

The ethical rule can be stated technically in the terms familiar to economists: *barring interference with the property of others, the owner of any productive factor³ should be free to choose the employment of the factor, and the output of that employment should be the property of the owner of the factor.* In nontechnical terms, this means that an individual is free to use his labor and his wealth as he sees fit so long as he doesn't impose costs on other people or infringe upon their freedom to employ their labor or other resources. What the individual manages to produce with his own labor and wealth is considered to belong to him. Implicit in the ownership of property and the freedom of the individual to use it as he sees fit is the acceptance and guarantee by others, including Government, of security in the rights of ownership. Theft or expropriation of property is recognized as being unethical. Exchange in the market is expected to occur, but no one can be forced to give up more than he gets in return. Acceptance of these rules can be recognized in the philosophical writings of Locke and other 18th-century philosophers and is generally conceded to be embodied in the Constitution of the United States.

In tracing out the logical implications of adopting this ethic, it will be necessary to employ some technical terms commonly used in formal economic theory. Such terms are defined where they are used, and

³ A factor is any input to a productive process. Thus labor, capital, land, entrepreneurial skills, etc., are all factors.

their use has been held to a minimum, so the nontechnical reader should have no difficulty in understanding the argument.

II. THE ECONOMIC MANDATE OF ETHICAL GOVERNMENT

In applying these rules, let us return to the basic problem of Government, i.e., how can the welfare of society be improved through Government action. Throughout this paper, the welfare of society will be considered to increase only if, as a result of Government action, some people are better off and no one is worse off. In studying the operation of a market economy, economists have usually started from their knowledge that perfect free market operation allows individuals to exchange resources and goods with each other and arrive at a position in which every individual maximizes the satisfaction (welfare) he derives from life. Perfect free market operation also leads to maximization of the value of national income. However, they are quick to point out that markets do not operate freely and perfectly, and the happy outcome of individual welfare maximization is not achieved. Whenever markets fail to operate perfectly, inefficiencies occur in production or in the final distribution of goods, and an opportunity exists to increase exchanges in the market, and thereby increase the welfare of individuals in society. Economic arguments for Government action are thus all derived from assertions that there are specific market imperfections that cause welfare (and national income) to be less than it could be.

It is important to understand that the argument which follows does *not* suggest that government should maximize national income. Despite the fact that it has been demonstrated that acts to maximize national income may *reduce* welfare,⁴ many economists persist in recommending such actions. As it turns out, many actions recommended under the ethical rule are the same as those recommended by national income maximizers, and the distinction may appear at first to be merely a semantic quibble. In some cases, however, actions to increase efficiency (or national income) are *not* acceptable under the ethical rule, and the importance of distinguishing between the two rules will be seen to be more than a quibble. Since welfare cannot be increased unless markets fail to operate perfectly, it is appropriate to start by listing market imperfections and examining the implications of the ethical rule for government action with respect to them. Generally, there are four types of market imperfections which concern us. They are:

- (1) Lack of competition in factor or product markets.
- (2) Restrictions on the movement of labor and products.
- (3) Lack of knowledge by buyers or sellers.
- (4) The existence of costs or benefits of individual action that accrue to others.

Consider the effect of a lack of competition in a factor market. Assume, for example, that we are dealing with the very real problem of an employer whose demand for labor represents a significant part of the total demand for labor in the market. If he wants to increase the number of workers he hires, he will have to offer a higher wage than he pays his current employees in order to attract workers away from

⁴ For a thorough exposition of the argument, see P. A. Samuelson, "Evaluation of Real National Income," *Oxford Economic Papers*, NS II, (January 1950), pp. 1-29.

other employment. This higher wage will have to be paid to his current employees as well because, if it were not, they would be free to quit and then apply for rehiring at the higher wage. The employer is thus in the classic position of a monopsonist, and this position can be shown graphically in the following way.

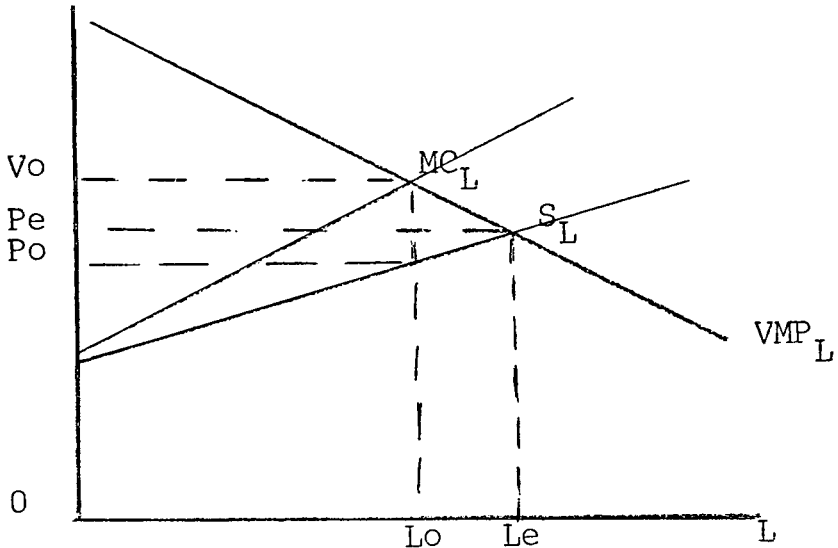


FIGURE 1

In the diagram, the employer's demand of labor is represented by a curve which shows what is technically called the value of the marginal product of labor (VMP_L). That line represents, at any level of employment, the contribution that an additional laborer would make to the value of the output of the firm. The supply of labor (line S_L) shows the wage that the employer would have to pay every laborer at any level of employment, and the marginal cost of labor (line MC_L) shows the additional cost the employer would incur to hire an additional laborer. MC_L lies above the supply curve S_L to show that the wage would have to rise for all laborers, not just the additional laborer hired, and thus the total cost of labor would rise faster than the wage rate. The amount of labor the employer would hire is L_0 , because if he hired less, the additional to output that would result from hiring more labor would be greater than the cost of hiring more, even though we had to increase the wages of all other laborers. If he hired more than L_0 , he would have to pay more for the additional laborer plus his currently employed labor, than the additional laborer would add to the value of output.

At the equilibrium employment level, L_0 , each worker would be paid P_0 while producing an increase in the value of output V_0 . But the ethical rule states that what a factor produces is the property of the factor. This divergence between what the laborer is paid and what he produces is therefore a violation of the ethical rule and if government and individuals in society are to adhere to the rule, such a violation must be corrected.

If the government were somehow to force the employer to pay each laborer the value of his marginal product, the eventual equilibrium would be at an employment level L_0 and a wage P_0 . This point has special significance to economists because it is not only "equitable" under the ethical rule, but it is also "efficient." Any increase in output of the product could only be achieved by giving up something of greater value elsewhere in the economy, and if any less were produced, something of lesser value could be sacrificed elsewhere in the economy to increase the output of this product. Thus, the policy recommendation of those who would seek to maximize efficiency and those who advocate following the stated ethical rule would be the same. The difference is that if we operate under the rule, the uncorrected inefficiency is seen to be derived from an unfair use of market power which should be corrected because of the unfairness involved, while the efficiency argument says only that a larger quantity of the good in question can be produced.⁵ Under the equity rule, we judge the fairness of the market allocation as a basis for government action. If we seek efficiency for efficiency's sake, we must judge the fairness of the allocation of the increase in output which results from government action, but without a specific guide to what is meant by "fair." This judgment can also determine whether or not it is "fair" to take action to seek efficiency for it is clearly possible that the increase in efficiency will hurt some people while adding to the totality of consumable goods.

The classic example of such a market imperfection is seen in the "company town" where a relatively isolated population has few alternatives to working for the single major employer. Although the advent of the automobile and modern communications have made this problem much less severe than it once was, it still exists. The problem may exist with regard to any specialized labor force even in a large labor market if there are few bidders. One could argue in the same way that public school systems are acting as monopsonists and that the wages of teachers are below what they should be getting paid. While empirical tests are practically impossible because education is provided through public schools throughout the country, one can imagine that if public education was decentralized so that every school principal was able to allocate his own budget free of constraints on teacher's salaries, building characteristics, et cetera, there would be a reallocation of school resources so that more teachers were employed at higher salaries than at present, with the higher level of pay resulting from competitive bidding among schools. The difference between the current number of teachers with their current pay, and the competitively hired number of teachers with their competitively determined pay would be an indicator of how far we are from a "fair" pay scale for teachers.

Similar inequities arise when the firm operates in competitive markets for goods and labor that go into production (factors) but sells in a market in which the firm's level of output affects the price of the good. Whenever this is so, the firm faces a marginal revenue curve which lies below the demand curve. The same rationale which in the previous example, led to the conclusion that factors were being paid

⁵ The efficiency argument does not say that more will be produced. Workers may be tempted to take longer vacations because of their higher incomes, or the good produced may be one which increases the enjoyment of leisure. Ideally, the value of leisure time should be included in national income, and if it were, efficiency increases and national income increases would always be seen to go together.

less than the value of their product, is applicable here. In order to sell more units of output, the producer must lower the price he charges for all units, not simply the price on the last unit sold. The value of the last unit sold is greater than what the firm is paying for its factors, and thus, the ethical rule is being violated. If the rule were upheld, the eventual equilibrium would be reached where the price of the product just equaled the increase in cost, and all factors would be paid the proper amount.

The two types of problems discussed should be familiar to all students of economics and government. The widely held attitude of general approval of union activities often seems to be based on grounds of "exploitation of labor" by employers. As such it represents tacit acceptance of the principle that employees should be paid the value of what they produce. Government antitrust activities have often been advocated by economists on the grounds that monopoly causes a divergence between the value produced by individual factors and the returns to those factors, but that complaint is less well understood, and probably less important in policy recommendations offered to government than that monopolists act in restraint of trade. It is claimed that the exercise of market power by very large producers prevents free entry into production by new competitors or the free flow of resources or goods between markets.

Constraints on entry or on movement of goods or factors is recognized and defined as another type of market imperfection. Consider the effect of monopolistic constraint on mobility in a factor market. For example, take the real problem of restrictions on the entry of labor into a specific field of employment.⁶ In the diagram, the supply of labor is drawn as the horizontal line S_1 , indicating that an employer can hire as much labor as he wants at the going wage rate. The line VMP_i is the same as in figure 1, and it shows the increment to the product of the firm realized by hiring an additional laborer. The amount of labor which would be hired in an unconstrained labor market is L_e , at which employment level the wage would be P_e . If some qualifications could be imposed on the hiring of labor such that many laborers became ineligible for employment, the supply of labor facing the firm would be S_1^1 , and he would pay a higher wage, P_o , to hire L_o of labor. But the labor which would otherwise be employed by this firm, $L_e - L_o$, would either be unemployed, or would have to find work elsewhere at a lower wage. The existence of the constraint on labor mobility into this occupation would thus prevent some laborers from selling their services for what they were worth. While this is not as clear a case of expropriation of the "property" of an individual as in the earlier example, it is nonetheless a violation of the ethic which allows factor owners to employ them without interference.

An example of a problem of this type came to national attention recently when several women were prevented from working as race-horse jockeys because of the exercise of market power by male jockeys. Other examples abound, of course, particularly in discriminatory union rules which prevent members of some minority group from entering certain fields of employment, and in many professional societies which set licensing standards very high to prohibit entry.

⁶ See, F. Machlup, "Monopolistic Wage Determination as a Part of the General Problem of Monopoly," Chamber of Commerce of the United States, *Wage Determination and the Economics of Liberalism*, 1947.

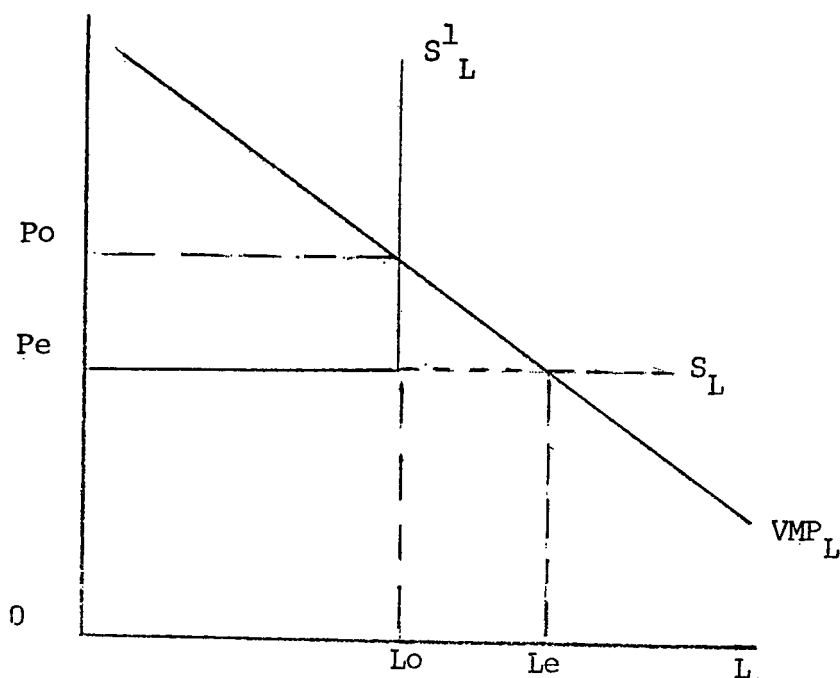


FIGURE 2

Governmental action to assure ethical behavior by all individuals would require that restrictions on mobility be removed, where unfair use of market power was preventing a factor from being paid its marginal product by interfering with the owner's ability to employ it in any productive activity. Such a condition is of necessity more difficult to identify even in theory than would be one in which a factor is paid less than the value of what it actually produces because the issue hinges on potential rather than realized production and because often there are at stake matters of individual choice of whom to employ and how and at what wage. If an individual does not *wish* to hire a factor, even if it would be profitable to do so, his failure to hire the factor can hardly be considered an unfair use of market power. Even among economists, definitional difficulties have not been resolved with respect to occupational discrimination. I think that the distinction must be made with respect to third parties interfering with contracting arrangements between two principals, but where to draw the line is not clear.

A third imperfection which prevents markets from reaching a welfare maximizing allocation of resources is a lack of knowledge on the part of buyers or sellers.* Market transactions between individuals are undertaken because each partner to the transaction perceives a gain to himself. If either party misapprehends the value he can derive from the transaction, however, nonoptimal exchanges, i.e. exchanges which do not maximize individual welfare will result. Most cases in which

*Further discussion of this issue is found in the paper by Davis & Kamien in vol. 1 of this collection.

people "make mistakes" do not violate the ethical rule which explicitly leaves to individuals the determination of how to spend their wealth. But in a system in which property rights are traded among individuals there must be some enforcement of contractual arrangements. If property is willingly given up by one individual on the promise of receiving something in exchange which is misrepresented or not then rendered, the failure to live up to contract terms represents a theft or expropriation of his property which is, of course, a violation of the ethical rule.⁷

Consideration of the problem of an imperfection in knowledge as one of misrepresentation or fraud is not usual in discussions of imperfect knowledge in welfare economies. It has been common to suggest that the efficient perfect market allocation of resources depends upon everyone knowing not only presently available investment opportunities, but also future market demands and supplies. It is certainly clear that if technological and consumption possibilities were known to everyone, the allocation of resources in the economy would be more "efficient" than in a condition in which not all individuals know all there is to know. But information is not free in the sense that there are costs of production and acquisition just as with other goods.⁸ As long as there are costs associated with increasing knowledge, efficient resource allocation will result from something less than total knowledge and the whole idea of what is perfect knowledge must be reexamined. Consideration of knowledge as a commodity subject to the rules of property leaves it very little of an explanatory role as a cause of market failure.

Policy recommendations offered in relation to the three types of imperfections discussed to this point should not cause any great controversy among economists. It is a familiar conclusion of microeconomic theory that an efficient allocation of resources is found when factors are paid the value of their marginal product, and whether the "efficiency" approach or the ethical approach is used as a basis for policy formulation, the recommendation is the same. Yet I should like to point out again that if the ethical basis for removing market imperfections is not made explicit, the decisionmaker is asked to decide on the distribution of wealth among individuals on the grounds of pure national materialism—that more national income is better than less—grounds which analysts have long recognized to be very uncomfortable indeed.

Another type of market imperfection which provides Government with an opportunity to increase the welfare of society, is the existence of costs or benefits of individual action which accrue to someone other than the individual.* Such problems are responsible for the bulk of Federal expenditures at present and, therefore, understanding of how Government *should* act in respect to them is most important simply because of the number of dollars involved. Beyond the level of expendi-

⁷ John Stuart Mill notes a view prevalent in his time, that "government, ought to confine themselves to affording protection against force and fraud: that, these two things apart, people should be free agents, able to take care of themselves and that so long as a person practices no violence or deception to the injury of others in person or property, legislatures and governments are in no way called upon to concern themselves about him." *Principles of Political Economy*. Book V. (Longman, Green, N.Y., 1892).

⁸ See G. Stigler, "The Economics of Information", *Journal of Political Economy*, vol. 69, June 1961, p. 213.

*Further discussion of this issue is found in the papers by Davis & Kamien, and Kneese & d'Arge in vol. 1 of this collection.

tures, however, problems of external effects arise because increasing levels of population and increasing geographical concentrations of people, both of which are characteristic of our society lead to new interactions of people and property. As more and more problems of external effects come into being, the importance of developing appropriate governmental procedures for handling them increases. It is also in relation to external effects that the ethical problem facing Government appears to be greatest, and policy recommendations based on efficiency in resource allocation carry the least weight.

Consider as a simple example of the problem the case of a farmer and a railroad owning adjacent pieces of land. If the railroad introduces a new production process which has an external effect, such as steam engines which drop hot sparks on the farmer's field and burn some of his crops, there will be a conflict over the rights of use of the property. The operator whose trains burn the crops can be viewed as either having "taken" some of the property of the farmer in the act of using his land for dropping sparks or, alternatively, of exercising the right to drop sparks into the air, no matter where they fall. Ronald Coase, whose article, *The Problem of Social Cost*,⁹ discusses this problem, points out that what is at issue is the ownership of the property rather than the production of railroad services or agricultural products. Once the property ownership has been defined by the courts, an opportunity for costless transactions between the two parties would lead them to the most socially productive use of the property. What results from the property ownership being defined clearly is determination of which partner in the transaction is to pay and which to receive payment for use of the property. If ownership is assigned to the farmer such that he can expect to produce without interference from the trains, the railroad owner will calculate the profit¹⁰ from dropping his sparks and offer to pay the farmer for the right to dump.

If accepting the payment and allowing the railroad to operate provides the farmer with more profit than he could otherwise make, he will accept. If not, he will reject the offer. Whichever course is followed, the end result will be to produce the goods, either railroad services or agricultural products, which are most desired by consumers as measured by the profitability of the alternative enterprises. If the right to use the land were assigned to the railroad and the returns from farming the land were greater than the returns to dropping sparks on it, the farmer would "rent" the railroad's right to drop sparks at such a price as would maintain the profit of the railroad at its initial level.

When the external effect is more generally spread, the characteristics of the problem may change to such an extent that individuals cannot make the proper transfers by themselves. If the trains passed the land of many farmers, a number of transfers would be called for and difficulties could be expected in even the simple business of bringing interested parties together. But the problem would be relatively minor if the property rights were held by the farmers. Payments from the railroad would be offered to the individual farmers such that, in each case the value of the land holding was maintained at a level at least as high as it was before the new technology was introduced. There

⁹ *Journal of Law and Economics*, vol. III, October 1960, p. 1.

¹⁰ If transaction costs are positive, the equilibrium condition can be restated such that the calculation of profit before and after the transaction includes the cost of transaction.

is every reason to expect that appropriate payments would be forthcoming irrespective of the number of such two-party transactions.¹¹

If the property right is assigned to the railroad, however, the appropriate level of transfer generally will not be achieved for if any farmer pays the railroad to restrict its droppings of sparks, that restriction will be as helpful to a farmer who has not paid as to the one who has. Thus all farmers would have an incentive not to pay while hoping that others do pay. In the event that some farmer does pay, he has produced something of value¹² which should belong to him, given the equity rule enunciated at the outset. A transfer of property has taken place, but no mechanism exists by which the donor can collect from others the value of the property he has bestowed upon them by his action. Left to operate freely, individual farmers will stop short of the point where the sum of the incremental values to each just equaled the incremental profit derived by the railroad's operation of its last unit of sparks dropped. Thus, while these might all hope to benefit from payments made by others, they would all be willing to pay more if *everyone else paid as well*.

Some institutional arrangement is called for if an optimal set of payments is to be achieved. If few farmers are involved, it may be possible for an agreement to be reached spontaneously which will lead each to make the proper contribution. In the extreme, described by Samuelson as a public good,¹³ voluntary contributions of the optimal amount will generally not take place. While many conceivable institutional arrangements could lead to the proper payments by each farmer, they would all have the role of assessment of value produced and the power to extract payments, i.e., the power to tax. Given the institution of private property, taxation by the agreed-upon agent for the calling forth of the desired collective action can be considered as enforcement of property rights by the agent over the value it produces. In order to avoid confiscation of private property in such an activity, this agent must perceive the profit functions of its constituents to determine the optimal level of payments and the distribution of the tax burden among the individuals—the sum of whose marginal profit were just adequate to bring forth the marginal unit of production. Of course, some farmers will end up paying nothing since the effects on their profits may drop to zero at the margin. Some others who get a profit on the margin may not pay if they *resent* the action, i.e. if they must bear some psychic costs as well as dollar costs. The tax is simply a necessary tool to get the “buyers” who want the marginal unit to compensate the agent for the value of the property the agent “sells” to them.

This role of agent cannot be well filled by the operation of the free market because necessary for its success is the power to enforce payment for the property transferred. The accepted agent is Government which is given the power to enforce property rights by the members

¹¹ Of course, some farmers may hold out for higher payments because they resent seeing “productive land” being “wasted”, i.e., they bear psychic costs from not using the land. Even were some farmers to hold out just to extort a high payment from the railroad, the most they could get would be the amount the railroad or the farmers had managed to save in bargaining—i.e., the total amount of cost saving to the railroad from using the new technology.

¹² The “thing” of value is the wealth increase generated by the reduction in the dropping of sparks. The value of this “thing” will be measured by the increase in the value of the land held by the other farmers.

¹³ P. A. Samuelson, “The Pure Theory of Public Expenditures” *Review of Economics and Statistics*. Vol. 36, No. 4, November 1954.

of society. If the proper collective action is carried out effectively, the existence of external effects in production will pose no problem for the observing economist with respect to the efficiency of resource allocation or to the distribution of incomes. Government will act in lieu of the market to produce a collective good with the product being bought by the individual consumers.¹⁴

The action of Government in collectivizing individual desires of farmers in the latter case where external effects exist has several characteristics worth dwelling on. First, collective payment by farmers is financed by taxing those who want to spend, not by taxing other farmers or other members of society who get no direct benefit from the expenditure. Second, the amount that individual farmers pay is a function of the value they receive from the last (marginal) reduction of spark dropping. Government taxes and spends only after the property right has been allocated and it is clear that the market process for making desired transactions (payments to get the railroad to stop dropping ashes) has broken down. In general, the essential characteristics of the problem are: (1) assignment of property rights; and (2) many individuals who want the same result but cannot exercise property rights over what their activity "produces." Essential characteristics of the solution of the problem are: (1) expenditure made by the Government on behalf of those who want to spend to achieve the desired objective; (2) taxation distributed appropriately so that beneficiaries (on the margin) pay for the expenditure.

Many similar cases can be found which are much more realistic than the given example of railroad and farmer. The existence of external costs is essentially what is involved in any problem of pollution, and in fact air pollution is really what was at issue in the railroad-farmer case. It was defined as an *expenditure problem* for government when the railroad (polluter) was recognized as the owner of the property right. In the case of widespread pollution of air and water in today's society there seems still to be some confusion over who owns what rights, and governmental action to reduce pollution seems to be stalled on that question. If it is eventually determined that the polluters have property rights in the air or on the water, and therefore have the right to pollute, then the appropriate action would be for the government to tax those who would benefit from pollution reduction and offer payments to polluters to reduce pollution. If polluters are recognized as being offenders, it could be left to the courts to assess damages against them in trials instituted by individuals and the polluter would then have to bargain with those individuals.¹⁵ If it turned out that the good being produced by the polluting activity was one for which substitutes could easily be found, production of which did not require the generation of pollution, the producer (polluter) might be unable to raise his price high enough to cover the tax and the firm would simply stop producing the pol-

¹⁴ The existence of transaction costs, i.e., costs associated with individuals getting together with the railroad and with each other is considered by some economists to be the only explanation for failure to achieve the appropriate level of payments. While I recognize that the proper level of payments could be achieved by voluntary action, it need not be. In any case, the existence of such costs makes the argument for Government action as a "convenience" a more powerful one.

¹⁵ As an alternative, the extent of damage could be assessed and collected through special taxes on effluent by government, with the proceeds then to be distributed to those who were hurt by the pollution. This latter action would be simply a case of government enforcing a court order against pollution.

luting good. If there were no easy substitutes, however, the tax could be passed on to the consumer by the firm raising its price.

Naturally, the higher price would tend to reduce consumption of the good in question, and lower levels of production would be accompanied by lower levels of pollution. Of course, even though pollution would continue, there would always be an incentive to reduce the tax by finding ways of reducing pollution, and this demand for pollution-reducing technology would work its way through the economy leading to research on and production and installation of the desired equipment.

The type of externality which is related primarily to physical effects is only one of many in which can be found the conditions calling for Government action. The argument that external benefits exist and desires for consumption should be collectivized through Government action is used to justify Government expenditures on defense, education, public health, roads, maintaining the family farm, irrigation, conservation, and a host of other programs. In each case, the collectivization of expenditure is justified on the grounds that individuals want to spend on the good in question, and the Government simply acts as the agent to coerce *beneficiaries* into paying for the benefits received.

One external effects problem of great importance is found in the fact that information is a commodity over which it is particularly difficult to exercise property rights.¹⁶ This externality problem is serious in that a man who produces new knowledge cannot expect to capture the full value of his production except through monopolistic exploitation of the knowledge in production, and hence the incentive to produce new knowledge (through research) is reduced below what is economically desirable. One possible treatment of this problem would be to enforce the producer's property rights to the value of the information in the market, although this solution would involve enforcing a noncompetitive price in the market. An alternative would be to tax users of the information and have the Government make payments to the researchers equal to the increase in profits which are realized through its use.¹⁷

In most economic arguments over public goods, the externality argument is accepted as the basis for asserting the desirability of public spending, but the relation between levels and patterns of expenditure on one hand, and levels and patterns of taxation on the other is not generally accepted. In the argument so far, Government action to tax and spend on public goods is consistent with the ethical rule only when the benefits of Government spending are directed to those who pay the tax, and the tax is justified only when the taxpayer is willing to pay the tax in order to get the good. *Unless taxpayers are satisfied that they are being provided with benefits equal in value to them of the taxes they pay, they are worse off than in the absence of Government action.*

¹⁶ For a discussion of the implications of this problem for Government activity in supporting research, see "The Economics of Research and Development," unpublished manuscript by L. Sjaastad, Department of Economics, University of Chicago.

¹⁷ Knowledge is peculiar in that it does not get "used up." One individual can pass it on to another without diminishing his own possession of it. As such, once knowledge is produced, it is not scarce in the same sense as other goods and its market price is thus the cost of transmission. Traditional attempts to enforce property rights over knowledge through copyrights have begun to fail because of the increasing ease of duplication.

The one major problem which has troubled economists and prevented them from accepting the idea that taxes and benefits should go together, is the fact that the wealth and income distribution is such that some individuals are very much poorer than others. It is widely accepted that the richer one is, the less satisfaction he derives from his last dollar of expenditure, and many people find it very tempting to assert that if wealth were taken from the rich and given to the poor, the increase in satisfaction of the poor would far outweigh the loss of utility by the rich. Such action, however, would be directly contrary to the rule that what a factor earns is the property of the factor, and cannot be "taken" by anyone including the Government. I have argued elsewhere¹⁸ that the existence of externalities in income redistribution makes the problem of redistribution no different from that presented by any other public good. We rely on Government to redistribute incomes because we cannot enforce payment by other would-be donors for the benefits we produce through individual action. But it is not necessary to rely on a social moral imperative to suggest that incomes be redistributed. So long as many individuals in our society desire to see poor people made better off, the question of whether Government should or should not redistribute income is answered with a clear affirmative.

Note that the problem has the same characteristics as the problem of pollution abatement. If one individual donates to the end of increasing the welfare of the poor, others who wish to achieve the same end will get the benefit of the donors' action without contributing themselves. If donors were able to exercise property rights over the value to society of the increase in welfare of the poor, they would charge other beneficiaries for their service. The final result would be that the donations would be made which increased the welfare of all donors, and there would be no question that the welfare of society was increased. No individual would donate who did not benefit, and it would be unnecessary to make the judgment that the increase in income realized by the poor was more important than the loss of welfare of the rich. Of course, such an arrangement is not a practical possibility and, as with the railroad and the farmers, defense, and other public goods, we leave it to the government to handle for us.

At first glance, this argument appears to be extraordinarily selfish; it suggests that we live in a society which considers that increasing the welfare of the poor should be dependent on the good will and feeling of brotherhood of the rich. Surely, however, this is the basis upon which we already transfer income.

Consider the course of action we now pursue. It is generally accepted that from the point of view of the recipient, the best way to increase his welfare is to transfer income to him directly. Yet we in this country have found it difficult to transfer money directly. Instead, we donate food, clothing, housing, education, training, etc., on the grounds that these are the only types of programs that can be pushed through Congress.

Those programs pass when direct transfers fail because they are the programs that the great majority of Americans (taxpayers)

¹⁸ *Efficiency, Distribution, and the Role of Government in a Market Economy*, IDA Research Paper No. P477. See also, R. Zeckhauser, "Optimal Mechanisms for Income Transfer." RAND Corporation, P3878, June, 1968.

want. If it is granted that current welfare expenditures reflect the concern of taxpayers, taxpayers, at least must appear to be not so selfish at all. The amount of money spent in "welfare" programs in this country is immense by any standard. It must be conceded that many so-called welfare programs redound to the direct financial benefit of high-income individuals, but they do reflect the generosity of the more affluent society in that they are usually initiated on the argument that they are designed to help the poor. As a final philosophical point, it is difficult to argue that a society is more benevolent when it provides for its poor as a matter of law rather than as a matter of genuine desire on the part of its affluent citizens to help those who are less fortunate.

The conclusions to be drawn about government and welfare in this analysis are that if the rules of property are to be rigorously enforced, the government should act to remove market imperfections as a matter of *social justice*, and without regard to possible changes in the *distribution of wealth*. With respect to the existence of external cost or benefit; that is, in the case of a public good, government should act in behalf of those whose willingness to spend is not expressed because of the externality. Thus it should avoid redistributing wealth when it collectivizes expenditure by distributing the tax burden in accord with the distribution of benefits. In many cases, it is very difficult to adjust taxes in this way, although some adjustments are possible which are not as difficult as some people make it appear. Many programs of the Federal Government provide benefits which are geographically localized; for example, local irrigation projects, water and sewer grants and loans, grants to cities for airport construction and modernization, or for purchase of other benefits peculiar to those cities. Many activities now carried on by local governments are at least partly national in the distribution of benefits, such as income redistribution to the poor, at least minimal amounts of education, preservation of natural beauty, and so forth. Acceptance of responsibility for collective expenditure at the appropriate level of government would go a long way toward arriving at the "correct" distribution of taxes and benefits.

A perfect distribution is obviously beyond us. At present we act, at the Federal level, on the assumption that the desire for the whole package of Federal expenditure is a function of one's income adjusted downward to the extent of one's personal responsibilities (dependents) and infirmities (age and blindness), and source of income (wages or capital gains), and so forth. In addition, we tax as if corporations desire Federal expenditure to the tune of about half of their profits, adjusted downward for the extent to which they deplete resources, invest in capital equipment, and so forth. While these measures do measure something, it is difficult to believe that corporations "desire" anything at all, particularly not in such relation to their earnings, and it is not at all clear that individuals desire collective expenditures on the bases described. It is certainly difficult to assess peoples' desires, especially when they would have every incentive to hide their preferences if they could thereby avoid being taxed. For this reason the assessment of individual desires for collective expenditure is left to the political process, and is the politician's major responsibility. But difficulties in assessment notwithstanding, the description of what is

desirable in government should not be passed off as unworkable without further comment. It should be an ongoing effort in government to align the distribution of the tax burden with the desire for collective expenditures. And in the meantime, it should be the main concern of policy analysts to expose existing desires for collective expenditures, and, with our current distribution of taxes, to identify the costs and benefits to taxpayers of existing and proposed government expenditure programs.

At this point, the reader must consider whether or not he is willing to accept the ethical rule stated at the outset, for his conclusions about what should be done by government do not match very well with what government in fact does. Peter Drucker has recently concluded¹⁹ that government is sick, that it does not do what it promises. It seems to follow no rules, and simply keeps on doing, in a rather mindless way, what it has done in the past—growing all the while. I think that the performance of government is poor not because government is unethical, or because a different ethic from Drucker's or mine is being followed, but rather because we have lacked a fully, clearly, and publicly stated ethic. It is always possible to complain about government, but unless one has a clear idea of what is good, it is impossible to show that any course of action is unqualifiedly better or worse than another. Unless Drucker can show what is right to do, he will have a hard time changing what he feels is wrong, and "rightness" or "wrongness" involve an ethical judgement.

III. How To Reclaim PPBS

This analysis as it has been developed does not call for judgments to be made through the political process of whether any individuals are better or more worthy than any others. It points only at the conditions under which an opportunity exists for Government to increase the welfare of society, given an initial statement of what is just or equitable about interpersonal relations. But in identifying the conditions which make possible Government action to increase welfare, nothing has been said of how the politician is to know the situations in which inequities exist, or the desires of the people he represents for collective expenditure. Nor has the matter of how taxes can be assessed against those who benefit from collective expenditures been approached.* In fact, it is these two questions which I conceive to be relevant areas for policy analysis. If the rule describing how individuals and Government must act in regard to property is accepted, a policymaker who has to decide whether to change a program or initiate a new one will want to know whether the program is being advocated in response to a real problem or if it represents simply an attempt by some group to exercise its political power over another group in order to enrich itself. In the administration of antitrust laws, the executive will want to know if unfair conditions exist which can be ameliorated through prosecution. In voting on minimum wage legislation, the Congressman will want to know whether all low wages are due to the exercise of monopsony power by employers, if establish-

¹⁹ Peter Drucker, excerpt from *The Age of Discontinuity*, published in *The Public Interests*, No. 14, winter 1969, p. 1.

* Further discussion of this issue is found in the papers by Krutilla and Milliman in vol. 1 of this collection.

ment of a uniform minimum will remove the inequity which may exist, and if not, whether other, less general legislation can handle the problem. In respect to Government lending programs, administrators and legislators will want to know if the capital market is operating imperfectly and if so, if it is due to a lack of knowledge or restrictions on the flow of funds to certain areas, and whatever the imperfection, how it can best be corrected.

With respect to Federal aid to higher education, the decisionmaker will want to know the returns to the taxpayer from providing a college education to other people's children.²⁰ Analysts should attempt to find the benefits of a health program to the taxpayer rather than the lifetime income of the man whose health is affected.²¹

We can point to every large area of Government expenditure and ask what the returns to taxpayers are. Always, however, program evaluation must be approached by defining the market imperfection which calls for Government action, and identifying whether the action is consistent with the ethical rule. It is worth pointing out that not all market imperfections call for Federal Government action. A local water resource project in the arid West hardly calls for collective expenditure by taxpayers other than those who will directly benefit from the project. Even water and air pollution reduction is predominantly a local problem calling for local taxation and expenditure. To the extent that Floridians are unhappy about the pollution of Lake Erie and want it abated, and New Yorkers are concerned about the draining of the Everglades and its effect on wildlife and are willing to pay for its abatement, these problems can be construed as proper areas for Federal Government action. But the question of whether taxpayers in general are concerned is to be determined by the policymaker and if they are, analysis can be an aid in estimating the values people place on conserving the environment.

This last point highlights the fact that it is not only the decisionmakers in Government who can use policy analysis. Typically, taxpayers want "something" to be done about a "problem" but they are not always aware of what the problem is or what they want done. Consequently, the policymaker needs to explain what is in the interests of his constituents and what is not. How many people who advocate expenditures on job training or education for the poor know how much such expenditures will reduce crime, welfare costs, or poverty—the problems they really want to solve? The fact that education is an "efficient" investment is by itself hardly an adequate justification for Government expenditure.

Now, if "relevant" policy analysis is what I have asserted it to be, how can the PPB system be used to generate it? First, and most obvious, it is necessary that the system should see to it that relevant questions be asked. The system is supposed to start off by grouping programs by problems they are trying to solve, and then compare existing programs and new ones in terms of how well they solve them. But the argument presented in this paper has suggested that the problems to be solved must be defined in terms of market imperfections

²⁰ See N. Singer and P. Feldman, "Criteria for Public Investment in Higher Education," in *The Economics and Financing of Higher Education*, forthcoming committee print, Joint Economic Committee, U.S. Congress.

²¹ For a fine exposition of the differences in policy which are suggested by following these two approaches, see V. D. Taylor, "How Much is Good Health Worth." RAND Corporation P3945 (Draft) October 1968.

which should be repaired. If every program is to repair one or another type of imperfection, then even programs using widely divergent techniques to deal with a specific imperfection should be grouped together. This is not done at present. Programs are usually grouped by some obvious but misleading characteristics. In the Department of Agriculture, for example, lending programs for housing go in one category, while lending for agricultural production are in another, and loans to start nonfarm enterprises are in another. The primary justifications for these programs are lack of knowledge of profitable lending opportunities by local lending institutions, or restrictions on the flow of capital into agricultural areas. But because the programs are dispersed throughout the program structure the Secretary of Agriculture does not see that he is "solving" the same problem many times. The same type of suboptimization in the Defense Department at one time led the three services to base their individual budget proposals for strategic weapons on a perceived requirement to be able to destroy the whole target complex. Only when we stopped viewing the problem as being separate for the Army, the Navy, and the Air Force was the true capability and expense of our strategic force exposed.

The clear implication is that programs should be grouped by the market imperfections they are intended to repair. Such grouping would help both managers, who have to concentrate on efficient operation of the program, and budget decisionmakers, who have to determine how much of their financial resources to devote to the program. Thus, I would suggest that as a first step toward making the system work, all domestic Government departments except for service agencies like GSA²² should start off with a program structure of four categories which would be the four types of market imperfections discussed. While the program structure is only a classification of programs according to their major objectives, it is really the beginning of analysis; it imposes a very important discipline on the measurement efforts which follow. In identifying the objective of programs, the program structure discards spurious objectives and determines the direction in which the analysis will proceed. It indicates what standard should be used in measurement and thus tells both analyst and decisionmaker what is really at issue for the Government.

The discipline imposed by such a program structure would have other benefits as well. The emotional appeals for fund allocations which characterized the budget process before PPBS (and still do) would have to be accompanied by evidence that specific market imperfections existed, and that something other than bureaucratic exigencies were motivating the agency. High level administrators could trade off among programs on grounds of achievement of basic policy objectives and their policy decisions would have a real affect upon the direction of expenditures in their agencies. Questions could be asked at the highest level about specific objectives and real alternatives for achieving them, and the long recitations of all the "advantages and disadvantages" of any program could be avoided.

²² For internal agencies like GSA, benefits offered to the public are to be found purely in the increased efficiency of other Government agencies. Thus GSA's activities must be justified on the grounds that office space, supplies, transportation, computer services, etc. can be supplied to other Government agencies at lower cost than would be the case if they were procured by those agencies from private suppliers.

The first step in analysis is only a first step, and settling on the right program structure does not automatically assure that analysis will be successful or that decisions will be improved. But the first step is by all odds, the most important. As Northrup observed:

The most difficult portion of any inquiry is its initiation. One may have the most rigorous of methods during the later steps of the investigation, but if a false or superficial beginning has been made, rigor later on will never retrieve the situation. It is like a ship leaving port for a distant destination. A very slight erroneous deviation in taking one's bearings at the beginning may result in entirely missing one's mark at the end, regardless of the sturdiness of one's craft or the excellence of one's subsequent seamanship.

Again and again investigators have plunged into a subject matter, sending out questionnaires, gathering a tremendous amount of data, even performing experiments, only to come out at the end wondering what it all proves and realizing after years of industry and effort that the real difficulty has slipped through their fingers. Others, noting the success of a given scientific method in one field have carried this method hastily and uncritically into their own, only to end later on in a similar disillusionment. All such experiences are a sign that the initiation of inquiry has been glossed over too hastily, without any appreciation of its importance or its difficulty.²³

If PPB is ever to go beyond measuring how well Government does what it does, that first step in the right direction will have to be taken.

²³ F. S. C. Northrup, *The Logic of the Sciences and the Humanities* (Macmillan, 1947), p. 1.

TODAY'S PPBS: THE FATAL TRIUMPH OF FINANCIAL MANAGEMENT OVER ECONOMICS

BY SAMUEL M. GREENHOUSE*

Samuel M. Greenhouse is a member the Administrator's Advisory Council at the Veterans' Administration.

The role of the Federal Government has undergone a substantial change in the past several decades. Mr. Greenhouse points out that "the modern executive branch, unlike earlier models, includes a large-scale economic enterprise." The emergence of this enterprise facet of government (distinct from "governing per se"—for example, tax collection, foreign policy) calls for the development of procedures for the economic evaluation of Government enterprise activities. Mr. Greenhouse suggests that there has been a lag in inaugurating procedures for such policy evaluation. The Government's cost accounting systems are not geared—as they will have to become ultimately—to differentiate the economic enterprise budget from the budget covering the tasks of 'governing per se.'"

Now that the Government is heavily engaged in production and distribution, it can and should identify its final outputs so these may be properly evaluated, not in terms of stated programs and associated objectives, or by reference to input workloads, but by ascertaining the value (that is, benefit) of each output, as determined by that portion of the public which obtains and consumes it. Mr. Greenhouse contends that these criteria are not satisfied by the current PPB system, which is too concerned with budget and financial questions, and not directed toward answering the important economic questions underlying policy decisions. He sets forth a proposal for a PPB system which he feels would be more responsive to the needs of modern Government decisionmakers. He concludes that "the executive branch enterprise does not operate in the market, subject to the discipline of sales volumes, profit margins, and the price system in general. Consequently, the production and distribution of final outputs will have to be regulated through a PPBS equal to the task of serving in lieu of a market system."

Introduction

The purpose of this essay is to set forth, singly and in interrelated significance, four positions:

First, an economic form of planning-programing-budgeting system (PPBS) is urgently needed in place of the "financial management" type used by the executive branch for the past 3 years. The modern executive branch, unlike earlier models, includes a large-scale economic enterprise which must be brought under the discipline of a fitting, pertinent monitorship and decisional apparatus.

Second, the evolution of this major economic enterprise within the American governing organization has led to the unavoidable question: What is the value (that is, benefit) of the enterprise's individual offerings? The marginal utility theory of value, because it provides the

*The views expressed herein are solely those of the author, and should not be taken to reflect the official policies, views, or opinions of his employer (the Veterans' Administration), or of any other Federal organization.

Government with a conceptual basis upon which to answer this question, is essential to an economic PPBS. The contribution which marginal utility could make is being overlooked in today's PPBS.

Third, applying marginal utility properly would mean identifying and relating: (1) the Federal enterprise's final outputs; and (2) the outside-the-Government "customers" who receive and/or use them. The economic PPBS (unlike the current type) would depend upon, and would systematically employ this information.

Fourth, the economic PPBS would have, of necessity, a very different design than the present PPBS. A specific design including a standardized Government-wide program structure and substructure, plus other features, is recommended in the latter portion of this essay. The recommended design is calculated to meet key information requirements at both the presidential and departmental levels of the executive branch, and to satisfy important congressional interests.

BACKGROUND

PPBS was heralded—for months before its Government-wide adoption in August 1965 and briefly afterward—as a new, economic approach to Federal decisionmaking, rooted in the marginal utility theory of value.

But as implemented, PPBS has never appeared to include an economic approach of any description (let alone marginal utility), except possibly in the sense that budgeting has always concerned money and, with PPBS, continues to do so.

It is not that the economic approach failed, but that it wasn't tried. The Budget Bureau's successive PPBS guidelines did and still do include a rhetoric befitting an economic approach with a marginal utility slant. But the procedural rules (as distant from the non-operative rhetoric) have marched to a different drumbeat. These rules have combined to call forth, at best, modest advances in budgeting techniques, rather than the epochal innovations in the decision-making process which constituted the special and rare promise of an economic PPBS.

To urge adoption of an economic PPBS in place of the current, financial management PPBS, is not to downrate the perennial need for either improved budgeting or more widely and intensively performed budget analysis.

Indeed, the very fact that these problems are important suggests that the economic PPBS was doomed from the moment when it was given into the Budget Bureau's charge. Expecting this organization—already heavily burdened with problems endemic to its own mission of budgeting—to find a whole new perspective may have been unrealistic and less than fair.*

An unencumbered governmental organization might have greater freedom to conceptualize. Once the results of such conceptualization were refined into a clear-cut operational routine, the Budget Bureau could oversee the performance thereof by all agencies concerned.

The financial management type of PPBS doesn't fit the decisional imperatives of the modern executive branch (at either the presidential

*Further discussion of this issue is found in the paper by Schick in this volume.

or departmental level), nor meet the needs of the Congress for a basis upon which to appraise today's executive branch operation.¹

This is because the executive branch operation is greatly changed—dramatically different from the historic model. Executive and legislative overview and direction are hampered because the change is at once too recent and too sweeping for full comprehension or widely shared realization. The organization has grown, in modern times, to include a great economic enterprise in fact, in purpose, and in functions.

ENTERPRISE ROLE OF THE MODERN EXECUTIVE BRANCH

The modern executive branch—unlike its forerunners from 1789 until perhaps 1933—is the largest, corporate-style, multiproduct economic enterprise, public or private, ever known to man. It is a major producer in its own right, with a wide range of product lines; and it is a nationwide (even worldwide) distributor of its own and other producers' outputs. To cite some examples: the Tennessee Valley Authority produces electric power and also, with one or two other Federal agencies, distributes it; the executive agencies together are the largest publisher in the world;² the Federal Aviation Administration produces and transmits radio messages (and directives in other forms) to commercial and private pilots; packaged foodstuffs are distributed abroad by the Agency for International Development; even the State Department proper is a producer-distributor (of passports and visas); while the Defense Department sells and otherwise dispenses billions of dollars' worth of military aircraft and weaponry to foreign countries.³ The Defense Department's production-distribution effort also includes domestic construction and electric power production.

For good measure, the executive branch is also history's foremost banking and finance institution, whose outputs in the form of loans, grants, and subsidies are generated on a grand scale.

The customers for these Federal outputs (commodities, services, and financing) include many individual Americans, virtually every State and local governing body, and more than one-half of the world's nations.

The annual budget of this gargantuan (and still growing) producer-distributor-financier is very likely in the \$75 to \$100 billion range—already almost certainly more than 50 percent of the substantive Federal budget total, and destined to become an even higher share of the future *peacetime* budgets.⁴ This is a gross estimate, all that can be derived at this juncture because the economic enterprise's presence within the executive branch is still unmapped—still unrecognized for what it is. In consequence, the Government's cost accounting systems are not geared—as they will have to become ultimately—to differentiate the

¹ In the latter half of 1968, the Budget Bureau contracted with McKinsey & Co. for an "information system" and a "Government-wide program structure" for PPBS. It would appear, judging from this action, that the Budget Bureau itself has reservations about the usefulness of the existing PPBS.

² The printing and selling, for the most part, are done by a legislative organization (Government Printing Office), but manuscript preparation and publication decisions are made by the executive agencies concerned.

³ Jack Raymond, "Growing Threat of Our Military-Industrial Complex," *Harvard Business Review*, May-June 1968, p. 59.

⁴ The term "substantive budget" is intended to describe the Federal budget exclusive of purely monetary items such as interest on the national debt, etc.

economic enterprise budget from the budget covering the tasks of "governing per se" as found in the U.S. Constitution (e.g., collecting taxes, raising armies and navies, arranging treaties with other governments, and the like).

There is no intention here to slight the traditional governing role which, since 1933, has itself grown. But this growth has been relatively modest, and probably is explained by population expansion rather than any appreciable addition of governing functions.

The enterprise role, on the other hand, has undergone a quantum jump—changing radically (and probably forever) the executive branch's very nature and vector of thrust. This development has raised an issue which is new for government in America; namely, What is the value of the executive branch's output in its role as producer?

THE QUESTION OF VALUE

There is not now and probably never has been any serious question but that the *traditional* tasks of governing are valuable. Centuries of time, during which all governments everywhere have found it necessary to perform virtually identical governing tasks, can be considered a sufficient test. The values of the governing tasks which have survived over the years may be indirect and difficult to calculate (because more often than not they involve Federal "intake" rather than output—e.g., collecting taxes or gathering foreign affairs "intelligence"). But the tasks themselves can be and are taken for granted or are regarded as inherently valuable. Only cost is at issue for such tasks, because their performance is accepted as mandatory.

This is why work measurement systems (which the American Government has used for roughly a half century⁵) have satisfied so thoroughly and well as basic decisional and appraisal apparatus of the *preeconomic* executive branch. These systems assure the industriousness and performing skill of the bureaucratic work force—ignoring all questions as to the value of the bureaucratic results—by comparing work performances against preestablished standards of "efficiency" (i.e., quantity and quality of work units completed).

But now that the executive branch includes an economic enterprise, efficiency alone no longer suffices as a managerial guideline. The economic operation quite naturally demands decisional and control mechanisms attuned to its own operational logic. The new role, and the production-distribution functions which give it life, require monitoring and administration in terms of economic criteria—which is to say economic *value* criteria. The first real question is: What must be evaluated?

Once the executive branch is recognized as a producer, however, this question answers itself. There can be little doubt that for a producer, the final outputs of the production process are the appropriate focuses for evaluation.

The value of particular executive branch outputs can neither be taken for granted nor considered infinite, because (unlike the governing role and its associated tasks) an economic enterprise role for a government is not mandatory but *optional*. And even if, somehow, the role itself

⁵ Work measurement systems were introduced into the U.S. Government (naval shipyards) about 1910-11, by F. W. Taylor.

came to be regarded as mandatory, the value of any *single output* would still be open to question (otherwise the enterprise's growth would be undisciplined and self-perpetuating).

This is the nub of the case for PPBS. But it must be an economic PPBS, rooted in either marginal utility or some other approach to value ascertainment,⁶ else it is merely a minor variant of the entrenched work measurement technique. The financial management form of PPBS has shown itself, in operation, to be exactly that and nothing more—a set of arrangements for examining *cost only*, not the *value and cost relationship*. Numerous Federal employees appear to believe that they are applying marginal utility theory when, under the existing PPBS procedures, they analyze issues of cost. But marginal utility (as distinct from marginal productivity) is basically a value theory.

The question is: How can the values of Federal outputs be established? The marginal utility theory addresses itself to this problem. Used appropriately, the marginal utility approach would become a worthwhile tool for consumer-conscious regulation of production and distribution in a context, namely the Federal one, where neither the price mechanism, nor profit motivation, nor sales volume is available to serve as a production guide.

MARGINAL UTILITY THEORY

The marginal utility theory holds that value is not intrinsic; is not embodied in commodities as a result of labor's effort in producing them; and is neither determined by the producer himself nor even established on the producer's side of the economy. Instead, value is regarded as wholly subjective, determined by the customer—the consumer—of whatever marketable commodity or service one happens to consider.

Now, what is the significance of this consumer sovereignty, as regards the economic PPBS versus the PPBS now in being?

Since marginal utility theory addresses itself to the value of marketable commodities and services (and the Government produces and distributes just such items), a PPBS rooted in marginal utility would depend upon their identification.

Some readers, whilst agreeing in principle that the Government's final outputs ought to be identified, may assume that the approach wouldn't work where services are concerned. Even if this were true, the Government purveys fewer services than is commonly believed. The Government's locations are often too remote from the customers for services, as such, to be supplied. Many of the so-called services (generically described), are actually commodities.

A similar argument is that the Government's outputs cannot be identified because they are intangible. This mistaken impression appears to result from confusing the producer's *objective* with the produced *thing* (as was done in Par. 4b of Bulletin 66-3, October 12, 1965, the very first PPBS directive). The fact is that objectives are often intangible but outputs aren't—and the latter are what must be identified.

⁶ Accordingly, this paper proposes the application of PPBS to the executive branch's economic enterprise segment *only*, and *not* to the governing tasks segment.

There has been no real attempt, under existing PPBS, to identify the Government's outputs and determine their respective values. Instead, elaborate proxies have been used in place of outputs, such as: missions, objectives, programs (defined very comprehensively), activities, efforts, workloads, functions, output "indices," "measures of output," and "public goods."⁷ The trouble is that in themselves none of these proxies possess value or confer benefit, so their use obscures rather than clarifies the value question.

In such terms, determining "benefit" is beyond human skill, because the current PPBS cannot provide the basis upon which to answer the questions: Benefit of what? and Benefit to whom? Accordingly, the initial marginal utility focus on benefit has remained purely rhetorical, during the three years of PPBS life, while the actual focus has been on "effectiveness."

In these terms, cost accounting is popular, but cost understanding and cost explaining are impossible. This is to say that output proxies are just as unsatisfactory for cost ascertainment as for benefit determination. The act of charging expenditures to an amorphous entity such as a mission, objective, or program category (as the categories are now constituted) creates an unanswerable question, namely: Which of these expenditures is properly definable as a cost? In short, the Government's supposed cost accounting systems have become mainly vehicles for classifying expenditures rather than ascertaining costs.

All this indirection (use of proxies for output) is unnecessary and appears to result from regarding the executive branch as the consumer-type organization it used to be, rather than the largely producer-type organization it is now. Once the fact is grasped that the executive branch itself generates actual outputs, both directly and indirectly,⁸ the need for proxies disappears.⁹

Furthermore, the marginal utility theory explains value as resultant of a *customer* verdict. Accordingly, the government ought, whenever feasible, to consider determining the economic value of its production in terms of whether the out-of-government customers want it.

A passage from a recent magazine article by President Nixon (proposing to establish a commission on Government reorganization), appears to espouse just this course of action:

"The commission's broadly-based membership would include the best management talent, the best government talent, and also the best academic talent from many disciplines. And one of its charges would be to start from a new premise: to search out what

⁷ Public goods, as the term is commonly used by economists, do not usually refer to produced commodities or services, but more often to desirable governmental objectives. Thus, the "defense" objective is considered a public good. When the writer speaks, on the other hand, of federal outputs, he is speaking of commodities and services actually produced by the government and/or distributed thereby to outside-the-government recipients.

⁸ Parenthetically, there is little difference, from an economic standpoint, whether the government produces outputs directly or (using private production facilities for the purpose), indirectly. The government's intent in both cases is to distribute the resulting outputs to its out-of-government customers. Direct vs. Indirect production, therefore, is merely an entrepreneur's option, exercised in the interest of assumed advantages in costs or other conditions.

⁹ It should not be too surprising that the financial management PPBS guideline writers mistakenly regard the executive branch as a consumer. Identical obsolescence is reflected in the forty-odd year old Gross National Product (GNP), which has not yet been redesigned so as to reflect federal productivity.

the people *want* from government today, and then proceed to the question of how those wants can best be satisfied."¹⁰

By contrast, the PPBS-in-being makes no systematic attempt to even identify the Government's customers, let alone ask them whether they want the items being produced for and distributed to them.

Instead, the Government perpetually asks itself what the value of its own efforts is. This approach is the direct opposite of the marginal utility theory. The Government is the producer, so when it asks itself about the value of its own efforts, it is returning (in the *name* of marginal utility) to the earlier theories of value which the marginal utility theory contradicted.¹¹

If the marginal utility concept is valid, the Government can't successfully judge the values of its own efforts. But would the customers be any better judges? After all, Federal outputs seldom have price tags. In the absence of market prices by which to quantify, how would the customers themselves measure benefits?*

OPPORTUNITY COST APPLICATION

The measurement problem, although outside the boundaries of marginal utility, yields to a companion principle—opportunity cost.

Applied to relative prices, this principle suggests that the would-be consumer who chooses any one item foregoes (at least theoretically if not knowingly in every case) the opportunity to enjoy other items he might have selected instead. If this be true, it follows that the value of the item *preferred* is greater (to the consumer doing the selecting) than the value of the item(s) foregone in the act of choosing.

Couldn't the Government apply this principle by asking customers to rank particular Federal outputs as against non-Federal products with well known market prices?

For example, the executive branch—the producer of output "X" at a unit cost of \$5—could decide to ascertain the benefit ranking of output X. A sample of the Government's customers (i.e., beneficiaries) for output X might be approached.¹²

The presumed beneficiaries might be asked (without being told the dollar figures shown below) to rank the following items in preference order:

<i>Item</i>	<i>Market price</i>
Item A -----	\$7.00
Item B -----	3.00
Output X -----	

¹⁰ "As Chief Executive, Here Are Management Changes I'd Make," *Administrative Management*, October 1968, p. 23.

¹¹ Notwithstanding this, the marginal "utility" discussion among PPBS practitioners has tended either to concentrate on marginal "productivity," (without the redeeming realization of what such a discussion as applied to the executive branch really signifies), or to focus upon the "incremental aspect," (i.e., the presumed consumer tendency to place progressively lower values upon successive increments of any commodity or service available) instead of the consumer sovereignty aspect discussed above.

¹² A modicum of care would have to be taken to assure that the sample contained beneficiaries only. In public transactions (and private ones) the recipient of a commodity or service is not always the beneficiary thereof.

*Further discussion of this issue is found in the papers by Freeman and Margolis in vol. 1 of this collection.

Were the Government's output considered preferable, it would be worth demonstrably more than $7/5$ (benefit/cost ratio). But if both items A and B ranked higher, suggesting an item X benefit/cost ratio of less than $3/5$, consideration might be given to ending or reducing output X's production, possibly in favor of another output.

To solicit quantified ratings for every federal output, or from every customer of the executive branch, would be neither practical nor necessary. Indeed, the Government stands to profit so greatly from gaining an identification of its final outputs, their respective production-distribution "networks," their costs, and some indication of customer reaction (however imprecise), that concern at this stage about a potential sophistication like quantifying benefits may be lily-gilding. The example may demonstrate, however, that when the executive branch decides to validate its operations through market research, suitable means may be at hand.¹³

As has been implied earlier, the traditional orientation of the Government's decisional processes has hardly been touched by the financial management form of PPBS. The orientation continues such that *all* issues—economic as well as noneconomic—come before the responsible decisionmakers (Cabinet and agency heads, Presidents, and Congressmen alike) posed in noneconomic terms and calling for the decision in virtually every instance to be reached on noneconomic grounds.

Priority belongs, therefore, to the question of how PPBS must be reshaped in order that the economic issues with which the modern executive branch is characteristically confronted may reach the Government's decisionmakers in relevant form.

RECOMMENDED PPBS DESIGN

Remaking PPBS will require development and application of a standardized, Government-wide program structure.

Output-oriented program categories would be established, as noted in figure 1. No other categorization pattern is appropriate or useful for an economic enterprise such as has evolved in the executive branch.¹⁴ Identifying an enterprise's outputs is essential for monitoring and decisional control. One must know what the final outputs of an organization (public no less than private) are before one can know anything specific about its production and/distribution processes, customers or costs. In other words, the final outputs of an enterprise may well comprise its lowest common denominators.

As an example, consider the relationship between the Bonneville Power Administration (Department of Interior) and the Corps of Engineers (U.S. Army). The former markets electric power and energy derived from "generating projects" constructed and operated by the latter (according to p. 266 of the U.S. Government Organization Manual, 1968-69). In effect, the two organizations are inter-related,

¹³ The list of possible approaches is not exhausted by reference to the opportunity cost principle.

¹⁴ The output-oriented categories of the proposed program structure easily could be summarized in objective-oriented groupings or listed under objective-oriented headings. This is emphasized because the Budget Bureau appears to want such groupings (judging from the program structure of the present PPBS).

* Further discussion of this issue is found in the papers by Wildavsky in this volume, and Carlson in vol. 2 of this collection.

FIGURE 1

PROPOSED PROGRAM STRUCTURE FOR PPBS

- I. Program Categories: A "program" is defined, for purposes of this proposal, as the entire effort to produce an output (i.e., an end-commodity or end-service) and to distribute that output to a group of American people outside the federal government, to a state or local governing body, or to another nation. Thus, the program categories would be: distributed output X_1 , distributed output X_2 ...distributed output X_n .
- II. Program Sub-Categories: Each discrete distributed output would be sub-categorized according to the input costs incurred by producing and/or distributing the output. Thus, output X_1 ... X_n each would be sub-categorized as follows:
- A. Production Costs
 - B. Distribution Costs
 - C. Ancillary Costs (e.g., administration related to the program)
 - D. Developmental Costs (if any)*
 - E. Capital Costs Attributable to the Given Output**

These five sub-categories together are designed to capture total costs, because any benefit cost-ratio which does not include all costs (pertinent to the item whose benefits and costs are supposedly being compared) is invalid.

- III. Program Elements: Each of the five sub-categories (II, A-E above) would be further "exploded" as follows:
- Cost of Agency X's own inputs
 - Cost of inputs budgeted by Agency X but obtained from other agencies
 - Cost of inputs budgeted by Agency X but obtained from private firms
 - Cost of inputs both budgeted by, and obtained from, other agencies

*Note that no provision is made for costs of "research." Such costs are not attributable to programs for the production and distribution of known outputs, but should instead be regarded as agency "overhead." However, an agency would establish a new, additional program at such time as the research effort resulted in the creation of an end-product which the agency began to produce and distribute.

**This program sub-category would include costs of program-related raw materials and producer goods, plus prorated costs of such agency-wide capital investments as office buildings or production facilities.

operationally and budgetarily. Indeed, where power and energy are concerned, the former agency is operationally and budgetarily *dependent* upon the latter, because as production increases (or decreases) the operational burdens and expense of marketing must grow (or decline). But only after having identified the common output (electric power) can one realize that there is any inter-relationship at all; and only by focusing upon the two agencies, respective production and distribution activities, individually and in depth, may one even begin to take the operational and budgetary measure of the inter-relationship. So it is with the government's other producing and/or distributing organizations as well. Therefore, final outputs are the only denominators upon which a workable and informative government-wide program structure can be predicated.

Figure 1 proposes also that all program subcategories be *cost-oriented*. In other words, each output would be subcategorized to reflect the cost of its production, distribution, ancillary endeavors, developmental effort, and capital requirements. Differentiating the costs in this manner offers the possibility to examine the efficiency with which each output is produced or distributed, and the validity of the overhead and capital expenditures for that output. Presumably costs could be lowered by increasing or decreasing production, by changing the methodology of production or distribution, or by changing the input mix—thereby improving benefit/cost ratios. But if costs are not distinguished in the detail recommended here, these avenues of improvement cannot open up.

Finally, figure 1 includes provision for subdividing each of the above cost-groupings (at the program-element level), so as to differentiate the costs borne by the producing agency itself from those incurred by other agencies participating, in some intermediate way, in the production or distribution of each final output. In this manner, the full range of agency-to-agency inter-relationships and inter-dependencies, of which the Bonneville case, above, is illustrative, may come to light.

Were the recommended program structure applied, the President and Congress would gain the means of regulating the Federal production apparatus and of optimizing the benefit/cost margin across the full range of Federal outputs. Inquiries such as the following would become answerable:

- How many agencies (if any) are producing or distributing identical final outputs?
- Which final outputs properly fit under national goals?¹⁵ (Or under agency objectives?) Should alternative outputs be considered for some of those now in production, on grounds that the alternative outputs might be better suited to achieve a given goal (or objective); or might be as well suited while cheaper?
- How many agencies are producing varied outputs in an attempt to achieve the same national goal, or comparable agency-level objectives? Is the across-the-Government range of outputs too broad? Too narrow? Duplicative?

¹⁵ Currently, a governmental list of national goals does not exist. Once the Government's end-outputs are identified, however, the knowledge will provide valuable clues for the generation of just such a list.

• How do production costs compare (as a proportion of total costs) in one agency versus another? How about a comparable look at distribution costs? Can changes (alternatives) in the methodology of production or distribution in a given agency bring about lower input costs or greater output volume?

• How do the various outputs of the Government compare in terms of apparent customer need? Or in terms of expense to produce and distribute? How valuable does each output appear to be, as adjudged by its respective beneficiaries?

The program structure and substructure reflected in figure 1 should apply in each Federal agency (or segment thereof) which regularly: (1) produces one or more final outputs, or (2) distributes one or more final outputs to customers outside the Government, or (3) produces one or more intermediate products (or other inputs) for a sister agency; or (4) ships one or more intermediate products (or other inputs) to a sister agency. Agencies, or segments thereof, which don't do these things regularly would be exempt from PPBS.

There is a final word to be said about program structure, with particular reference to its information-bearing properties. Implicit in the current PPBS is the assumption that both a program structure and an information system are required. One must assert, however, that the program structure *is* the information system of PPBS (leaving aside computation procedures), else it serves no real function in PPBS at all. In other words, a program structure's only significant function is to provide basic, essential information. If an information system for PPBS is wanting, it is because a program structure pertinent to this purpose is not in use. If both an information system and a program structure are suffered to coexist, one of them is unnecessary or neither is any good.

The recommended program structure, however, corresponds with the informational demands of the benefit/cost approach. The output categories will open the path to benefit data while the subcategories will provide cost information.

SUMMARY

The economic PPBS would both require and make possible a greatly different perspective than that which characterizes the system currently in force. The economic PPBS would reflect (1) a new program structure and substructure, with a format uniformly applicable to all executive departments and establishments concerned; (2) a changed concept as to what constitutes a Federal program (per item 1 of fig. 1); and (3) another viewpoint than the currently prevalent one regarding "alternatives."¹⁶

These developments are needed because the operational-level executive branch has grown, in our time, to include a major economic enter-

¹⁶ In the proposed, economic PPBS, agency-level alternatives would be of two main types: (1) alternative *outputs*, considered for their respective merits in satisfying a given objective; and (2) alternative *methods* of producing or distributing a particular output, considered for their respective costs or other advantages. The writer originally recommended this approach to the problem of alternatives in 1966. See "The PPBS: Rationale, Language, and Idea-Relationships," *Public Administration Review*, December 1966, p. 275. The suggestion to predicate PPBS upon final outputs was put forth originally by the writer in "A 'Distributed Output' Concept for PPBS," *Personnel Administration*, July-August 1967.

prise as well as the traditional governing duty.¹⁷ Unfortunately, this fact is not generally recognized, nor are its implications widely comprehended as yet.

In a very real sense, the presidency and legislature, respectively, now embody "chief corporate officer" and "board of directors" responsibilities in addition to their traditional, better recognized ones. But a knowing and sensitive execution of the latter day responsibilities is not possible at this stage, because the dimensions and demarcation lines of the Government's economic enterprise are not now clearly discernible. Although discrete in purpose and results, the enterprise and governing roles are too thoroughly intermingled conceptually and budgetarily.

One would surely incline toward pessimism concerning the course and prospects of any private company whose chief corporate officer and board of directors were unaware of (and lacked any established procedural mechanism for learning about) all of the end items the firm was in the business of producing and/or distributing, and the customer reactions thereto. Yet the Government's equivalent officials (i.e., the President and the Congress) occupy just such a position vis-a-vis the executive branch's gigantic "company." To grant that this is understandable because the Government's economic enterprise has emerged, for the most part, only during the last 35 years, is not to say that a continuation can be tolerated.

For those aspects of the Government which fall into the economic enterprise category, a PPBS rooted in marginal utility and employing benefit-cost techniques is not only possible and appropriate, but has become mandatory.

For those aspects which fall into the governing tasks per se category, PPBS is inappropriate because benefit-cost analysis as an application of marginal utility is impossible. The values of governing tasks are not customer established. Fortunately, these values may safely be taken for granted.

The *enterprise's* values, however, must be proven, by repeated customer assessments and approvals of the Government's final outputs. But the executive branch enterprise does not operate in the market, subject to the discipline of sales volumes, profit margins, and the price system in general. Consequently, the production and distribution of final outputs will have to be regulated through a PPBS equal to the task of serving in lieu of a market system. In other words, PPBS will have to provide the means of monitoring the Federal enterprise by determining (in as many instances as may be feasible) :

- The value (that is, benefit as construed by the customers) of each final output.
- The cost, to the Government, of producing and distributing it.
- The "profit margin" (that is, benefit minus cost) attributable to it.

Value determinations will be made possible by applying a combination of the marginal utility theory of value and the opportunity cost

¹⁷ The origins of the enterprise may be said to go back as far as the Post Office Department's origination in 1789. A further spurt may be identified in the establishment of the regulatory function circa 1887 (when the Interstate Commerce Commission—the first regulatory agency—was formed). But the transforming quantum jump unquestionably began with creation of the Tennessee Valley Authority and other producers, in 1933, and has accelerated in the post-World War II era.

principle to the Government's final outputs. Related input-cost determinations will become obtainable once the production lines and distribution networks pertinent to the various outputs become visible (at least in their broad dimensions). The same visibility will serve to differentiate the enterprise from the governing portion.

Ultimately, preparation of the annual Federal budget in two sections—one for the enterprise and the other for governing—would appear to be inescapable and desirable.

Vital interests of the American people, the President, and the Congress are at stake in the development and purposeful application of proper decisional controls to the executive branch's economic enterprise. An economic PPBS, as recommended, is essential and would be workable.

SECTION C

PPB IN TWO PROMINENT AGENCIES : SOME LESSONS FROM EXPERIENCE

THE PLANNING, PROGRAMING, AND BUDGETING SYSTEM IN THE DEPARTMENT OF DEFENSE: SOME LESSONS FROM EXPERIENCE

BY ALAIN ENTHOVEN

Alain C. Enthoven is vice president of Litton Industries. Until January 1969, he was Assistant Secretary of Defense for Systems Analysis.

Past experience in implementing planning-programing-budgeting techniques is important in developing improved decisionmaking procedures in the Federal Government. Because these techniques have been formally practiced in the Department of Defense over the longest period of time, the experience of PPBS there should contain lessons for the future. Here, Dr. Enthoven discusses the application of analysis to policy decisions in the Department of Defense and draws some practical conclusions.

In describing the PPB process in the Department of Defense, Dr. Enthoven emphasizes several characteristics which have made the system successful in that agency. Among them are the establishment of explicit criteria, the tying together of outputs and costs, the open consideration of alternatives based upon the best information available, and the existence of a plan in which future costs of current decisions are clearly laid out. The existence of a system with these characteristics has enabled the Secretary of Defense and the President to "choose a defense budget in full awareness of its implications for our military posture and in light of information on whether extra spending would bring military benefits which justify the sacrifice of the other programs which are also competing for the budget dollar."

In relating some practical lessons from experience, Dr. Enthoven emphasizes the need for analysis to start by first looking at the overall context "McNamara's first law of analysis", the need for the limited supply of analytical effort to be concentrated on simple and basic analyses of questions involving major decisions and expenditures, and the importance of an independent research program existing in the functional area to which analysis is to be applied. In describing the relevance of policy analysis to decisionmaking in the Department of Defense, Dr. Enthoven makes reference to a number of major defense decisions including those related to the Polaris program, the anti-ballistic missile program, and the allocation of NATO strength in Europe.

Introduction

These thoughts are based on 8 years of experience developing, installing, and using the planning, programing, and budgeting system (PPBS) in the Department of Defense.* First, I will discuss what I believe are the essentials of PPBS as practiced in the Department of Defense. Second, I will describe some of the lessons we learned from our experience with PPBS in DOD.

I. WHAT IS PPBS IN DOD?

The foundation of PPBS is decisionmaking based on explicit criteria of the public interest as opposed to decisionmaking by compromise among various institutional, parochial, or other vested interests.

* Further discussion of this issue is found in the paper by Enthoven & Smith in this volume.

Thus, PPBS starts with a search for clear statements of the openly defensible public purposes each program is meant to serve, ways of measuring how these purposes can be achieved, and criteria for judging alternatives.

Our experience has been that analyses in most areas are very crude at first, and the criteria originally selected to evaluate alternatives do not seem very satisfying. The initial crudeness of the criteria may cause some critics of PPBS to reject analysis altogether. But one has to start somewhere, and the value of even a rough statement of criteria is that it generates a debate on better ways of defining the public interest. Armed with a crude analysis, the Secretary of Defense can say "if you don't accept these criteria, please come up with better ones."

The value of beginning with even crude criteria has been further impressed on me as a result of my work as a member of the board of directors of Georgetown University. A universitywide task force on PPBS is developing data on the cost per course, the cost per student credit hour produced, and similar information. These criteria are admittedly rough, and they do not measure the value of the courses, but they are much better than nothing. They serve as a point of departure for a debate about what the criteria ought to be in allocating university funds. I fully expect that as time goes by we will develop more refined criteria, but it takes a process of constructive debate to insure that this happens.

The second element of PPBS is the tying together of needs or output and cost. In the Defense Department this means identifying the costs of accomplishing major military missions such as strategic retaliatory forces, continental air and missile defense forces, airlift and sealift forces, and antisubmarine warfare forces rather than merely identifying costs by object classes of expenditure, such as manpower, operations, research and development and the like. In a university, PPBS means tying curriculum and courses to the budgets that support them.

Some critics resent the intrusion of cost data into the choice of military forces. One still frequently hears charges of "overemphasis on cost." I have also found that some professors resent the injection of cost data into the design of curriculum or the determination of faculty requirements. In the Defense Department and in universities, the plain fact is that total resources are inevitably limited by other needs in our society, so that the way to get the most effective total program is to put each dollar where it will add the most to total effectiveness. If cost is ignored in program design, the result will be a less effective total program. Those who argue most vocally against the introduction of cost considerations into the choice of weapon systems and forces are usually the proponents of programs that yield low effectiveness per dollar.

The third basic element of PPBS is the explicit consideration of alternatives, rather than a single staff solution, at the top decision level. By alternative I mean a balanced, feasible solution to the problem, not a strawman chosen to make an alternative preferred by the staff look better by comparison.

Some might say that the Secretary of Defense has always considered alternatives. For example, because the Joint Chiefs of Staff regularly recommend forces costing roughly 25 to 35 percent more than the budget the President believes the Nation should provide, there is implicitly a set of alternatives which includes both the Joint

Chiefs' force levels and implied budget and the administration budget and force plan. Moreover, the Comptroller's staff and the Bureau of the Budget have, at times, argued for financial ceilings or target budget totals without explicitly taking into account the strategic and military implications of staying within these limits. The idea of PPBS is to insure that the Secretary of Defense can consider alternatives in which costs, forces and strategies have been considered together. Thus, the Secretary and the President can choose a defense budget in full awareness of its implications for our military posture and in light of information on whether extra spending would bring military benefits which justify the sacrifice of the other public programs which are also competing for the budget dollar.

The fourth element of PPBS is the use of analysis as the servant of judgment. The purpose of analysis as we used it in the Department of Defense is not primarily to determine the "best" solution, given a certain set of assumptions. Generally speaking, there is no "best" solution to complex matters of policy choice and program formulation. Rather, the purpose of analysis is to sort out which assumptions are important to the decision, why they are important, and how they affect the outcome, so that judgment can be focused on the really crucial issues. The purpose of analysis is to illuminate and inform judgment, not to replace it. This is the opposite of the view that one gets from reading much of the formal literature on analytical methods, which seems to suggest that making the assumptions and collecting the data are uninteresting preliminaries and that the action really starts with calculating the "optimum" solution given the assumptions and data.

The fifth important idea of PPBS, at least as we have practiced it in the Pentagon, is that analysis should be open and explicit. That is, each analysis should be spelled out explicitly and clearly and made available to all interested parties so that they can see what assumptions were used and so they can retrace the steps leading to the conclusions. Open and explicit analysis is our best protection against persistence in error and reaching conclusions on the basis of hidden assumptions. It helps to build confidence in the results. All calculations, assumptions, empirical data, and judgments should be described in the analysis in such a way that they can be subjected to checking, testing, criticism, debate, discussion, and possible refutation. And the analyses *should* be tested, checked and debated by all interested parties. Analyses should not be believed simply because they are analyses.

The sixth basic idea is that of a forward force and financial plan, that is, a plan projected into the future as far as the clearly foreseeable implications of current decisions. The forward plan is not meant to be a blueprint for the future, or a set of goals that must be achieved. Rather, the plan is a projection of the implications of past decisions, a set of official planning assumptions—the point of departure for a continuing search for improvements. The plan forces one to look into the future to the time when today's decisions will have their most important effects and to judge programs versus needs in the light of their consequences over time. If the decisionmaker insists on seeing costs over a period of years, proponents of new programs find it harder to conceal the future cost implications of decisions made today,

and thereby drive the "thin edge of the budget wedge" into the program.

II. SOME PRACTICAL LESSONS FROM EXPERIENCE

Over the years we have developed a number of practical rules for how to go about analyzing and planning the Defense program. I think many of these rules would have useful application in other areas.

The first rule is what I like to call "McNamara's First Law of Analysis," that is "always start by looking at the grand totals." Whatever problem you are studying, back off and look at the overall context. Don't start with a small piece and work up—look at the total first and then break it down into its constituent parts. Thus, if cost is the issue, look at total system cost over the useful life of the system, not just this year's procurement costs. If you are analyzing a particular strategic offensive weapon system, start by looking at the total strategic offensive forces.

I can remember a Navy briefing to the Secretary of Defense on the Polaris program in 1961. In a very orderly way it laid out the targets to be attacked, the probabilities of destroying each of the various targets, the number of missiles on station and the number of submarines, the ratio of total submarines to submarines on station, and therefore, the total force required—a very fine job on a small piece of the problem. The trouble with the briefing was that throughout the whole analysis of requirements for Polaris, there was not one mention of our bomber force or our intercontinental-ballistic-missile force. But one can't make sense out of how many Polaris submarines we ought to have without looking at our total strategic offensive forces.

One of the striking things about McNamara's first law of analysis is how few people understand and act upon it, and how many people behave on the opposite principle. One of the main reasons that overall program decisionmaking in the Pentagon is so controversial is that so many people are concerned exclusively with a small piece of the total. Not only do they not see the total context in which decisions must be made, they tend to be skeptical that broad questions can ever really be understood and answered.

Second, for years I have taught my staff "It is better to be roughly right than exactly wrong." One must resist the temptation to concentrate on pinpoint accuracy on a part of the problem rather than approximate accuracy on the total problem. The ability to recognize, make judgments about, and be comfortable with roughly right information and analysis is a most valuable but scarce executive talent. It is the opposite of the habit of suspending judgment until measurements are precise and until "all the facts are in." In most important policy issues, all the facts will never be in, and in the meantime decisions have to be made with the best information available.

Third, it is important to recognize that policy analysis is not a search for "the best solution." Generally speaking, there isn't a best solution. There are only good solutions and bad solutions. The objective of good analysis ought to be to avoid disaster and gross waste. For example, in evaluating the decision to deploy a full-scale, anti-ballistic-missile (ABM) defense, the important thing is not to design a mix

of ABM's and air defense that is optimum for some single set of assumptions. Rather, the important things are identifying facts like (1) a full-scale ABM would be ineffective in saving our cities if the Soviets were to react to our deployment by deploying penetration aids, multiple warheads, and more forces of their own, and (2) the ABM would be ineffective in saving lives after a full Soviet attack if we were to deploy it without a large scale civil defense program.

Or, to take another example, in evaluating alternative levels of strategic offensive forces, we plotted curves of the number of Soviet people killed or U.S. lives saved versus number of U.S. offensive missiles. As one would expect, the curve rises steeply at first and gradually flattens out as we have to attack less and less remunerative targets or reattack targets probably already destroyed. There is no single best point at which to cut it off. A judgment must be made. What analysis can do and has done is to help the Secretary of Defense avoid committing many billions of dollars for additional weapons whose effect would be, at best, to raise the damage probability a few percent on the assumption that the Soviets do not react to our deployment, and perhaps to achieve virtually nothing if the Soviets do react. Thus, these curves did not tell us what is the best answer, but they did identify a lot of bad answers.

Next is the question of priority of analytical effort. Almost inevitably any analytical staff is going to be small relative to the number of problems to be addressed. Priorities must be set by the head of the agency and by the head of the analytical staff. He should start with the most important problems, where importance is measured either in effectiveness or cost. It usually would not make sense to devote many man-hours to the refinement of an analysis that would save a few million dollars when there are multibillion-dollar programs around that have not been analyzed. But some programs have implications for overall defense effectiveness far out of proportion to their cost.

In considering the order of priority of effort, it is valuable to direct effort to those places where it will be the most rewarding. Thus, one should consider the difficulty of the problems being attacked, as well as the payoff from solving them. In making this judgment, I suggest that there is merit to attacking the simplest problems first and attacking the more difficult ones after solving the easier ones.

For example, we are now spending about a fifth of the defense budget on tactical air forces. For years we have struggled with the question, "How many tactical air forces should the United States have?" In the early 1960's, we spent a lot of time and effort approaching it the way a classical economist or operations analyst might approach it. That is, we recognized that the main purpose of tactical air forces is to augment the effectiveness of our land forces by interdicting the movement of opposing land forces, by providing close fire support for our land forces, and by keeping the enemy aircraft from attacking our land forces. So, for each of these missions, we hypothesized that there would be a "tradeoff curve" describing the land and air forces yielding the same effectiveness. The trouble was that it simply did not prove possible to get any very good data on what the actual position and shape of these curves might be, and it became clear that the position and shape of the curves would vary a great deal with the particular local circumstances.

After blunting our lance for several years on attempts to solve these very difficult problems, we noticed that the actual decisions were being based on much simpler reasoning. So we switched over to the development of some simple indexes of the combat effectiveness of our tactical air forces. We noticed that the services were basing many of their arguments for procurement of more aircraft on comparisons of our inventory in 1961 with our inventory, say, in 1966 or 1967. We recognized that inventory was a very poor indicator of total force effectiveness. So we developed a payload index which measured the number of tons of bombs that the force could deliver over a representative combat sortie, and we noticed that the payload of our total force had doubled between 1961 and 1967. We have been trying to develop a target destruction index for a representative array of targets, but it has proved to be more difficult to get agreement on this more complex output measure.

Right away the use of the payload index was met with the argument "payload is not the same as force effectiveness." But the Secretary of Defense was able to reply, "Then I suggest you come up with a better index." This experience showed that it was more productive to take a simple step in the direction of developing criteria of effectiveness than it was to try to develop a sophisticated solution to the whole problem. I wish we had taken the simple steps earlier.

Fifth is an observation on simple and complex methods of analysis. The complex mathematical and computerized methods of analysis certainly have their place and have proved to be very useful in such problems as the decision on the antiballistic missile, and the analysis of our strategic mobility posture. Nevertheless, it is impressive how much can be done with the simplest tools of analysis. Indeed, I believe that most of the really important contributions made by the Systems Analysis Office in the Defense Department were based on the simplest tools of analysis. Let me give an example:

Years ago we were told that the NATO forces in the center region of Europe, numbering somewhere between 20 and 30 divisions, were hopelessly outnumbered by those of the Warsaw Pact, numbering somewhere between 160 and 175 divisions. Moreover, none of the NATO governments apparently had the manpower or financial resources required to remedy that huge imbalance. Indeed, the belief that NATO was hopelessly outnumbered created severe pressures to cut the forces that were being provided. It was almost impossible to develop a sensible strategy and force plan in the face of such a situation.

Put simply, the way we resolved the problem was to count soldiers, equipment, weapons, and logistics, instead of divisions. This cannot tell us who will actually win a war, but it can tell us how we stand on some of those factors which contribute heavily to winning wars. By using that method we now know that NATO not only has as many soldiers as the Warsaw Pact in the center region, but more in the total alliance (not counting those added by the United States for the war in Vietnam).

At one point, this debate centered on the development of "equivalent effectiveness" ratios for a United States and a Soviet division. Some terribly complex methods—war games, simulations, and the like—were produced in an attempt to make this assessment. But none of these studies was convincing because of the great differences in strength,

equipment, and training between United States and Soviet divisions, and the complex problems of attempting to develop satisfactory measures of the effectiveness of division forces. Instead, we asked the Army to estimate what it would cost to buy a complete Soviet division force if its equipment were made in American factories and its soldiers were paid American wages and supported at the American standard of living. We found that we could buy three Soviet-type division forces for the price of one of ours.

Now, of course, cost does not equal effectiveness; but additional cost plus good judgment ought to yield additional effectiveness. That is, if one of our division forces costs us as much to buy as it would to buy three division forces, if we organized as the Soviets do, then our division force ought to be at least as effective as three of theirs, or else we should redesign our forces along Soviet lines. A cost analysis is a pretty simple analytical tool, but some of the most important contributions that the Systems Analysis Office has made have been based on such simple tools.

My point is that the complex methods are useful in their place. It would be wrong not to use them on something like the worldwide strategic mobility problem. It would be equally wrong, however, to try to use them on some problems where they do not work, especially on problems where their use would require data that we do not have or cannot obtain.

In any case, the emphasis must be on the definition and solution of problems by whatever tools are well suited to them rather than on the application of analytical tools for its own sake.

Sixth, I believe that an important factor in the successful development of a PPB system for any program or agency is the existence of an independent research program that can take the long-term view, that can do work the eventual rewards of which may be great but the immediate payoffs of which may look small. In the Defense Department in 1961, we had the benefit of a decade of research by a very talented group of researchers at the RAND Corporation. At RAND, they were able to ask and answer questions that would have been too fundamental, too far reaching, and would have taken too long to answer for busy staffs in the Pentagon. I think that every agency trying to develop its own PPB system needs to have the benefit of an independent, continuing research program.

These experiences point up the importance of adversary proceedings as a part of the PPB system. Debates between the Systems Analysis Office and the services have served to clarify and refine criteria, to improve the accuracy and quality of the data, to identify and highlight otherwise hidden but important assumptions, and to catch mistakes. The Secretary of Defense can have much more confidence in his calculations of the effectiveness of the antiballistic missile defense system if the adversaries in a debate have agreed to the validity of a given set of calculations. Also, the adversary proceedings stimulate analytical progress.

Finally, I believe that an absolutely necessary condition for the successful development of a PPB system is that the head of the agency want it, understand it, be prepared to use it, and act upon it. The analysts must be able to know that their analyses are either acted upon or rejected for cause; the analytical and planning process must

really interact with the decision process or else it will waste away. Good systems analysts were willing to work for Secretary McNamara because they knew that when they did good work—work that stood up well in debate—it would be acted upon and would influence decisions. Analysis was not simply window dressing. If the head of the agency does not understand and want a PPB system, it cannot be forced upon him. Thus, I think that we have a difficult job of public education ahead of us to get men chosen to head Government agencies to understand what are the possibilities of a PPB system so that they will want to have one and will develop it into a practical and effective tool of policy decisionmaking.

THE PLANNING, PROGRAMING, AND BUDGETING SYSTEM IN THE DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE: SOME LESSONS FROM EXPERIENCE

BY ALICE M. RIVLIN*

Alice M. Rivlin is Senior Fellow at The Brookings Institution. Until March 1969, she was Assistant Secretary for Planning and Evaluation, Department of Health, Education, and Welfare.

The PPB System is an experimental system and is in its infancy. As with all experimental endeavors, improvements depend upon the accurate appraisal and evaluation of experience to date. Dr. Rivlin here discusses the implementation of the planning-programing-budgeting system in the Department of Health, Education, and Welfare and describes "what was accomplished, what the difficulties were, and what could be done better or differently in the future."

Dr. Rivlin argues that systematic analysis and program evaluation are important components of an effective agency and efficient programs. She describes the evolution of systematic analysis in HEW, describing the program budget and information system, the quantitative evaluation of program accomplishment, the analysis of alternative policy actions, and the development of a 5-year plan in the context of the planning cycle. Dr. Rivlin evaluates each of these components of the decision process in HEW and offers her judgment on those elements which contributed most to the development of an effective decision process. She argues that analysis is able to reduce the uncertainty surrounding domestic civilian decisions, even more than it is able to reduce the uncertainty in the defense area, but that evaluating the differential impact of decisions on people is "far more . . . troublesome in the domestic than in the foreign area." Dr. Rivlin concludes by asserting that "a Secretary of Health, Education, and Welfare who wants to do a good job [cannot] . . . get along without planning ahead, evaluating the effectiveness of programs, analyzing alternatives of programs, and making decisions in an orderly way in the light of maximum information. It does not matter what he chooses to call it, but he badly needs the basic tools of PPBS."

Introduction

I am happy to take this opportunity to look back on an operation in which I have been, until recently, deeply immersed. I have just left the Department of Health, Education, and Welfare after 3 years of working to implement the planning, programing, budgeting system. This is a good chance to set down briefly my own thoughts on what was accomplished, what the difficulties were, and what could be done better or differently in the future.

To implement the PPB system, Secretary John W. Gardner established a new office under an assistant secretary for program coordination (later and more aptly called planning and evaluation). I suspect he would have done this even without the impetus of the President's directive on PPBS. A new Secretary trying to understand and manage the vast, sprawling Department of Health, Education, and Welfare clearly needed a staff of his own to analyze where the Department's resources were going, what was being accomplished, and how the job could be done better.

*The views expressed are the author's own, and do not purport to be those of The Brookings Institution or the Department of Health, Education, and Welfare.

We conceived of our mission as that of helping the Secretary make better—or at least more informed—decisions about the allocation of resources among the many programs and possible programs of the Department. These decisions would be reflected primarily in the Department's budget and legislative program.

We proceeded on six assumptions:

1. Decisions will be better if you know what you are trying to do—if objectives are stated and resources devoted to the accomplishment of a particular objective are grouped together.

2. Decisions will be better if information is available on how resources are presently being used—by major objectives, ways in which objectives are being carried out, types of people being served, and so forth.

3. Decisions will be better if the effectiveness of present programs is evaluated.

4. Decisions will be better if alternative ways of accomplishing objectives are considered and analyzed.

5. It makes sense to plan ahead—to decide first what the Department should be doing several years in the future, and then what immediate legislative and budgetary changes are needed to move in the desired direction.

6. It is good to be systematic about decisionmaking—to follow an explicit procedure for reviewing long-range plans periodically in the light of new information, evaluation and analysis, and translating changes of plans into budgetary and legislative consequences.

We worked on all six of these premises at once. What follows is a brief attempt to describe what we did and what we learned from the experience.

PROGRAM BUDGET AND INFORMATION SYSTEM

The Secretary of HEW now has some new tools which he did not have 3 years ago. He has a program budget and information system which enable him to get a better grasp of what HEW does and where the money goes than he could get from the budget in appropriations terms.

Making up a program budget involves identifying the major objectives and subobjectives of the Department to which resources are devoted. In a complex operation like HEW, where many programs have multiple objectives, there is certainly no unique way of organizing a program budget. We tried several ways and did not find an ideal one. Our first attempt at a program budget was organized under three major objectives of Department activity: (1) "human investment"—improving the earning capacity and ability to function of individuals and families, (2) providing income and other benefits to individuals and families, and (3) institutional and community development. These three categories cut across the organizational lines of the Department. Manpower training programs managed in various parts of the Department, for example, were grouped together in category (1) while construction programs of various sorts were put together in category (3). This crosscutting was useful for some purposes, but not for others. It made it difficult, for example, to look at education as

a whole and see the relationship between Department programs to provide services for children and those for training the teachers needed to provide those services. To facilitate looking at these questions, we moved in the next program budget to the more conventional major objectives of (1) improving health, (2) improving education, (3) income maintenance, and (4) social and rehabilitative services.

The program information system sorts out Department funds not only by program objectives, but also by population group served, type of activity, method of finance, and so forth. Using the information system, the Secretary can see, for example, what portion of the Department's resources go for health; within health, how much is for the development of health resources; and within health resources, how much is for the training of physicians. He can see who is helped by HEW programs—how much goes to the old, the young, the poor.* He can see what means are used to further objectives—how much for construction, how much for research. He can see how much goes to the States in formula grants, and how much in the form of project grants. He can also see how all these proportions have changed over the last several years.

These are important questions, and the answers do not leap out of the appropriations budget. Some of these questions had, of course, been asked before by Secretaries or by Members of Congress, and estimates of the answers had been painstakingly put together, but now the Secretary has ready access to this kind of information on a regular basis.

In my opinion, the greatest impact of the program information system has been in facilitating some simple calculations at high levels of aggregation. My favorite example is Secretary Cohen's astonishment at a table showing that most of the Department's recent budget increases had been devoted to older people and relatively little to children. Why the father of Medicare should have been surprised at this, I do not know, but he was; and he immediately began talking about a new emphasis on programs for children.

Granted that a program budget can provide useful information to decisionmakers and new ways of looking at programs, how useful is a program budget as a decision tool? Our HEW experience indicates, I think, that a program budget is a useful *planning* tool, but at the moment of budget decisionmaking the program budget cannot be substituted for the appropriations budget. Both are necessary. Let me explain this.

HEW operates under several hundred legislative authorities and separate appropriations categories. Sensible planning necessitates organizing these activities in terms of major objectives and subobjectives of the Department and deciding on the relative emphasis to be given to these various objectives and subobjectives. For example, planning for health has to involve such questions as these: What should the Department be doing to improve the access of individuals to medical care by providing them the means of paying for such care? How much effort should the Department be making to increase the supply of medical services by training doctors or building hospitals, or other

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

means? To what extent should the Department be investing in future medical discoveries rather than present provision of services? On the education side, planning for the Department must involve questions such as these: Should the Department expand its aid to elementary and secondary education at a more rapid rate than its aid to higher education? Should it increase the proportion of resources devoted to improving the education of the poor rather than that of the whole population? How much emphasis should be put on finding new methods and approaches to education as opposed to increasing resources going into the present system?

Obviously a program budget cannot answer these questions, but it is a useful framework for laying out the choices so that decisions can be made about them. Once these major decisions are made, however, they cannot automatically be translated into a budget to be sent to the President and the Congress. Each of the several hundred programs operated by HEW has unique characteristics. It has a legislative history and an authorization level. It is handled by a particular committee or subcommittee whose chairman may have definite views. It may have a strong lobby supporting it or gunning for it. It may be administered by States or localities or other non-Federal institutions. All of these particular characteristics or programs are relevant to a decision to translate a program budget decision into budgetary and legislative terms. For example, a Secretary of HEW may decide to increase the resources devoted to experimenting with new methods in education. Once he has decided that, however, he is confronted with where to put the money. Should he use title III of the Elementary and Secondary Education Act, which is largely controlled by the States? Should he use the regional laboratories under title IV of the same act which are administered in an entirely different way and have different strengths and weaknesses? Should he ask for a new authorization and run the risk that a committee which has shown itself reluctant to fund new programs will deny him the funds? No matter how useful the program budget proves, as a way of organizing information and as a planning tool, the final decisions on the budget must be made in appropriations terms and in the light of all of these complicated considerations which, though they may not be desirable, are facts of life for a Secretary of Health, Education, and Welfare.

The answer to the question "Have decisions been made in program budget terms in HEW?" is both "Yes" and "No." * Since the advent of PPB, major decisions have been made in program budget terms—decisions to emphasize health services for the poor, family planning, education research and innovation, efforts to help welfare recipients become self-supporting, etc. The process of translating these major decisions into appropriations terms, however, necessitates continuous walking back and forth between the two sets of budget categories. At some points in the decision process the program budget formulation was extremely helpful to the decisionmakers, especially, I think, in the health area where it facilitated joint consideration of health programs administered in several different agencies. At other points the program budget seemed to make decisions more complicated be-

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

cause a particular appropriation was either buried in a larger total or split among several program categories. For example, Title I of the Elementary and Secondary Education Act is primarily an education program, but provides some funds for health services for disadvantaged children. An estimate of the health expenditures from Title I showed up in the program budget under "health." At the moment of decision on "how much for Title I?" it was necessary to add the two pieces together and make a decision on Title I as an entity.

I see no simple solution to this problem, although simplification of the HEW appropriations structure would help. Both kinds of budgets are necessary to good decisionmaking, and HEW executives simply have to be adroit at considering decisions both in program and in appropriations terms, and translating back and forth frequently.

EVALUATION: MEASUREMENT OF PROGRAM ACCOMPLISHMENT *

The second result of PPB in HEW has been a new emphasis on evaluation of what programs actually do. The first step was to collect information on a regular basis about the "outputs" of programs. The Secretary now has available in the program information system a continuing series of measures of the "outputs" of individual programs—hospital beds constructed, teachers trained, patients served, persons participating in basic literacy programs, etc. In some cases it was almost impossible to find a meaningful output measure for a program. "Number of research projects supported," for example, is not an interesting statistic. Yet, it is the only readily available measure of output of a research program. At best, these output measures are rough guides to what the program is buying, and can be useful in showing the Secretary what he would give up if he shifted money from one program to another. For example, how many nurses does one give up to train a psychiatrist, or how many teachers could be trained for the price of a hospital bed? These statistics are better than no information on what the program is buying, but they do not throw much light on what is actually being accomplished. They do not tell the Secretary what the program is contributing to the health or education or welfare of the Nation.

Evaluating the effectiveness of most HEW programs is difficult—not because the people who run them are incompetent or falsify the information, but for at least three more basic reasons. First, it is usually far from obvious what one would *like* to have happen—what the measure of success of the program should be. For example, Title I of the Elementary and Secondary Education Act gives money to school districts to improve the education of disadvantaged children. Should we look for a measure of success of this program in the test scores of these children, in their dropout rates, in their future ability to hold a job, or in some measure of their attitude toward themselves and their environment? Second, most HEW programs are designed to help individuals function better. Their success can only be gaged by following the individuals over some considerable period of time to find out what actually happened to them. Followup is expensive even if done on a sample basis. Third, it is difficult to disentangle the effects of HEW

* Further discussion of this issue is found in the papers by Grosse, Brandl, Mangum, and Levine in this volume.

programs from all the other things which affect the health, education, and welfare of individuals. If infant mortality drops in a particular locality, it may be the result of a prenatal care program, or better nutrition, or higher incomes, or a lower birth rate, or a combination of all of these things. In some cases, control groups and sophisticated statistical techniques can help sort out these various factors; in some cases, they cannot.

Can a Government agency be expected to evaluate its own programs? In particular, should program managers be expected to participate in evaluation? Can they be objective about their own programs?

Before trying to answer these questions, I think it is important to distinguish two kinds of evaluation. The first (and the one in which Congress seems to be most interested) is overall evaluation of the accomplishment of a program. It is the attempt to answer the question: To what extent is a program meeting its objectives? It amounts to giving a grade—often a pass or a fail grade—to a program as a whole. For example, one might want to know how many additional doctors have been trained as a result of a program of aid to medical schools, how many welfare recipients have become self-supporting as a result of training and day-care programs, or what has happened to the incidence of measles as a result of the measles vaccine program.

While program managers must cooperate in providing the information necessary for these overall evaluations, it is too much to expect them to carry out the evaluation themselves. No one wants to admit failure. In order to insure objectivity, it is necessary to have the information analyzed and judgments made by someone not directly responsible for the execution of the program—perhaps someone outside the Government altogether.

For many important HEW programs, however, I think this kind of overall pass-or-fail evaluation is next to impossible. For example, it is not really possible to answer the question, What is title I of the Elementary and Secondary Education Act accomplishing? Title I provides only a small part of the resources used to educate disadvantaged children, and school itself is only one of the influences (and probably not the most important one) on the performance of these children. If a national testing program showed an increase in the test scores of disadvantaged children, everyone would be happy, but it certainly would not be clear what proportion of this increase, if any, should be attributed to Title I. A negative finding—no change in the tested performance of poor children—might suggest that Title I money was being wasted, but would not prove that nothing could be done through the schools to help these children. Title I funds (as well as other education resources) are spent in many different ways in different localities presumably with varying degrees of effectiveness. The really interesting problem for the evaluator is not to figure out what the average effectiveness of the program is, but to identify the kinds of education projects which are successful with low-income children so they can be replicated and expanded.

This second kind of evaluation—that designed to identify successful ways of spending money for a particular objective and to improve the average effectiveness of a program—should be of tremendous importance to a program manager who wants to do a good job. It should have his full support and participation. The manager of a manpower

training program should have a strong interest in discovering which types of training projects are most successful. The manager of a family planning program should have a strong interest in discovering which ways of delivering family planning services are the most effective. In the long run, I think this kind of evaluation is of more importance to the wise use of Government resources than is the overall pass-or-fail type.

Evaluation—of both types—is still in its infancy in HEW. The planning and evaluation staff has succeeded in getting some funds authorized for evaluation in various legislation and in some cases in getting the funds appropriated. We worked with the staffs of several of the HEW agencies—most notably with the Office of Education—to design evaluation plans, and we funded a number of pilot evaluations. But designing sound evaluation techniques and collecting, processing, and interpreting the information takes time and expert staff resources. These resources are not presently available in HEW, nor is it easy to find them outside the Government. To do a good job on evaluation, it will be necessary to recruit a staff of competent people who can work closely with managers of major programs to define what kinds of information are needed, to design a system for collecting this information, and make sure that it does get collected and analyzed.

ANALYSIS OF ALTERNATIVE COURSES OF ACTION

Systematic analysis of alternative ways of reaching objectives is the heart and soul of PPB. A good analysis specifies an agreed-on objective or set of objectives, outlines alternative ways of reaching these objectives, and brings together as much information as possible about the costs, benefits, advantages, and disadvantages of each. The analyst uses the results of program evaluation and goes beyond them to try to estimate the effectiveness of new programs. In a sense he is an evaluator of programs which do not exist yet.

Analysis of alternatives is, of course, not a new idea. Studies of program alternatives of various sorts have been done in different parts of HEW for years. What was new in the last 3 years was the existence of a staff of economists and other analysts in the Office of the Secretary which was specifically devoted to studying the major options open to the Secretary with respect to budget and legislation. Perhaps even more important was the presence of an Assistant Secretary in budgetary and legislative decision meetings whose job it was to see that relevant analysis was considered at the decision moment. By way of illustration, let me describe briefly two recent analyses carried out by the planning and evaluation staff at HEW which should be of considerable interest to the Congress and the general public as well as to the executive branch.

The first is a study of higher education undertaken at the request of President Johnson, and released by Secretary Cohen just before he left office.¹ In this study we made an attempt to specify the various objectives which the Federal Government has in supporting higher education—objectives such as improving access to higher education on the part of all students, improving the quality of higher education

¹ U.S. Department of Health, Education, and Welfare, *Toward a Long-Range Plan for Federal Financial Support for Higher Education*, Washington, D.C., 1969. Estimates are for the 1960 high school graduating class.

by increasing the resources available to institutions, and preserving diversity and autonomy in American higher education. The study examines the available information on the degree of equality of access to higher education on the part of students from different income levels, documenting the fact that students from low-income families have relatively low chances of going to college even if they have high ability. A student of good college potential, scoring in the top two-fifths on high school achievement tests, is more than twice as likely to enter college if he comes from a family in the top quarter of the income distribution than if he comes from one in the bottom quarter.² The study also examines available information on the financial health of higher education and the relative strength of public and private institutions. One interesting and somewhat surprising finding of this part of the study was the widening gap in resources per student available in public and private institutions, with *private* institutions enjoying a more rapid increase. Between 1959-60 and 1965-66 the study found:

“There was a marked disparity in the rates of increase in revenues per student in public and private institutions, with public institutions’ revenues per student increasing 4.0 percent annually while the comparable rate of increase for private institutions was 8.1 percent.”³

An attempt was made to lay out the major options available to the Federal Government in support of higher education—the student-aid through loans, grants, the work study-program, and institutional aid of a variety of types—and to evaluate the advantages and disadvantages of these various alternatives as ways of furthering the particular Federal objectives. The report contained a set of recommendations, but these may well be of less importance than the analysis itself.

A second analysis which I would like to discuss briefly concerns major alternatives to the present welfare system.⁴ Estimates were made of the number of people who would probably still be in poverty 5 years from now if present welfare programs were continued, and of the cost of such continuation. It was estimated that the cost of present welfare programs would rise from about \$3.7 billion in 1969 to perhaps \$6.2 billion by 1974, but that poverty would not disappear in this period:

“* * * poor households will still number some 8.8 million by 1974, compared to 10.8 million in 1966, even if we are successful in maintaining a high rate of employment and economic growth.”⁵

An attempt was made to analyze major alternatives to continuation of the present welfare system. The alternatives considered included a children’s allowance and a negative income tax-type program which would give aid to the working poor as well as those aided by present welfare programs. The various alternatives were evaluated with respect to cost, coverage, contribution to closing the poverty gap, savings to the States, and their effects on incentives to work, incentives to establish separate households, incentives to move from low to high income States, and other factors.

² *Ibid.*, p. 5.

³ *Ibid.*, p. 11.

⁴ U.S. Department of Health, Education, and Welfare, Office of the Assistant Secretary (Planning and Evaluation), *Program Memorandum on Income Maintenance and Social and Rehabilitation Services Programs of DHEW*, November 1968, pp. III.1-III.20.

⁵ *Ibid.*, p. III.1.

Analytical effort in HEW has been hampered by two main factors—lack of staff and lack of information. Studies of the sort just described take many man-months of effort. The present staff of analysts under the Assistant Secretary for Planning and Evaluation can handle only a small number of studies a year, and must choose the three or four issues which seem likely to be of importance in upcoming budgetary or legislative decisions, perhaps leaving aside issues of more basic long-run importance. Better use could be made of analysts in universities, foundations, and elsewhere if funds were available to finance more outside studies.

The second difficulty (that of lack of information) is more basic. Indeed, it may be that the most important result of the PPB effort in HEW so far has been the discovery of how little is really known, either about the status of the Nation's health, education, and welfare, or about what to do to change it. A recent report of the Department prepared in the Office of Planning and Evaluation, attempted to measure the Nation's progress toward certain widely accepted social goals.⁶ The study was an attempt to see what could be said about such questions as: What is the state of the Nation's health? Are we getting healthier? Are we better educated? Are we winning the war on poverty? If nothing else, the volume served to illustrate the thinness of social statistics and how little is really known about the state of the Nation even with respect to such apparently measurable factors as physical health and intellectual capacity.

The child health study discussed by Dr. Wholey is a good example of an analysis which uncovered more questions than it answered.* I remember being astonished when we first started that study that doctors could produce no evidence that children who saw doctors regularly were healthier than children who did not. They all believed it (and I do, too), but they did not have any statistics to prove it. I was equally astonished to find that educators have little or no evidence that children who get more expensive education (newer buildings, higher paid teachers, more teachers, etc.) learn more than children who get less expensive education. They believe (and so do I), but the available statistics do not prove it, nor does presently available information give any solid clues about what kinds of schools are best, or whether particular educational methods are more effective than others.

Analysis, experimentation, and evaluation must proceed together if we are to make progress in providing decisionmakers with good bases for decisions in health, education, and welfare. Serious effort on the evaluation of Federal programs would give the analyst more data. Additional analyses will also provide a better idea of where the gaps in information are and what kind of statistics should be collected.

THE FIVE-YEAR PLAN

The major objectives of the Department of Health, Education, and Welfare—improving health and education and eliminating poverty—

⁶ U.S. Department of Health, Education, and Welfare. *Toward A Social Report*, Washington, D.C., January 1969. See also the paper by Isabell Sawhill in vol. 1 of this collection.

*Further discussion of this issue is found in the paper by Wholey in vol. 1 of this collection.

all take time and are highly interrelated. Successful compensatory education, manpower training programs, and provision of health services would reduce the need for income maintenance in the future or, conversely, provision of income to large numbers of poor persons through a new kind of income maintenance system might reduce the need for special services in health and education. Moreover, most of the programs of the Department involve the provision of services by skilled manpower or in highly specialized facilities. It would not be possible to provide good quality pre-school education or pediatric care for all children next year. Neither the facilities nor the trained people exist. For all of these reasons, planning ahead in the health, education, and welfare area is essential to realistic decisionmaking.

Probably the most important thing about the HEW 5-year plan is that it exists. For the first time at the Secretary's level a real effort has been made to look at the Department as a whole, to address alternatives and priorities, and to lay out at least a tentative program for 5 years into the future.

The present HEW 5-year program and financial plan is a plan only in this sense: it shows how assumed budget totals would be allocated by objectives and sub-objectives in the future in order to reflect presently conceived priorities. It shows how an increased emphasis on such priorities as reducing welfare rolls, educating disadvantaged children, and providing health services for the poor would be reflected in future HEW program budgets.

Some elements of the HEW plan are only projections. In the income maintenance area, for example, the HEW plan is not a plan at all. It is simply a projection of the future costs of present income maintenance programs. A great deal of work was done on alternative income maintenance programs (as noted above, these were transmitted to the Bureau of the Budget in a program memorandum), but the last administration did not commit itself to the choice of a particular alternative to present income maintenance program.

Only a few segments of the HEW plan are real plans in the sense that they show how specific objectives could be reached over a multi-year period. Planning for the work incentive program (WIN) is the best example. A plan was designed which, it was thought, would result in removing a specific number of people from the welfare rolls by 1975 through provision of training, day care, and other services.

Ideally, the HEW plan should state performance goals—elimination of poverty, elimination of specific diseases, reduction of infant mortality to specified levels, availability of health services to all persons, raising average reading scores of poor children to given levels, etc.—and show how these goals are to be accomplished. Right now, it is not possible to lay out this kind of plan because in most instances we cannot yet specify the connection between money expended and results achieved. It is not now possible, for example, to make any estimate of how much it would cost to raise the reading levels of poor children to the national average, or how one would go about it.

The process of putting together the plan revealed dramatically how little is known about the connection between expending money and specific accomplishments. If nothing else, producing the plan provided a strong case for increases in systematic experimentation, evaluation, and analysis so that more informed planning can be done in the future.

The Bureau of the Budget, after initially requiring a comprehensive multi-year program and financial plan backed away from this requirement.* Present Bureau of the Budget instructions require the Department to submit estimates of the future costs of present decisions, but specifically do not require laying out the implications of future decisions. For HEW, this change in signals was equivalent to rejection of planning on the part of the Bureau of the Budget. Only a few HEW programs—those involving construction or multi-year research projects—have built-in future costs. Most HEW programs involve services to people and can be run at various levels depending on the number of people one has the resources to serve. Hence, a plan which reflects only future implications of present decisions is really not a plan at all.

I think it was a mistake for the Bureau of the Budget to back away from forward planning. Indeed, HEW ignored the backing away and submitted a full program and financial plan as required by original PPB directives. Presumably, the Bureau of the Budget changed its mind because of the fear that it might prove embarrassing to the administration to have plans laid out in advance which might reveal or seem to reveal a hidden strategy or cause unwarranted criticism of tentative plans on which decision had not yet been made. These risks are real, but I suspect that as planners become more sophisticated and better able to specify the connections between goals and expenditures, the obvious advantages of planning ahead begin to outweigh the disadvantages. Indeed, Congress itself may some day require the administration to submit multi-year plans in addition to single year budgets.

THE PLANNING CYCLE: EXPERIMENTS IN SYSTEMATIC DECISION-MAKING

Evaluation, analysis, and program budget categories are all useful tools, but the moment of truth is the decision. The real point of PPBS is to establish a process for bringing the relevant tools to bear at the right moment so that decisions are made in the light of maximum information about present and future consequences of alternative courses of action.

Until the advent of PPB, budgets in HEW had been made by building on the previous year's base and adding additional funds where administration priorities, Congressional interest, or the bargaining power of program managers dictated. New legislation was handled separately, usually after the budget was put to bed, and with little explicit consideration of trade-offs between funding old programs and adding new ones.

What we tried to do in implementing the PPBS idea was to institute a regular process in which (1) the Secretary and his principal advisers sat down together to consider major alternative ways of using Department resources over the next several years; (2) decisions were made in the spring or early summer about the major directions in which the Department wanted to move; (3) these major long-run decisions were translated into budgetary and legislative decisions in the fall.

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

There have been two full budget and legislative cycles at HEW since the implementation of PPBS. We tried out the new procedure first in the formulation of the fiscal year 1968 budget and 1969 legislative program. We began the process of getting an fiscal year 1968 budget by having the Secretary ask the constituent agencies of the Department to examine the missions of their agencies and their priorities for the future, and to submit a program budget for the fiscal year 1973 indicating how they would allocate resources to achieve objectives for that year. Each of the constituent agencies was given a high and a low planning figure for fiscal year 1973. When the agencies' plans for that year were submitted, the Secretary's Planning and Evaluation staff analyzed them, suggested alternatives, and presented the Secretary with a series of decision memoranda outlining major long-run options, and giving as much analysis as possible of the advantages and disadvantages of each, together with indications of positions taken by the constituent agencies and by members of his own staff and the reasons therefor. After considerable back and forth discussion, the Secretary made decisions on program objectives for fiscal year 1973. These were then transmitted back to the agencies with instructions that they develop a plan for moving from the present to those 1973 objectives (i.e., fill in the intervening years) making their fiscal year 1968 budget consistent with this plan. When the budgets were submitted, they were reviewed in the Office of the Secretary to make sure they were indeed consistent with the 1973 objectives—that programs scheduled to receive increased emphasis by 1973 had indeed been increased in the budget, and those scheduled for decreased emphasis had been cut back or held level. In other words, the plan provided a rough set of guidelines for accepting or rejecting agency budget requests.

In formulating the fiscal year 1969 budget, a similar procedure was used except that, since the Department now had a plan for the years 1968 through 1973, the agencies were asked to start with that plan, extend it through fiscal year 1974 and submit revisions consistent with new information or new priorities. As in the previous year, the revised agency plans were worked over by the Planning and Evaluation staff, alternatives were suggested, and secretarial decisions sought so that guidance could be prepared for formulation of fiscal year 1969 budgets and 1970 legislative proposals.

What we were trying to achieve in the planning cycle in both of these years was an orderly process in which the Secretary was able to make major decisions early enough in the year to allow the staff work necessary to translate these decisions into budgets and legislative proposals. In both years, however, we ran into considerable difficulties in obtaining early decisions on major matters (the second year happened to be an election year and the last year of a lame-duck administration, which made planning more than usually difficult). There were at least three reasons for the difficulty. First, the Secretary himself was reluctant to make major decisions early in the year, and was always eager to hold open as many options as possible. Second, much of the initiative on new legislation came not from the Department itself but from the White House. The White House procedure was to organize legislative task forces in August or September to report about the first of November with major legislative proposals for consideration

by the President. This procedure interfered with early decisionmaking in the Department. Third, the procedure we were using depended on the constituent agencies making the first move. They were required to submit plans or revisions of plans for reaction by the Secretary and his staff. Since the agencies tended to drag their feet and turn their plans in far after the deadline, the time available for analysis by the Secretary's staff was short, and Secretarial decisions tended to occur later in the year than desirable. The result was that major decisions on the long-range plan were delayed until August and September and tended to be overtaken by the shorter range budget decisions which had to be made at the same time.

I do not think there is any complete solution to this problem. Being human, Federal executives will tend where possible to avoid commitments, to preserve their options, and to put off major decisions as long as possible. However, the planning cycle procedure in HEW could be altered so as to facilitate earlier decisionmaking. One way to do this would be to shift the initiative to the Office of the Secretary which would issue specific guidance on major program emphasis for the agencies to follow in formulating their plans. This would take the Secretary's Office out of the position of reacting to the agencies and put the agencies in the position of reacting to the Office of the Secretary. This would make it more difficult for the agencies to slow down the whole process by coming in late with plans. Such a procedure presupposes a capable and knowledgeable staff both in the agencies and in the Office of the Secretary.

SOME CONCLUSIONS FROM EXPERIENCE

Anyone who thought that PPBS was a magic formula to make the allocation of Federal resources easy had better think again. There is no magic formula because these decisions are inherently difficult. They are difficult, first, because they are made in the face of great uncertainty and, second, because the outcomes affect different groups of people importantly and differently. Far from making the decisions easier, the PPB system has undoubtedly made decisionmakers more aware than ever before of how hard the decisions they have to make really are.

In the defense area, uncertainty is the dominant difficulty. Good analysis of the costs and effectiveness of alternative U.S. actions is highly useful, but it can only reduce the uncertainties by a small percentage. A tremendous amount of guesswork about enemy motivations and intentions is still necessary. There is little room for experiment. Decisions are not made in small, discrete steps but tend to be of the all-or-nothing variety, and the cost of making a mistake is great.

In the domestic area, the uncertainty surrounding decisions *need* not be so great, although at present it probably is. It would be possible to run domestic programs as a continuous series of experiments—to try different things, to evaluate the results, to expand those that work well, and cut back on those that do not. Good evaluation systems will certainly not be quick or easy, but they can be used to make programs far more effective than they are now, not just at the Federal level but at all levels of Government. The potentiality of PPBS for reducing the uncertainty surrounding decisions seems to me far greater in the domestic than in the defense area.

The other difficulty—the differential impact of decisions on people—is, however, far more obvious and troublesome in the domestic than in foreign area. Defense decisions result in some people being better protected or bearing a heavier burden than others, but these differential effects are not nearly so obvious as in domestic programs. In domestic programs of direct service to particular types of people, everyone knows who the immediate beneficiaries are. A good PBB system can illuminate these distributional decisions, but cannot make them any easier. Indeed, assembling and publicizing information on who is helped by particular government programs may intensify political conflict.

I view PPBS as a commonsense approach to decisionmaking. The terminology may well change—and probably should—but I fail to see how a Secretary of Health, Education, and Welfare who wants to do a good job can get along without planning ahead, evaluating the effectiveness of programs, analyzing alternatives carefully, and making decisions in an orderly way in the light of maximum information. It does not matter what he chooses to call it, but he badly needs the basic tools of PPBS.

The progress made in the Department in the last 3 years is clearly a start toward improved decisionmaking, but it is only a start. More attention needs to be paid to evaluation, and more resources need to be devoted to building up a continuous flow of useful information on the effectiveness of programs. Far more resources need to be devoted to good analysis, especially to understanding the complicated interactions between Federal programs and what happens in the State, local, and private sectors. Better ways need to be found within the Department for focusing attention on major longrun decisions and considering budgetary and legislative options at the same time. Above all, the Secretary himself has to use the system. If he wants good analysis, he will get it. If he wants good information, he will get it. If he wants to make decisions in an orderly way, considering all important options carefully and systematically, he will, with patience, be able to do so.

SECTION D

THE PPB SYSTEM AND THE INSTITUTIONS OF GOVERNMENT

PUBLIC EXPENDITURE ANALYSIS AND THE INSTITUTIONS OF THE EXECUTIVE BRANCH

BY FRED S. HOFFMAN

Fred S. Hoffman is vice president of Laird Systems, Inc., in Los Angeles, Calif. Until January 1969, he was Assistant Director of the U.S. Bureau of the Budget.

It is widely recognized that the impediments to sound and objective economic analyses of public policy alternatives are great. Many of these impediments are rooted in the institutions of the executive branch of the Government. In this paper, Mr. Hoffman analyzes the origins of systematic analysis in the Federal Government, the appropriateness and value of this kind of analysis in both the military and civilian areas, and the obstacles in the executive branch that have been and are being faced in the introduction of the PPB system. He discusses the problems raised by the multiobjective nature of many social programs, the lack of sound theory and data with which to analyze most domestic programs, and the greater political sensitivity of civilian outlay programs.

After analyzing the progress made to date in implementing the PPB system, Mr. Hoffman makes several suggestions designed to increase the effectiveness of benefit cost-type information in the decision process. These include making public the analytic studies generated by the PPB system, the development and release of a modified 5-year budget plan in which both the commitments already made by the Government as well as Presidential objectives for program directions are displayed, the undertaking of independent analysis by the Bureau of the Budget to stimulate department and agency heads to improve their own analyses, the development of needed data, the assembly of groups of competent people who are technically trained in analysis and who have an understanding of the substantive areas in which they are involved, and an inter-agency effort led by the Bureau of the Budget to develop a Government-wide program structure.

Introduction

The planning, programing, and budgeting (PPB) system, as the name suggests, was conceived as a system of interrelated elements. The system was intended to improve Federal decisionmaking about resource allocation in several important ways. The system is designed to:

- Compare the efficiency of alternative ways of carrying on Government resource-using or resource-affecting activities, as the market tests the efficiency of private resource-using activities.
- Relate tests of efficiency to the proper objectives of public action (not necessarily the historical or organizational objectives of Government agencies).
- Present major issues for decision in a useful way to high officials who have no time to be specialists on even a fraction of the matters they must decide.

To accomplish the desired improvements in governmental decision-making, PPB has strived to introduce or strengthen three aids to the making of choices about resource allocations:

1. *Analysis*.—Comparisons of the cost and effectiveness of alternative ways of achieving the objectives of public policy.

2. *Program Budgeting*.—Presentation of the agency budget in a classification system related to the major program choices to be made.

3. *Planning*.—Presentation of information about the future implications of current program choices for cost and effectiveness beyond the budget year.

Analysis, program budgeting, and planning are distinct elements of PPB, and might have been introduced separately. Moreover, their development has proceeded at different rates during the 3 years since the introduction of PPB, and in response to different stimuli and impediments.

Although analysis, in my view, is at the heart of PPB, I believe that there were good reasons for linking the three elements. The reasons are related to the nature of systems analysis and to the nature of decisionmaking in a large organization such as the Federal Government. The paper first reviews the nature and origin of systems analysis and then turns to the relations among the three elements as they have developed in PPB.

1. THE ORIGINS OF SYSTEMS ANALYSIS IN NATIONAL SECURITY PLANNING*

The PPB system is extending to the other major departments and agencies of the Executive the approach to resource management developed by Secretary McNamara in the Department of Defense. Systems analysis played a central role in the system that evolved in the Defense Department, and an understanding of its salient characteristics is necessary to understand the PPB system.

Systems analysis is a term whose meaning has been eroded by very wide and diverse usage. The sense of the term most relevant to PPB is the one that describes the approach to national security problems developed during the early 1950's. The approach evolved in response to the planning problems of the early post-World War II period; it was not the result of a grand intellectual design.

Operations analysis in World War II provided the immediate antecedents for systems analysis. The successful application of applied mathematics, physical science, and systematic data collection to tactical problems of increasing complexity, created the presumption that people trained in these activities could contribute to post-war planning. The Air Force, in 1946, at the urging of Secretary of War for Air Lovett (later to become Secretary of Defense) and under the leadership of Gen. H. H. Arnold, created Project RAND, later to become the RAND Corp., to bring together people of the sort who had done wartime operations analysis, in order to advise the Air Force on its research and development activities.

The nature of the problem facing the Air Force in 1946 was vastly different and more difficult than the wartime operations analysis

*Further discussion of this issue is found in the papers by Enthoven, and Enthoven & Smith in this volume.

problems had been. The postwar problem was one of maintaining for an indefinite period, peacetime readiness for combat, rather than conducting ongoing combat operations. After 1946, the problem became one of making decisions that would shape our military forces for many years in the future, subject to the resource constraints encountered in a peacetime economy. This was a far less determinate problem than that of choosing the best search tactics for a destroyer hunting a submarine, or firing tactics for a fighter in pursuit of a bomber.

The indeterminacy of the post-war planning problem was augmented by the post-war technological revolution. In addition to increasing the urgency of national security questions and the cost of providing and maintaining military forces, the flood of new technology so changed the character of prospective conflict that very little past military experience could be considered relevant to the problem of deterring World War III, or of protecting the United States if conflict should come. Under the circumstances, it was necessary to gather such data as might be available from physical science, engineering analysis, and past military operations, and from these, to synthesize by analytic means, predictions and comparisons that would be useful for policy decisions.

The analysis that developed in response to this need could be no respecter either of organizational boundaries or of the limits of traditional disciplines. The analysis of air defense could not stop within the limits of a fighter squadron, for fighter aircraft became dependent on information provided by a radar network connected by an elaborate communications system. In the choice of bombers, also, the wide range of alternative bomber designs offered by technology made it necessary to take account of the overseas bases or tanker aircraft needed in larger quantities by the shorter range aircraft, and also of the greater vulnerability to enemy attack involved in operating from overseas bases. And, as the linkages among elements of the problem forced the boundaries of the analysis outward, more and more diverse skills were required in intimate interaction during the course of an analysis.

One important effect of the systems approach is to call into question the narrower, organizationally oriented objectives of many programs. During the 1950's, for example, there was an important reorientation of air defense to take account of the growing importance of protecting the country by deterrence of attack rather than by defeating the attack if it came. Under the conditions of that period, the highest priority tasks of the air defense system became the provision of warning to the strategic bombing forces, permitting them to survive a surprise attack and retaliate. Such reorientations of objectives have generally followed a long series of analyses with steadily widening perspectives. Thus, the neat, logical characterization of analysis as beginning with a clarification of objectives is somewhat misleading.

In reality, systems analysts have more often than not begun by accepting current objectives and later proposed changes in them only after repeated analyses of broadening scope have shown objectives to be in conflict with one another. Certainly, the most fruitful systems analyses have not indulged in argument about objectives for

argument's sake, but have been forced to review existing objectives and priorities as inconsistencies among them have appeared or because they proved to be inappropriate under changing conditions. The analytical process is an educational process revealing new objectives and often new means of achieving them.

In summary, then, systems analysis was a response to the complexity of the national security planning problems encountered after World War II. It is not a discipline or a technique, but rather a style for dealing with complex problems of choice. The style is characterized by two salient characteristics:

Explicitness.—From its origins in the application of scientific method to the analysis of military operations, systems analysis has emphasized explicit treatment of objectives, assumptions of fact, criteria for choice, and above all, the alternatives among which choice is to be made. Explicit statement permits reproducibility of results, and by isolating points of disagreement, permits the policy process to converge to agreement.

Orientation to decision.—The scope of the analysis is determined by the scope of the decision. The systems analyst attempts to include all those elements which interact strongly in determining the implications of a choice among the alternatives, regardless of the boundaries of academic disciplines or of bureaucratic organization. Such an approach inevitably calls into question existing objectives from time to time and suggests new policies for inclusion among the set of alternatives. It also calls for consideration of effects that go beyond a 1-year budget period.

2. THE APPLICABILITY OF SYSTEMS ANALYSIS TO DOMESTIC PROGRAM CHOICES

2.1 A MISLEADING DISTINCTION

Those who question the relevance of the experience in the Department of Defense to the analysis of domestic programs have often done so on the grounds that quantitative analysis is appropriate for programs that are essentially concerned with *things* such as aircraft, missiles, and radar, but that it is not appropriate for programs which deal with *people*.

This distinction is misleading, first of all, because it is not true that domestic programs are concerned entirely with people any more than that national security programs are devoid of such considerations. Deterrence of war, the central concern of national security policy, is a question of how people and governments will react. Analysis has been useful in decisionmaking about the requirements to maintain deterrence, despite the fact that no one has devised a quantitative, objective measure of deterrence. Similarly, analysis can be useful in allocating resources among programs to combat disease, despite the fact that no way exists to establish a socially accepted dollar value for a human life.

Secondly, domestic programs are far from devoid of questions involving large, expensive, and long-lived items of equipment for capital. Schools, hospitals, dams, air traffic control radars, and highways are all objects of decisionmaking about domestic programs.

There are, nevertheless, real and important distinctions between domestic and national security programs from the point of view of analysis. These differences stem largely, I believe, from differences in the basis for the Government's role.

2.2 THE BASIS FOR THE ROLE OF GOVERNMENT IN RESOURCE ALLOCATION

Economists call national security a "public good."* A public good is one that is either consumed in common or not consumed at all. Either nuclear war is deterred for all of us, or it is deterred for none of us. In the case of a private good, like shoes, for example, if I buy a pair of shoes there is one less pair available for use by everyone else. In the case of national security, my enjoyment of the benefits of peace does not detract from that of others. It is therefore impossible to use a private market mechanism to determine the amount of a public good to be provided. Everyone would sit back and wait for everyone else to pay for his security. The decision about the level of spending on national security must, consequently, be made by Government.

Examples of public goods may be found also in domestic programs, although national security is probably the purest and most extreme example of a public good. Education provides an example of a good that has both public and private elements. Individuals want education for themselves and for their children in order to increase the quality of themselves and their earning power. But there are also a variety of public motives for education. In particular, the requirement that a self-governing electorate achieve certain minimal educational standards, establishes education as at least partly a public good and one about which public decisions are, therefore, required.

In the domestic area, there are a number of additional Government roles in resources allocation decisions. Since 1946, the Government has had a statutory responsibility for the maintenance of full employment. More recently, the maintenance of a suitable growth rate without excessive inflation has become a widely recognized responsibility of Government. There are, however, a number of more specific roles for Government which bear upon the analysis of Government programs.

*Dealing with spillover effects.***—Spillover effects exist when the consequences of a decision affect not only the individual making the decision, but others as well, and the decisionmaker need not or cannot take into account his effect on others. Spillovers become more important as we live ever closer to our fellow citizens. When John Doe burns leaves with a prevailing wind toward Richard Roe's garden party down the street, a spillover effect exists. The two individuals have conflicting interests if John Doe finds burning leaves preferable to having them removed; but this is not the essence of the problem. Richard Roe may be bothered enough by the smoke to be willing to have the leaves removed, but if there exists no social mechanism which will permit this, the spillover situation is an appropriate subject for

*Further discussion of this issue is found in the papers by Steiner and Arrow in vol. 1 of this collection.

**Further discussion of this issue is found in the papers by Davis & Kamien, and Kneese & d'Arge in vol. 1 of this collection.

governmental action. (In the case of near neighbors there exists a variety of informal mechanisms by which this situation may be resolved; in the more general case, this is not so.)

In the particular example, the Government might either subsidize the removal of the leaves until removal is so cheap that John Doe will prefer it to burning, or it might tax their burning. In place of monetary incentives, Government often resorts to regulatory action to deal with spillover effect. Each way will induce John Doe to act in a way that takes account of the effect of his actions on others. Less trivial examples than leaf burning are actual environmental pollution, traffic congestion, etc.

The effects that spill over need not always be bad. Examples of beneficial spillovers occur in unpatentable research activities, social welfare programs which not only benefit the individual but reduce the costs to society by making him less of a public charge and less likely to break the law, etc. Spillover effects it can be seen, are closely related to public goods, and like them, require public action.

*Making the distribution of income and opportunity more equal.**—For the last 35 years Government has become more and more concerned to increase the equality of income and especially of opportunity, to limit the risks of old age or ill health, and to compensate for the handicaps of poverty, racial discrimination, or disability. There has been a great variety of programs with these objectives, including direct transfers of income, food subsidy programs, social insurance programs, training programs, programs to provide social services, and more recently, medical insurance and assistance.

Managing publicly owned resources.—As a result of history, political preferences, or economic factors, the Government owns a wide variety of resources and provides to the public many different kinds of services. The publicly owned resources range from the electromagnetic frequency spectrum to the highways and federally owned mineral and forest lands. The services provided include such large-scale and diverse activities as the post office, the Federal health service establishments, and the FAA's air traffic control facilities. Government is expected to manage its resources and provide the services it is engaged in in an efficient manner. However, there is very rarely, if ever, a market test of the efficiency of the Government's activities, and since the Government is very often a monopolist, profit-maximizing behavior would not lead to a socially desirable result.

*Large-Scale Risk-Bearing and Innovation.***—There are many activities which would be socially desirable but which involve operation on a scale beyond that which is feasible for private enterprise, or that involve risks that are too large for an individual decisionmaker to assume but that may be tolerable when spread over society as a whole. Examples that illustrate both the problems of scale and risk are attempts to stem the decay of central cities (urban renewal requires in addition to the scale and risk-spreading available to Government, the exercise of Government's power of eminent domain) and attempts to open major new areas of technology such as nuclear power. These

*Further discussion of this issue is found in the paper by Weisbrod in vol. 1 of this collection.

**Further discussion of this issue is found in the paper by Zeckhauser in vol. 1 of this collection.

activities must be carefully examined and periodically reviewed to insure that they do not merely provide windfalls for private activities that would be undertaken even without Government subsidies.

Increasing the Efficiency of Private Markets.—Government attempts to increase the efficiency of private economic activity in a number of ways, including the strengthening of competition, and the regulation of injurious trade practices. Especially important in this role, however, is the provision of information to labor, businessmen, and consumers, permitting them to behave more efficiently.

2.3 SIGNIFICANT DIFFERENCES BETWEEN THE ANALYSIS OF NATIONAL SECURITY PROGRAMS AND DOMESTIC PROGRAMS

The principal difference between the application of systems analysis to domestic programs and to national security programs is to be found in the differences between the Government's role in these two areas. The overriding objective in national security, as discussed above, is the provision of a public good. Most domestic programs of any significance, however, are involved in more than one of the several Government roles discussed above. The multiplicity of Government's roles in domestic programs is reflected in a multiplicity of objectives for individual programs, greatly complicating the analysis of the program and the comparison among alternatives.*

Consider, as an extreme example, programs for the education for the disadvantaged, such as title I of the Elementary and Secondary Education Act of 1964. Virtually all of the Government roles are involved. Like other educational programs, this one is intended to provide the public good of a better educated electorate; it is clearly an attempt to redistribute resources in a way that will equalize the opportunity of the children involved; to the extent that it leads them to become more productive members of the labor force, it will increase the efficiency of private markets; and, to the extent that it results in less dependence on social services and less frequent criminal behavior, reducing the cost of social services and law enforcement, it will realize spillover benefits. Moreover, it is likely that aid furnished to local school districts to improve the education of the disadvantaged partly displaces local resources for that purpose (although such is counter to the intent of the legislation). Therefore, it represents a contribution to a developing role of the Federal Government not discussed above, that of supporting State and local governments with Federal revenues.

Clearly this large program has very complicated objectives. Some of them are joint products, others are in conflict with each other. But whether they are mutually reinforcing or in conflict, the comparison of an alternative way of improving the status of disadvantaged children with the title I programs, would involve comparisons in many dimensions. Since there is no hope of finding a common unit to measure the public good of a better-educated electorate against the benefits due to redistribution of income or increases in individual productivity, it is highly unlikely that an analysis of alternatives will result in a conclusive preference for one program over another.

*Further discussion of this issue is found in the paper by Freeman in vol. 1 of this collection, and Feldman in this volume.

This complexity is also encountered in the analysis of national security programs, but to a lesser degree. Although the objectives of national security programs are simpler, there are some respects in which analysis in that area encounters more severe limitations than in the domestic area. Since the predominant objective in national security programs is the provision of a public good, and since a public good cannot be evaluated in the marketplace, it follows that the outputs of national security programs cannot be measured in terms of dollars. Consequently, the Department of Defense has developed cost-effectiveness analysis. Cost is used as a measure of the inputs to the program, but the outputs are measured in physical terms. This means, for example, that one cannot compare the efficiency of resources spent on improving our Strategic Air Command against resources spent on improving our forces for guerrilla war. It also means that analysis must be supplemented by judgment to arrive at the desirable level of spending on national security programs as a whole.

To the extent that Government domestic programs are involved in the provision of public goods, or the redistribution of income or opportunity, the same limitations apply. However, many Government programs are predominantly concerned with spillover effects, with the provision of marketable goods or services by the Government, or with improvements in the efficiency of private markets. In these cases, analysis can aspire to go beyond cost-effectiveness analysis to measure both costs and at least some benefits in dollars. Even in such cases, however, the analysis is limited by a variety of conceptual problems and data gaps.

The lack of sound theory and data with which to analyze most domestic programs points to another difference between systems analysis in the national security and domestic program areas. For over 20 years, the Department of Defense and the military services have spent large sums of money on systems analysis of national security choices. As a result, there exist substantial analytic organizations within the Department of Defense as well as large independent ones outside. Large numbers of highly trained people from many disciplines have for years been working together on the analysis of national security programs. They have developed the data and analytic models needed to evaluate programs. Prior to 1965 this situation was a very rare exception in most domestic agencies. As a result, the newly developing analytic organizations in the domestic agencies must begin with very little in the way of accumulated knowledge or experience in the program areas concerned.*

Finally, and in some way perhaps the most pervasive of the differences between the analysis of national security and domestic programs, is the greater political sensitivity of decisions about domestic programs. Although individuals, special interest groups, and sectional interests do sometimes play a role in national security decisions, that role is circumscribed by the overriding common interest in providing the Nation's security. In most domestic programs the question of who benefits is much more important. Analysis can shed light on the distri-

*Further discussion of this issue is found in the paper by Wholey in vol. 1 of this collection.

bution of the benefits among the beneficiaries, but the resolution of conflicts among individual interests must be done by the political process. In the leaf-burning example above, analysis may reveal that either a tax on leaf burning or a subsidy to leaf removal would be a more efficient solution than allowing the leaf burning to continue, but analysis will not be able to choose between a subsidy for leaf removal, borne by the community at large, or a tax on leaf burning, which will be borne by the man who has to get rid of the leaves. Thus analysis, properly conceived, complements the political process and cannot replace it.

3. PROGRAM BUDGETING IN PPB

Program budgeting is, of course, a good deal older than either PPB or systems analysis. Students of budgeting assert that the basic ideas have been in evidence since at least the early part of the 20th century, and the idea received powerful support from the postwar Hoover Commission.

Program budgeting is often described as involving the presentation of budget data in a classification system based on output categories rather than input categories. Sometimes it is described instead as involving categories based on objectives. When the PPB system was introduced, agencies were asked to review their objectives and to devise program categories that were based on the systems of objectives resulting from the review. In particular, in order to free the so-called "program structure" from the traditional budget classification schemes, agencies were instructed to disregard traditional classification systems in developing their program structures.

The attempt to construct program structures that cut across organizational lines and existing appropriations accounts, is closely related to the tendency of systems analysis to ignore predetermined definitions of a problem and follow instead the boundaries of the decision. Thus, it was natural for an alliance to occur between systems analysis and program budgeting. The program budget was to show *all* the costs and outputs relevant to each program decision.

The alliance has, however, resulted in some confusion and excesses in abandoning the older classification systems. To begin, there were some who confused the presentation of costs and outputs in the program budget with the analyses themselves. A program budget does not directly assist in choosing among alternatives, for it shows only the cost of outputs of one specific set of choices. It does not present a comparison among alternative choices, which is the essence of program analysis. The program budget serves instead as a standard reference document displaying the costs and output implication of the approved programs. It also serves as a base for subsequent reviews of the program. Thus the program budget provides the link between the analysis and the budget.

In doing so, however, it imposes significant administrative costs which need to be considered in the design of the program structure. These costs arise because the program structure supplements rather than replaces the older classification systems based on appropriations and organizational lines. So long as the Appropriations Committees of Congress choose to enact the budget into law in terms of the older appropriations structure, it will be necessary for the executive branch

to continue to prepare the budget in those terms. Moreover, apart from the preferences of the Appropriations Committees, so long as there is divergence between organizational structure and the structure imposed by the analysis of program decisions, it will be necessary to have separate classification systems for planning and for program execution. Execution must, of course, be manageable mainly within organizational boundaries or a reorganization is indicated.

For these reasons, it appears that most agencies will have to develop and maintain at least two distinct classification systems for their activities and must sustain the workloads of doing so. This being the case, divergence between the classification systems should be justifiable on the grounds of contribution to program analysis, and should not be undertaken merely in a spirit of innovation.

4. PLANNING IN THE PPB SYSTEM

An unsophisticated approach to Federal planning would simply attempt to prejudge, in its entirety, the budget for each year of the planning period. Such an approach ignores the uncertainties inherent in planning. It would also raise arguments concerning decisions which need not yet be taken. A more realistic approach to planning treats the projection of programs as having several tiers.

The bottom tier of the planning structure is the projection of the cost and output implications of decisions *already made or currently proposed*. A second tier consists of the cost and output implications of those future decisions necessary to achieve *currently approved* goals in *specific* programs, such as the 10-year housing goal adopted by President Johnson.* And, finally, it is possible to project the aggregate level of agency budgets by working from the top down, estimating a likely or desirable level of future Government expenditure as determined by fiscal policy, and then allocating portions to agencies on the basis of broad priorities. Estimates of the output implications of this tier may not be possible since the program activities will not generally be sufficiently defined.

A multitiered approach is essential to planning in a bureaucratic environment. If upper levels of an organization request plans from subordinate levels without specification of several distinct tiers as indicated above and without imposing future resource constraints, they will get blue-sky estimates. Or, if budget ceilings are imposed, the subordinate levels will often omit some high-priority programs from the plan to bring pressure to bear to restore them and thus to raise the budget.

We might call this response "Portia's Ploy" from its resemblance to the way in which Portia protected her client against Shylock in the *Merchant of Venice*. Shylock, it will be recalled, after proving his case at law, was told to take his pound of flesh—but only flesh and no blood and from the victim's heart—and he was threatened with execution if the victim died. The analog, when the Budget Bureau plays Shylock, is that the agency will all too often offer up that program which is closest to the President's heart in response to a request for

*Further discussion of this issue is found in the paper by Ross in this volume.

their priorities within a budget ceiling. This device plays hob with the development of a decentralized planning process.

In addition to problems like Portia's Ploy, planning must cope with uncertainties about the general state of budget stringency in future years and about future priorities. Because of these problems, attempts to develop *overall* agency and Government budget *totals* for a period as long as 5 years are more valuable for the stimulation derived from the process of developing them than for their reliability as a guide to future activities. Consequently, a planning process that deals only with the total of the three tiers discussed above is likely to intensify bureaucratic bargaining unnecessarily and embarrass an administration that must defend future budget totals with little basis in analysis or fact.

A multitiered approach, on the other hand, offers several useful contributions. The projection of commitments resulting from past or currently contemplated decisions is essential to preserve the future flexibility of the President and to avoid the problem of starting a number of programs which later will not only absorb the resources that might be wanted for future new program starts, but which themselves may be underfunded as a result of failure to take account of the future growth in the resource demands implied by current program decisions. In some cases, the future growth is obvious but is nevertheless not systematically taken into account without a formal planning process. "Commitments" in the sense intended here is a broader term than contractual or statutory commitments. It also embraces commitments that arise as a logical consequence of program decisions. When we buy a hospital or a truck there is no legal commitment to operate it over a period of years, but there is a clear, logical implication that will do so. In fact, for the Government to walk away from a newly built hospital would be a very embarrassing act. Commitment projections offer a way to take such implications into account.

In other cases, the implied commitment may be less obvious but is, nonetheless, real. For example, if the Federal Aviation Agency were to request funding for a new ground control radar system at one or two heavy traffic airports on a demonstration basis, it would be difficult to withhold subsequent installation at other airports with equally heavy traffic. The commitment to install systems at other similar airports implied by the decision to install the first one or two should be considered in deciding whether to proceed at all. The projection is also necessary to test whether the aggregate of currently contemplated decisions will exceed any likely or desirable level of commitment of future resources.

The second tier of projection, comprising the costs and outputs necessary to reach selected future goals, permits the President to espouse high-priority goals, gain public support for them, and provide guidance to the bureaucracy to achieve them in an orderly fashion. Such commitments to major future goals should require the approval of the President in order to avoid the tendency for each agency to rush in with its high-priority programs with the likelihood of overcommitment of resources.

Finally, if the first two tiers of projection are clearly identified, agencies may usefully be requested to propose an overall plan for the expenditure of likely levels of total budget resources in order to

permit them to display their priorities to the President and the Budget Bureau. In my view, however, the political and administrative costs of attempting to arrive at approved set of government-wide agency budgets for a 5-year period is not worth the cost.

5. PROBLEMS ENCOUNTERED IN THE INTRODUCTION OF PPB TO DOMESTIC AGENCIES*

This section will recapitulate some of the problems mentioned in the discussion above, and also introduce some new ones. The problems are largely related to the differences between PPB in the domestic and the national security areas already discussed. They fall into three main groups:

- The lack of trained and experienced people, conceptual frameworks, and data.
- Bureaucratic problems.
- Problems arising from conflict of individual interest among the public.

The first of these has been adequately discussed above. The other two will be taken up in turn.

5.1 PROBLEMS ARISING OUT OF THE OPERATION OF A BUREAUCRACY

Perhaps the root problem besetting the operation of the PPB system during its first 3 years of operation has been the inability, except in a relatively small number of cases, to state issues for decision in terms of a range of relevant alternatives. Most often, issues will be stated in terms of acceptance or rejection of a specific program proposal. When alternatives are stated they are very often a mere mechanical compliance with the requirement to do so imposed by the Budget Bureau.

An almost classical style for the statement of alternatives has evolved. The alternatives most often will be stated in terms of three possible levels of funding for a given program—zero, quadrupling of the current program level, and a 10-percent increase. It is not terribly difficult to guess which alternatives the agency wants chosen. Issues involving a reorientation of the objectives of the program, or the mix of the programs intended to serve a given purpose are much less frequently encountered.

The absence of realistic, relevant alternatives is related to both the dearth of experienced analysts outside the Government and to the way in which a hierarchial bureaucracy like that of the executive branch normally tends to operate. Each level wants to receive alternatives from those below—and to pass only the preferred course of action to those above. To rely upon a bureaucracy to generate alternatives is to encounter a dilemma.

Most of the operational experience necessary to suggest a practical new approach to the problem is to be found at relatively low levels in the line organizations of the agencies. On the other hand, people at that level may lack the breadth of view as well as the incentives to suggest

*Further discussion of this issue is found in the papers by Carlson in vol. 2, and Marvin & Rouse in this volume.

changes. As one moves toward the peak of the hierarchy, in the White House and the Budget Bureau, the breadth of view increases, and certainly the general level of ability is high, but the familiarity with operational details is missing. Even though the people involved at those levels may have had substantial experience in one or another part of Government, their experience is too selective and their exposure not recent enough to afford the kind of familiarity that generates approaches that are both new and practical.

Moreover, the conflict of interest between higher and lower levels of the Government, and the resulting bargaining situation, has affected the overall development of PPB. The decision to introduce PPB comprehensively, to all of the major domestic agencies at once, made inevitable the assumption of a major role by the Bureau of the Budget. There were both advantages and disadvantages of this decision and it is not my purpose now to evaluate it in retrospect.*

The resulting identification of PPB as a system, of, by, and for the Bureau of the Budget has, however, been a substantial disadvantage in the development of PPB. If, instead, the system had developed in a way that led agency and department heads to identify themselves with the system more, I believe that some of the bureaucratic problems would have been alleviated. It is true, of course, that bargaining occurs not only between departments and the Budget Bureau, but also between the office of the secretary of a department and his bureau chiefs. Nevertheless, the responsibility of a department head is more direct than that of the Budget Bureau and it is possible for him and his staff to provide more continuous, better informed, and more forceful guidance for the activities of his agency.

Bureaucratic bargaining also manifests itself in attempts to make agencies establish priorities. The usual response to a request for a ranking of activities by priority is that all agency activities are vital to the welfare of the country and would not be otherwise undertaken. If a more operational approach is taken, and the agency is given a budget planning figure within which it must make allocation decisions, the response described above as Portia's Ploy is often the result.

Finally, bargaining within the Government is not restricted to internal jockeying in the executive. Relations between the executive and the Congress are, of course, also a mixture of cooperative and adversary proceedings. Moreover, whereas the executive is organized, at least roughly, in a hierarchial way, the Congress represents a much more complex set of arrangements of diverse interests and responsibilities.

The problems that have arisen between the Congress and the executive in the development of PPB are correspondingly diverse. They are characterized under three headings; lack of PPB output, lack of congressional access to the existing PPB output, and lack of interest (or actual antipathy) in some quarters of Congress to the things that PPB is striving to do.

Of the outputs of PPB, the Congress probably is and should be most concerned with the alternatives and the comparisons among them that PPB is to generate. Because of separation from the operations of

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection

Government agencies (even more so than in the case of the White House and the Bureau of the Budget), and because of staff limitations relative to the executive branch, the Congress will probably continue to be dependent upon the information and analyses generated by the Executive. The presentation of decisions in terms of choices among relevant alternatives, together with analyses comparing the alternatives and presenting the basis for the choice proposed by the Executive, should permit the Congress and its staff to ask more relevant questions, and should provide the data base to make it possible for even a small staff to test the sensitivity of the conclusions reached by the Executive to changes in key assumptions. Thus, when PPB has overcome the problems of generating alternatives and of making systematic comparisons among them, it should offer output of high value to the Congress.

There is, however, a further problem, the problem of congressional access. With the exception of data concerning the current budget year, the executive branch has preferred not to release PPB material for either congressional or public examination. The reluctance of large organizations to make explicit the basis for their choices would result in sterilization of PPB material if it were all routinely to be released for scrutiny outside the executive. As it is, the discussion above indicates that it is very difficult to get a high degree of candor even within the executive family.

An answer to this problem may be found in distinguishing the degree of sensitivity of the different kinds of PPB materials. The most sensitive documents of all are the program memorandums which are intended to present, for review by the White House and the Budget Bureau, the choices recommended by the heads of the agencies and departments, together with the basis for those recommendations. Less sensitive are the analytic comparisons among alternatives which often are far short of conclusive with regard to policy choices. Because of the limitations of quantitative analysis, especially in regard to the treatment of conflicts among individual interests, the results of the analytic comparisons will require a considerable mixture of judgment and advocacy before a decision will emerge. But it is precisely in regard to the quantitative analysis that the Congress needs most to rely upon the executive. A possible solution, therefore, would be to continue to treat the program memorandums as documents privileged to the executive, but to make the analytic studies available for public use.

Planning data for future years beyond the budget year has also been a sensitive item of PPB output. So long as the 5-year plan was comprehensive and appeared to commit the Executive to decisions which the President had not yet either considered or resolved, the data on costs, outputs, and budget totals for the future years was also considered highly sensitive. If, however, the kind of multitiered planning discussed above is developed and applied, there should be much less sensitivity about making public the commitments implied by decisions already taken by the executive. Moreover, in the case of selected future program goals, the President will have positive reasons for wanting to make them public in order to mobilize public understanding and support.

5.2 CONFLICTS AMONG INDIVIDUAL INTERESTS

Apart from, but related to, the bargaining among the various elements of the Government, is the bargaining that goes on to resolve conflicts among the interests of the public. In the example discussed above, it is quite likely that John Doe, the leaf burner, would attempt to resist by political means, any attempt to impose a tax on leaf burning. Richard Roe, on the other hand, the unwilling inhaler of John Doe's smoke, would probably resist an increase in taxes to subsidize the collection of leaves. As indicated above, analysis cannot say whether it is preferable to deal with the spillover effect by a tax or a subsidy. It can, however, say that either would be preferable to a situation in which John Doe goes on burning leaves, and Richard Roe goes on inhaling the smoke. All too often, an impasse is reached because John Doe merely sees the impending tax on leaf burning and Richard Roe sees the tax to support a subsidy on leaf removal and neither is clearly aware of the implication of one course or the other or of the possibility of some compromise that might be acceptable to both.

Analysis cannot be expected to replace the political horse-trading by which many conflicts are resolved in our society. Rather, it has great potential for making that horse-trading a more effective process by clarifying the implications of alternative choices or by generating new and more effective alternatives, and where there is an over-riding common interest, by helping to clarify and present the case for that interest.

Another limitation of analysis is its inability to establish, without the aid of the political process, desirable priorities among such broad aggregates as health, education, economic development, conservation, and national security. In order to make analytic comparisons among alternative patterns of resource allocation, it is necessary that costs and outputs of the various activities be comparable. Obviously, there is no unit of measure that will establish the relative benefits of education against those of health or national security. No computer, therefore, will ever produce an allocation of resources among these activities that has any claim to optimality, let alone compelling appeal to the electorate. Choices at this high level of aggregation must be developed on the basis of public preferences that largely find their expression in the political process.

Nevertheless, analysis does have an important role to play in the making of such decisions. All too often, at present, the choice between spending an additional billion dollars on urban transportation as opposed to, say, the education of disadvantaged children, is a choice between putting resources into one black box as opposed to another. For public preferences to find intelligent expression, it is necessary that the public know something about the benefits to be gained from an increment of expenditure on urban transportation and an increment to education. The comparison will still have to be made in the minds of individual citizens and elected officials, but the factual basis for making such a comparison can be greatly improved.

6. SOME DIRECTIONS FOR THE IMPROVEMENT OF PPB

6.1 GREATER INVOLVEMENT BY DEPARTMENT AND AGENCY HEADS*

The interest and attention of the head of each department or agency or of his deputy are crucial to the success of PPB. The prototype of PPB was created by Secretary McNamara who considered the job of managing the resources of the Defense Department as among his prime responsibilities. Other cases where PPB has taken hold almost all show some similar pattern of interest on the part of the agency head or his immediate deputy. Although the President expects his cabinet officers and their immediate subordinates to assist him in many capacities, he must accord high priority to the role of resource manager if Government resources are to be used more efficiently. The Bureau of the Budget will continue to have an important role to play in monitoring the development of PPB, but steps should be taken to make PPB responsible to the needs of department and agency heads, and to encourage initiatives on their part in developing the system further. Increasing the ability of the Budget Bureau to do independent analysis can help to stimulate the departments and agencies to improve their own analyses.

6.2 MORE AND BETTER ANALYSIS AND DATA

If we are to improve our understanding of the program choices open to us, it will be necessary to devote much larger resources on a long-term, continuing basis to efforts to improve our conceptual understanding and to increase and improve the data available. Some significant steps in this direction have already been taken.

In particular, a start has been made on the systematic evaluation of the performance of programs already in existence. In several recent pieces of legislation, the statute contains authorization and direction to the secretary of the agency involved to spend up to 1 percent of the authorized funds on evaluation of the program, either in his own office, in the field organization of the Federal department, or (in the case of grant-in-aid programs) at the State and local level. Such provisions should be contained in more legislation, perhaps in all new authorizing legislation, and other methods should be sought to indicate the intent of the Congress to encourage and support program evaluation.

Improvement is also needed in our ability to evaluate new programs that have no existing counterparts. An example of an attempt to do this is the OEO experiment to determine the effects of a negative income tax on such aspects of behavior as labor force participation and consumption patterns.** Imaginative, controlled experiments of this sort are essential to improving our ability to design new programs. But it is also essential that both the Executive and Congress require more systematic presentation of the implications of proposed new legislation and comparisons with alternative ways to accomplish the desired ends.

*Further discussion of this issue is found in the paper by Marvin & Rouse in this volume.

**Further discussion of this issue is found in the paper by Levine in this volume.

Most of all, it is necessary to assemble groups of people who are technically trained in analysis and who have an understanding of the substantive areas which they are to analyze. Because of the twin problems of institutional blinders on those currently involved in operations, and lack of relevant experience on the part of outsiders, a pattern of rotation from analytic positions to operating positions should be developed. Such rotation between program analysis and program operation could occur within Government between an agency line organization and the office of the secretary, between line organizations and an expanded program evaluation staff either in the Budget Bureau or elsewhere in the Executive Office of the President, or between Government line organizations and private independent research organizations of the sort that have proven useful in the national security area. A start has been made in increasing outside expertise, but much more needs to be done, and the domestic agencies must develop patterns of long-term continuing funding of research like those in the Defense Department. To the extent that the problems encountered are interagency problems, it will be especially necessary to develop groups of analysts either in the Executive Office of the President or outside Government.

To help meet the requirements for trained people, Government should encourage universities, through graduate fellowships, to develop curricula that combine analytic training and substantive courses in the applied fields of health services, urban transportation, the evaluation of education, and so on.

6.3 INCREASED ACCESSIBILITY TO PPB MATERIAL

Although the program memorandums should continue to be privileged documents if they are to be useful at all, analytic studies displaying alternative programs and comparing their costs and benefits should routinely be made available to the Congress and to the public. Such analyses need not and should not attempt to be conclusive and reach definite program recommendations. That can be left to the program memorandums in the executive branch and to the legislative process in Congress. The analyses should, however, provide a common basis in fact for making program choices. In addition, as the executive develops the projection of the commitments implied by decisions that have been made, it should make these projections available, together with the projection of selected programs to realize future goals approved by the President.

6.4 THE FURTHER DEVELOPMENT OF PROGRAM BUDGETING

Since the maintenance and development of program budgets will be superimposed on the requirement to maintain budgets along traditional lines in many cases, the existing program structures should be reviewed to determine whether the added workload is justified by their contribution to the understanding of the agency's program activities. The criterion by which the program structure should be judged should correspond to the lines of definition of the analyses required to assist in making program choices. Other things being equal, ease of translation between program structure and appropriation structure or organizational structure should be considered in reviewing the program structure. Wherever possible, of course, a single, integrated

classification system should be devised and proposed for acceptance by the executive and the Congress.

As yet, little has been done to establish a Government-wide program structure. In the many areas where departmental responsibilities overlap, the development of departmental program structures that are consistent with one another and that will correspond to the program decisions is necessary to improve our understanding of resource allocation and to provide a common starting point for analyzing subsequent program decisions. A substantial interagency effort, led by the Bureau of the Budget, should be undertaken to develop a Government-wide program structure and consistent definitions of costs and outputs in areas where agency programs overlap.

6.5 IMPROVING THE PPB PROCESS

One of the purposes of PPB is to permit program evaluation to go on continuously during the year rather than under the crisis atmosphere of fall budget review. The schedule for development of PPB material and its submission to the Budget Bureau has been extremely late in each of the three PPB cycles so far completed. In part, this has been the result of changes in the process from year to year, but more fundamentally, it has stemmed from lack of interest on the part of the department heads and unwillingness to commit themselves to decisions in the spring. Greater interest on the part of the Secretary will help to alleviate this situation, but it is also necessary to distinguish the analysis preparatory to decisionmaking from the decision itself. The spring review should concentrate on reaching an understanding of the costs and benefits of the alternatives. Formal choices among them by high-level officials should be separated from this process in order to avoid delaying the analytic process. Such choices can be made in the fall if their implications are understood as a result of the spring review.

POLICY ANALYSIS AND CONGRESS*

BY NELSON W. POLSBY

Nelson W. Polsby is Professor of Political Science at the University of California at Berkeley.

The role of Congress in allocating the Federal budget and determining public policy direction is crucial. For this reason, many would argue that the lack of analytical activity in the Congress is a serious bottleneck to increasing the role of economic analysis in the decision-making process. Professor Polsby in this paper analyzes the organization and functioning of both Houses of the Congress and "explores ways in which congressional decisionmaking can be made more receptive to the kinds of policy analysis that are carried on elsewhere, both within the Government and outside it."

Professor Polsby argues that Congress does play an analytic role in policymaking. "Congress, in the normal course of events, gathers great amounts of information, processes this information according to reasonably well-known criteria, and matches what it learns against goals." That is, it conducts a tremendous amount of policy analysis. However, while the volume of policy analysis is large, Professor Polsby argues that it is mostly implicit, and takes place under conditions of fragmented and adversary decision-making. He offers two recommendations designed to increase the quantity and quality of explicit policy analysis in the Congress. Both of them relate to improvements in committee staffing that would help to increase the capacity of Congress to deal meaningfully with "sound and sophisticated explicit policy analysis."

Introduction

This paper will discuss the U.S. Congress as a machine for making decisions about public policy. It will ask in what sense Congress engages in analytic activity in the process of decisionmaking, and will explore ways in which Congressional decisionmaking can be made more receptive to the kinds of policy analysis that are carried on elsewhere, both within the Government and outside it.

The fact that Congress is organized differently from conventional bureaucracies leads many observers to assert overhastily that congressional decisionmaking is inefficient, cumbersome, and in need of instant reform. Consider, for example, the fact that Cabinet officers are asked to justify certain aspects of their programs in much the same language before authorization and appropriation committees in both Houses—sometimes adding up to four presentations in all. Clearly an inefficient use of a busy executive's time, according to the busy executive and his friends.¹ Yet this same busy executive as a matter of course insists that programs coming up the line to his office be justified repeatedly to program review committees, bureau chiefs, department level staff, and departmental budget officers, and he would think nothing of justifying the program again to other interested executive branch departments, the President and the Budget Bureau.

¹ A recent summary of this sort of complaining is contained in John P. Leacacos, *Fires in the In-Basket* (Cleveland and New York: World, 1968), pp. 180 ff.

*Copyright © 1969 by Nelson W. Polsby.

Cabinet-level officers quite commonly make presentations, formal and informal, justifying their programs to the general public, to interest groups, to newspapermen. Why, then, does the alleged inconvenience of an executive officer of the Government provide an excuse for the recommendation that Congress change its structure if the same reasoning does not lead (for example) to an outcry to consolidate those three well-known extra-constitutional entities "Face the Nation", "Meet the Press", and "Issues and Answers"?

This is one of the little mysteries of Washington politics wrapped inside the bigger enigma that the organizational structure of Congress presents to most of the outside world. As an outsider myself I cannot pretend to know all the ins and outs of congressional decisionmaking, but I believe nevertheless that some attempt has to be made to comprehend the unique qualities of the two Houses in order to capture a sense of why they interact as they do with one another, with the executive branch, and with the rest of their environment.

The structure of an organization, after all, maps the topography of its economizing devices. So in viewing the structures of the House and the Senate whole and from a distance, it may be easier to see how rational calculation enters into the wiring diagram of congressional decisionmaking, how Congress does research, how "politics" aids and deters rational calculation and how increased professionalization in policy analysis can improve the political position of generalist politicians.

I

As institutions, the House and Senate differ markedly in their essential character. The House is a highly specialized instrument for processing legislation. Its great strength is its firmly structured division of labor. This provides the House with a toehold in the policymaking process by virtue of its capacity to farm out and hence, in some collective sense, to master technical details. House Members are frequently better prepared than Senators in conferences,² and usually have the better grasp of the peculiarities of the executive agencies they supervise. This is an artifact of the strong division of labor that the House maintains: Members are generally assigned to one or two committees only; floor debate is generally limited to participation by committee members. There is an expectation that Members will concentrate their energies rather than range widely over the full spectrum of public policy. Patterns of news coverage encourage specialization; general pronouncements by House Members are normally not widely reported. Senators, because they are fewer, more socially prominent, and serve longer terms (hence are around long enough for newsmen to cultivate) and allegedly serve "larger" districts, can draw attention to themselves by well-timed press releases almost regardless of their content.

The coordination of an organism like the House is difficult because it cannot entail excessive centralization of power. Decentralization is

² Although the question has not been systematically studied in great detail, this seems to be a fair conclusion from a number of case studies. See, for instance, Richard F. Fenno, Jr., *The Power of the Purse* (Boston: Little, Brown, 1966), pp. 616 ff.; Gilbert Y. Steiner, *The Congressional Conference Committee* (Urbana: University of Illinois Press, 1951); James M. Landis, "The Legislative History of the Securities Act of 1933," *George Washington Law Review* 28 (1959-60), pp. 29-49; or the following recent comment by Senator Lee Metcalf (formerly a member of the House Ways and Means Committee): "No matter what the Finance Committee does or the Senate does, when we come back from conference with the House we have given in to Wilbur Mills. He runs both committees." *Washington Post*, January 14, 1969.

necessary for the House to maintain its capacity to cope with the outside world (that is, through its complex and specialized division of labor). And this in turn produces the House's major career incentive, namely the opportunity accorded a tenth to a fifth of its members to possess the substance of power in the form of a committee or subcommittee chairmanship or membership on a key committee. At present seniority acts as a bulwark of this incentive system, by guaranteeing a form of job security at least within the division of labor of the organization.³

Thus, as I once observed in another connection:

To that large fraction of members for whom the House is a career and a vocation, the longevity of members above them in the many hierarchies of the House—not the entirely predictable congressional election returns in their home districts—is the key to the political future.⁴

The essence of the Senate is that it is a great forum, an echo chamber, a publicity machine.⁵ Thus "passing bills," which is central to the life of the House, is peripheral to the Senate. In the Senate the three central activities are (1) the cultivation of national constituencies (that is, beyond State lines) by political leaders; (2) the formulation of questions for debate and discussion on a national scale (especially in opposition to the President); and (3) the incubation of new policy proposals that may at some future time find their way into legislation.

This is, in some respects, a novel conception of the Senate since it focuses upon an aspect of Senate life that is much deplored by aficionados of the "inner club" conception of the institution, who often defend the curious thesis that the persons anointed by the mysterious chemistry of Senate popularity are the very elite that keeps this Nation from the mob scene in *The Day of the Locust*.⁶

I think, however, there is considerable use in a democratic republic for an organization that encourages—as the Senate presently does—the generation of publicity on issues of public importance. One must grant there have been abuses in the pursuit of publicity by Senators; but Senate "great debates," investigations and hearings have also performed great public service.

Where the House of Representatives is a large, impersonal and highly specialized machine for processing bills and overseeing the executive branch, the Senate is, in a way, a theater where dramas—comedies and tragedies, soap operas and horse operas—are staged to enhance the careers of its members and to influence public policy by means of debate and public investigation.

³ Nelson W. Polsby, Miriam Gallaher and Barry Spencer Rundquist, "The Growth of the Seniority System in the U.S. House of Representatives," *American Political Science Review*, in press, September 1969, goes into the history of seniority more fully. See also Michael Abram and Joseph Cooper "The Rise of Seniority in the House of Representatives," *Polity* I (Fall, 1968) pp. 52-85.

⁴ Nelson W. Polsby, "Two Strategies of Influence: Choosing a Majority Leader, 1962" in R. L. Peabody and N. W. Polsby (eds.), *New Perspectives on the House of Representatives* (Chicago: Rand McNally, 1963), p. 244.

⁵ I am currently completing a paper, "Policy Initiation in the U.S. Political System," that makes the argument in detail that follows here.

⁶ Different points of view on the nature of the Senate are expressed by William S. White *The Citadel* (New York: Harper, 1956), Donald Matthews *U.S. Senators and Their World* (Chapel Hill: University of North Carolina Press, 1960), Joseph S. Clark *et al.*, *The Senate Establishment* (N.Y. Hill and Wang, 1963), and Ralph K. Huitt in Huitt and Robert L. Peabody *Congress: Two Decades of Analysis* (N.Y.: Harper and Row, 1969) especially pages 159-208. Huitt's position is closest to my own in Polsby, *Congress and the Presidency* (Englewood Cliffs, N.J.: Prentice-Hall, 1964) pages 31-46.

In both the House and the Senate the first commandment to newcomers is "specialize." But this means vastly different things in each House. "Specialize" to a Representative means "tend to your knitting": work hard on the committee to which you are assigned, pursue the interests of your State and region. In the Senate everyone has several committee assignments. Boundaries between committees are not strictly observed: occasionally a Senator who is not a committee member will sit in on a hearing if a subject interests him. On the floor, quite unlike the House, virtually any Senator may speak for any length of time about anything. So the institution itself gives few cues and no compulsions to new Senators wondering what they should specialize in. For the Senate, specialization seems to mean finding a subject matter and a nationwide constituency interested in the subject that has not already been preempted by some more senior Senator.⁷

It is a cliché of academic political science that in legislative matters, it is the President who initiates policy, and Congress which responds, amplifying and modifying and rearranging elements which are essentially originated in the executive branch.⁸ Not much work has been done, however, on following this river of bills-becoming and not-becoming-laws back to its sources. Where do innovations in policy come from *before* the President "initiates" them?

Old Washington hands know the answer. There is very little new under the sun. A great many newly enacted policies have "been around," "in the air" for quite a while. In the heat of a presidential campaign or when a newly inaugurated President wants a "new" program, desk drawers fly open all over Washington. Pet schemes are fished out, dusted off, and tried out on the new political leaders.

There is often a hiatus of years—sometimes decades—between the first proposal of a policy innovation and its appearance as a presidential "initiative"—much less a law. Commentators have greatly underestimated the role of the Senate in gestating these ideas, by providing a forum for speeches, hearings, and the introduction of bills going nowhere for the moment. This process of gestation accomplishes a number of things. It maintains a sense of community among farflung interest groups that favor the innovation, by giving them occasional opportunities to come in and testify. It provides an incentive for persons favoring the innovation to keep up to date information on its prospective benefits and technical feasibility. And it accustoms the uncommitted to a new idea.

Thus the Senate is in some respects at a crucial nerve-end of the polity. It articulates, formulates, shapes, and publicizes demands, and can serve as a hot house for significant policy innovation.

Hence, proposals to increase the structuredness of the Senate, to force germaneness in debate, to tighten committee assignment procedures, and reduce the number of assignments per Senator, misunderstand the nature of the Senate and the contribution it can uniquely make to the political system. What is needed in the Senate is as little structure as possible; its organizational flexibility enables it to incubate policy innovations, to advocate, to respond, to launch its great

⁷ A more familiar view of Senate specialization which in my judgment still has considerable merit may be found in Matthews (*op. cit.*) pages 95-97.

⁸ A good summary statement is in Charles E. Lindbloom *The Policy-Making Process* (Englewood Cliffs, N.J.: Prentice-Hall, 1968) page 86.

debates, in short to pursue the continuous renovation of American public policy through the hidden hand of the self-promotion of its Members.

II

What has this to do with analysis in policymaking? It suggests that the analytic roles that Congress plays in the process are somewhat more varied than the customary "President proposes, Congress disposes" overview would suggest. Let us decompose the policymaking process into stages.⁹

1. *Initiation.* How are policies initiated in the American political system? The process is by no means uniform, or clear. It is certainly not generally true that policy innovation begins with a Presidential message to Congress. For behind each Presidential message lurk months of man-hours of work and sometimes years of advocacy and controversy. The two great fountainheads of policy seem to be: (1) Sudden demands upon Government that spur bureaucrats to ad hoc problem solving which ultimately has to be codified or rationalized as "policy"; and (2) a longer range buildup in the society of some demand upon the Government where the formulation of a "solution" may first be made by a professor, or by technical support personnel attached to an interest group, or by a Government "expert." On rare occasions, experts attached to a congressional committee will initiate a policy. More often, I think, Congress is in on the beginning of a policy innovation because it provides the first sympathetic ear for an innovation concocted by outside experts.

2. *Incubation.* Many of our most important policy innovations take years from initiation to enactment. Surely, the idea of medicare, to take an obvious example, was not "initiated" by the Johnson administration in the 89th Congress. Proposals incorporating its main features had been part of the Washington landscape since the early Truman administration. Medicare, like other great policy innovations, required *incubation*—a process in which men of Congress often play very significant roles. Incubation entails keeping a proposal alive while it picks up support, or waits for a better climate, or while the problem to which it is addressed grows. Senators and (to a lesser extent) Representatives contribute to incubation by proposing bills that they know will not pass, making speeches, making demands for data and for support from interest groups favoring the proposal. Sometimes a sympathetic committee chairman can be persuaded to allow hearings on such a proposal. Hearings focus public attention, mobilize interest groups for and against, and provide an occasion for the airing of a proposal's technical justifications.

3. *Formulation.* When, finally, a proposal moves toward enactment, it is usually the executive branch that focuses the energy sufficient to overcome inertia. A Presidential priority is a tremendous advantage in clearing away obstacles, but the President's support is usually purchased at a price: the proposal becomes his. This is not merely a matter of credit, although who gets credit is no trivial matter. The executive branch begins the process of bargaining by including some features of the proposal and dropping others, adding bait here and

⁹ These are inspired by Harold D. Lasswell's "The Decision Process: Seven Categories of Functional Analysis," reported in N. W. Polsby, R. A. Dentler and P. Smith (eds.), *Politics and Social Life* (Boston: Houghton-Mifflin, 1963), pp. 93-105.

padding there. In some cases (e.g., foreign aid, civil rights), executive branch control over bargaining is tight and continues right through the legislative mill. In others (e.g., surtax, medicare), influential Members of Congress establish which provisions will survive and which will be sacrificed. Sometimes (e.g., HUD bill in the Kennedy administration) the most significant battle is precisely over who will control the bill.

4. *Modification.* The legislative gauntlet is too well known to require discussion here. The analytical questions at the focus of attention during this part of the policymaking process are: Who wants the proposal? Who wants it to fail? How resourceful and well mobilized are they? By what means (invocation of party loyalty, promises of future help, logrolling, the sacrificing of certain provisions, etc.) are coalitions for and against the proposal built? In addition, committee staffs generally assemble competent justifications on the merits for legislation. Often these reports reflect work done by the downtown bureaucracies. Hearings provide additional evidence on the merits, as do interest group representatives on a more informal basis.

5. *Appraisal.* After a bill is enacted, it goes into effect. Presumably this has an impact upon members of the general public who in turn communicate with their Congressmen about this and myriad other topics. By monitoring the tides of complaint and appeals for assistance from constituents, Congress keeps track of the activity of the entire Federal Government. Congressmen learn quickly enough which agencies are throwing off benefits to their constituents, which cause the people back home grief, which preoccupy them, which they ignore.

This appraisal process operates day and night on a piecemeal basis, and separately from the more formally organized oversight activities of the Congress: Investigative hearings, budgetary hearings, confirmation hearings, onsite inspections of physical plant, informal briefings, conferences, and so on.

III

In short, Congress in the normal course of events gathers great amounts of information, processes this information according to reasonably well-known criteria, and matches what it learns against goals. That is, it conducts a tremendous amount of policy analysis. This simple fact is generally somewhat obscured by two important conditions under which policy analysis takes place on Capitol Hill. Much congressional policy analysis takes place under adversary circumstances. Thus, congressional decisionmakers ordinarily cannot enjoy the luxury of examining alternative means to stipulated ends. In an adversary process, ends are not stipulated but contested. Agreement on means is often sought as a substitute for agreement on ends. Ends are often scaled down, pulled out of shape, or otherwise transformed. In short, from the standpoint of an outside observer whose focus is as often as not upon some pressing problem in society, the congressional process of policy analysis looks chaotic at best, perversely insensitive at worst.

Insensitivity in congressional policy analysis is not altogether curable. It can come about because the strength of a demand in society as it is felt by an observer has no counterpart equally strong within the congressional process itself. Sometimes Congress does not reflect "needs" as defined in the society at large because Congress itself is

malapportioned, or because the "wrong" sorts of people dominate the relevant committee. Thus, a wave of shortrun, intense demands may break futilely across the superstructure of any institution. Given the stately metabolism decreed for it by the Founding Fathers, Congress could hardly be expected to operate efficiently with respect to short-run demands in the best of circumstances.

The second basic condition under which Congress conducts policy analysis is inexplicitness and fragmentation. All knowledge on a particular topic is rarely collected in a single spot or systematically marshaled. Nevertheless, the executive branch does impose some order, principally because the congressional division of labor is organized according to executive agencies so as to provide oversight. Thus jurisdictional anomalies in the executive are echoed in the legislature. Fragmentation can be spatial—as when a bill's best friends and worst enemies are not members of the relevant committee—or temporal, when excellent analytic work is done in the incubation process but is not picked up in formulation or enactment stages. There are often structural as well as coincidental reasons for this phenomenon when it occurs—jurisdictional jealousies between committees may prevent efficient communication, for example.

All this suggests that the analytic activity undertaken by Congress, while formidable in amount, is inexplicit with respect to some matters regarded as crucial outside and systematically skewed toward the reduction of the sorts of uncertainties about which most members of society are indifferent. Yet Congressmen, as elected officials, *must* ask who will get the credit—or the blame. They must know who is for what and how badly because these matters affect not only their own future efficacy but also their present chances of assembling a coalition "on the merits."

Is there a practical alternative to policy analysis in which alternative policies are put to such tests? The alternative, for a legislature, is total passivity. Legislative arenas, as contrasted with legislative institutions having transformative effects—and therefore insensitivities—can faithfully reflect the balance of forces as they are generally arrayed in society, and they can if they like commission policy analysis. But they are powerless to incorporate such analysis into their deliberations because legislative arenas do not deliberate, they merely transmit. The sponsorship of research by parliamentary bodies that are principally "electoral colleges" is at best a means of lobbying the cabinet or the prime minister.

IV

Under the circumstances, is there any use in considering the improvement of explicit policy analysis by Congress? I believe the answer is yes, principally because most substantive policy that Congress is concerned with affords nearly complete freedom from constituent knowledge, much less pressure. The adversary process may be muted or perfunctory, or capable of drastic modification by the infusion of detailed technical knowledge. Thus explicit policy analysis, although it comprises only a fraction of the policy analysis actually going on at any one time in Congress, is well worth improving.

Where does Congress get technical knowledge? Principally from committee staff personnel, who virtually monopolize the activity of

explicit policy analysis in most subject matter areas. But while the executive branch has systematically been engaged in professionalizing its search for technical detail over the past decade or more, Congress on the whole has not done so. It is romantic for Congressmen to think of themselves as not in need of expert and detailed explicit analysis because they are "generalists". Generalism is too often a genteel name for ignorance. Like all other modern institutions, Congress can only preserve its autonomy and effectiveness by reducing ignorance.

Are there means by which Congress can do so? Two such come readily to mind. Both seek to apply to congressional committee staffs lessons from the executive branch, where the professionalization of economic forecasting and defense procurement has led to tremendous increases in the power of political decisionmakers to identify options and choose among them. This is precisely the battle many Congressmen feel they are losing. Yet if they choose to do so, they can professionalize their own committee staffs, thereby increasing the efficiency of their explicit analytical activities and enhancing their own knowledge and power.

To "professionalize" implies continuous contact with a community outside the world of Capitol Hill. Professional economists, operations researchers, psychologists, and so on, maintain standards of performance by participating in professional communities through meetings, scholarly journals, and similar specialized communications media. Typically, nowadays, the top economists of the executive branch—the men who formulate fiscal policy, antitrust policy, international trade policy, and so forth—are first and foremost professional economists. The primacy of loyalty to professional craft standards on the part of executive technical personnel vastly increases the probability that the options presented to political executives will be feasible and technically sound.

Typically, congressional committees are staffed by an older, less effective process of patronage.¹⁰ This produces loyal service, and by the standards of an earlier day, highly competent service. But unswerving loyalty to the chairman is seldom enough to produce technically advanced criticism of executive proposals, sophisticated insight into alternatives, or sensitive awareness of emerging problems in the world. Yet these are what Congress needs. Hence, two modest proposals, both of which have already been tried out in small ways on Capitol Hill. Committees should be encouraged to constitute outside advisory groups to advise the chairman on the technical competence of the work they are receiving from their staffs.¹¹ Secondly, exchanges

¹⁰ The only scholar who seems to have devoted close attention in recent years to the professional capabilities of committee staff is John F. Manley, who has looked with some care into tax policy. See his "Congressional Staff and Public Policy-Making: The Joint Committee on Internal Revenue Taxation" *Journal of Politics* 30 (November, 1968) pp. 1046-1067.

Manley gives very high marks to the staff of the Committee on Internal Revenue Taxation, in particular for its even-handedness under the present staff director, Lawrence Woodworth, in dealing with members of the Committee alike who are defenders and critics of current tax policy. This was far less true, Manley reports, under Woodworth's predecessor, the legendary Colin F. Stam. Under both the Stam and the Woodworth regimes the Committee staff has enjoyed a high reputation for technical accuracy in forecasting.

¹¹ The Select Committee on Government Research of the U.S. House of Representatives (1963-64) under the chairmanship of Representative Carl Elliott used this device. I know of no published evaluation of the efficacy of the committee's General Advisory Committee, but have the impression from talking with members of the committee, advisers and staff that the net effect of the advisory committee was to help. No doubt other committees have experimented from time to time with similar bodies.

for 1- or 2-year hitches of service should be instituted between congressional committee staffs and staff persons in the executive branch, private business, labor unions, social service organizations, and universities.¹²

The purpose of these proposals is to bring to bear upon explicit policy analysis on Capitol Hill the standards—and the considerations—that are commonly employed in policy analysis within the executive branch and elsewhere in society. It is not contemplated that steps such as these will necessarily bring Congress into harmony with the executive branch in areas where they now disagree, since there is no reason to suppose that a large number of disagreements over national policy are *based* upon ignorance—though some may be. These disagreements should be resolved. Other disagreements may rear their heads if Congress chooses to equip itself with more professional analytic personnel, since not all executive branch proposals are free from controversy even when they are grounded in thorough professional knowledge. Thus more professionalism in explicit analysis can assist Congress in finding disagreements and weak spots in executive branch recommendations and can increase the probability that Congress itself can initiate policy. These proposals, therefore, genuinely attempt to strengthen Congress rather than the opposite—as is the case with so many proposals for congressional reform.

Many of these proposals, in my opinion, make a fundamental **mistake: They attempt to force Congress into an organizational format that mimics the hierarchical arrangement of the executive branch.** For example, PPBS, where it has been used successfully in the executive branch, is in part a political device for forcing decisions upward in the hierarchy. There is a point, however, beyond which this technique cannot go. PPBS is a technique for comparing alternative and **substitutable methods of achieving specific goals.** But it is not and cannot be a device for selecting desirable goals. In rare cases, where costs differ greatly, PPBS can help decisionmakers identify goals as more **or less achievable given limited resources,** but in the end political not technical decisions are the outcome of PPBS analyses.

Thus Congress is presently satisfactorily organized to assess the results of PPBS analysis in each of the sectors to which it can be or has been applied. The Armed Services Committees have for some time received the benefit of detailed discussion of cost-benefit analysis by Pentagon planners. There is no reason to assume that the same sort of information would not be forthcoming from other agencies that succeed in using PPB techniques, and presented to the appropriate committees of Congress.

Some observers may assume that because on the executive side an aggregation process takes place in the Bureau of the Budget and the Executive Office of the President that establishes priorities in the funding of programs, some similar explicitly synoptic act of aggregation ought to be instituted on the legislative side. This is a mistake. It assumes that the act of establishing priorities is a technical, not a political matter in the Executive Office Building, when in fact judgments are being made about items that are incommensurable and non-

¹² I am aware that a small number of programs like this are presently operating. I think it would be useful to see an evaluation of their effects. This recommendation reflects my judgment that such an evaluation would be strongly favorable.

substitutable by technical criteria. Secondly, it ignores the fact that just as the executive branch employs a method of aggregation suitable to its organizational design, Congress does too. Decomposition of budgetary and program proposals by subject-matter specialities and the assembly of successive majorities around specific proposals is a process of setting priorities and aggregating preferences no more political than the analogous activities in the top of the executive branch. It merely entails politics. It is appropriately responsive to the relatively decentralized character of a complex legislative institution.

Unlike the Supreme Court, the legitimacy of Congress does not rest even in small measure upon the intellectual excellence of its work. Congress legitimizes policy because it embodies the will of the people by giving voice to the collective judgment of their duly elected representatives. Technical competence and intellectual excellence in one phase of policy analysis are therefore not strictly necessary for Congress to be important in the American system of government. But sound and sophisticated explicit policy analysis can increase the capacity of Congress to contribute to the solution of the problems besetting America. Since Congressmen must choose among solutions, they may as well equip themselves as best they can for the task. This does not require the revamping of Congress itself, but rather greater attention to unused resources presently well within the power of Congress to command.

Part VI

ANALYSIS AND EVALUATION IN MAJOR POLICY AREAS: UNRESOLVED ISSUES AND NEXT STEPS

SECTION A

NATIONAL SECURITY AND FOREIGN AFFAIRS

THE PLANNING, PROGRAMING, AND BUDGETING SYSTEM IN THE DEPARTMENT OF DEFENSE: CURRENT STATUS AND NEXT STEPS

BY ALAIN C. ENTHOVEN and K. WAYNE SMITH*

Alain C. Enthoven is vice president of Litton Industries and a consultant to the RAND Corporation. Until January 1969, he was Assistant Secretary of Defense (Systems Analysis).

K. Wayne Smith is a staff member at the RAND Corporation and a former Special Assistant to the Assistant Secretary of Defense (Systems Analysis).

While the PPB system has substantially improved the decision-making process in the Department of Defense, Drs. Enthoven and Smith emphasize that there are still major problem areas and much unfinished business. In discussing the improvements generated by the application of systematic analysis, they emphasize the principal management tools that make the PPB System work. These are the Five-year Defense Program, the Draft Presidential Memoranda, the Development Concept Papers, and the Systems Analysis Office. These tools have "helped to give the Secretary of Defense the relevant information and analysis . . . to make a reasoned choice among [alternatives] . . . and to structure debate over defense issues along relevant objective lines. In an organization as large and diverse as the Department of Defense, where many issues are highly emotional, where the 'facts' are hard to pin down, and where parochial and institutional interests constantly compete with the national interest, these are not small accomplishments."

In citing the decisionmaking areas requiring improvement, Drs. Enthoven and Smith discuss the lack of usefulness of some current analysis, inadequacies in cost estimation and data on performance measurement, an inadequate theory of requirements in many important areas, and the lack of economic discipline within the military services. Analyses they conclude, could be substantially improved if they were less complex and more pertinent to basic decisions which must be made, and if they placed more emphasis on the development of accurate data necessary for meaningful analysis. Drs. Enthoven and Smith emphasize the need to apply performance measurement procedures in improving decisionmaking. Because "many of the major program decisions in DOD concern the introduction of new equipment, realistic estimates of performance must be available if the choices are to be good ones."

Drs. Enthoven and Smith stress the failure of the services to exercise financial discipline and their reluctance to set priorities and make hard choices. "Choices are not faced up to by the services and the Joint Chiefs of Staff, since they have the option of avoiding choice by simply adding all 'requirements' together. Since it is almost always possible to develop a set of assumptions that, in isolation, will 'prove' the worth of any new military device, it is extremely difficult to make a convincing case against introducing and producing a new system. The result is that the national interest, in terms of resources spent unwisely, sometimes suffers. The problem of how to provide incentives for the services to face up to hard choices * * * is a difficult one. * * * Part of the answer is creating a political environment which demands that they do. * * * A service which lets its costs go up disproportionately should not be 'rewarded' by simply having its budget increased."

* Any views expressed in this paper are those of the authors. They should not be interpreted as reflecting the views of the RAND Corporation or the official opinion or policy of any of its governmental or private research sponsors.

In addressing the question of next steps, Drs. Enthoven and Smith recommend the development of a procedure for a broader periodic review of Defense policies and alternatives than is possible in the context of a current year budget, a reorientation of the DOD study effort toward understandability and the needs of the decisionmaker, and a more comprehensive overall integration national security programs and operations.

CURRENT STATUS

The purpose of this paper is to describe briefly the current status of the planning, programming, budgeting (PPB) system in the Department of Defense (DOD), the changes and improvements it has brought in the management of the Department, and the major problems and future prospects of the system.*

The PPB system was developed in the Defense Department in the early 1960's to give the Secretary of Defense the management tools and information he needed to shape Defense programs in the national interest, i.e., to develop explicit criteria, openly and thoroughly debated by all interested parties, that could be used as measures of the need and adequacy of defense programs. This fundamental idea is reflected in both the approach to issues which the system encourages and in the major tools which have been developed to put this approach into actual operation. Understanding this idea is essential to understanding the PPB system.

If the Secretary of Defense is to shape Defense programs in the national interest, he must be able to judge the effectiveness versus the cost of each of a large number of proposals and set priorities, to judge, if you like, at what point the marginal utility of further spending on a given military mission is so small that it is no longer justified. And, as much as possible, the choice should be based on explicit criteria of the national interest, however crude these criteria may be initially. Moreover, the Secretary must be able to choose among real alternatives. This is the only way he can effectively translate his judgments about national security policy into action. This fundamental concept—decisionmaking based on explicit criteria of the national interest—provides both the rationale and the goal of the PPB system in the Pentagon.

One way to understand the PPB system is to look at the principal management tools that make it work. The most important of these, at present, are the Five-Year Defense Program (FYDP) and the programming system, the Draft Presidential Memorandums (DPM), the Development Concept Papers (DCP), and the Systems Analysis Office.

The FYDP is an 8-year projection of forces and a 5-year projection of costs and manpower arranged in mission-oriented programs. The FYDP ties together force and financial planning and provides a vehicle for insuring that the process of changing the approved program is orderly and that the changes are accurately recorded. The FYDP also provides an official set of planning assumptions. It is an authoritative record of what the Secretary of Defense has tentatively approved

*Further discussion of this issue is found in the paper by Enthoven in this volume.

for purposes of force and financial planning and a common reference point for subsequent changes. In other words, all interested parties within DOD know how many and what kinds of divisions, squadrons, ships, et cetera have been authorized and how many men and how much money it will take to support them. By clearly relating forces to their costs and to the defense budget, the FYDP gives financial planning the same output orientation as force planning.

This is a major improvement over the pre-1961 system. At that time there was an almost complete gap between force planning, which was long range, expressed in terms of combat units and performed by military planners in the Joint Staff and the military departments; and financial planning, which was short range, expressed in terms of objects of expenditure, and performed by civilians in the Comptroller organization. Given this situation, it is not surprising that there were imbalances in our defense posture—Army divisions without ammunition and the required personnel, aircraft without spare parts, spare parts without the transportation to move them.

With the FYDP, there is a common base for planning in the literally hundreds of separate agencies and offices throughout the Department. Logistics planners can see how many armored divisions are planned and budget for tanks accordingly. Each service can see what is planned for the other and thus better determine what forces are needed for common missions. Air Force planners, for example, can see how large an Army is planned and plan their airlift capability accordingly. Moreover, with a common set of planning assumptions, the wastefulness associated with starting or continuing a great many individual service projects which will all do the same job can be and has been reduced.

The FYDP, and the related backup data which supports it, also permit reasonable estimates to be made of the overall cost of existing and proposed new programs. These estimates include all the procurement, construction, personnel, and operating costs, in current and future years, that are related to a given program. Development of these kinds of output-oriented costs, which are essential for rational decisionmaking, was one of the original purposes of the PPB system and this objective has generally been achieved. The system does provide a framework for giving the Secretary of Defense an understanding of the financial implications of his program decisions.

Obtaining relevant cost information of this kind, however, turned out to be a difficult task, and there is still considerable improvement needed. The difficulty of estimating the cost of new equipment, and the tendency for gross underestimates, is well known and still remains. Equally important, however, has been the problem of "tip of the iceberg" cost estimates. Only in recent years has the importance of indirect and support costs become apparent. In order to deploy one squadron of aircraft an enormous tail of training base support, logistics, communications, and so on is required. The cost of these support elements sometimes exceeds considerably the equipment and direct operating cost for a system or program, but until recently the relationship of these costs to the major programs was not normally considered explicitly, and even today the relationships are not well understood.

The point is that obtaining comprehensive and relevant costs, which is of course a fundamental objective of any PPB system, is a difficult

analytical job, not just a simple accounting problem. Doing it right requires a clear understanding of the operations and characteristics of the programs themselves. DOD has made impressive strides in this direction under the PPB framework, but considerable work remains to be done.

In addition to insuring that the Secretary's program decisions are known and carried out, the FYDP is also the main basis for the services' budget submissions each fall. The major program and force issues are argued out in the annual force review cycle, which begins in the spring. The services then submit a budget which prices out the latest update of the FYDP (although they may and do submit supplementary requests). This means that the budget review, which is a highly demanding task in itself, can concentrate mainly on a thorough review of the financial requirements for an approved program, and does not have to address all of the major program issues of the Department. One of the important contributions of the programing system is that the real functions of a budget review—such as deciding how much money is really needed for the approved program, identifying funds that can be deferred because of slippages in production schedules, and so on, can be accomplished more effectively, since the whole program does not have to be reviewed at the same time.

It is neither realistic nor desirable of course to expect *no* program decisions to be made during the budget review. Some decisions are so important or uncertain that their final resolution is deferred until the last minute, when the latest technical information and intelligence estimates are available. In addition, it is reasonable to expect that the impact of the defense budget on the total Federal budget, which only becomes clear fairly late in the cycle, will lead to further program reviews to pare down the total. The important contribution of the programing system is that these decisions can be treated as exceptions: all of the program decisions do not have to be reviewed during the relatively short budget review.

The FYDP and the programing system, then, have created a vehicle which is in fact used by the Secretary of Defense to make program decisions and to tie them into the preparation of the annual budget. Both the FYDP and the programing system are now (January 1969) well-established, functioning systems. Yet nobody pretends they are final or perfect. A major piece of unfinished business is the development of a way to review how authorized resources have actually been used to accomplish defense missions. For budget planning, estimates are made of the resources needed for the operation and maintenance of the forces and funds are apportioned accordingly. However, there has been no mechanism to provide systematic feedback on how these resources have actually been used and whether the apportionments are accurate. Project PRIME, recently set up by the Comptroller of the Defense Department, represents an important step in establishing a flow of feedback information. By establishing accountability for resource use at subordinate levels in DOD by output-oriented program elements, better information on actual operation, maintenance, manpower, and other costs should be obtained. This information can, in turn, be used to improve force and financial planning.

Frequently, to meet a special need, force tables, displays, and management controls are needed in more detail than shown in the FYDP.

One example of such a specialized planning system is that developed to record (by month and quarter) the approved plan for the deployment of U.S. forces to Southeast Asia, their activity rates, their consumption of major material items, and the projections of these figures. These tables are essential for the controlled deployment or withdrawal of balanced increments of U.S. forces in Vietnam. Another example of such a specialized planning system is that being developed for Army and Marine Corps land forces. By dividing these forces into meaningful categories that emphasize the functions performed by their major components, this system holds promise of improving our knowledge and understanding of the functions and capabilities of our land forces. Eventually these tables will become a functional subsystem of the FYDP. Similar subsystems which emphasize indices of the effectiveness of our forces in other areas, such as tactical air forces, theater nuclear forces, antisubmarine warfare forces, and mobility forces, are now being developed.

These additions and extensions of the PPB system suggest its flexibility. It is not simply a set of management tools to be mechanically applied. Indeed, in the broadest sense, it is more of an approach to management that seeks to carry out a number of underlying principles than a specific set of management tools. It is a "system" mainly in the sense that the management tools are all designed to help put these principles into operation.

The very characteristics of the PPB system that make it effective have led to much of the political opposition to it. Since the FYDP does constitute an official and explicit record of program decisions and tentative planning assumptions, it requires the Secretary explicitly to make controversial decisions. This is quite a different procedure from simply setting a 1-year budget ceiling, without nailing down choices between competing claims for resources. Setting a 1-year ceiling inevitably leads to starting and continuing more and larger programs than the budget can adequately finance since the long-term financial implications of decisions are not explicitly considered, and the pressure for approval of competing programs does not have to be met head on. This, in turn, leads to holding on to prestige items at the expense of their unglamorous support elements causing a progressive deterioration in the effectiveness of the programs and the combat readiness of the forces. In the short run, however, a simple budget ceiling will generate less political heat than a system which requires explicit, long-range program decisions, simply because it presents a more vague target for critics.

While the FYDP is an indispensable management tool for the operation of the PPB system, it is not enough. The Secretary also needs an analytical staff that will assist him in determining the alternatives available, obtaining the relevant information bearing on the decision, and seeing that this information is presented in such a fashion that the key judgments that must be made are clearly understood. It is vital that such a staff look at the problem from the Secretary's point of view, i.e., integrating all of the conflicting specialized considerations that bear on a program decision. This has been the job of the Systems Analysis Office in the Office of the Secretary of Defense (OSD). The systematic effort to do this staff job has been the second of the two major innovations in DOD (the FYDP and the programing system being

the first) that make up the PPB system. In effect, the programing system has been the vehicle for making explicit program decisions with the Systems Analysis staff pulling together the substantive information in the area of forces and related major requirements needed to make these decisions intelligently.

A 1961 organizational chart of the Department of Defense would show the Joint Chiefs of Staff as the principal advisers to the Secretary of Defense on military strategy and forces; the Director of Defense Research and Engineering as the principal adviser on research and engineering matters; the Comptroller on financial matters; the Assistant Secretary for Installations and Logistics on production matters; the Assistant Secretary for International Security Affairs on foreign policy matters; etc. Each adviser was concerned primarily with his own specialty rather than with the defense program in its entirety. The Secretary of Defense was expected to integrate all of this specialized advice without staff assistance. The Systems Analysis Office was established to gather and analyze information relevant to forces and other major requirements from these different areas and assemble it in a way that would help the Secretary of Defense to fit the pieces together.

The Systems Analysis Office integrates the cost, effectiveness, and requirements data and the recommendations of the four services into groupings arranged so that the Secretary can understand what capabilities he is buying, at what cost, and how they relate to overall defense needs. The Office routinely provides the Secretary with the staff assistance necessary to identify and analyze alternative levels and mixes of forces. This insures that his choices are not limited to the alternatives proposed by the military services and the Joint Chiefs of Staff. In short, the Systems Analysis Office helps to broaden the range of alternatives available to the Secretary, to develop explicit criteria for defining the national interest in defense programs, and to structure the debate over issues in such a fashion that it focuses on the key judgments that must be made in choosing among the alternatives.

The Systems Analysis Office does not—as some of its critics have claimed—make decisions. It can only make recommendations, and these only in the areas specified by the Secretary of Defense.

The analysis and debate of issues is carried on within DOD in a variety of ways. One key vehicle is the Draft Presidential Memorandum (DPM). A series of these memorandums, each addressing a major functional area, are prepared annually by the OSD staff. Each DPM is initially prepared based on the guidance given by the Secretary and using the latest available analyses and intelligence information. The DPM expresses not only the Secretary's initial, tentative decisions, but his rationale for those decisions. The services and the JCS then are asked formally to comment, beginning a process of debate and interaction which lasts most of the year, and which generates innumerable special studies and memoranda. Every interested party not only has his say but has an opportunity to say it several times.

Another key vehicle for structuring debate is the Development Concept Paper (DCP). The DCP's, which as a regular procedure are about a year old, represent an attempt to document the performance, cost, and schedule estimates, as well as the technical risks, which were the basis for the decision to start or continue a development program. Each interested party is required to concur in these estimates or state

explicitly his objections. The goal is not to insist on completely accurate cost and schedule estimates but to combat tendencies to grossly overstate expected performance and understate costs and risks merely to get a project underway.

The DCP's also provide thresholds in these estimates which, if exceeded, would call for a reconsideration of the project by the Secretary of Defense. Estimates in a DCP are periodically updated, and the new estimates compared with the original estimates, so that the Secretary can see if expectations are being realized and if the reasons for continuing the project are still valid. If there are conflicting ideas about the potential usefulness of a proposed new weapon system, the DCP's help bring these disagreements out into the open for debate and discussion before a great deal of time and money have been spent and perhaps wasted.

One of the most significant achievements of the PPB system in DOD has been the stimulation of this kind of intense debate over relevant program issues, not just over arbitrary allocations of budget ceilings. This aspect of the system is especially interesting, in view of the charges that it has "shut off discussion" and "frozen the services out of decisions." In fact, one of the most successful aspects of the PPB system has been the focusing of the inevitable conflict and debate within the Department onto a much more constructive and objective level than before.

Open and explicit analysis, reviewed and commented on by all interested parties, is fundamental to the working of the PPB system in the Pentagon. Through such vehicles as the DPM's and DCP's, the analyses underlying the Secretary's decisions are circulated for comment and review by all interested parties, and their comments go directly to the Secretary. The procedures are designed so that the Secretary will hear all sides, so that no one has a monopoly on the information going to him. This open and explicit approach has proven to be the best protection against persistent error; it has made it virtually impossible for any group to rig the analysis without that point being made clear to the Secretary. It insures that all important assumptions are made explicit and that all opinions are fully considered.

Evaluation of the success or failure of this attempt to improve the substance of decisions by improving the analytical input and the decisionmaking instruments is difficult. The only real measure of the contribution of this effort is whether the actual program decisions were better than they would have been otherwise; this is a highly speculative question, since no one knows what would have happened in the absence of this attempt. At present, evaluation of the effort must rest on the general observation that it has provided the Secretary more relevant information and meaningful alternatives than he had before, that the analysis underlying these decisions was open and explicit, and that the debate over them has been much more orderly and relevant.

Any number of examples of this could be given, but the analysis of our strategic nuclear forces is probably the best. This analysis has progressed to the point where the main participants in the process agree that the key assumptions have been isolated, although they may not agree on the substance of the assumptions themselves. Whether and how the Russians would react if we were to build a full-scale anti-

ballistic-missile defense is still hotly debated, but the fact that this is the critical assumption, and its impact on the decision, is not.

The question of the impact of the PPB system on the decisions of the Vietnam war naturally arises in any evaluation of the system. The answer to this question is that it had very little impact on the major decisions of the war. It did help to manage the resources much more efficiently than would otherwise have been the case. In 1965, at the direction of the Secretary of Defense, the Systems Analysis Office established a Southeast Asia deployment plan and a system for keeping it up to date. This extension of the PPB system served as the basis for financial, logistic, and manpower planning, and helped to insure that the planning of forces and resources was integrated. But the key Vietnam decisions involved either issues of military strategy in the use of force, which for the most part were not seen by the top policymakers as being subject to systematic, quantitative analysis in Washington, or issues of international politics, which were beyond the scope of the PPB system. The potential value of systems analysis in making decisions on the conduct of a war has yet to be tested.

In summary, the PPB system has provided an effective framework for making and carrying out major program decisions in an informed and orderly way. By unifying programing and budgeting, the PPB system has closed the "gap" between force and financial planning. By providing an official force plan, it gives the planners and analysts in the whole Department a firm foundation for their planning and a solid point of departure for their analyses. It has also led to a major improvement in the quality and relevance of debate over requirements. The idea of open and explicit analyses, reviewed and commented on by all interested parties, is firmly established. The systematic search for real alternatives to prevent the Secretary from being the prisoner of a single staff solution is now the rule rather than the exception. The systematic viewing of all requirements on an overall mission basis, rather than on the basis of a single service, has led to the elimination of much unnecessary duplication. Most importantly, the PPB system has helped to give the Secretary of Defense the relevant information and analyses to see what the alternatives are and to help him make a reasoned choice among them. It has given him a way to structure debate over defense issues along relevant, objective lines. In an organization as large and diverse as the Department of Defense, where many issues are highly emotional, where the "facts" are hard to pin down, and where parochial and institutional interests constantly compete with the national interest, these are not small accomplishments.

MAJOR PROBLEM AREAS

The experience obtained from the development and operation of the PPB system, however, has uncovered additional opportunities for improved decisionmaking and management. Indeed, one of the more desirable aspects of the system is that it has helped to identify places where additional improvements are needed. The PPB system as it now exists is a good foundation on which to build, but it is by no means complete. Some basic problem areas are: (1) the quality and usefulness of analyses; (2) the lack of good cost estimates and test data on new equipment; (3) the lack of an adequate theory of requirements in

many important areas; and (4) inadequate economic discipline in the services.

While some of the study effort on defense programs has been excellent, much has been relatively poor. The amount of really useful analysis relevant to major defense decisions has been limited. Ideally, the large-scale study effort within the services, the Joint Staff, the major contract study organizations, and other agencies should provide a base of knowledge which can be drawn on when specific program decisions are being considered. Yet, while hundreds or possibly thousands of studies are turned out each year, few of them are of any real use for decisions at the Secretary of Defense level. For example, the number of major studies in the services and contract study organizations of overall tactical air requirements that have been done over the last 5 years number in the dozens. Many of them involved large study groups and complex computer models. All of them came up with recommendations and conclusions. None of them shed much light on the subject. In fact, what few confident conclusions that can be drawn on the subject of tactical air requirements have come from the development of much simpler analyses and measures, mostly by the OSD staff.

This is not necessarily a criticism of the individuals participating in these studies, many of whom are highly capable. Nor do we believe that the problems addressed by these studies are so complex that they can never be understood. In part the problem stems, in our judgment, from the fact that nearly all such studies are oriented to near-term program decisions. Few, if any, ever attempt any "basic research" on underlying areas where data and knowledge are lacking. Most of the studies have fairly short deadlines and specific terms of reference. Normally, the study group is neither capable of—nor authorized to—investigate and question basic data supplied by other agencies, such as data on intelligence or technical performance.

The result is that the study group starts with whatever data or assumptions it can quickly get its hands on. The available data are frequently highly unreliable and the assumptions used in service studies are generally chosen to put their proposal in a favorable light. Not enough work is done on the underlying analytical issues and data which all these studies proceed from. For example, studies of tactical air capability and requirements depend heavily on estimates of bombing accuracy, target acquisition, target hardness, ordnance effectiveness, and the impact of tactics and training. Until recently, however, no systematic data on these subjects was available and even today the data are spotty.

Another important aspect of this problem area and one that helps to explain in part the poor quality of many analyses is the inherent bias toward the specialist's view over that of the generalist's. The Defense Establishment includes many men who are pushing their own programs very hard and who see the whole defense of the United States as being tied up with their own programs. They are often personally affronted and publicly outraged if their program is cut back a little. There are too few people in DOD who appreciate the problem of getting a total program that makes sense. The military and civilian participants in this process must learn to take a larger view and recognize that the perspective that is appropriate for a project officer is not one that is ap-

propriate for someone who is participating in the development of the total national defense program.

The PPB system is on the side of the generalists. One of its main purposes is to translate the specific technical criteria of the specialists, that are not meaningful to the generalist, into terms that are. For example, probabilities of damage that different nuclear forces could achieve against various lists of targets are sufficient for the specialist. On the other hand, probabilities of damage are very difficult for a generalist to interpret and judge. By translating these numbers into, say, the number of Russians and Americans what would survive a nuclear war, they can be more easily grasped. The PPB system has gone a long way in helping to organize information along these lines, but it cannot substitute for a more widespread awareness in DOD that the whole takes priority over the individual parts.

One line of attack for improving the quality of studies is for the Secretary of Defense to insist on better studies. This is not always easy to accomplish even for studies done by the OSD staff. Moreover, the vast majority of studies are not done within OSD but by the service staffs or contract study organizations. The Secretary's insistence alone has proven to be a feeble counterweight within the service staffs to that of more immediate superiors, the pressures for biasing assumptions to put proposals in a more favorable light, the frequent turnover of personnel assigned to analytical staffs, and the lack of training in modern analytical techniques of most such staff members.

One proven way of getting better studies from the services has been for the Systems Analysis Office to do a pilot study in a particularly sensitive area and then send it to the services for comment and review. This approach should probably be used more frequently in the future.

A second and related problem area is the lack of relevant and reliable test data for equipment. Many of the major program decisions in DOD concern the introduction of new equipment. Realistic estimates of performance must be available if the choices made are to be good ones. Indeed, the problem of reliable performance estimates has become more significant in the last 10 years as missiles and electronics have become the key elements in the effectiveness of many new weapons systems.

Much more needs to be done to insure that reliable and accurate performance information on new weapons is obtained. Frequently the only performance data available on a new system come from the contractors who are developing or producing it, or the officers who are managing the project. Granting the best intentions, it is not surprising that estimates from these sources are frequently too optimistic.

Not enough emphasis has been placed on the use of prototypes for testing. Even where prototypes have been developed, the time allotted for testing has frequently been too short to do the job thoroughly.

One serious result of this situation is the lack of reliable data on the performance of new systems under conditions approximating actual combat, and in comparison with older systems under similar conditions. Too often, such "operational" tests as are conducted are done under ideal conditions not likely to be found in actual combat. These conditions include using highly-trained technical personnel—instead of average troops—to operate the system; excluding from reported results any failures due to "special or exceptional" problems; testing

only under favorable conditions of altitude, range, weather, tactics, and terrain; neglecting possible countermeasures; and the like. The difference between results under ideal conditions such as these, and actual combat conditions, can be large enough to have reversed a decision. Currently, however, very little objective operational troop testing of equipment is done, even on existing equipment which would provide a benchmark for evaluating new systems. Such data is almost never available when decisions on production of a new system have to be made. (This is not usually a problem of time; very few systems are really so urgent as to warrant a production decision before being fully tested.)

A third important problem area is the incompleteness of the theory of requirements for some major components of the defense program. In some areas, particularly strategic nuclear forces, requirements have been clearly and thoroughly analyzed. While there remain considerable disagreements over what assumptions are most likely to be valid, no one argues that an important assumption is missing or that the results of the calculations based on these assumptions are inaccurate. A similar situation exists in the field of strategic mobility. In other areas, however, progress toward developing good requirements analysis is just beginning. In land forces, for example, simple indicators of force capability such as helicopter lift in total troop miles per month and artillery fire in total lethal area per minute have just recently been developed. These indicators, when combined with older ones such as firepower and maneuver indices, hold promise of greatly improving our ability to analyze land forces requirements. Much more work remains to be done, however, before any high confidence statements of land forces capability can be made. Much the same situation exists in the areas of tactical air forces and antisubmarine warfare forces. While progress has been made in identifying the major factors involved and in developing objective methods for comparison, very little progress has been made toward developing a satisfactory theory of requirements in either of these important areas.

In still other areas, communications and intelligence programs for example, we are quite far from having satisfactory principles for determining aggregate requirements. Given the huge sums of money that are spent in these areas it is imperative that more progress be made toward understanding explicitly what these resources are buying.

A final, major problem area is the lack of adequate financial discipline by the services themselves; in other words, the reluctance of the services to set priorities and make hard choices. General Eisenhower's remarks of a decade ago describing this problem are still appropriate:

Words like "essential" and "indispensable" and "absolute minimum" become the common coin of the realm, and they are spent with wild abandon. One military man will argue hotly for a given number of aircraft as the "absolute minimum," and others will earnestly advocate the "indispensable" needs for ships, tanks, rockets, guided missiles, or artillery, all totaled in numbers that are always called "minimum." All such views are argued with vigor and tenacity, but obviously all cannot be right.

The way the PPB system has operated thus far, the services and the Joint Chiefs of Staff have been able to propose specific programs (such as a new aircraft or tank or ship) generally without explicit

consideration of the impact on the overall service budget or on the overall defense budget.

It is possible, as an example, for the Navy to argue for the advantage of nuclear-powered aircraft carriers, and have the other Joint Chiefs support this recommendation, without having to specify whether the total defense budget should thus be increased (with appropriate reasoning and evidence to support that conclusion), or whether the greater cost of nuclear carriers should be paid for by a reduction in the number of carriers. In fact, if nuclear carriers are really more "efficient," then their advantages should permit a corresponding reduction somewhere else, possibly in the total number of Navy ships. The current system does not force the Navy or the JCS to address this fundamental question. The result is that choices are not faced up to by the services and the JCS, since they have the option of avoiding choice by simply adding all "requirements" together.

In these circumstances, the burden of choice rests almost entirely on the Secretary of Defense and his staff. We believe too much of the burden of proof has been on the Secretary of Defense for not accepting service proposals and not enough of the burden has been on the services for proving that their new proposals should be added to the defense budget rather than being substituted for an existing program. Since the analysis of complex defense issues is almost never clear cut and "provable," one way or the other, the pressure on the Secretary for continuous budget increases is very great.

Similarly, although the PPB system has meant an enormous improvement over what went before, the current procedure still favors the procurement of costly new weapons systems just because they have been developed, without adequate knowledge of whether they are the best way of doing a given job. The fact that a new device has been invented is not a sufficient reason to increase the total cost and capability of our Armed Forces. The rate of new inventions is simply not a good indicator of how fast we should increase the cost and capability of these forces. However, since it is almost always possible to develop a set of assumptions that, in isolation, will "prove" the worth of any new military device, it is extremely difficult to make a convincing case against introducing and producing a new system. (This is made doubly difficult because of the lack of reliable test and experience data, discussed above.) The result is that the national interest, in terms of resources spent unwisely, sometimes suffers. The services themselves suffer because the claims made for many new systems often are not realized in the field, while maintenance and support problems are even greater—and more costly—than predicted. We badly need a more rigorous application of cost-benefit criteria to the starting of engineering development projects.

Part of the reason for the increases in the cost and complexity of our forces is that the numbers of major force units (such as divisions and wings and carriers) have been largely treated as being inflexible. These units are the most widely known aspect of the services' structure and the number of such units has remained relatively fixed for some years. The pressure for increased capabilities and increased budgets has therefore been manifested mainly through introducing more expensive equipment, communications, support, and the like. The cost to buy and operate an A-6 Navy attack bomber is much more than that

of an A-1, for example, so that the replacement of A-1's with A-6's means a major cost (and performance) increase, even though the nominal force structure in terms of wings remains constant. This trend has not only increased substantially the overall defense budget, but has inhibited service incentives to develop simple, low-cost equipment which often could be introduced in larger numbers and possibly with far greater effectiveness per dollar.

The problem of how to provide incentives for the services to face up to hard choices and get the most capability for the money they spend is a difficult one that does not lend itself to simple solution. Part of the answer is creating a political environment which demands that they do. Until such an environment prevails, however, reliance will have to be placed on building up the "case law" of equal-cost and equal-effectiveness trades for new weapons systems that are made; a limited use of budgetary guidance, when appropriate, as a prod in this direction; and continued efforts by the Secretary of Defense to have the services separate, in their proposals, issues of force level and force mix. In any event, a service which lets its costs go up disproportionately should not be "rewarded" by simply having its budget increased.

NEXT STEPS

None of these problem areas is a result of poor choices in how the PPB system has been set up and operated to date; each of them was in fact a serious problem in DOD management before PPB. They do, however, represent possibilities for further improvements in defense management which are enhanced by having reached the current "state of the art." Actions taken to meet these problem areas should be viewed as extensions and improvements in the PPB system as it now stands, not as alternatives to it.

Some possible next steps which, in our judgment, appear warranted are discussed below:

First, some procedure should be developed for a periodic review of alternatives that are broader than those considered for the current year budget alone. The last such broad review was conducted in 1961-62 and resulted in the shift from the policy of "massive retaliation" to that of "flexible response." With only minor changes, the broad outlines of this policy have continued until today. Such a review is not necessary or desirable every year. (It would result in chaos if basic policy changes were made and attempted to be implemented every year.) But periodically, as important circumstances dictate, a thorough review of broad alternatives would be very useful.

The foundation provided by the PPB system and the methods and tools which have been developed to put the system into operation would be invaluable to such a review. Ideally, this review would describe for each of several alternative postures the types of military contingencies the force would be capable of meeting, the risks involved, and the costs. The domestic and foreign political implications and the economic impact on the rest of the U.S. budget would also be explicitly considered. Other means of meeting our broad foreign policy objectives, such as economic aid, technical assistance, training and educational programs, would be considered and compared with military options, before a military program and budget were decided upon. To be meaningful, this process should culminate in setting a target for

defense spending over the next several years, a target which would then be used internally in DOD as a guide when making specific program decisions. Such a procedure would seem to fit in fairly well with the renewed emphasis on the National Security Council by President Nixon.

A second desirable change in the system is to reorient the basic thrust of the analytical effort which supports it. With respect to the Defense Department, at least, certain points which bear on this problem have become very clear. First, too many study groups spend too little effort attempting to define the problem they are working on and to develop a logical way of relating data to the problem. The design phase of a study is usually by far the toughest and most critical part. It may require a lot of preliminary estimating and rough trials. It may take up a significant portion of the total time spent on a study. It also often takes an iron will to carry out, because there is little to show bosses who are anxious to see results. It is essential, however, that the design phase be done right. There is little to be gained from charging off to gather data and make detailed calculations until one knows what is really needed and how the parts of the problem fit together.

Second, too many studies are becoming so complex that they are almost impossible for anyone except (and sometimes including) the authors to understand. The most compelling reason to make analyses understandable is that it increases the probability that decisionmakers will use them. We all recognize that decisionmakers usually add judgment to the facts and the logic they consider. This is as it should be; analysis is the servant of judgment, not a replacement for it. However, in most cases where the decisionmaker doesn't personally understand at least the basic structure of analysis, he must not only add judgment but is forced to rely on it entirely. Given the importance of defense decisions and the enormous costs they involve, few would argue that this is a desirable situation.

One good test of the understandability of an analysis is a simple description of the essential features of its logic in layman's language. While, in principle, there can be valid analyses of any degree of complexity, there are few real life situations where such complexity is truly necessary. In short, if the study's logic can't be explained to a layman in a comparatively simple fashion, it almost always means that there is something wrong with it.

A redirection of the DOD study effort toward simplicity and understandability is not only necessary, but will ultimately produce a greater number of useful studies.

Third, too many studies are seriously out of balance, treating some parts of the problem in great detail (usually because it happens to be possible to do so) and ignoring other important parts. This is a danger which most analysts recognize and strive to avoid. At a minimum, however, the aspects of a problem that do not lend themselves to quantitative treatment should be explicitly recognized and brought to the attention of the decisionmaker.

Fourth, too few studies in a given mission area (for example, NATO, antisubmarine warfare, tactical air forces, et cetera) are done on the basis of a consistent set of assumptions. A great deal of the work of the Systems Analysis Office has consisted of trying to enforce the use of a consistent set of assumptions. What is needed is to look at several

sets of consistent assumptions that can be applied to all of the competing weapon systems in order to build a balanced posture that will be effective under a broad range of alternatives. Reaching this goal is doubtful so long as each new study starts with its own particular set of assumptions.

Finally, as we discussed earlier, much of the basic input data that is being used in these studies, particularly in studies of general purpose forces, is suspect. Most of this data is supplied by sources which cannot be considered totally objective. Moreover, little research on underlying areas (ordnance effectiveness, target acquisition, the impact of tactics and training, et cetera.) that bear on most of these studies has been done. Basic data on intelligence and technical performance are too frequently accepted without being questioned. Until some action is taken to remedy these problems and provide systematic and objective input data, the conclusions of these studies will continue to reflect these shortcomings.

A comprehensive review of that part of the DOD study and testing effort which is aimed at providing data and analysis relevant to program decisions would be useful. The current effort, although extensive and expensive, provides a minimum of really useful information. A comprehensive review should seek to define the key gaps in existing knowledge and make recommendations on the organization, budgeting, and guidance of a study and testing effort designed to be responsive to the needs of the Secretary of Defense.

Finally, one of the most significant contributions of the PPB system in the Defense Department has been toward integrating related things in an intelligible way. An extension of the same ideas should improve the integration of our overall national security programs and operations.* The PPB system has already contributed to this kind of integration by tying together within DOD things that were previously treated separately; for example, strategic retaliatory forces, airlift and sealift forces, and the like. But the problem is at the next level up, at the broader level of national security policy and operations. Vietnam shows this problem quite clearly. Examples abound of where we have destroyed with the left hand what we were building up with the right. For instance, one of our basic goals in Vietnam is to establish strong armed forces and an honest civil service. Yet the heavy deployment of American forces has contributed directly to the great inflation which has eroded the economic position of both the Vietnamese officer corps and civil servants, making them more susceptible to corruption and disunity.

Various ideas—such as consolidated country programing, putting the Ambassador in charge of all U.S. programs in an area, program budgeting by country or region, et cetera—have been proposed for how to move in this direction, but none have received much more than lip-service thus far. One of the great challenges and unsolved problems in the national security field remains that of developing a way to effectively integrate all of the U.S. operations and programs affecting national security in overseas areas. We believe an imaginative and vigorous extension of the PPB system at the national policymaking level offers one promising way of meeting this challenge.

*Further discussion of this issue is found in the paper by Rowen & Williams in this volume.

POLICY ANALYSIS IN INTERNATIONAL AFFAIRS

BY HENRY S. ROWEN and ALBERT P. WILLIAMS, JR.*

Henry S. Rowen is president of the RAND Corporation.

Albert P. Williams, Jr., is an economist at the RAND Corporation.

It has been argued that policy decisions in the area of international affairs are not likely to be improved through explicit and open analysis. This point of view is not shared by Drs. Rowen and Williams. In this paper, they evaluate "the state of analysis in international affairs and how this analysis and its use might be improved."

In the first section of their paper, Drs. Rowen and Williams deal with the analytic problems which the decisionmaker confronts in the international affairs area. These include matters relating to the structure of the budget and the formulation of country and functional decision issues. In their appraisal of these problems, Drs. Rowen and Williams offer recommendations concerning the need for unified budgeting in the international affairs area, and for increased interaction of the pertinent variables and information regarding country and functional issues. With regard to the need for a unified budget, they assert that, "the fragmentation of budget decisionmaking within many agencies means the absence of a consistent policy input to these decisions."

In their discussion, Drs. Rowen and Williams present a set of guidelines for the framing of analytical studies in the international affairs area. These guidelines pertain to the modes for clarifying policy problems and formulating objectives, the evaluation of program costs and outputs, and the appropriate response to uncertainty factors involved in any decision.

In the latter portion of their paper, the authors deal with organizational issues pertinent to the successful implementation of policy analysis. In this discussion, they emphasize the need for a decisionmaker who is prepared to use policy analysis. "The sine qua non for analysis to serve a useful purpose is to have a decisionmaker who will use it. Decisionmakers can do without analysis, and the proof of that fact is that they have so often done without it in the past; but good analysis and analysts cannot do without decisionmakers." They also discuss the composition of analytical staffs and the types of personnel most essential to such staffs. They conclude with a discussion of the structure and quality of analysis in the staffs of the President, the State Department, the Department of Defense, the Central Intelligence Agency, and other Federal organizations concerned with international affairs decisions.

I. Introduction

Foreign affairs is almost the last hideout of the intuitionists who distrust and dislike attempting to subject the political affairs of men to systematic analysis. For domestic political issues, the potential of

*We are grateful for the subcommittee's invitation to contribute this paper on "Policy Analysis in International Affairs." The subject is both intriguing and humbling. We greatly benefited from the comments on an early version of the paper by our RAND colleagues, Wayne I. Boucher, Charles A. Cooper, Daniel Ellsberg, Hans Heymann, Jr., Malcolm W. Hoag, Malcolm A. Palmatier, Guy J. Pauker, Peter L. Szanton, Helen Turin, Marshall W. Wiley, and Charles Wolf, Jr. The views expressed here are ours; and they should not be taken as reflecting the views of the RAND Corporation or the official policy or opinion of any of its research sponsors.

analysis has been both observed and accepted (along with its limitations). But for problems that cross our national borders even strong advocates of analysis elsewhere are dubious. Thomas C. Schelling, for example, has commented:

I should like to see the Department of State enjoy the benefits of modern analytical techniques of the kind Secretary Enthoven has brought to the Department of Defense, as well as other kinds. But I cannot—I wish I could, but I cannot—declare with any confidence that this can be done * * *. Foreign affairs is complicated and disorderly; its conduct depends mainly on the quality of the people who have responsibility; decisions have to be based on judgments, often too suddenly to permit orderly analytical processes to determine those decisions.¹

This paper explores some of the reasons underlying such skepticism concerning the role of analysis in international affairs, and sets forth some simple guidelines for increasing its usefulness in the future.

Foreign affairs never was primarily a question of reinsurance treaties and diplomatic covenants. This is perhaps clearer today than in the past. To be sure the diplomatic game still includes such stuff, but increasingly it also involves the wide range of particular programs and policies that we are engaged in elsewhere in the world: defense, trade, economic assistance, information gathering and dissemination, international financial matters, and scientific cooperation, among others. All of these activities together are what foreign affairs is about. Moreover, trends in technology, economics, and culture all make inevitable a high, and probably growing, level of international involvement which will persist despite our current flirtation with some of the trappings of neoisolationism. Accordingly the subject of this paper is the application of analysis to foreign affairs, broadly defined.

In the past 20 years, the U.S. Government has responded to our increased involvement abroad by making major institutional changes. New agencies have been created to carry out new functions, and there has been a gradual evolution in the style with which we do our foreign business. But this response has not been enough, especially with respect to the ways in which foreign policy decisions are made and carried out. However, our central concern in this paper is a more limited one: the state of analysis in international affairs and how this analysis and its use might be improved. Since policy analysis can be usefully examined only in relation to the mechanisms for reaching and implementing policy decisions, we also touch on organizational problems. Our purpose in this paper is not to deal with any of the many substantive issues the United States faces in the world but rather to seek out ways of improving the capacity of the U.S. Government to deal with these issues.

A final point of clarification: the term "analysis" does not conjure up in our minds visions of computers, and it should not do so in the minds of our readers. What we mean by analysis is more orderly, comprehensive treatment of problems, and this is a job for people, not computers.

¹ Thomas C. Schelling, "PPBS and Foreign Affairs," memorandum prepared at the request of the Subcommittee on National Security and International Operations, Committee on Government Operations, U.S. Senate, 90th Cong., first sess. (Washington: Government Printing Office, 1968), pp. 9-10.

II. ANALYTIC PROBLEMS AND APPROACHES

Politics is an unusually difficult subject, and international politics is especially so. The interactions between international and domestic interests, national defense and foreign affairs, trade and aid, bureaucratic and substantive considerations, means and ends, are extremely complex. Some of the difficulty arises from the fact that foreign affairs comprises many classes of problems of widely differing character. And for many of these problems, there are strict limits to what can be done, limits imposed by a fundamental absence of knowledge about crucial relationships. And hard data are often missing. But some of the difficulty arises from the fact that all too often we do not make the best of the knowledge we have—or might be able to acquire.

In this section, we shall describe the various types of problems in international affairs and discuss some methods of analysis that can be applied to them.

TYPES OF PROBLEMS

In matters involving programs, where specific activities are carried out involving the expenditure of funds, there is a *prima facie* case for being able to do a certain kind of analysis. The logic of economizing behavior can be applied. At least one can describe the proximate “outputs” of programs, often quantitatively, compare alternative ways of achieving these proximate outputs—and perhaps invent new ones—and enhance program effectiveness relative to program cost by better choice among alternatives. For problems such as flood control and power production on the Mekong River, or the signal density of Voice of America’s radio coverage, or fertilizer production in India, there is much that can be done, and is being done, by way of analysis at this level. It is not always easy to do nor is it necessarily always well done; there are more than a few economic development or defense projects that have received justified criticism. Moreover, some programs involving sizable sums of money do not lend themselves to direct and concrete analysis even of a narrow sort. It is no small task to assess the effectiveness of Voice of America broadcasts to Eastern Europe or the consequences of providing program loans (i.e., balance-of-payments support) to the Government of India.

Whatever the ease or difficulty in analyzing programs in this sense, programs are not ends in themselves. They relate to such broader U.S. purposes as strengthening our security, sometimes as specifically as getting concessions in return (e.g., base rights), sometimes as generally as energizing other governments to take internal measures to promote their development and internal stability. Assessing programs in terms of their contribution to such broader objectives is usually quite difficult. For that reason it is often not attempted at all. The objectives themselves are often vague, the functional relationships connecting program activities to these objectives are difficult to specify, and relevant data are often poor or even nonexistent. But clearly it is these higher purposes that are of greatest interest to U.S. policymakers.

Other problems do not directly involve program activities at all, or do so only in small part. Such policy areas might include efforts to control the spread of nuclear weapons, or to decrease the probability of conflict in the Middle East, or to improve our trade relations with

foreign countries. Specific programs play a minor role compared with a wider set of nonprogram aspects.

It is our view that much can be done not only on "program" but also on these broader "policy" matters to improve the quality of analysis bearing on decisions. But what can be done, and how, will depend on the kinds of issues at stake.

A. Budgetary Issues

Program decisions inescapably involve budgetary outlays. Thus, the budget provides the most convenient occasion for tackling many issues. For foreign affairs and related national security the sums involved, of course, are very large. Outlays for international affairs and national security programs are expected to total \$85 billion in fiscal year 1970, 44 percent of the Federal budget (see table 1). From one point of view, the aggregate of the resources available for these programs, the entire \$85 billion, is available to be allocated in the most efficient way to our various international and related security purposes. Thus, we might, in principle, aspire to define a set of highest level objectives in weighted value terms (or better yet, a set of alternative ones), devise mixes of military, economic, propaganda, intelligence, and other programs to meet these objectives, and choose the mix that promises the best performance within the budget available. To do so explicitly is overly grandiose. Yet implicit in budgetary decisions is the view that the purposes are right, that the sums to spend for these purposes are about right, and that these are the right programs to support.

TABLE 1.—INTERNATIONAL AFFAIRS AND NATIONAL SECURITY PROGRAMS

[Outlays in millions of dollars]

Program or agency	1968 actual	1969 estimate	1970 estimate
International affairs:			
Conduct of foreign affairs:			
Department of State.....	339	358	370
U.S. Arms Control and Disarmament Agency.....	11	10	10
Tariff Commission.....	4	4	4
Foreign Claims Settlement Commission.....	1	1	1
Economic and financial programs:			
Agency for International Development.....	1,936	2,092	1,973
International financial institutions.....	201	140	216
Export-Import Bank.....	790	165	140
Peace Corps.....	111	106	110
Other.....	15	22	26
Food for freedom.....	1,204	1,037	925
Foreign Information and Exchange Activities:			
U.S. Information Agency.....	187	191	195
Department of State and other.....	66	53	41
Subtotal, international affairs.....	4,864	4,180	4,011
National security:			
Department of Defense—military ¹	77,373	77,790	78,471
Military assistance ¹	654	610	529
Atomic energy ¹	2,466	2,451	2,571
Defense-related activities.....	139	282	171
Subtotal, national security.....	80,632	81,134	81,742
Total.....	85,496	85,314	85,753

¹ Entries net of offsetting receipts.

There is much controversy at the present time about the magnitude of these sums and especially the amounts allocated to military programs. Questions are being raised about the extent of our foreign commitments, the contingencies for which we should be prepared, the structure of our military forces, the size and character of our AID program, the allocation of resources among regions and countries. Also, are our various programs mutually consistent? What are the theories or beliefs and the underlying evidence in support of the budget allocation? How certain are we about these theories and beliefs? What contrary hypotheses or beliefs, and programs, might be advanced, and what is the evidence for them?

These are legitimate, indeed necessary, questions to address. But the foreign affairs-national security budget is not constructed in a unitary way nor is it subjected to the kind of systematic process we have suggested. Rather, it is an assemblage of largely independent components, and some important ones receive relatively little analysis.

But other components, mainly large parts of the defense budget, are subjected to systematic analysis today. The quality of the analysis is variable, and sometimes—and inevitably—bad decisions get made. Nevertheless, there is a serious effort to address precisely the kinds of questions we have stated (of course, at a greater level of concreteness and detail). And some comparable analysis is also done in other program areas, for example, on some economic development programs.

However, there is a shortage of analysis which cuts across budgetary categories and organizational lines. The funds included in this \$85 billion are administered by a dozen different agencies, and their appropriations sometimes appear out of line with their responsibilities. For example, the Department of State, the agency charged with coordinating foreign affairs, receives less than one-half of 1 percent of the total budget, much of this for administrative expenses and salaries of Foreign Service personnel. But many of our problems do not come packaged in the way Congress appropriates funds or the executive branch administers them.

Yet budget decisions are policy decisions. Budget decisions on bilateral versus multilateral aid, military lift capacity versus foreign bases, nuclear versus nonnuclear military forces, food aid versus money, Latin America versus Africa, all have profound policy implications. The fragmentation of budget decisionmaking within many agencies means the absence of a consistent policy input to these decisions. The importance of an essentially unified national security budget to the management and policy innovations in the defense side of international affairs has often been stressed. Schelling has commented:

When Secretary McNamara assumed office, he was at least 15 years ahead of where the Secretary of State is now in having a recognized budget. There is a "defense budget;" there is not a "foreign affairs budget." Both legally and traditionally the defense budget is fairly clearly defined; around the edges there are the Atomic Energy Commission, some space activities, perhaps the Maritime Commission, that one may wish to lump into a comprehensive "defense total," and over which the Secretary of Defense does not exercise direct budgetary authority * * *. The Secretary of Defense makes an annual comprehensive presentation of his budget * * * it is a "state of the Union" insofar as

national security is concerned. The committees in Congress that deal with the defense budget have no doubt about what budget it is they are considering.

Not so the Secretary of State, whose own budget of about a third of a billion a year corresponds, to take a very crude analogy, to the budget that the Secretary of Defense might present for the Pentagon building and the people who work in it.²

Some modest steps have been taken toward integrated foreign affairs program budgeting during the past two budget cycles. During the review for the fiscal year 1969 budget, the Budget Bureau began systematically consulting the regional Assistant Secretaries of State on interagency program issues arising out of various agency PPB submissions. During the past year, a few interagency papers for individual countries were prepared on an experimental basis. These papers dealt with U.S. objectives and the resource inputs of the major foreign affairs agencies devoted to achieving these objectives. The joint State/AID Latin American Bureau has made the most progress in this area through its country analysis and strategy papers (CASP), prepared in the field each year on the basis of guidance from Washington. Finally, during the fiscal year 1970 budget review process (fall 1968), the Budget Bureau used the Senior Interdepartmental Group (SIG) and the subsidiary Interdepartmental Regional Group (IRG) as forums to inform member foreign affairs agencies of budget issues affecting international affairs.³

Charles J. Zwick, then Budget Director, recognized both the limited nature of this progress and the obstacles to further progress when he commented last May, "Because of our concern for the complexities of the problems, we are moving forward pragmatically and deliberately."⁴ To be sure, the steps taken have been in the right direction, but they do not take the executive branch very far down the road toward consolidated consideration of foreign affairs budget matters.

B. *Country Issues*

Many of the most important policy issues involve selecting and trying to reach objectives with the governments of other countries. Programs and policies serving U.S. global objectives have to be tailored to the conditions obtaining in individual countries. And U.S. programs in foreign countries, whatever our reasons for conducting them, usually must be acceptable to host governments. We can assume that the need to structure U.S. programs in light of these requirements will persist.

Thus, we need to examine the full range of interests and objectives we have with respect to a given country, the full range of policies and programs we are using to try to reach these objectives, the effectiveness of these policies and programs, and their consistency with each other. Alternative means of pursuing U.S. objectives in individual countries must be weighed in terms of their likely costs (in both monetary and

² Schelling, "PPBS and Foreign Affairs," memorandum, op. cit., pp. 4-5.

³ As a result of the new administration's reorganization of the national security process centering on the National Security Council, the SIG and the IRG, as such, no longer exist. However, the reorganization provides for forums whose membership is essentially the same as the SIG and IRG. Hence, the precedent established for using these forums to air budget issues is by no means insignificant.

⁴ Charles J. Zwick, "Commentary on Recent Developments in the Planning, Programming, and Budgeting System," in "Budget Bureau Guidelines of 1968," *Planning-Programming-Budgeting*, Subcommittee on National Security and International Operations, Committee on Government Operations of the U.S. Senate, 90th Cong., second sess. (Washington: Government Printing Office, 1968), p. 19.

nonmonetary form) and benefits. There are also the effects of our policies on third countries and the effects of the problems of third countries on us. Some of these country issues are treated during relevant budget reviews, but clearly the scope of these issues is not limited to budgetary decisions.

The great majority of the information for foreign policy decision-making is collected in—and collectable only in—country form. This includes information that is economic (GNP, prices, balance of payments), political (attitudes, power relationships), and social (literacy rates, birth rates). Even where such information relates to the achievement of global or regional objectives, it must be first analyzed on an individual country basis and, to the extent possible, standardized to make cross-country comparisons more meaningful.

Yet much of the relevant program data is scattered. It, too, must be brought together on a country basis, along with that related set of policy issues that now frequently remains the central concern of different agencies. Doing so should facilitate the exploration of often neglected interactions among programs and policies and the constructing of larger “packages” for negotiating purposes. On this latter point, although there are sometimes good reasons for treating some foreign activities in isolation from the central thread of foreign policy formulation (e.g., the Peace Corps in order to try to keep it nonpolitical), in most cases we need to break down these barriers, many of which are bureaucratic artifacts.

Finally, our emphasis on the need for better country analysis is motivated not merely by a desire to tidy up the process by which decisions are made, but by some evident failures of the present system. Consider Vietnam. Until recently, no group has been responsible for seeing to it that the full range of relevant information, hypotheses, ideas—including strongly divergent ones—is collected from inside and outside of the Government and made available to senior decision-makers. Consider the scope and the complexity of the factors involved—the military, intelligence, economic, political factors within South Vietnam; Hanoi’s capabilities and perceptions; the interests of Peking and Moscow; those of U.S. allies; U.S. domestic opinion; and many others. There are grounds for believing that some of our mistakes might have been avoided had we established a better system for collecting and evaluating what was going on and what our alternatives were.

Of course, some issues need to be considered on a regional as well as a country basis. This is true of many military issues, trade (e.g., with the Common Market countries), and the operations of regional development organizations. Although the number of important regional issues is smaller than is suggested by much official rhetoric, where they exist they can be handled in part by aggregating the country data and analyses described above and, remaining, by examining the relevant problems of the region as a whole.

C. Functional Problems

Many important international issues are of a global or functional character. And such issues often are not equipped with a good “budgetary” handle. The workings of the international monetary system, many international trade problems, some international communication and transportation matters, and the regulation of immigration are

examples. The global aspects of defense problems are growing in importance as the military globe becomes less bipolar and the threat of nuclear proliferation increases.

Two questions are relevant about these global or functional issues: Are they treated competently in their own right? And are important interactions between these issues and others adequately taken into account?

Many of these issues usually receive a high level of technically competent attention. International financial matters are subjected to a good deal of analysis by Treasury and Federal Reserve staffs, by the Economic Bureau of State, by private bankers and by academic economists; and trade matters generally get thoroughly examined by governmental and industry groups. International transport and communications policies are sporadically analyzed in depth by high level interagency groups.⁵

Without asserting that all such issues get adequately examined, we would emphasize here (as elsewhere) the need for cross-cutting analysis not limited to some narrow concern but rather directed at broader issues. Some of the "gold flow" actions taken by the United States in recent years in order to effect balance-of-payments savings have had costly side effects in other areas.⁶ The tendency for international financial matters to be decided by Ministers of Finance and central bankers means that crucial issues profoundly affecting the foreign affairs of countries are decided by groups that have little understanding of many of the broader consequences of their actions.

Interactions between problems are too often neglected. The external resource requirements of less developed countries can be met directly by foreign aid or indirectly by granting trade preferences for their products. Although "trade versus aid" tradeoffs clearly exist, the resource transfer implications of trade preferences or commodity agreements are difficult to calculate, whereas AID budget issues are regularly submitted to exhaustive—and recently devastating—treatment. Another troublesome problem is the tendency of some technologically oriented agencies to promote the transfer of their technology abroad (for example, in the field of atomic energy) even though such promotions increase our difficulties in achieving other objectives (for example, slowing the spread of nuclear weapons).

D. Background Knowledge

Implicit in much of what we try to do abroad are assumptions about the ways in which institutions work, the strength of forces making for change or for stability, the prospects for increased economic growth, the effects of such growth on political stability, the prospects for changes in the birth rate, the consequences of increased urbanization, and so forth. Yet we infrequently examine these matters in depth. And when we do so it is usually on rather narrow, albeit often important, questions: agricultural progress, birth control programs, the status

⁵ Examples are the White House International Air Transport Study of 1962-63, and the President's Task Force Study on Communications Policy of 1968-69.

⁶ Not all the side effects have been unforeseen or even costly. "Gold flow" reductions abroad have also been used as an excuse for cutting down on overseas activities deemed to have low productivity.

of a certain dissident group. Usually neglected is a systematic effort to get deeper and broader understanding of the societies with which we deal.

Many people in government feel that they have a good knowledge of Western European countries through family ties, education abroad, reading of professional and popular literature, foreign assignments, and occasional visits. Even here we may often exaggerate the depth of our understanding. But it is clear that few in government service have a deep knowledge of much of the rest of the world, including countries of great importance to the United States. Yet there is far from an adequate effort underway to correct these deficiencies. Some exceptions are a modest program of language training, commendable efforts by intelligence agencies to deepen their knowledge, increased specialization in the assignment of foreign service officers, and some research efforts by DOD and much smaller ones by AID and other agencies. The International Education Act of 1966, designed to promote international studies at both advanced and undergraduate levels, will no doubt eventually have payoffs for the U.S. Government's understanding of these problems. But these will not accrue quickly, and a general program is no substitute for a concentrated effort by the U.S. Government to increase its own intellectual capital.

One particular aspect of background problems deserving of more attention is the connection between the development process and U.S. interests. Implicit in programs designed to influence the development process is a conviction that U.S. interests are involved in the outcomes, and country achievements are often cited as proximate U.S. policy objectives. Yet it is usually difficult to establish the connection between outcomes and U.S. political objectives.⁷ This does not mean that the connection is missing but rather that too little is known about causes and effects of these aspects of the development process to determine the connections.

These problems have not all suffered from a lack of attention in academic and other research circles. But at least for U.S. foreign relations purposes, the research has too often lacked a real-world policy orientation. Econometric growth models, for example, are of value as a means of improving the understanding of the economic growth process, but they may be of little value in helping a country overcome the political obstacles to establishing sound economic policies. We know that urbanization changes the political complexion of a country, but not always the same way in every country. As a result, it is very hard for a U.S. policymaker to know whether or how to try to influence it—or if he can. We do not argue for policy research at the expense of basic research but for more attention to both, and to better linkages between them.

METHODS OF ANALYSIS

It is hard to tell where one foreign problem ends and another begins. But, despite this, we believe that a better analytic job can be done and that it can be done by the application of existing analytical concepts.

⁷ The absence of a clear notion of the U.S. interests in outcomes does not preclude programmatic attempts to influence outcomes. The growing U.S. disillusionment with foreign aid is, we believe, but one example of the frustration at U.S. inability to influence outcomes.

A. *Clarifying the Policy Problem*

The analyst's first task is to tidy up the problem package to the point where it is manageable, carefully taking note of parts he temporarily sets aside. (It is the decisionmaker's job to put the missing analytically intractable part back in.) The package must include those parts of the problem that strongly interact with one another. This criterion will permit analysis of some discrete, manageable problems, but it will not, of course, reduce the importance of parts set aside. The inevitability of overlaps should be clear from our earlier discussion of types of problems. Indeed, some particular problems need to be packaged and examined in several different ways before the analysis is complete.

Take the example of a U.S. base in a foreign country. Inevitably, the existence of the base is an important factor in U.S. relations with the host country—involving a specific security commitment, often raising local problems involving the presence of U.S. personnel, necessitating some "status of forces" arrangements, accruing direct and indirect economic benefits to the host country, and frequently requiring some quid pro quo. The base will constitute a part of some larger U.S. regional security posture. It may have characteristics that are duplicated by or can be substituted for a base in another country; it may be viewed by neighboring countries—friendly and hostile—as an indicator of the credibility of a U.S. regional security commitment. The base will also pertain to global problems. The existence of the base will determine, to some extent, the kinds of forces the United States needs to protect its regional security interests—with it, short-range lift capacity may be adequate; without it, more long-range troop lift capacity may be required. On the economic side, the cost of operating the base may result in a balance-of-payments drain. Finally, the base will affect budget decisions of several agencies.

Perhaps foreign base questions are near the more complex end of the international affairs and security problem spectrum, but there are few problems for which a "single cut" of analysis will suffice. The U.S. protectionist policy for textiles impinges on the economic development programs of foreign countries to which AID gives assistance. Arrangements between the United States and the United Kingdom on nuclear weapons affect French acceptance of the UK within the European Common Market.

The list is long, but the lesson is simple: most foreign policy problems are not analyzable until they have been reduced in size. This cutting down to size usually results in several problems, none of which is complete, but all of which are more analytically manageable than the original complex. Following the thoroughgoing analysis of each of the component problems, there remains one final task, that of bringing the pieces of analysis back together again as an input to decisionmaking.

In foreign affairs decisionmaking, this last task is performed haphazardly, and many times not at all. This is where a "foreign affairs budget" comes in. The decision by President Johnson to use PPBS as the instrument for improving the process of decisionmaking within the Government did not mean that all or even most policy decisions would be made in the context of budget considerations. However, the budget is an extremely useful device for policy review and control, and the creation of a foreign affairs budget could serve to focus at least

the consideration of program issues in foreign affairs. Moreover, the existence of such a budget would certainly not mean that one agency would do all the analysis or that it would administer all the funds. Most program analysis needs to be done in various agencies. Even if the President so proposed, the Congress would neither authorize a single agency to administer the necessary funds for the multitude of overseas programs and activities nor appropriate such funds. A foreign affairs budget, at least in the clearly foreseeable stages of development, should be viewed as a means of assembling the scattered pieces of data and analysis.

Since the objective of having such a foreign affairs budget is to make better policy and program decisions, it follows that an important question is the way the budget is structured. When Secretary McNamara took over the Department of Defense in 1961, the budget format he inherited was organized in terms of line items—such as Military Personnel, Operations and Maintenance, Procurement, and so on—that told him little about the objectives of the Department. One of his tasks was a reorganization of the budget into a program structure—a structure that introduced program categories which reflected rather more closely the principal aims of DOD activities and planning: Strategic Retaliatory Forces, Continental Air and Missile Defense Forces, General Purpose Forces, Airlift and Sealift Forces, and so on. To be sure, a dichotomy still exists between the form in which the Defense Budget is submitted to congressional appropriation committees and the program form that is the basis for force structure planning, but policy decisions are clearly made in a program context.

The Department of Defense approach indicates that the first step in developing a foreign affairs program budget will thus be to decide what constitute the basic “program packages.” We feel that the “individual country” should constitute the basic program package.⁸ The individual country is the building block of both foreign policy and foreign programs. Although country analysis is not sufficient, the country form is for many problems the most illuminating. Finally, both in Washington and abroad, the organization of the foreign policy community favors the country as the point for integration of management as well as policy control.⁹

However, the country programs would not include a large part of the Defense budget since these, for the most part, have a regional or global character. Moreover, many foreign policy questions requiring analysis—even those with large cost implications—may not develop in a manner or at a time conducive to examination in a PPBS context. The foreign affairs program budget should be viewed as a mechanism for periodically drawing together various kinds of analysis on individual countries.

⁸ See, for example, Thomas C. Schelling, “PPBS in Foreign Affairs,” memorandum to the Jackson Subcommittee, *op. cit.*, pp. 7–8; Charles L. Schultze, *Planning-Programming-Budgeting*, hearings before the Subcommittee on National Security and International Operations of the Committee on Government Operations, U.S. Senate, 90th Cong., first sess. (Washington: Government Printing Office, 1967), pp. 28–29; U. Alexis Johnson, *Planning-Programming-Budgeting*, hearings before the Subcommittee on National Security and International Operations of the Committee on Government Operations, U.S. Senate, 90th Cong., second sess. (Washington: Government Printing Office, 1968), p. 267.

⁹ For the foreseeable future any foreign affairs program budget will have associated with it an analog of the traditional (nonprogram) defense budget that is the basis for congressional appropriations. This analog will comprise the foreign operations portions of the budgets of the various agencies with programs overseas.

B. Formulating Policy Objectives

The formulation of policy objectives would seem to be an important part of policy analysis. However, the results of formal policy planning processes of the last three administrations do not strongly support this contention. In practice, statements of objectives have tended to be series of homilies that were unobjectionable in principle but not of much use as measures of policy success or program effectiveness.

Statements of policy objectives in the past have characteristically been forged in an interagency process (the National Policy Papers were drafted by interagency committees, and the Eisenhower NSC Planning Board was an interagency committee). An agency's participation in the process has been taken to imply its general approval of the resultant policy statement. The predictable results have been "lowest common denominator" statements of objectives which are either bland enough for all agencies to accept or vague enough for each agency to interpret to its satisfaction. In fact, getting agreement on objectives is often much more difficult than getting agreement on specific actions—unless the objectives have been largely drained of content.

What is needed is more nearly the opposite: the surfacing of conflicting views on policy and the reasons for them. It is the confrontation of differing viewpoints that produces much of the payoff from policy analysis. Of course, formulating objectives is a difficult analytical task even if consensus is not required. Objectives or ends are often difficult to distinguish from means. For instance, economic growth of less-developed countries is often cited as a national objective, but a close examination of U.S. foreign aid policy does not support the notion. Funds are not allocated so as to maximize third world economic growth, but rather to support the economies of countries in which the United States has substantial political interests. Aid may support economic growth policies in order to impart a progressive image to the recipient government. Or aid may be aimed at preventing economic collapse in a country where such collapse would seem to be disruptive of international order. Thus, in practice, economic aid has been a means to a greater end which is essentially political in nature—from the Marshall plan to the present.

The acceptance of an objective also depends on what is required to achieve it, assuming, of course, the latter is known—which is frequently not the case. The means may be too expensive in terms of budgetary resources, requiring a revision of initially stated objectives. Situations of this sort inevitably arise in the present circumstances of declining appropriations for economic and military assistance. In other circumstances, ends are rejected because they "do not justify the means" in some broader sense. For example, the means, though associated with a legitimate end, may require a degree of involvement in another country's affairs which makes the U.S. Government vulnerable to embarrassment or is simply contrary to the decisionmaker's notion of what it is proper to do.

If forcing agreement on general objectives tends to be self-defeating and ends and means are difficult to separate, it follows that the ordering of objectives by priority presents a logically very difficult task. However, even if these practical problems were overcome, we question the value of attempting to rank objectives by priority, in the first-

things-first sense. Certainly various policy objectives will be valued differentially, and it is theoretically possible to rank objectives in the order of the value attached to achievement. But such a ranking considers only the benefit side of a decision, and it will probably not be very useful unless it considers divisibilities within objectives and the utilities of pursuing them at the margin.¹⁰ The policymaker will certainly also want to consider the cost side, as his most difficult problem is allocation—how to get the most from the political capital and fiscal resources he has available. He may wisely reject an expensive program which makes a small contribution to the achievement of his highest objective in favor of a less costly program which contributes substantially to the achievement of a lower priority objective.

The aim in formulating policy objectives should be to expose the decisionmaker to a set that is relevant. In doing so the analyst should indicate where he thinks objectives conflict and possible means of resolving the conflicts. To the extent possible, objectives should not be treated as fixed goals but rather as desiderata, for which varying levels of accomplishment are possible. In short, it is as important to identify for the policymaker the existence of alternative packages of objectives as it is to identify alternative packages of programs to meet these objectives.

C. *Developing Program Alternatives*

The task of indicating to the decisionmaker the alternative means by which he can pursue his objectives is exceedingly important. But developing real program alternatives in international relations presents some difficult problems.¹¹ First, targets of opportunity often appear not because of anything the United States does but because of developments largely internal to a particular country; for example, a change in a government or ministry, or the need for a particular type of assistance. Second, existing programs usually have considerable momentum of their own which makes change difficult. The momentum exists within the U.S. Government—operators of particular programs form constituencies within the bureaucracy, and Congressmen whose own constituencies benefit from particular programs represent a force for continuation. The momentum exists overseas because programs can quickly become part of the general relationship the United States has with a particular country, and some individual government officials or some particular ministry usually has a stake in the continuation of each program. Hence significant program changes tend to disturb the bilateral relationship and create problems for the U.S. mission.

By all odds, the first step toward developing program alternatives should be an examination of current U.S. relations with and activities in the particular country.¹² This may sound trivial, but it is not. As a general rule there exists no complete compilation of U.S. programs in individual countries, not to mention evaluations of program effectiveness. Hence an important order of business is simply finding out what

¹⁰ It is useful to recall Adam Smith's classic treatment of value using the example of water and diamonds. Adam Smith, *The Wealth of Nations* (New York: Modern Library, 1937), p. 28.

¹¹ "Program" is used here in the broadest sense—foreign economic and military assistance membership in an alliance, diplomatic activity directed toward a particular purpose, etc.

¹² For a country program package examination in the PPBS context, this examination should cover a wide range of interactions. A more narrow analysis aimed at a specific problem need deal only with interactions relevant to the issue at hand, but even then the list is likely to be long.

is going on—what the programs and activities of the various agencies are; in what directions various members of the U.S. mission are trying to exert their influence.¹³

Stock taking itself is illuminating. It should be followed by an evaluation of what is actually being accomplished and why. In making this evaluation it is important to understand that the “real” objectives of a program may or may not have been used as the rationale for the program. Even if the program has been rationalized on the basis of what seem to be its “real” objectives, quite different outputs may justify the program. Where U.S. policy objectives are hard to establish, what is actually going on may shed a good deal of light on what real interests are. Therefore, the analyst’s list of program accomplishments should cover all significant outputs that seem relevant to U.S. interests.

The evaluation of existing programs should also shed considerable light on questions of new program feasibility. Domestic political factors in host countries which reduce the effectiveness of existing programs are constraints in designing alternatives. This applies equally to domestic budgetary and human resource constraints. On the positive side, the evaluation effort may point to areas where U.S. and host country interests closely coincide, suggesting new program patterns.

Experience is certainly not the only basis for judging the feasibility of new programs. But it is probably the best available indicator in most program areas in the absence of major changes in the environment of bilateral relations.

Finally, to serve the decisionmaker well, the analyst must attempt to be as rigorous in assessing the prospects for the success of alternative programs as he has been in pointing up the shortcomings of existing programs. This is difficult to do because for the analyst, as for everyone else, hindsight is sharper than foresight. Yet the effort must be made. One way the analyst can help ensure that his results will be balanced as to subject new program alternatives to more rigorous evaluation criteria than those of the existing program.

D. Program Costs

Program alternatives can scarcely be evaluated adequately in the absence of cost estimates. Yet lacking a compilation of total U.S. activities in a given country, the decisionmaker cannot estimate the cost of pursuing objectives with any degree of confidence. Probably the most useful approach to the cost question is to begin by costing current programs. This can and should be done concurrently with the compilation of activities proposed above. Combining these two exercises has the virtue of presenting the various types of data in common program terms.¹⁴

Data on current costs of existing programs may not, in many cases, serve as a very adequate guide for costing new program alternatives.

¹³ We do not mean to suggest time and motion studies of mission activities. Rather we are interested in what the United States is attempting to do programmatically and diplomatically.

¹⁴ We do not conceive of this cost work in elaborate terms. For these purposes it is not necessary—and perhaps not even desirable—to have the kind of detailed cost breakdowns produced by the comprehensive country programing system (CCPS) experimented with by the State Department several years ago. The cost data produced by CCPS included hour-by-hour breakdowns of how junior Foreign Service officers spent their time. Such data might be very useful in managing an embassy, in an administrative sense. But our concern is with a reasonably accurate description in program cost terms (which cut across agency lines) of how U.S. resources are being used.

Current data often show only the cost of continuing what is going on, whereas there are often substantial costs to initiating a program. Sunk costs in existing programs should, of course, be neglected in comparing prospective with present programs. But if such costs are substantial, they should serve as a warning of the potential for underestimating alternative program costs (which potential is great in any case).

One of the important functions of PPBS is to give an improved perspective to the costing questions. Too often decisions are made without an adequate understanding of cost implications, either because costs are incorrectly or incompletely calculated or because the presentation of costs, though technically correct, is misleading. The "Budget Bureau Guidelines for 1968" provide an instructive example of this latter problem:

. . . if a project will ultimately cost \$200 million, and if the first year budget authority would be \$40 million, the PFP (program and financial plan) should show for the budget year:

- (1) A program level of \$40 million if, as a practical matter, the project could be stopped at that point.
- (2) A program level of \$200 million, if, as a practical matter, the project would have to be completed once begun.
- (3) A program level between \$40 million and \$200 million if there is an interim stopping point.¹⁵

The discussion of costs up to this point has centered on straightforward, directly measurable program costs. However, many of the costs associated with foreign policy decisions are difficult to identify in program terms. And they often are of much greater importance than the program ones.

We do not pretend that indirect policy costs can be estimated—much less measured—with any degree of confidence. However, judgments on such matters are implicit in many foreign policy and national security decisions—the stationing of U.S. troops abroad, the deployment of naval forces, or the development or relinquishment of base facilities, to mention a few of the more obvious ones.¹⁶ Analysis may not provide a very adequate assessment of the liability side of policy costs. Rank ordering of the liability aspects of alternative policies and programs may be possible, and, at the very least, analysis can make explicit the liability aspects of policy.

E. *Uncertainty* *

Some irreducible uncertainty must be dealt with in most "real world" analytical problems, but we can think of few classes of problems in which the uncertainty component is greater than in international affairs. In the first place, it is frequently impossible to forecast political and economic developments in a friendly country, where we have access to a great deal of information, with much confidence, not to mention developments in or actions of an adversary country where we may have little information.

¹⁵ "Budget Bureau Guidelines of 1968", *op. cit.*, p. 11.

¹⁶ Concern has been voiced in recent years, notably by Senator Fulbright, that even U.S. economic assistance carries with it some implicit commitment to come to a country's assistance. And this argument has been advanced as a reason for amendments to the Foreign Assistance Act limiting the number of countries to which the United States may give economic assistance. (See "Foreign Assistant Act of 1961, as Amended," "Legislation on Foreign Relations, 1968," sections 201(b), 211(a), 401, 504(a).)

* Further discussion of this issue is found in the paper by Hirshleifer & Shapiro in vol. 1 of this collection.

The degree to which programs promote U.S. objectives also is commonly a matter of uncertainty. Often it is difficult to determine the effects of a program even on proximate objectives (the effects of aid on the economic growth rate, for example) not to mention the program's effect on basic U.S. objectives (strengthening the recipient country's political and economic fiber, for example).

Finally, there may be considerable uncertainty regarding the costs of a particular program. Military assistance program objectives are commonly stated in terms of the recipient country's force development, but equipment attrition and the maintenance capacity of the country are frequently unknown. Hence the cost of hardware needed to develop and maintain a desired level of effectiveness is a matter of uncertainty. Of even greater uncertainty are the costs of attaining the more fundamental objective, enabling the recipient country to deal with a specific threat.¹⁷

A useful analytical step toward dealing with the problems of uncertainty is to enumerate events or contingencies that might significantly affect the attainment of program objectives. Developing this list should be much more the task of experts in the program area (e.g., experienced political observers, military experts, economists, and technicians) than of any centralized analytical staff. The analyst's role should be that of probing the experts to be sure that the resultant list is as complete as possible.

The experts may be able to impute a probability distribution to some of the uncertain events identified, but their basis for doing so is usually subjective. Still the assignment of subjective probabilities may be useful to clarify or to point out inconsistencies in the analysts' and others' thinking. The decisionmaker may be wary of accepting subjective estimates of probability at face value, even when experts have reached a near consensus—and he should be wary. But what are his options?

He may, of course, decide that the information at hand is not sufficient to permit him to make a decision. He may then ask for more information. For example: Will a new seed variety triple the output of the crop? Can the country's technicians maintain the sophisticated aircraft? Whether this will help will depend on the extent to which the initial analysis used the available data, and whether additional relevant data can be collected. It will also depend on the direct cost of collecting and analyzing the additional data, and finally, on the costs of postponing the decision. Buying more information may marginally reduce, but will rarely eliminate, uncertainty.

The decisionmaker always has the option of buying time—postponing a decision. By waiting, some uncertainties may be resolved by the course of events. The election returns will be in; the need for the road may be clarified. However, as pointed out above, there is often a cost associated with waiting—in terms of opportunities lost, for example. Thus, U.S. silence might be taken as tacit support of a coup that is contrary to U.S. interests.

In many circumstances, the decisionmaker may choose a hedging course of action that preserves some of his options. This may involve

¹⁷ A case in point on the equipment side of this latter problem was the U.S. realization, after the Tet offensive of 1968, that the South Vietnamese Army had to be equipped with new, high-cost M-16 rifles to permit them to match firepower with Vietcong units newly outfitted with AK-47 rifles.

initially proceeding, in effect, along several paths with the full knowledge that all but one path must be abandoned eventually, and that the sunk costs and costs of abandonment must be accepted as the price of ascertaining feasibility. AID may finance several types of village radios on an experimental basis, knowing that it is infeasible for district offices to develop maintenance for more than one. In other circumstances, options may be preserved by selecting a course of action that will solve only interim problems, but will retain future options.

In rare circumstances, one of the decisionmaker's alternatives may appear superior to all others in each of the relevant contingencies. Of course, the existence of such a dominant policy or program can put an end to the decisionmaker's worries. Unfortunately, absolute dominance is rare in circumstances where analysis has concentrated on producing sound alternatives—not straw men—but a search for it may eliminate one or more inferior alternatives.

Except in the rare cases where one alternative dominates all others, the decisionmaker will have to cope with some residual uncertainty when he makes his decision. In the final process of deciding he will probably resort—perhaps subconsciously—to a form of sensitivity analysis. That is, he will attempt to take account of the degree to which contingencies will affect outcomes under various alternative courses of action open to him. Depending upon his preferences, the decisionmaker may opt for a course of action whose results promise to be very favorable under the most probable course of events. Or he may select an alternative whose results promise not to be quite as favorable under the most probable course of events but promise to be acceptable under a much larger range of contingencies. Even though the decisionmaker's consideration of the problem may involve either approach implicitly, he is best served by analysis that treats the matter of sensitivity explicitly.¹⁸

The systematic examination of uncertainties which we have prescribed may itself appear laborious and "uncertain." It is, but the stakes are high. To us many painstaking *ex ante* examinations of the "what if's * * * ?" seem justified if they can avoid a few hopeless *ex post* "but I had assumed * * *" excuses.

F. Evaluation of Alternatives

Our prescribed methods of analysis are aimed at one primary objective: developing a system of analysis which will better serve the decisionmaker by providing him with more relevant information and by widening the range and increasing the quality of the choices open to him.

If irreducible uncertainty is as pervasive in international problems as we assert, analysis will produce few, if any, clear solutions to policy problems. What analysis can and should produce is a series of policy or program options, some of which promise to work better in certain circumstances than in others, or which serve certain objectives better

¹⁸ For a discussion of this and other aspects of uncertainty see Albert Madansky, "Uncertainty," pp. 81-96, and H. Rosenzweig, "Technological Considerations," pp. 115-123, in E. S. Quade and W. I. Boucher (eds.), *Systems Analysis and Policy Planning: Applications in Defense* (New York: American Elsevier, 1968). Rosenzweig suggests that the performance of a system (policy or program) should be viewed as a band of different widths instead of a fine single line. Madansky recommends going further "to include subjective probabilities across the band, since the extreme of the band may not be as likely as is the 'fine single line' somewhere in the middle of the band."

than others. The quality of this analytical product will depend upon how well the problem is defined, and how effectively it handles objectives, program alternatives, costs, and uncertainty. But the value of analysis to the decisionmaker will often be determined, in large part, by how all relevant factors are integrated into a concise, relatively short document that presents and evaluates alternative courses of action. What the decisionmaker has received too often in the past is a memorandum which, in effect, reads, "Here is the problem * * * I recommend * * *." The originator may have systematically examined all aspects of the question, but by not making his interim conclusions and his basis for arriving at them explicit, he leaves the decisionmaker little choice but to accept or reject his judgment.

Finally, the analyst will serve the decisionmaker well if he insures that any analytical document he prepares enumerates all conflicting opinions of any merit. This, of course, is not a function of analysis *per se*. But since judgment will almost always be an important element in a decision, the decisionmaker deserves to have the benefit of all that is available. Knowing where differences of opinion exist should help him to conserve his effort and focus his judgment on the more crucial aspects of a problem.

III. ORGANIZATIONAL PROBLEMS AND APPROACHES

The foreign affairs system—in the broadest sense—should primarily be designated to serve the President by enabling him better to fulfill his responsibilities in directing U.S. foreign affairs.¹⁹ This does not flow simply from the President's authority under the Constitution, but from his position as political and administrative leader of the United States. The responsibility for foreign affairs could not reside elsewhere.²⁰ The predominance of Presidential authority distinguishes the conduct of foreign affairs from that of domestic affairs, where responsibility is diffused widely. However, since it is obviously neither desirable nor possible for the President to involve himself in all, or even many relatively important, policy decisions, he must delegate a great deal of authority. But while he can delegate authority, the President cannot unburden himself of responsibility. Therefore, it is essential that those to whom the President delegates such authority act on behalf of the President, in the President's interest—in short, that they adopt insofar as possible a Presidential perspective.

This does not mean that everyone is expected to or should have an Olympian view of the world. When the President turns to the Joint Chiefs for military advice, or to the Arms Control and Disarmament Agency for advice on our arms negotiation with the Soviet Union, he should not expect to get a balanced overall judgment. Instead,

¹⁹ The foreign affairs system we refer to is that of the executive branch. A separate question is the role of analysis in supporting the Congress in the field of foreign affairs. This aspect is especially pertinent in light of the growing congressional tendency to check executive authority in foreign and defense matters. Without arguing that it is in the national interest for the power of Congress to be increased in this area, there is little doubt in our minds that the relevant committees of Congress could do a more effective job in illuminating issues, in eliciting information from the executive branch, and in generating alternative policies. They could do so by equipping themselves with larger and better staffs who are able to do independent work and to draw more on the analytic resources of the academic and research communities.

²⁰ The extent to which the President exercises his role personally, relies more upon the Office of the Presidency (special assistants, NSC staff, BOB, etc.), delegates considerable authority to one or more Cabinet officers, or uses the formal National Security Council apparatus will determine to some extent the organizational structure needed.

what he is looking for is a competent treatment of the issues from those with *particular responsibilities* and expertise. On the other hand, because he *is* dependent on these agencies for advice, and because every bureaucracy can be expected to have certain biases and vested interests, he should take precautions to try to assure that issues get examined in the round. One way is for him to choose people for senior positions in agencies who are competent in running their agencies and in representing their expertise and who are sensitive to Presidential needs. Another is to equip himself with independent analytic capabilities.

The function of such Presidential staff analysts is not to collect the pieces submitted by agencies and simply staple them together but (1) to elicit ideas, evidence, options, and beliefs on issues held throughout (and outside of) the Government; (2) to make independent investigations and raise sharply pointed questions on matters of importance on which there is a basis for raising questions; and (3) to do comprehensive analysis, which can be done only at a high level. These are not easy tasks. There are frequently bureaucratic barriers to the flow of information upward, and people with good ideas do not always know how to articulate them or where to place them. Perhaps most difficult is the discipline of not letting the analysts' own views unduly distort or color the advice coming from other quarters.

The role of analysis depends most of all on the attitude of the decisionmakers. Unless it is demanded by the President, and unless the President organizes not only his own office but the entire system to this end, the foreign affairs bureaucracy will not provide him with the materials needed to make better decisions. Agency doctrines, interests, and perceptions have a very strong influence on agency behavior. And the process of interagency coordination often involves a good deal of logrolling as a means of resolving conflicts. This mechanism is the only means of dealing with many day-to-day problems. Accommodation and adjustment are necessary if the system is to function at all, but on important issues such compromises frequently result in poor decisions. At the very least, among the logrollers there should be a strong representative of the Presidential interest.

In short, the *sine qua non* for analysis to serve a useful purpose is to have a decisionmaker who will use it. Decisionmakers can do without analysis, and the proof of that fact is that they have so often done without it in the past; but good analysis and analysts cannot do without decisionmakers.

But for all of the importance of analysis at the top, policy analysis in international affairs should not be the function of a single staff but rather that of many analytic staffs within the various foreign affairs agencies, and at different levels within these agencies. For example, some of the major benefits of introducing systems analysis in the Office of the Secretary of Defense have been the effects external to that Office. The use of systems analysis by the Secretary of Defense has contributed to improving the quality of staff work done within the services. The introduction of better, more systematic analysis of international problems should have a similar demonstration or competitive effect. But for this to happen, much of the product of the senior staffs will have to be (1) of high quality, (2) visible in the form of written analyses to those with a need to know throughout the Government, and (3) taken seriously, because they often form the basis for action.

What makes a good analyst? It is true that people with formal, quantitative analytic training tend to be found in these jobs. And there is much to be said for the value of quantitative skills. However, the way an individual thinks about problems is often more important than his academic discipline. On this subject, Charles Schultze, former Budget Director, observed:

[PPBS has tended to attract] people who attempt to pin things down and use analytical processes as opposed to the intuitional approach * * * If you look at our [Budget Bureau] staff or the staff of Alain Enthoven in Systems Analysis, you will find people of all kinds of backgrounds. Law, for example, is very good training for this.²¹

Some characteristics of a good analyst can be summarized: He must be interested in problems and the process of problem solving; he needs to be persistently curious, willing to dig for relevant information; he can certainly have predispositions regarding solutions to the problems he deals with, but he must be able to separate predispositions from the findings of analysis; he should understand when problems are complex, but not be totally cowed by that complexity; above all, he needs to recognize and acknowledge limitations in his work.

We have used the term "analysts" as though this is a distinct group of people from "decisionmakers" or "operators." This is perhaps misleading. For although there are some groups that have a staff advisory function, there are many in line operating positions who can and should provide organized analytic advice to their superiors on certain matters. Thus, it used to be said that Secretary McNamara was the senior systems analyst in the Defense Department. Moreover, even those clearly in a staff advisory capacity bear a certain responsibility for "decisionmaking" in the way they formulate issues, in the data they decide are relevant, and, of course, in the recommendations for action that they make.

Knowing more about the decisionmaker's presentational preferences and how he uses the product can significantly increase and sometimes even determine the value of analysis. While the manner of personal presentational preferences may seem trivial to some readers, few who have served on an analytic staff will deny their importance. A knowledge of how much time the decisionmaker will spend on the problem is virtually essential to effective analysis. If he has only 15 minutes to devote to the problem, a 25-page analytic study will be of little value. But a tightly written, two-page memorandum sometimes can summarize the most relevant points, outline options, and provide either the basis for a decision on the question at hand or a determination that it is important enough to merit more time and study. Finally, there must be guidance down from decisionmakers. Analysts need to know which kinds, or what aspects, of problems most concern the decisionmaker—particularly when he is the President. Analysis should, of course, cover what is relevant, but it should not dwell on an aspect of the problem the decisionmaker already understands or is not interested in. More knowledge about any particular problem might always be useful but there are also other problems. Here the analyst must do his

²¹ *Planning-Programming-Budgeting*, hearings before the Subcommittee on National Security and International Operations of the Committee on Government Operations, U.S. Senate, 90th Cong., first sess., Aug. 23, 1967 (U.S. Government Printing Office, Washington: 1967).

own cost-effectiveness analysis of the decisionmaker's time, using his knowledge of how the product will be used.

Foreign affairs problems usually are not settled by "one time" decisions. More often it is a process involving a series of actions. This implies that the analyst needs to monitor operations. This should not mean becoming immersed in cable reading or engrossed in day-to-day operations, but rather staying abreast of what is going on and being alert to how the bureaucracy is carrying out decisions made at the top. Bureaucracies can frequently construe guidance to mean something contrary to the decisionmaker's intent. But the utility of the monitoring function is greater than that of merely being a watchdog over the bureaucracy. Because of the complexity of international problems, the decisionmaker is almost sure to have to deal with a gap of uncertainty even after analysis has been pushed to the limits of feasibility. Situations change, new data become available, old hypotheses become questionable. Here the effort to oversee what is occurring can have its greatest value.

SOME KEY ORGANIZATIONAL FEATURES

How problems are dealt with by different foreign affairs agencies reflects not just differences in the nature of the problems, but also differences in the organizational character of the agencies. These differences are a function of bureaucratic traditions and styles, the disciplines and personalities of the people involved, and individual and group loyalties, as well as the kinds of activities an organization is charged with carrying out.

A. *The Office of the Presidency*

How any organization operates depends, to a considerable extent, on who heads it, but for no other organization in the U.S. Government is this as true as it is for the Office of the Presidency.²² The selection of Presidential Special Assistants, the Budget Director, members of the Council of Economic Advisers, and other such key positions will obviously reflect the President's personal preferences for people as well as his mode of operation. However, even an organization as institutionalized as the Budget Bureau can be changed substantially to conform to Presidential preferences, as was demonstrated by the change between the Eisenhower and Kennedy-Johnson administrations. And radical changes in the functions and composition of the National Security Council staffs from the Eisenhower through to the Nixon Presidencies show the importance of the President's personal preferences in determining how he will use his own staff. Given this, what relevant observations and generalizations can be made regarding the role of the Office of the Presidency in foreign policy analysis, decisionmaking, and execution?

²² We include here the White House Office staff and all the staff organizations of the Executive Office of the President. The latter group includes the Bureau of the Budget, the Council of Economic Advisers, the National Security Council Staff, the Office of Science and Technology, the Office of the Special Trade Representative, the National Aeronautics and Space Council, and the Office of Emergency Preparedness. The proposed total of authorized positions for these organizations in fiscal year 1970 was 1298. See, "The Budget of the United States Government: Fiscal Year 1970, Appendix" (Washington: Government Printing Office, 1969), pp. 1012-1014. However, in the past there has been a substantial number of professional and clerical personnel "on loan" from other agencies to the White House staff for indefinite periods who do not show on the White House rolls.

Presidents rely on their own immediate staffs to view issues consistently from a Presidential perspective. A President may call on particular individuals within his Cabinet for judgment and advice on a broad range of questions, presumably not only because he holds them in high regard and trusts them, but also because he expects them to view matters from his perspective with his broad interests in mind. And this is more likely to happen in foreign affairs and national security matters than in domestic ones. But even Cabinet members with the broadest sense of the public interest have a responsibility to represent their departmental interest. Not so with members of the President's staff. Their job is to serve the President by adopting his perspective in a national political interest sense.

Each of the past three Presidents relied to some degree upon analysis done within the Office of the President. Even during the Eisenhower Administration, when the Interagency Planning Board prepared most of the analytical backup material for the National Security Council's policy deliberations, a "special staff" of senior NSC staff professionals was assembled. The special staff, directed by the NSC's Deputy Executive Secretary, was used as an independent source of analysis of Planning Board papers and departmental recommendations, and as the briefer of the President before council meetings.

Downgrading the importance of the National Security Council and abolishing its interagency support groups, President Kennedy chose to place greater reliance upon the Presidential staff as a personal source of analysis and advice on foreign and national security policy. Commenting on the role of the Kennedy Presidential staff, McGeorge Bundy wrote:

This staff is smaller than it was in the last administration, and it is more closely knit. The President uses in these areas a number of officers holding White House appointments, and a number of others holding appointments in the National Security Council staff. He also uses extensively the staff of the Bureau of the Budget. These men are all staff officers. Their job is to help the President, not to supersede or supplement any of the high officials who hold line responsibilities in the executive departments and agencies. Their task is that all of staff officers: to extend the range and enlarge the direct effectiveness of the man they serve. * * * There remains a crushing burden of responsibility, and of sheer work, on the President himself; there remains also the steady flow of questions, of ideas, of executive energy which a strong President will give off like sparks. If his Cabinet officers are to be free to do their own work, the President's work must be done—to the extent that he cannot do it himself—by staff officers under his direct oversight.²³

Initially, President Johnson used his staff in essentially the same manner as President Kennedy, although he was somewhat less deeply involved in most foreign affairs issues than his predecessor. Three significant changes occurred in early 1966: Walt Rostow replaced McGeorge Bundy as Special Assistant for National Security Affairs; a separate staff for Vietnam nonmilitary affairs was established under

²³ McGeorge Bundy, Letter to Senator Henry M. Jackson, dated Sept. 4, 1961, in "Administration of National Security: Selected Papers," Subcommittee on National Security Staffing and Operations, Committee on Government Operations, U.S. Senate (Washington: Government Printing Office, 1962), p. 7.

Special Assistant Robert Komer; and NSAM 341 established regional and worldwide interdepartmental policy groups under State's leadership. However, despite his formal delegation of greater authority to State, the President continued to rely heavily upon the Presidential staff for analysis, and routine procedures were established for staff analysis and Presidential decision on issues involving financial matters, food, and military aid.

However well this system may have worked in general, it is clear that there have been some important deficiencies in the nature of the policy analysis available to the President in recent years. For example, the most urgent foreign problem the U.S. has faced during this period, Vietnam, has not had the attention of a full-time senior staff addressing all aspects—military, political, economic, psychological. Regular “Tuesday luncheons” attended by senior officials, all of whom had other major responsibilities as well, were an inadequate substitute for such a full-time group.

The Kennedy-Johnson national security staff, although containing many excellent people, was small and often—of necessity—focused on current operational problems. The joint result, combined with the poor quality of much of the material routinely submitted by the departments, left many issues inadequately treated. Further, the Budget Bureau, which conducts the only overall review of some issues from a national viewpoint, is poorly placed to provide comprehensive analytic advice that takes proper account of nonbudgetary foreign issues.

The staff organization of the Nixon administration is still in the process of development, and the role of the staff will be determined more by Presidential behavior, over time, than by organizational directives and charts. However, the new President has made clear his intent to use the office of the Presidency as a source of independent analysis. At the same time that he revitalized the National Security Council's role in foreign policy formation, President Nixon selected Henry Kissinger, a highly respected foreign policy analyst, as his Special Assistant for National Security Affairs. Kissinger, in turn, has assembled a group of highly competent professionals in an NSC staff a good deal larger than those of the Kennedy and Johnson administrations. Within this staff are essentially three groups: regional and functional specialists, a planning group, and a small program analysis staff. While revoking NSAM 341 (President Johnson's attempt to give a larger policy and coordinating role to State), President Nixon has essentially kept the Assistant Secretary-level Interdepartmental Regional Groups (IRG) and the Under Secretary-level Senior Interdepartmental Group (SIG), but he has placed them within the National Security Council system.

The attraction of serving on the Presidential staff is sufficiently great to assure a supply of highly competent professionals to fill the positions—on the condition that their talents are used. The consistently high quality of the professional staffs of the National Security Council, the Budget Bureau, and the Council of Economic Advisers supports this premise.

Notwithstanding this, there are limitations to what the Presidential staff can do by way of analysis. Their numbers should remain small to prevent overbureaucratization. And the staff must perform other functions: monitoring operations, responding to Presidential requests

for information, communicating to the various agencies their impressions of what the President expects. Even if the constraints of small size and preoccupation with other activities were overcome, there would remain a fundamental limitation on the analysis that the President's staff can do itself. For inputs to its analysis, the staff must depend almost entirely upon the operating agencies with their large bureaucratic resources and information. And to a large extent the necessary inputs will not be available in the agencies unless the agencies themselves are also performing similar analyses. Thus, the President's staff cannot do its own analytic job efficiently unless others in the agencies are doing theirs.

B. *The Department of State*

The Department of State has strong institutional characteristics that have been little affected over time by changes in its own top leadership or the Presidency. To some this is virtue; to others it is vice. But there seems little disagreement about the fact that it is so. While we are less convinced than some that what has been in the Department of State will necessarily continue to be, a discussion of State's organizational structure and functions and its other institutional characteristics is less subject to being "dated" than any such discussion of the Presidency.

The Secretary of State has the responsibility for overall direction, coordination, and supervision of U.S. activities overseas. The tradition of the Department is that it serves as a staff for the Secretary to enable him to fulfill his responsibilities. In fact, State has many more of the characteristics of an operating agency than of a staff agency. Diplomacy is a global operation which engages most of the Department in day-to-day matters that are little connected with the "seventh floor" (residence of, and shorthand for, State's top command and their staffs). However, there are four staff groups within the Department who are sufficiently free of day-to-day operations to permit them to provide substantial staff services for top leadership; the Bureau of Intelligence and Research, the Policy Planning Council, the Bureau of Economic Affairs, and the Political Military Group.

The Bureau of Intelligence and Research (INR) has roughly 150 professionals whose function it is to examine the intelligence intake of the U.S. Government and the research output of private individuals, institutes, and the academic community. INR also is responsible for representing State on the formal interagency U.S. Intelligence Board and for contributing State's views on intelligence issues. Its principal staff output takes the form of memorandums on selected topics which are designed to provide policymakers with a different analytic perspective from that of other members of the intelligence community.

The chief problem with INR is that its staff is too operationally oriented and spread too thin. One of INR's office directors recently wrote about his staff:

. . . as research analysts, they simply do not have sufficient time both to keep on top of current issues *and* to remain adequately steeped in all those other aspects of academic and other external research and reflection which could enrich their more fundamental studies and ultimately, their current analysis.²⁴

* E. Raymond Platig, "Research and Analysis," *The Annals*, November 1968, p. 57.

The Policy Planning Council has the broadest charter to examine foreign policy issues. Traditionally, the Council has attracted a group of foreign affairs specialists from both within and outside Government who have dealt with a wide range of issues cutting across political, military, economic, and other matters.

In recent years, a considerable part of its effort has been devoted to the preparation of national policy papers (NPP's) on specific countries. However, these papers are generally regarded in the foreign affairs community as not being very useful, despite the talent that goes into their preparation. It is worth considering why this is so. For one thing, the papers have tended to be very general. This reflects, on the one hand, a proper interest in having a broad perspective; but it also reflects a remoteness from actual decisions, a lack of relevance or "bite" in the discussion of issues. For instance, they usually have not dealt with foreign programs in any detail. Fisher Howe, a former Council member, described the articulation of objectives as "largely unsystematic and haphazard" in which "precision and comparability are not achieved."²⁵

The Bureau of Economic Affairs, in addition to many other duties, performs State's economic analysis. It has often served an important function within Washington's economic policy community by giving a broader policy perspective to economic policy considerations. The Bureau has attracted some highly competent, policy-oriented economists, but it has consistently had difficulties staffing in depth and scope. The Bureau's effectiveness has been limited largely by two factors: First, the Secretary of State's responsibility in formulating international economic policy is shared with strong domestic agencies, and recent Secretaries have not been assertive in the role that have—much less sought to expand it. As a result, the Bureau has often acted more as a staff to the Assistant Secretary for Economic Affairs or to other agencies or interagency groups than to the Secretary. Second, State provides generally weak career incentives for economists. The career Foreign Service economist's function is generally viewed as "reporting," not "analysis," and because State's role in economic policy formation is circumscribed, the Bureau has difficulty attracting economists from outside the Foreign Service.²⁶

The Politico-Military Group (G/PM), headed until recently by a deputy assistant secretary within the Office of the Deputy Under Secretary for Political Affairs, is State's institutional answer to policy coordination with the Pentagon.²⁷ This staff is generally good but small. It does not begin to have the staff resources to stay abreast of the range of security issues bearing on foreign policy. As a consequence it is forced to be highly selective. Although the Secretary of State has had the opportunity to review the annual force structure program of DOD before it goes to the President, Secretary Rusk did not choose to involve State very deeply in security questions. Thus G/PM's analytic capabilities have been used more in liaison functions on particular

²⁵ Fisher Howe, "Policy Planning in the New Diplomacy," *The Annals*, November 1968, p. 46.

²⁶ During the past several years the Foreign Service has begun to stress the importance of economics in recruitment and in midcareer training. No doubt they are better off for having done so, but increased demand for economists in the professional marketplace has probably left State less competitive than before.

²⁷ In the new Administration, G/PM has been restyled J/PM, and is now attached directly to the Office of the Under Secretary for Political Affairs.

issues than for a generally substantive input to defense decisions affecting foreign affairs.

A great deal of the power in the State Department below the Secretary and Under Secretary levels resides within the regional bureaus, and this power is jealously guarded. The regional bureaus view themselves as staffs to the Secretary and have institutionally resisted the creation of independent analytic staffs for the "seventh floor." Nor have the regional assistant secretaries created analytic staffs for themselves within the bureaus, but have relied on the "country desk" organization for analysis as well as day-to-day operations.²⁸ Although this regional integration of analytic and operational functions is consistent with the bureaus' views of how the Department should function, the result is that neither the Secretary nor the assistant secretaries have anyone whom they can ask for routine substantive analysis other than busy operators.²⁹ And the operator's perspective is constrained by deep involvement in day-to-day matters. One of the chief weaknesses of the interdepartmental coordination structure (the SIG and the IRG's) established in 1966—and recently changed by the Nixon administration—was the lack of staffs to develop and analyze agenda items.

No discussion of the State Department—however brief—would be complete without some mention of the Foreign Service as an institution. The Foreign Service is generally regarded as the professional corps of highest caliber within the U.S. Government. Yet many of the inadequacies in formulating and executing foreign policy are attributed to the Foreign Service. And the "Young Turk" movement and the soul searching that has begun within the Foreign Service during the past 2 years indicate that many of its members are seriously concerned about their personal future and that of the institution's.³⁰

Despite the competency of many Foreign Service Officers, there is probably no group, as a whole, within the U.S. Government less disposed toward systematic decisionmaking than the senior members of that corps—officials who either head or dominate our missions abroad. By background, by experience, by selection within the system, they epitomize the intuitive operator. Since they have been trained mostly in the liberal arts, have usually served for much of their careers as generalists and political officers (as distinct from being specialists in administration, intelligence, or information), and have been selected for promotion in part because they are *not* specialists in any particular field, it would be surprising if this group had characteristics different from those that they possess.

Notwithstanding this, there are good analysts among the Foreign Service. More important, the Foreign Service is by far the largest source of expertise on foreign affairs in the U.S. Government, and unless this expertise is mobilized, good analysis in international affairs will be slow in coming.

²⁸ The one partial exception is the combined State/AID Latin American Bureau (ARA-LA), which has several groups that partially fulfill this staff role.

²⁹ Obviously, the Secretary can call on one of the four analytic staffs discussed above, if the matters fall within their area of expertise. However, the particular institutional characteristics of the two broadest-gauge staffs, the Policy Planning Council and INR, make them inappropriate for what might be called routine analytic tasks.

³⁰ One manifestation of these developments was the publication of *Toward a Modern Diplomacy* (Washington: American Foreign Service Association, 1963).

C. *The Department of Defense**

On this subject so much has been written in recent years that there is little that we want to add. The Defense Department probably has gone further than any other part of the U.S. Government in doing systematic analysis and research, much of which is relevant directly or indirectly to international matters. For example, analysis that improves the capability for quick response air deployment of U.S. forces to trouble spots overseas may lessen our dependence on foreign bases, with direct consequences for our overall relations abroad.

Among the central features of the DOD analytical system relevant to the foreign affairs analytical system is, of course, its consolidation within the planning, programming, budgeting system (PPBS), which was devised and applied in the DOD before being introduced in other agencies beginning in 1965. This system generates several planning documents in recurring cycles that serve an extremely useful role in communicating concepts, decisions, and a common basis for policy—and provide a forum for constructively organized debate about policy disagreements. One document is the 5-year force structure and financial program, which describes decisions about approved military programs and their fiscal implications for a 5-year period. Still another is the Secretary's annual posture statement (which appears in both classified and unclassified form), analyzing, in broad scope, key defense issues and programs to deal with them. Behind this statement lie more detailed analyses and reasons for decisions in the form of draft presidential memoranda, which serve as the focus for internal review and debate about programs and policies. A relatively new innovation is the development concept paper, which serves a similar function on research and development issues. In addition, the Joint Chiefs of Staff continue to produce the joint strategic objectives plan, which recommends forces and programs for long-term requirements. Finally, there are many special analytic studies that articulate particular issues in depth.

Another feature of the DOD analytical system is the great use made of the academic and research community. This is, of course, true for a wide range of DOD's activities, of which research on foreign problems constitutes only a small part.

The activities of the Joint War Games Agency in the Joint Staff are also worthy of mention. Among other things, the Agency conducts very useful political "games" in which hypothetical crisis situations around the world are simulated. These games involve the participation of people throughout the foreign affairs and defense agencies and provide a useful forum for review and discussion of issues of wide concern.

What is particularly relevant to our topic is that, although the DOD does its best to take a broad view, incorporating considerations going beyond the narrowly military, it remains primarily responsible for military affairs. No countervailing system of comparable degree of organizational strength and analytic competency exists to represent nonmilitary interests.

*Further discussion of this issue is found in the papers by Enthoven, and Enthoven & Smith in this volume.

D. *The Central Intelligence Agency*

The Central Intelligence Agency has a major analytic role, and has maintained a clear distinction between analytic and operational functions, which in its case seems vital. The analytic function is central to CIA's role as intelligence estimator and forecaster. Because its analysts often have strong academic interests, they tend to be receptive to ideas from outside the government. And because his interests lie primarily in the country (or problems) he analyzes, not in either U.S. programs or policy toward that country, his views are less encumbered by a need to justify U.S. actions. Because this type of work tends to attract the intellectual and because he is relatively free of program or policy commitment, the quality of much of CIA's analysis is quite high. (The analyst is not, of course, wholly free of involvement with policy; or of constraint imposed by earlier forecasts he may have made; or of institutional biases.)

The types of policy problems we described earlier are ones to which the intelligence community makes a substantial contribution by providing facts on foreign countries, evaluation of facts, estimates of intentions of foreign governments, warnings of possible foreign actions, and assessments of the consequences of possible actions by us. Over the last several decades the intelligence analytic function has been greatly increased in importance. Together with budget-oriented analysis, it is one of the two areas in the foreign affairs-national security area in which analysis has been most developed. But the analysis of the intelligence community has important limitations, some of which may be inherent.

These limitations result, in part, from CIA's detachment from policy—the very detachment which gives the CIA analyst his independence of view. But a better balance between policy involvement and detachment might be struck. If the intelligence analyst is unable to interact strongly with policymakers, especially in State, it is not easy for him to focus on the most relevant issues. (This limitation, of course, also applies to personnel in State's INR and the Defense Intelligence Agency.) But for this greater degree of interaction to happen, an initiative must be taken by policymakers, especially in State, to bring the intelligence analysts more intimately into contact with them.

Another difficult problem in using the analysis of the intelligence community is distilling the good from the bad. In some areas there is by now a substantial record of analysis and prediction by intelligence analysts which suggests that, if their advice had been taken seriously by policymakers, some bad decisions might have been avoided. But how might policymakers have known which intelligence to take most seriously? There is no clear answer. Perhaps it would be worth a serious effort to explore the accuracy of expert forecasts, to attempt to determine the characteristics of both successful and unsuccessful predictions.

The analytical work of the intelligence community is vital. But can its value be increased? This question cannot be answered until we have a better understanding of the use that is made of intelligence analyses. Some of the questions that need to be addressed are these: How good, how timely, and how relevant have intelligence analyses been? How often has good analysis been done but not been acted upon?

What seems to be the reason for neglect? Is it a failure to treat issues that are most important to the operators? If so, why have the operators not communicated their needs to the intelligence analysts? Or is it a bias on the part of operators against analytical inputs? Or is it something about the pressures of the decisionmaking environment? Or are there other explanations?

E. Other Agencies

The UNITED STATES INFORMATION AGENCY (USIA) is an operationally oriented organization largely comprising reporters, linguists, broadcasters, public relations men, and so forth. Analysis tends not to interest such specialists, and their input to policy is slight. Attempts have been made to evaluate the types of coverage provided by various media and to obtain better cost data for various activities by the application of PPBS, with some useful results.

The AGENCY FOR INTERNATIONAL DEVELOPMENT (AID) has perhaps progressed further with program analysis than any other foreign affairs agency apart from Defense. Many of AID's activities lend themselves to systematic, quantitative analysis. In its organization is an Office of Program and Policy Coordination (PPC), a central coordination, analysis and information staff whose function is to serve the Administrator. In the regional bureaus are development planning staffs whose principal function is to provide the Assistant Administrators with independent analysis. AID's activities have tended to attract people with an analytical orientation: economists, engineers, technical specialists of various types. Finally, AID has faced many difficulties, some inherent in its work abroad, some associated with its lack of support at home. Much analysis, though not in its most constructive form, has focused on finding program vulnerabilities and dealing with adversity.

It is hard to separate the difficulties of AID's problems from the shortcomings of its analysis. At times, AID has given too much attention to external resource constraints on economic growth and not enough to poor economic policies deeply rooted in the domestic politics of recipient countries. In other instances, AID has perhaps undertaken projects that were doomed to failure because they ran head on into traditional values, and AID's development analysis has frequently given insufficient attention to deepseated cultural factors. More often AID's programs have suffered from the lack of coherence in U.S. foreign policy—AID has too frequently found itself with a program in search of an objective.

Many agencies have a share in the formulation of international economic policy. The TREASURY DEPARTMENT takes the lead in the field of international economic policy. The Secretary of the Treasury is the U.S. Governor of the International Monetary Fund (IMF), the World Bank (IBRD), the Asia Development Bank (ADB), and the Inter-American Development Bank (IDB). Treasury Department preeminence in international financial matters dates back to the Bretton Woods Conference in 1944 and the establishment and Treasury Chairmanship of the National Advisory Council on International Monetary and Financial Problems (NAC) by the Bretton Woods

Act of 1945.³¹ The importance and scope of Treasury's authority in international financial matters has increased with the establishment of international development lending institutions (that is, IDB, IDA, and ADB). On international monetary issues, which have been prominent among U.S. foreign policy problems during the past several years, the Chairman of the FEDERAL RESERVE BOARD (in effect, the U.S. central banker) assumes an important share of foreign relations responsibility along with the Secretaries of Treasury and State.

State is only one of a half dozen agencies among which the responsibility for international trade policy formation is fragmented. "The OFFICE OF THE SPECIAL TRADE REPRESENTATIVE (STR) was created in the Executive Office of the President for the purpose of negotiating the Kennedy Round. The SECRETARY OF COMMERCE is a representative of both the export promotion and protectionist interests. The TARIFF COMMISSION becomes involved in the latter class of problem. The DEPARTMENT OF AGRICULTURE is concerned with trade policy when U.S. agricultural produce is involved. And finally, when trade issues become balance-of-payments issues—as they frequently do—the Secretary of the Treasury must assume some responsibility for international trade policy.

F. Overseas Missions

A large U.S. embassy is much more than an overseas extension of the State Department. It may house representatives of two dozen different U.S. government agencies. The total number of agencies with overseas programs is in the neighborhood of 40. But for our purposes, overseas missions fall into two groups: those in which there are sizable operating programs under the overall direction of the Ambassador and those in which there are not. (We exclude those operating activities of U.S. combat forces because of the severe limits to ambassadorial authority over these forces.) Where the United States has operating programs, the Ambassador has managerial responsibilities; where it does not, the Ambassador and his staff nevertheless have an important role in policy formation.

The Ambassador's authority over all U.S. Government activities in his country (except U.S. combat forces) is clear.³² But how the mission operates depends in part on how its performance is measured in Washington. But because of the inadequacies in Washington coordination and overview, what the field sees is many distinct "counterparts" in Washington, each with its own criteria for evaluating performance. This fact, plus the lack of experience in administration

³¹ The NAC comprises the Secretaries of Treasury, State, and Commerce, the Chairman of the Board of Governors of the Federal Reserve System, and the President of the Export-Import Bank. The "old NAC" was abolished on Jan. 1, 1966, under the provisions of Reorganization Plan No. 4 of 1965, but a new NAC was established (changing "problems" in the title to "policies") by Executive Order No. 12689. Although there was considerable internal debate regarding a larger role for the Secretary of State in the reorganization, Treasury's interest in retaining its bureaucratic prerogatives in international financial matters and State's distinct lack of eagerness to engage in a jurisdictional controversy led to only a minor circumscription of the NAC charter. The most significant change in the NAC from an operational standpoint relieved the NAC Staff Committee from the responsibility—but not the right—to conduct a review of AID loans apart from that held in the Development Assistance Staff Committee. For a comparison of the original and present charters, see Committee on Foreign Relations, U.S. Senate, Committee on Foreign Affairs, U.S. House of Representatives, 90th Cong., 2d sess., *Legislation on Foreign Relations with Explanatory Notes* (Washington: Government Printing Office, 1968), pp. 634-636, 644-645.

³² President's [Kennedy] Memorandum of 27 May 1961 on "The Responsibilities of Chiefs of American Diplomatic Missions."

of many senior Foreign Service officers, tends to make for weak management control of the "country team."

Moreover, despite the fuzzy nature of much of what the mission deals with, it is hard to believe that many of the issues dealt with by ambassadors would not benefit from deeper knowledge, more data, and more systematic evaluation of objectives and alternatives than these issues often get. The reason is that they sometimes get very little thoughtful analysis at all. This seems to be true even in the management of some operating programs. With some important exceptions, operating programs tend to have a life of their own—to be run by the local agency representative without being integrated into an overall mission effort. And, of course, just as there is no overall foreign affairs budget in Washington, there is no "country budget" in the field.

APPROACHES TO ORGANIZATIONAL PROBLEMS

Some actions of great promise have been taken by the Nixon administration to improve the analysis of foreign policy issues: the establishment of a strong central staff reporting to the President, the institution of a foreign affairs program analysis group within the NSC, the installation of a procedure for eliciting divergent views from among the various agencies. One area of major concern remains. It is the role and organization of the Department of State. As we have seen, the State Department plays an extremely important role in staffing missions and guiding their operations abroad, in generating and interpreting information, and in executing policies. Unless there are major changes in State, there are grounds for doubting the depth of the reforms now underway, and also their persistence when some key people leave office.

State's general country strategy orientation gives its Secretary his best grip on foreign policy formulation. To be sure, other major foreign affairs agencies (Defense, Treasury, Commerce, and CIA) all have some equivalent of the country desk organization, but none matches the depth and scope of the country resources to which State has access.³³ In addition to its own organization, State can draw upon the country-oriented U.S. Information Agency and Agency for International Development, which, though semi-autonomous, are nominally under the control of the Secretary of State. Because it is the focal point for communications with U.S. missions overseas, State also has the best operational channels for dealing with country problems.

The Secretary of State's hold on the global aspects of foreign policy is tenuous, at best. Treasury, Commerce, Agriculture, and other specialized agencies often play a more important role in international economic policy determinations than the Department of State. The principal institutional sources of advice to the President on national security policy are the Secretary of Defense and the Joint Chiefs of Staff.

³³ It is generally accepted that State has a better grip on broad country questions than other agencies, and more emphasis on country problems generally means more power for State. A principal objective of organizational changes prescribed by NSAM 341 (March 1966) was to improve State's interdepartmental leadership and coordination of country matters. To accomplish this, the desk officer was elevated to the position of country director where he would serve as "the single focus of responsibility for leadership and coordination of departmental and interdepartmental activities concerning his country or countries of assignment." See "Department of State Foreign Affairs Manual Circular 385, March 4, 1966." Department of State Newsletter, March 1966, No. 59.

As noted earlier, the Secretary of Defense submits the annual force structure program to the Department of State for review and comment before it is sent to the President. However, the substantive input of State to defense policy has generally been rather small.

We do not wish to suggest that the responsibility for U.S. foreign policy—either the country or global aspects—should reside within the Department of State. Indeed, as we have suggested previously, this responsibility can reside only with the President. But many of the coordinating functions will fall to State, and the Secretary or some member of the Department will often be cast in a position, in effect, of exercising Presidential authority. Inevitably many important issues are going to be affected or decided within State, at the Secretary and Under Secretary level, at the regional Assistant Secretary of State level, or by desk officers, or in the field. This is so not because of deliberate preemption of Secretarial or Presidential authority, but by the ways events are interpreted and analyzed, hypotheses formed, data sought, and questions asked.

Therefore, it is of the first importance that the Secretary of State and his principal aides have available to them the effective analytical apparatus that they now lack. This point can hardly be overemphasized. We do not have a blueprint for such an apparatus, but some of its main futures would seem to include the following:

- Analytic staffs created to serve the five regional assistant secretaries. These staffs should include but not be limited to Foreign Service officers.
- Stronger connections and interactions with the academic and research community to stimulate more relevant research in that community, to increase the flow of data and ideas to Government, and to help improve the training of people in the field of foreign affairs.
- A program analysis and planning staff to assist the Secretary of State in his review of the foreign affairs budget discussed above. This staff might incorporate the existing Policy Planning Council. It should focus on global issues that cannot be adequately dealt with at the country or regional level, and on any other matters on which the Secretary wants an independent analysis.
- Increased opportunities for research and specialized education by Foreign Service officers.

IV. CONCLUSION

Foreign affairs is indeed “complicated and disorderly,” as Schelling suggests. Analysis can make it no less complicated, and analysis that attempts to do so is probably more a disservice than a service. But we are convinced that analysis can make the U.S. conduct of foreign affairs more orderly.

Order is, of course, only a proximate objective, and it is of little value unless it enables the policymaker to cope better with complexity. Can policy analysis in international affairs perform this function? We believe it can, if it is not only orderly but comprehensive. Too often the decisionmaker has been shown only a small part of the problem. Or he has not been made aware of the full range of relevant options. Analysis that “assumes away” part of the problem, without saying so, is not better than intuition that overlooks it. Analysts should strive to deal with a problem comprehensively and systematically, but they

should be equally comprehensive and systematic in pointing out the limitations of their work.

The foreign affairs community will not be able to develop a sophisticated analytic capability quickly. The application of PPBS to the Department of Defense in the 1960's benefited by analytic know-how acquired in the 1950's. Analysis in foreign affairs does not have to start from scratch, but it will suffer from past years of relative inattention. Some—perhaps many—early products will be unsophisticated. More than a few will be bad.

There are some who doubt that foreign policy decisionmakers would use good analysis if it were available. Say's law—that supply creates its own demand—may not always apply in the case of analysis. Certainly organizational innovation within the foreign affairs community can only make analysis available to the policymaker; it cannot make him use it. However, we are reasonably confident that if much is available, some will be used, and that those who use it wisely will find it of value.

One lesson gained from the application of PPBS to the entire Federal budget is that there is great potential for misunderstanding at all levels of the Government. In particular, analysis was often taken to be synonymous with quantification. It is true that analysis thrives on and often involves quantification, but analysis that either excludes or attempts to quantify the unquantifiable is wrong analysis.

Finally, we should like to say a word about the value of policy analysis in international affairs outside the small foreign affairs community within the executive branch. Most analytic papers inevitably move in a closed circuit among analysts, operators, and decisionmakers. The process is a continuing one with many revisions, formulations, and reformulations, aimed mainly at better articulation of U.S. foreign policy within the executive branch. But if the executive branch can better articulate foreign policy internally, it can also better articulate foreign policy to the Congress and the Nation at large. A clearer, more widespread understanding of what U.S. policy seeks to accomplish, and why, can only serve to raise the level of debate as to whether, in the broadest sense, the benefits justify the costs.

SECTION B

THE "NEW TECHNOLOGY" BUDGETS

PROSPECTS FOR PPB AT AEC

BY MILTON F. SEARL*

Milton F. Searl is Technical Assistant at the Office of Science and Technology in the Executive Office of the President. He was formerly Chief Economist at the Atomic Energy Commission.

Many of the programs administered by the Atomic Energy Commission involve investment expenditures in which the outputs and some of the costs are deferred until far in the future. Such investment undertakings are clearly subject to the application of economic analysis and public investment criteria. Mr. Searl discusses the application of PPB-type analysis to such investment undertakings and more generally, the application of economic analysis to the full range of Atomic Energy Commission activities. After reviewing the origins of the U.S. Government's activities in the atomic energy area and the organizational structure by which planning and analysis are applied to these activities, he concludes that "The advent of PPB * * * does not appear to have done much to increase the use of economic analysis in the formulation of program plans." He cites the grounds on which AEC has expressed objection to the application of economic analysis to its programs.

In the second section of his paper, Mr. Searl discusses the role of planning and analysis in a number of nonmilitary AEC programs. These include the central station nuclear power program, the breeder reactor program, the nuclear desalting program, and programs dealing with gaseous diffusion plants. In all of these programs, Mr. Searl asserts that economic analysis and PPBS techniques have wide application and, if appropriately implemented, could play a substantial role in improving policy decisions.

Mr. Searl concludes with a discussion of the next steps for implementing PPB at the Atomic Energy Commission. He notes that while the AEC has largely met the formal requirements of the PPB system, it has not made economic analysis an integral part of its decision-making process. He notes several things which could be done to improve the quality of analysis in this agency. One is to require the publication of all AEC benefit-cost and other analytic studies. "Exposing these studies to public view would provide the public and the Congress with an opportunity to judge for themselves the merits of the programs." He also proposes that the analysis of program benefits should be separated from the analysis of program costs and should be undertaken by different groups. He notes a number of areas in which the Atomic Energy Commission might pioneer in the application of benefit-cost and cost-effectiveness studies. He asserts that "continued Bureau of the Budget interest in better PPB analysis is a prerequisite for further progress. Congressional interest in the continuance and improvement of PPBS is, of course, vital to success of the system."

Introduction

In the following pages an attempt is made to examine the current status of PPB and related analytic techniques at the Atomic Energy Commission (AEC), the planning and analysis underlying some current atomic energy programs, and the steps that might be taken to

* The author bears sole responsibility for the views expressed herein. They do not necessarily represent the views of AEC or any other organization or individual.

further the application of the PPB system to the Nation's atomic energy program.

The material should be considered in a specific framework—namely, that of the PPB system and more specifically the economic analysis requirements of that system. There are clearly valid considerations that cannot always be factored into economic analysis which should also enter into public expenditure decisions.

The discussion is centered on the economic aspects although the PPB system encompasses much more than economic analysis. An economic focus appears most appropriate for this compendium and, furthermore, the budget and planning aspects of PPB at AEC generally appear to be either in a reasonably satisfactory state or evolving in a satisfactory manner.

AEC budgeting has been along program lines for many years and adaption to PPB system requirements was not difficult. Planning along program lines had also made some progress at AEC prior to the inauguration of PPB systems. The section 202 hearings (state of the industry) which the 1954 Atomic Energy Act required, as well as other hearings held by the Joint Committee on Atomic Energy, encouraged AEC to lay out at least nominal plans for many of its programs. Perhaps the best known planning effort is the Commission's 1962 report to the President on civilian nuclear power. In recent years extensive planning has been carried on by AEC, particularly in connection with the programs of the Production Division and the Division of Reactor Development and Technology. These efforts will be described in more detail subsequently.

A convenient place to begin an analysis of the role of PPB system and economic analysis in the atomic energy program is with consideration of the Atomic Energy Act and the institutional arrangements controlling its application. The failures, successes, problems, and potential of the system in the atomic energy field can be better comprehended in such a framework.

The Atomic Energy Act of 1954, as amended, declares it to be the policy of the United States that—

“(a) the development, use, and control of atomic energy shall be directed so as to make the maximum contribution to the general welfare, subject at all times to the paramount objective of making the maximum contribution to the common defense and security; and

“(b) the development, use, and control of atomic energy shall be directed so as to promote world peace, improve the general welfare, increase the standard of living, and strengthen free competition in private enterprise.” (Sec. 1. Declaration.)

It is further stated to be the purpose of the act to effectuate the above policies by providing for—

“(a) a program of conducting, assisting, and fostering research and development in order to encourage maximum scientific and industrial progress;

. . .

“(d) a program to encourage widespread participation in the development and utilization of atomic energy for peaceful purposes to the maximum extent consistent with the common defense and security and with the health and safety of the public;

“(e) a program of international cooperation to promote the common defense and security and to make available to cooperating nations the benefits of peaceful applications of atomic energy as widely as expanding technology and considerations of the common defense and security will permit;” (Sec. 3. Purpose.)

There is little in the above-quoted portions, or elsewhere in the Act, which can be interpreted as calling for an economically efficient approach to the development of atomic energy. The emphasis on the non-military aspects of the atomic energy program is on promotion of atomic energy, scientific progress, and use of atomic energy as a vehicle for furthering world peace and development.

It is hard to argue that the emphasis in the Act should have been other than it was in 1954. The potential of the atom was clearly great, although perhaps poorly understood, and facts were not available as to costs of development or as to benefits that would have permitted meaningful analysis. The decision to embark on the major development effort required to harness the atom was not based on detailed economic analysis but rather on the fact that the potential benefits were so large that the beginning effort was clearly justified.

The responsibilities for scientific research placed on AEC by the act are a second factor complicating economic analysis of atomic energy programs. The economic framework for a valid analysis of basic research efforts is still in its infancy. However, even for basic research efforts, where benefits are very uncertain, cost-effectiveness techniques can still be applied in analysis of the relationship of project scheduling to funding levels.

Projects and programs which contain a modest basic research content, some applied research, and a substantial development component present particular problems of analysis.

A third factor complicating the application of economic analysis to atomic energy activities is the military origin of the atomic energy program. This is not so much a result of AEC's responsibilities under the Atomic Energy Act, as were the previously discussed promotional and scientific aspects, as it is an historical accident. Many of AEC's key personnel were associated with the early military programs. For years the overriding priority of the national atomic energy program was the production of weapons and weapon materials. The primary object was regarded as “getting the job done,” with economic niceties being of secondary importance. Some of this philosophy, supported by the statutory mandate to promote atomic power, still carries over into current activities, and it has been argued that without this managerial attitude some of the applications of atomic energy would not have achieved their present status. Nevertheless, the fact that the atomic program has become of age suggests that the PPB program does have application to its future funding, as AEC itself has come to recognize.

The institutional framework within which the Atomic Energy Act is carried out, and on which the PPB system has been superimposed, is perhaps even more fundamental to the role of PPB and economics in the atomic energy program than the act itself.* Consequently, it is appropriate to describe this framework.

*Further discussion of this issue is found in the paper by Marvin & Rouse in this volume.

AEC is managed by five Commissioners appointed by the President and confirmed by the Senate. The Commissioners are men of stature, generally selected from the scientific, academic, and legal communities. At times a member of the industrial community is also included.

The staff is headed by a General Manager, who is assisted by a Deputy General Manager, an Assistant General Manager, and Assistant General Managers for various areas of Commission activity. Operating divisions, mainly organized along program lines, report to each Assistant General Manager. Basic program responsibility resides in the operating divisions. In addition, the Controller and the General Counsel have special responsibilities to the Commission as well as heading their respective offices. (The Director of Regulation also reports separately to the Commission.) There are also nonoperating divisions with special responsibilities, such as the Divisions of Operations Analysis and Forecasting (which does little PPB work) and the Division of Plans and Reports (which assists with PPB system planning activities).

AEC programs are for the most part carried out through operations offices, national laboratories, and industrial contractors, all with specific program interests.

In the national laboratories, the operations offices, contractor organizations, and elsewhere, there are highly competent scientists and engineers with a national stature in and sometimes beyond their professions and, therefore, entitled to be heard on policy matters—all of which complicates application of the PPB system.

On the congressional side, AEC operations are reviewed by the Joint Committee on Atomic Energy (JCAE). The JCAE is a very effective committee which takes a close and constructive interest in the work of AEC.

Finally, there are community and industrial interests with a stake in AEC programs and which naturally seek to influence Commission activities.

In this complex of decision points the introduction of a new decision-assisting tool—PPB economic analysis—presents obvious difficulties. A fundamental difficulty, of course, is that the basic facts on costs and benefits are not readily available because most AEC programs deal with research and development activities in which future costs and benefits contain large margins for dispute. Few decisions have been based on economic analysis, as opposed to cost, budget, and engineering analysis, and some of the analysis has been in efforts to provide economic backup for previously established program plans.

The formulation of program plans and the conduct of detailed studies at AEC are basically the responsibility of the program divisions. However, AEC has established the position of Assistant to the General Manager for Program Analysis to guide and help the divisions in conducting analysis. The Assistant for Program Analysis has three analysts working with him.

Budget aspects of PPB are handled by budget officers in each division who work closely with the Controller's office on the budget for each program. The Division of Plans and Reports works with the program divisions in preparing the planning documents required by the PPB system.

Economic analysis of division programs may be carried out by the division itself, by contractors specifically employed for given project, or by contractors with a continuing relationship with the division. Most program divisions contain few practicing economists. One economist works for the Assistant to the General Manager for Program Analysis. Consequently, the bulk of the economic analysis is carried out by scientific and engineering personnel.

The Division of Production and the Division of Reactor Development and Technology, the two divisions with the largest civilian-oriented programs, carry on extensive analytic efforts. The Division of Reactor Development tends to use outside contractors with an extensive knowledge of the technical details of AEC reactor programs for analysis of its program.

The Divisions of Production and Military Application have established their own contractor-operated "think tanks," partially staffed by technical people from various production division operating sites on a temporary (1- or 2-year) assignment basis, and partially by permanent staff. They also use some outside contractors.

It would appear that the advent of PPB has strengthened the technical analytic capability of AEC, or at least organized and provided better direction for it. On the other hand, it does not appear to have done much to increase the use of economic analysis in the formulation of program plans.

At various times, AEC has expressed objection to or at least concern about the application of economic analysis, and particularly rate of return, benefit-cost, and cost-effectiveness measures, to its programs on the following grounds:

1. The preference of many economists for high-productivity projects; for example, those with a social rate of return of more than 5 percent;*
2. The long-term nature of some of AEC's programs, which means that benefits to society are long delayed, and which delay tends to reduce the productivity of the programs as measured by the rate of return;
3. The failure of analysts to give adequate weight to the intangible or external social and economic benefits of AEC programs;**
4. The failure of the analysis to consider the benefits of early versus deferred investment in an inflationary economy;
5. The omission of possible tax benefits to the Government in evaluation of the merit of AEC programs;
6. The lack of confidence by some people outside AEC that there will be benefits from new technological developments even if such benefits are currently unforeseeable; and
7. The lack of confidence at AEC in the validity of applying cost-effectiveness analysis and similar techniques to "basic research" programs.

*Further discussion of this issue is found in the paper by Baumol in vol. 1 of this collection.

**Further discussion of this issue is found in the paper by Margolis in vol. 1 of this collection, and Knetsch in this volume.

Presumably the merit, or lack thereof, of the above items falls within the province of other authors in this compendium, so no attempt is made to resolve the issues here.

It may be appropriate to make explicit where this paper considers economic analysis to start and stop. Herein, economics is not considered to cover subjects such as purchasing, contracting, employee relations or the manner in which facilities are operated. Economic analysis frequently assumes that all these things are being done efficiently so that, given the specific plans and programs, there is no more efficient way of doing things. In technical terms, we are on the production opportunity frontier—output cannot be increased without more resources. Of course, we know that in general all through the economy opportunities for increasing economic efficiency exist, but the items cited are frequently the dividing line between economics and other disciplines.

The economic issues rather center around the plans and programs and whether AEC's, the Government's, and/or the Nation's resources could be better allocated within and between programs to increase benefits to society. It is not, of course, the economist's function to specify society's goals, but rather, once these goals are specified, to use the science of economics to allocate scarce resources among competing ends in order to maximize the achievement of the ends.

Having perhaps to some extent explained AEC's attitude toward economic analysis and the complications of economic analysis of atomic energy programs, the next task is to review the economic aspects of some of AEC's programs involving substantial expenditure of public funds. As a prelude to this task, it is desirable to recognize a basic conflict between economics and engineering. This stems from the difference between engineering (or technical) efficiency and economic efficiency. In general, a plant, device, or technology which is optimum from the standpoint of technical efficiency is nonoptimum from an economic standpoint (e.g., the powerplant with the highest efficiency—best heat rate—is not the lowest cost power producer). This difference between what is technically "best" and what is economically optimum tends to carry over into the design and conduct of research and development programs. Scientists and engineers tend to design high quality research and engineering programs and ones with lots of backup against possible R. & D. uncertainties. Such a program tends to push past the point of diminishing returns from a strict economic standpoint and thus to be criticized by economists.

PLANNING AND ANALYSIS IN SPECIFIC AEC PROGRAMS

No attempt is made to cover all AEC programs in the following discussion. Selection is based on the author's knowledge of specific program efforts, availability of public information on the various programs, and attempts to keep the paper within reasonable limits. In general, no discussion of military or militarily oriented programs is included since these rapidly run into classification problems.

CENTRAL STATION NUCLEAR POWER PROGRAM

This is one of AEC's major programs and is administered primarily by the Division of Reactor Development and Technology. Total re-

actor development costs for this program in fiscal 1968, including construction and allocation of appropriate supporting activities, appear to have been about \$250 million. Program plans call for a substantial increase in the level of funding in the next 5 or 6 years. Aggregate future government expenditures for power reactor development, including general support, safety, R. & D. and fuel, could conceivably reach \$7 billion to \$10 billion, if various technically interesting and potentially economic concepts which are still under consideration were more fully explored.

From a practical standpoint, the central station power reactor development program appears to be at its first goal—the demonstration of economic nuclear power. Now that nuclear power has come of age, and in view of the large expenditures contemplated to develop the breeder reactor and other advanced types, it is timely to assess whether economic analysis cannot play a large and more constructive role in decisionmaking. The Joint Committee on Atomic Energy, in its report of May 1966 on the fiscal year 1967 authorization legislation, suggested an updating of AEC's reactor development plans. A number of volumes presenting parts of such an updating have been published by AEC, including "The 1967 Supplement to the 1962 Report to the President." As yet no comprehensive overall reactor development plan to guide future expenditures has been made available.

The comprehensive plan should afford a role for economic analysis in further refining or redefining AEC's reactor development objectives and laying out AEC's strategy for achieving those objectives.

The problem is not that the President or Congress will approve AEC's spending \$7 to \$10 billion on the reactor program without careful review. Rather, the problem is that looking at the program on a year by year, or even several-years-at-a-time basis, it is difficult to exercise meaningful control. It is often hard to abandon unpromising concepts or make new starts without an overall plan that would provide the framework for doing so.

A typical situation is the availability of two or more competing reactor concepts, any of which could be expected to serve the purpose of generating low cost electricity equally well and on about the same time scale. In the absence of overall economic criteria for the reactor program, there is little basis for AEC to choose among them so there is pressure to continue both. Yet, only in some cases will benefits be large enough and the uncertainties great enough to warrant pursuing more than one.

BREEDER REACTOR PROGRAM

The main emphasis in AEC's central station nuclear power program is currently on the breeder reactor program. A plan for AEC's mainline breeder effort, the liquid metal fast breeder reactor (LMFBR), is contained in the "Liquid Metal Fast Breeder Reactor Program Plan, Volume 1—Overall Plan (WASH 1101)." The volume does not give cost estimates for implementing the plan, but it appears that total breeder reactor development costs, including the LMFBR, could cost from one-half to two-thirds of total future estimated central station nuclear power reactor development costs. There is a need for the LMFBR plan to be integrated into an overall breeder reactor plan and this, in turn, made a consistent part of the previously

suggested overall plan for all central station nuclear power development.

AEC is developing an economic analysis of its breeder reactor development plans. Such a plan needs to be concerned with the pace and scope of breeder development and the establishment of a competitive manufacturing industry.

The successful development of breeder reactors has always been the main vision of the nuclear power program. Breeder reactors will make available the full potential of uranium and uranium is sufficiently abundant in the earth's crust that with breeders, our ability to supply mankind's energy needs for thousands of years is assured. Breeder reactors can banish the fear which has haunted scientists for much of this century; namely, that our increasingly energy dependent civilization would eventually grind to a halt due to the exhaustion of energy resources.

It appears that this objective is now virtually at hand. Present breeder technology, or at least that to be developed by present programs within the next few years, seems to assure that the world will have available essentially unlimited amounts of energy. This fact seems to have gone largely unnoticed in the excitement over the Nation's dramatic commitment to light water reactors. (This paragraph assumes that there will be no problem with radioactive waste disposal.)

The basic goal of the breeder development program now appears to be to reduce the cost of power from breeder reactors to the point where breeders are commercially viable and, perhaps, to reduce the cost of power well below the 4 to 5 mills per kilowatt-hour commonly used as the cost (to investor-owned utilities) of generating power from the best nuclear and fossil plants today.

The task of reducing the cost of power from breeder reactors to "more reasonable" levels is largely an engineering one—albeit a difficult and expensive one, requiring perhaps \$3 billion or more in additional Government expenditures and substantial expenditures by the private sector.

The fact that the breeder reactor program that is still ahead of us is essentially an engineering development program aimed at reducing the cost of production of an established commodity, electricity, which can be produced by other means seems to make it a natural for economic analysis within the PPB system framework.

PACE OF THE BREEDER PROGRAM

AEC's LMFBR program plan, referenced above (but, not yet, approved by the President), calls for initial criticality of the first commercial plant in 1986 and initial criticality of two more plants before 1990. Considering the time span involved, an acceleration of the program is probably possible and, conversely, the program could be operated at a level designed for a later introduction date. Additional benefits from lower than otherwise electricity generating costs should result from earlier introduction of breeders.

The behavior of R. & D. costs and benefits with a change in program pace are unclear, and the rates of return for both earlier and later introduction dates needs to be calculated. AEC has drafted a liquid metal fast breeder study and may have finalized, and perhaps

published it, by the time this paper is published. The LMFBR plan and study represent a major PPB effort.

SCOPE OF THE BREEDER PROGRAM

The main questions regarding the scope of the breeder program involve the number of breeder designs on which AEC should be working and how far AEC should seek to develop breeders before withdrawing from development work and allowing industry to finance further development.

AEC apparently has a very high degree of confidence that costs of the LMFBR can be substantially reduced to the point where it can generate power on a utility system for about 4 mills per kilowatt-hour by 1990 (constant dollars). The LMFBR apparently has potential for further cost reductions below 4 mills, with operating experience and continued development beyond that date.

The public expenditure decisions that the Nation faces in connection with the scope of the program are concerned with the number of alternative breeder concepts which AEC should pursue, given expectations for the LMFBR. Such decisions can be aided by economic analysis which can be readily undertaken, given whatever distribution of likelihoods AEC assigns to the achievement of various generating costs at specific dates with the LMFBR.

Breeder reactors using the thorium-uranium-233 fuel cycle are conceptually feasible as well as breeders using the uranium-plutonium cycle, such as the LMFBR system, which is currently AEC's mainline breeder effort. Arguments for the thorium-uranium-233 breeder system center around its possibly favorable economic features, uranium conservation, and thorium availability.

AEC has chosen to make the uranium-plutonium system its mainline effort and has considerable confidence in the success of the system. There is sufficient uranium to fuel breeders for many centuries. From an economic standpoint, therefore, it is questionable if the added costs of developing the thorium system can be justified, even if in the long run it has lower costs in commercial operation. At any rate, such studies should be a part of the decisionmaking process.

Although, thorium is estimated to be somewhat more abundant than uranium, this is irrelevant from the economic standpoint unless its use results in net economic benefits to society when R. & D. costs are included in the evaluation.

Dr. Paul MacAvoy, of the Massachusetts Institute of Technology, has carried out some pioneer research work in possible breeder reactor development strategies under a grant from Resources for the Future, Inc. This work involves program scope, among other things. It is to be published in the near future and the methodology could form a model for further analysis of program scope by AEC.

NUCLEAR DESALTING

The possibility of making large supplies of clean, fresh water available to water short or arid areas of the world is one that captures the public imagination, perhaps even more than the prospects of large supplies of low-cost energy. However, the two are not necessarily

separate. The achievement of desalting goals by the route being pursued by AEC requires large amounts of low-cost energy.

The desalting program is a joint program between the Atomic Energy Commission, the Department of Interior, and various utility systems. An international cooperative program is also carried out in the desalting field.

Atomic Energy Commission costs for desalting applications were approximately \$3 million in 1968. Government costs for proposed demonstration plants would involve much larger expenditures.

The first question is whether the energy for desalting should be supplied by fossil fuel or nuclear power. It is doubtful if any general answer can be given to this question, particularly in the United States, where nuclear power and fossil fuel are frequently quite competitive. Each project needs to be analyzed on its own merits.

The question of nuclear versus fossil fuel becomes even more controversial abroad. Many areas in the Middle East and north Africa which need water have prolific oil production and are flaring or re-injecting natural gas. The marginal cost to the country of using this oil or gas appears to be quite low. Such comparisons on a national basis must, of course, be based not on the market price of the fuel but rather on the net cost to the Government after considering the tax and other revenue which it gets from each unit of production. This may mean 5 to 10 cents per million British thermal unit oil and gas costs and minimum foreign exchange requirements.

Determination of the relative amounts of water and power to be produced in a dual-purpose plant, and the allocation of costs between water and power, is an area in which it seems there is a need for application of economics to desalting. Given an unambiguous economic objective for the plant, there are perfectly straightforward mathematical economics techniques for optimizing this objective.

In fact, it is possible to apply either product transformation and isorevenue curve or marginal benefit and marginal cost theory. Although this may sound a little complicated, it is likely to be easier and is more accurate than the empirical methods frequently used. Although there seems to be some reluctance to deal with demand, isorevenue, and marginal revenue curves, no strictly economic optimization is possible without them or equivalent concepts.

It needs to be emphasized that allocation of true joint costs is not needed to determine the project economic optimum, including the amounts of each product to be produced and prices. Furthermore, attempted allocation is likely to be confusing and, if improperly done, to force the project off of the economic optimum. If cost allocation of true joint costs is required for accounting purposes, it can be best done by working back from price and quantity data. The economic conditions which determine project optimum also effectively determine the appropriate allocation of costs (i.e., the unique allocation which is consistent with the optimization, the acceptance of any other allocation being inconsistent with project optimization.)

GASEOUS DIFFUSION PLANT CAPITAL EXPENDITURES

A major public expenditure question which has been raised is whether the large capital expenditures required to enrich uranium for

the electric utility market should be made by the Government, private industry, or some combination thereof.

Uranium is currently enriched (in the isotope uranium 235) in three gaseous diffusion plants operated for the Atomic Energy Commission by industrial contractors. AEC is planning a cascade (gaseous diffusion) plant improvement program which can be expected to cost about \$500 million through fiscal year 1977 and is considering a follow-on power uprating program costing perhaps \$150 million. Furthermore, AEC has estimated that construction commitments for new enrichment facilities, probably running into billions of dollars, will need to be made starting in the mid-1970's. (The level of construction costs, the status of enriching technology, and the size of the market in the late 1970's and early 1980's make more precise estimates difficult.)

It goes without saying that PPBS techniques have wide application to evaluating and scheduling gaseous diffusion plant capital expenditures, and such techniques are being used by AEC for programming plant improvement.

GASEOUS DIFFUSION PLANT OPERATION

For many years the Government's gaseous diffusion plants were operated at high levels to enrich uranium for Government programs. In recent years, Government military requirements have been much less than plant capacity and operations have been sharply cut back. However, AEC projections indicate that the full capacity of the plants will be needed by the mid-1970's, primarily to supply electric utility markets for enriched uranium. Furthermore, projected requirements substantially exceed diffusion plant capacity in the late 1970's. Consequently, AEC has embarked on a campaign to pre-produce enriched uranium and thereby take advantage of the favorable characteristics of the diffusion plant marginal cost curves and to defer the time at which new plants (or major expansions) are required.

Although a preproduction program is rather clearly desirable, there are economic questions as to the proper extent of preproduction.

Very extensive analysis of the question of the proper preproduction level has been undertaken by AEC. The basic underlying concept of this analysis is that of equal discounted incremental cost. This principle indicates that units should be preproduced each year up to the point where the out-of-pocket cost of such production, plus interest and other holding costs on preproduction, will just equal the minimum future production cost by a feasible alternative. This, of course, requires establishing future sales prices, which in this case are derived from estimates of the cost of additional capacity to be built about 1980.

Although the basic procedure for determining preproduction levels seems sound, questions have been raised about many of the parameters going into the actual calculations and, consequently, the results. A primary question is the appropriate rate for discounting. AEC prefers a 5-percent discount rate with risk analysis. This results in higher production levels than would result from use of a higher discount rate.*

*Further discussion of this issue is found in the paper by Baumol in vol. 1 of this collection.

Since this program clearly involves market and other uncertainties, attempts have been made to compute the optimum strategy for pre-production under such uncertainty, given the appropriate basic discount rate, but there is not yet general agreement on such calculations.

FURTHER IMPLEMENTATION OF PPBS AT AEC

AEC has largely met the formal requirements of the PPB System. It submits plans and budgets as required and provides special analytic studies. Organizationally, it has established the position of assistant to the general manager for program analysis (assistant and three analysts—one an economist) to help the divisions in conducting program analysis.

However, AEC has not, by and large, made the economic tools and techniques of PPBS an integral part of its decision-making process. This is not surprising, considering AEC's scientific, engineering and military orientation.

Any substantial further progress in the use of economic analysis by AEC is unlikely to come except as the result of outside stimulus, such as the trend in government toward increased use of economic techniques.*

It is tempting to suggest that AEC be induced to establish a division of economics or some high level economic group to foster and improve economic analysis at AEC. However, until the AEC becomes convinced that economic analysis can be helpful to it in its decisionmaking and funding problems, no form of staff organization is likely to make its mark. In fact, it is not clear that at present economists could even be readily found for such a group. AEC has difficulty recruiting economists and even the young economists who join AEC's intern program soon drift into personnel, contract or similar work, or move on to other agencies.

There are several things which could be done rather easily and would probably improve the quality of the economic analysis of atomic energy programs. One is to require that AEC promptly publish cost-benefit analyses and special analytic studies which it makes. There are, of course, cases where prompt publication of studies is precluded for policy reasons. Even there I would urge in most cases that the studies be published as soon as possible after the policy decision.

Exposing these studies to public view would provide the public and Congress with an opportunity to judge for themselves the merits of the programs. And there is increasing interest in and competence to evaluate the costs and benefits of atomic energy programs at universities and non-profit institutions. There is no reason why the government should not have the benefit of the reviews of atomic energy studies which these groups would make. Such outside review and criticism would almost certainly gradually increase the quality of economic analysis carried out in the Commission.

Perhaps the least painful and most productive method of obtaining better analysis of the benefits of government programs would be to separate responsibility for the estimation of the costs from responsi-

*Further discussion of this issue is found in the papers by Marvin & Rouse in this volume, and Carlson in vol. 2 of this collection.

bility for the measurement of the benefits. This would more closely approach our traditional market system, where the producer offers his goods and customers place a value on them. In many cases, there is no special capability in an individual agency to measure the benefits resulting from its programs. Manpower must be diverted from the agency's main function to scout other agencies for needed "market" data or to develop it. Because of lack of adequate experience in evaluating benefits, it is also quite possible that errors will be made and benefits may be underestimated as well as overestimated. However, the natural tendency of an analyst to view with some favor his agency's programs may result in a consistent bias toward overestimation.

The idea of separating benefit-cost and related analytic work from the operating agencies is not, of course, original. It has been proposed by others and further research on it is being done. What I have suggested here for AEC's work is a less drastic step and one that could be implemented more quickly. It could be a transition step on the way to the more complete separation proposed by others. It does not appear that separation of the estimation of the technical characteristics of the technology and of the research and development costs of achieving those characteristics is presently feasible in AEC's case. I doubt if there is enough independent technical know-how to make these estimates. Lack of widespread technical know-how outside of AEC and the nuclear industry should not, of course, preclude legitimate questioning of AEC's estimates where they appear to differ from other available data or to be inconsistent with other AEC data or studies. On the other hand, on the benefit side, there is probably greater capability outside of AEC (and the AEC contractor complex) as far as methodological know-how, experience, and background information availability are concerned and less chance of bias creeping into the results. AEC might, of course, still desire to make its own estimate of benefits for comparative purposes.

On the methodological side, various improvements are possible, although some studies are better in this respect than others.

In view of differences of opinion over the appropriate discount rate to be used in analysis, it might be desirable to abandon the direct application of specified discount rates in AEC analysis. Instead, from the raw (undiscounted) data, the discount rate which will make the benefit-cost ratio equal to one (or other desired value) should be calculated; i.e., the internal rate of return should be found. This technique is being increasingly used outside of government, chiefly because the results are easier for management to understand—i.e., for most people it is more meaningful to measure the productivity of an investment by stating that it is earning 8 percent than that it has a 2:1 benefit-cost ratio at 5 percent.

Analyses for programs where costs are shared by industry, or where industry is carrying on a program of its own, should incorporate the best possible estimates of industry expenditures in the analysis. It is possible that in some programs both industry and government judge the programs to have a favorable rate of return because both assume *full* national benefits in their calculations but only *their* part of the costs. This could result in carrying out programs which are not economically justified from a national economic interest standpoint. In other cases, failure to take account of industry efforts could result in overestimating the cost of the government development program and thus understating the benefit-cost ratio.

Studies involving estimates of future consumption of goods or services should pay more attention to the concept of demand instead of fixing on "requirements," which are assumed to be independent of price. While demand for many commodities may be inelastic, there appear to be relatively few for which demand is independent of price.

Analysis of alternatives which arrive at rates of return (or other productivity measures, including benefit-cost ratios) should be carried to the point of choosing between the alternatives as far as the economic criteria are concerned. At least some AEC studies terminate the analysis by listing the rates of return (or benefit-cost ratios). In general, under a fixed budget constraint—Government, agency, or program—it is not possible to pick the economic optimum without further analysis. For example, a 14-percent rate of return on an investment is better from an overall productivity standpoint than a 15-percent rate of return on one-half as much investment and 10 percent on the other half. There are accepted, mathematically valid methods of carrying the analysis of investment productivity to its optimum value and these should be used. It is relatively easy to do when the basic computations are being made, but somewhat more difficult for another analyst without the basic data to do later.

There is a need, at least on a total basis, if not on a project basis, of finding some method of evaluating the benefits of basic research and also determining the value of "spinoff benefits" from such research.* Chairman Seaborg of AEC has recognized this need. In a recent talk, he noted:

"One of the big problems in establishing a growth rate for basic research, like it or not, is determining the economic value of a discovery. This is tending to become a basic need in the physical and biological sciences, perhaps also in the social sciences, and to a lesser extent in the arts and humanities * * *"

"* * * the problem of quantifying the value of "spinoff" from basic research is not trivial. I look forward to the time when some group of economists, perhaps supported by a grant from the National Science Foundation, makes a breakthrough in establishing realistic criteria in this field."

(The Government-University Partnership in Graduate Education—AEC press release S-50-68, Dec. 4, 1968.)

It might be appropriate for AEC to take the lead or at least carry on economic research aimed at quantifying the value of discoveries and of collateral benefits of development work. Such an effort might receive better cooperation from the scientific community, if so sponsored, and there would be a greater likelihood of scientific values being properly reflected in the study than if undertaken independently by economists.

AEC might also pioneer work in the application of cost-effectiveness techniques to the scheduling of research, and particularly applied research, in view of its strong scientific interests. Even where the benefits are unknown, as in the high-energy physics program, but can be assumed to be a fixed amount or vary in some known way, the costs of realizing those benefits can be incurred in various ways and according to varying time patterns, some of which will be more cost effective than

*Further discussion of this issue is found in the paper by Sewell in this volume.

others. And the lowest undiscounted cost is not necessarily optimum, so simply minimizing project costs does not suffice.

Along a similar line, it would appear worth some AEC effort to attempt quantification of various external benefits which are frequently emphasized qualitatively. In view of the phenomenal ability of science to measure the elusive in physics, biology, et cetera, it is hardly satisfactory to maintain that many of the much more tangible externalities in economics and the social sciences are immeasurable.

To the extent that it has not already been done, reviews of the reliability of AEC initial estimates of project costs and benefits and of changes in estimates over time should be made. The results of such review should help correct the planning process and indicate any systematic bias and/or methodological deficiencies in the analysis. Dr. MacAvoy has made some estimates of the accuracy of early cost estimates in connection with his previously mentioned study of the economic strategy for developing nuclear breeder reactors. Dr. Robert Haveman, under a Resources for the Future, Inc., grant to Grinnell College, is conducting a study concerned with methodology of assessing the reliability of projected benefits and costs used in initial project justification in the water resources field and of the use of the results of such an assessment to improve current estimating procedures. This work should, when published, be reviewed as a possible source of methodology for carrying out similar assessments in the nuclear field. Such an assessment should probably be carried out by an independent group rather than the agency.

Regardless of what new measures are taken, continued BOB interest in better PPB analysis is a prerequisite for further progress. Congressional interest in the continuance and improvement of PPB is, of course, vital to success of the System.

POLICY ANALYSIS IN THE NATIONAL SPACE PROGRAM

BY BRUNO W. AUGENSTEIN*

Bruno W. Augenstein is Vice President for Research at the RAND Corporation.

The crash effort of the United States, beginning in 1958, to accelerate achievements in space gained widespread support for massive space program expenditures in these programs undertaken by the National Aeronautics and Space Administration and the Department of Defense, to gain American technological supremacy in the space area. However, asserts Mr. Augenstein, "now is a natural time to review and reevaluate * * * the goals and purposes of the national space program as achievement of the current decade's primary mission, the manned lunar landing, is within reach." The growing demands on our resources by numerous social programs provide added incentive to conduct more rational analysis of the objectives and alternatives involved in our space program.

Evaluation of space programs, however, is confronted by numerous difficulties. Thus far, "questions of priorities for space activities have not been directly addressed * * * there is less than complete agreement on what our national objectives in space ought to be (in the sense of objectives that can be compared with other defined national objectives) * * * and in particular, neither the goals nor the accomplishments of the DOD space program are widely acknowledged." Analysis is also hindered by the difficulties in quantitatively measuring many space benefits, as well as by the long leadtime between initial expenditures and final results. This latter circumstance necessitates a long-term funding commitment while at the same time increasing the difficulty of evaluating the expected benefits.

Mr. Augenstein discusses the progress of our space program in the past decade. He feels that whereas there has not yet been excessive duplication between NASA and DOD programs, steps should be taken to ensure that such duplication will not occur. He also suggests that whereas, until now, new technological discoveries and civil applications from space activities have been limited by a concentration on a few chosen mission objectives, future space efforts should attempt to broaden the scientific and economic returns.

Mr. Augenstein then presents some recommendations for space policy and concludes with a discussion of the types of analysis which must be undertaken if rational decisions on major policy questions are to be reached. "Some of these analyses would be more qualitative ones, providing background and context for consideration of the national space program; other analyses would be quite quantitative, and in the spirit of the PPB system within which NASA and DOD are now asked to plan and propose their programs."

I. WHAT THIS PAPER IS ABOUT

In the 10 years since it was patched together from a varied assortment of projects and first rate but little known institutions, the U.S. space program has changed in character. It has always been exciting.

*The author is indebted to Mr. R. Perry, The RAND Corporation, for major assistance in critical review and comment during preparation of the paper.

NOTE: This paper, which was submitted to the committee in February 1969, does not necessarily reflect the views of the RAND Corporation.

It began as a frantic response to put something—anything—into space. There is today still as much excitement, or more. But now it can be the excitement of solid achievements, not only of imaginative but uncertain efforts. The difference can be sensed in many ways. There is no longer any compulsion to doubt Soviet accomplishments, or merely to simulate pride in American achievements. Apollo, Surveyor, Orbiter, and Mariner testify openly, objectively, and eloquently to the status of the United States in any prestige competition with the Soviet Union.

Rather, the space program has matured in many ways in the current decade. We can now begin to consider soberly what investment of national resources in space activities is in the public interest; what tangible and intangible benefits the space program confers, and on whom, and in what quantity; and to what ends those resources should best be bent in the space program. Those are reasonable questions, although it is probably not yet reasonable to require full answers. It is also reasonable now to consider systematically which should be selected from among the many possible future courses for the national space program. Not broad objectives only, but technologies and specific program goals can be compared against one another. The alternatives are many and real. The attention of the Nation can be focused on new problems and ambitions, not only those of a decade ago. National priorities have changed, warranting a corresponding reevaluation of priorities for the national space program. A future course in space that is a straightforward extension of the past is not necessarily or certainly desirable. What is certain is that the choice of a space policy for the future should be made—at least in part—with consideration of the probable costs and benefits, and that the decisions leading to such a choice are sufficiently crucial to warrant deliberate attention.

This paper then suggests the importance of (1) recognizing, (2) discussing and analyzing, and (3) resolving each of several interrelated issues. It obviously is pointless and futile to continue separate arguments over space program goals, funding levels, societal benefits, and such matters; these are interactive elements of a larger national policy issue that has yet to be accepted as such.

Uncertainty about public and congressional acceptance of a space program stabilized at some artificially established funding level has characterized discussion of the future of space activity in the recent past. But it is a hard fact that the national interest will probably not permit the resource base for space enterprises to shrink past a certain minimum size, because, among other reasons, of the possibility that a rapid mobilization of resources may be needed to counter some presently unforeseen circumstance or threat. The American public, and the American Congress, have come to understand and accept the need for maintaining an adaptive defense establishment—one that can cope with a variety of conceivable threats and at the same time provide a base for relatively rapid expansion. There should be no particular difficulty in securing public and congressional acceptance of the same reality for space programs, once the alternatives have been publicly examined.

Examination of these alternatives may indeed require holding now the debate on space activities that was put off in 1961. But is there assurance that the public, or the Congress, is or will become as concerned with really careful scrutiny of space programs, on their merits,

as with more pressing or immediate issues like civil disorder, phasing down a very costly war effort, or air pollution (to take but three of a dozen similar issues)? A debate might more easily have been held in 1961, when there was considerable dissatisfaction with the status and prospect of the existent space program. In 1969 there is much less dissatisfaction of that sort; and there may be, consequently, a slighter chance that a candid significant public debate on future space policy can or will be held, and a greater chance that preemptory or ill-considered decisions could be made. On intellectual as well as capability considerations that would not be a very satisfying circumstance. But there are strong reasons for undertaking assessment and review of our purposes and objectives for being in space.

Development of additional guidance for public expenditure decisions in the national space program is appropriate; and it presents significant challenges, as well as opportunities, for analysis. The challenges stem from the fact that space policy intersects so many other broad, and unresolved, policy issues—such as basic research and development strategy, and the setting of priorities in national efforts as one determinant of the funding level appropriate for such efforts. The opportunities arise because the cost streams for space expenditures are large, and will remain large—and because now is a natural time to review and reevaluate, and perhaps restate, the goals and purposes of the national space program as achievement of the current decade's primary mission objective, the manned lunar landing, is within reach.

Thus, while the public policy decisions in the national space program are not the most pressing problems facing us—certainly there are problems both more important and with greater fiscal impact on us—the national space program poses issues with a special kind of urgency, because of its essentially unique mix, for a single program, of major engineering, development, scientific, and economic consideration.

The general plan for the remainder of the paper is to discuss the general policy problems and framework for policy considerations in sections II and III. In section IV the histories of NASA and DOD are reviewed, primarily because these histories reflect past policy considerations and have some lessons for the future. Section V reviews the last decade's space program in the context of some of the guidelines of the 1958 Space Act. Section VI recapitulates the main threads of the discussion, and suggests, in broadest outline, some features of a space program for the next decade which would build on the general conclusions of the previous discussion. Finally, section VII describes some explicit studies which can help clarify many of the relevant decisions and issues involved in space policy formulation.

II. INTRODUCTION AND THEME

The main thesis of this paper is that it is timely now to assess the course of the last decade in space, and to reverify or restate the nature of the goals and the policy issues which can shape the space endeavors in the next decade.

The national space program—which is here considered to be the programs undertaken by NASA and by the DOD—has thus far been marked by monumental, predominantly successful endeavors and by the accomplishment of highly important objectives. In the process, a

vast national resource of skilled personnel, administrators, and facilities has been created by NASA, and is ready for new missions. The DOD space programs, while less in the public eye, have likewise been comparably successful in carrying through missions of great importance to national security.

Nevertheless, the national space program, and most particularly that part of the national space program represented by the programs of NASA, is now being subjected to an evaluation and reassessment that will strongly influence its composition in the near future.

Both the budget and the basic mission of NASA have become the focus of heightened congressional scrutiny, as the costs of the Vietnam war have mounted, and as demands for more funds to deal with internal social problems of the United States have intensified. NASA funding levels have already dropped well below the levels sought by NASA. Also, with the successful manned circumlunar navigation, and with the imminent fulfillment of NASA's first major goal of landing a man on the moon and returning him to earth, one era is coming to an end and another is beginning.

Other factors have also played a major role as causes of the NASA funding level shrinkage. We tend to forget that the creation of the NASA and the national space program were greatly motivated by singular events—the launch of Sputnik I and the following shocks of an apparent U.S.S.R. scientific and technological threat. Other consequences of those unique events are beginning to diminish, and there is no reason to believe that space budgets can continue to shelter behind such relatively ancient history. For example, interest in the United States in science and technology, as measured by university and college student enrollments in these areas, is beginning to drop after a substantial increase in the 1958–64 period.

These shocks, together with what was then a relatively smaller national concern with issues which are strongly competing for money now, made it both possible and feasible to key the NASA space program for the current decade to a very demanding mission objective—manned lunar landing and return. There is no such consensus on a natural and compelling major mission objective for NASA in the next decade. This lack of agreement on a major mission—or, alternatively, the odd insistence of some space proponents that all interesting space projects should be undertaken—has probably had much to do with the current lack of enthusiasm for a \$5 to \$6 billion NASA annual space budget.

Another powerful influence on NASA and the space budget is independent of NASA and its mission. The U.S. R. & D. community has been favored by liberal Federal financing for 10 to 15 years. But recently now the U.S. R. & D. budget has grown large enough to attract attention and to draw questions. Those questions are not always relevant and meaningful, which perhaps prompts the unfortunate tendency of some segments of the R. & D. community to believe that they should be the automatic and unquestioned recipients of continually increased Federal support. The result has been to alienate many people who want to see and understand the overt consequences to society of this Federal support. The NASA space program is caught up in this questioning, as are many other scientific and technological endeavors. A not uncommon feeling, which has become ever stronger since the

mid-1960's is that the days of passive and unquestioning public support of massive Federal R. & D. expenditures are about over; and that some discipline, and much more reasoned persuasiveness on the part of the R. & D. community will be necessary in the future to sustain a reasonable level of R. & D. support. The level of this support will probably remain high; but a shift of emphasis in the direction of more applied research is quite likely. The danger is that such a shift, which has many desirable features, may endanger the continuation of the absolutely essential menu of varied basic research which provides the technological and intellectual capital for the future.*

It is difficult to ignore the combined effects on the NASA budget of the decay of the Sputnik-Vostok shocks; the lack of agreement on objectives or on a major and compelling new NASA space mission, or missions; the fact that even the very demanding manned lunar landing and return mission, which for the large part drove the NASA program in the last 8 years, has a natural cycle of peaking and decline as the massive capital investments and launch system procurements are accomplished; and the increased doubt that continual and automatic jumps in R. & D. funding in general are unmistakably in the national interest.

The current governmental and public questioning of the rationale for large R. & D. expenditures—which promises to remain a most important factor shaping the future trend of Federal allocations of resources—is not a peculiarly American phenomenon. Rather, it is a concern which has a more global aspect, particularly among societies with a strong technological thrust. This is especially seen in a little-noted article in the Soviet newspaper *Izvestia* (Oct. 24, 1968, pp. 1-2) decreeing major reorganizations of R. & D. in the U.S.S.R. Portions of the article are translated in the following excerpts:

A prevalent shortcoming in the work of scientific research, design, projection and planning, and technological organizations, and in scientific subdivisions of higher educational institutions, is that their activity is not properly geared to solving the most important scientific and technical problems, especially those connected with accelerating the development of labor productivity in industry, agriculture, construction, transport, and other branches of the national economy.

The Central Committee of the CPSU and the U.S.S.R. Council of Ministers, proceeding from the problems of further development of the U.S.S.R. national economy, have proposed that the State Committee on Science and Technology under the U.S.S.R. Council of Ministers, Gosplan of the U.S.S.R. Gosstroj of the U.S.S.R., the U.S.S.R. Academy of Sciences, the ministries and their departments, and the councils of ministers of the union republics:

- (1) Provide for a broader use of the latest achievements of domestic and foreign science and technology and advanced experience in the development of long-range, 5-year, and annual national economic plans;

* Further discussion of this issue is found in the paper by Sewell in this volume.

(2) Take urgent measures to significantly increase the efficiency of scientific establishments, to improve the organization and control of scientific research and technological development, and to strengthen management responsibility in enterprises, scientific organizations, and higher educational institutions in the creation of new hardware and utilizing it in the national economy.

. . . . It is considered necessary to provide economic incentives to staff personnel of scientific research establishments, scientific subdivisions of higher educational institutions, and industrial enterprises, and also their workers, depending directly on the actual economic effect obtained in the national economy from using scientific and technical developments and new techniques. . . .

It seems evident from these excerpts that scientific research has now become a major industry in the Soviet Union also, and is no longer simply a glamorous activity into which unlimited and uncontrolled funds can be poured. This emphasis on the utility of research work—like the “relevance” and “results” arguments in the United States—indicates that the Soviets are becoming increasingly interested in the problems of introduction of research results into actual practice.

In any event, it is against these backgrounds of questioning attitudes current both in the U.S. and the U.S.S.R. that basic problems of the public policy aspects of the national space program arise:

1. Are the resources allocated to the national space program rationally and effectively applied to satisfying valid national goals?
2. Considering the many competing alternative users of the resources so allocated, are the statements of the national goals, definitions of national priorities, and the allocation mechanisms adequately well defined so as to permit decisions on preferred utilization of such resources?

III. PROBLEMS OF STATING PUBLIC POLICY FOR THE NATIONAL SPACE PROGRAM

In the sense of having available a quantitative mechanism for assigning some level of priority to a national space program, and for guiding the development of a budget commensurate with that priority, in the context of other claimants for the resources (in terms of funds, personnel, facilities, and industrial base), it seems reasonably clear that no truly adequate statement of policy and purpose for the national space program currently exists.

It should at once be emphasized, however, that the same is true of many other national endeavors; the space program cannot and should not be faulted on that basis alone. An indirect and relatively informal mechanism for assigning priorities nevertheless exists, in terms of the annual appropriations made by Congress, which is one implicit measurement (there are no explicitly stated criteria for judgment) of the assignment of relative importance to various national endeavors. As has always been the case, appropriations are shaping priorities and fixing goals, not the other way around. But there is clearly particular concern that both the rationale for the costs of the space program, and the relevance of the U.S. total space endeavors to the state of the national well-being (including national security) and to the national economy, present a more pressing need for planning, and for review of

present prospects and possible future courses of activity, than almost any other major area of national expenditure.

Part of this particular concern is undoubtedly generated by the circumstance that most, if not all, of our other heavily funded activities are characterized by a historically long acceptance by tradition and habit (e.g., farm income stabilization) and/or by more obvious, or at least more simply stated, goals (e.g., national security expenditures). Most major new budget line items such as NASA probably receive comparably close scrutiny.

Existing guidelines for the NASA space program, and for the reservation of some highly important parts of the national space program to the DOD, are reflected by the legislative language of the "National Aeronautics and Space Act of 1958." It should be clear that policy implications of the act are neither well-defined nor quantitative. We cite briefly from the act because we shall have occasion to refer to it and its implications:

"DECLARATION OF POLICY AND PURPOSE"

(a) * * * it is the policy of the United States that activities in space should be devoted to peaceful purposes for the benefit of all mankind.

(b) * * * the general welfare and security of the United States require that adequate provision be made for aeronautical and space activities. * * * such activities shall be the responsibility of, and shall be directed by, a civilian agency exercising control over aeronautical and space activities sponsored by the United States, except that activities peculiar to or primarily associated with the development of weapons systems, military operations, or the defense of the United States (including the research and development necessary to make effective provision for the defense of the United States) shall be the responsibility of, * * * the Department of Defense: * * *

(c) The aeronautical and space activities of the United States shall be conducted so as to contribute materially to one or more of the following objectives:

(1) The expansion of human knowledge * * *;

(2) The improvement * * * of aeronautical and space vehicles;

(3) The development and operation of vehicles capable of carrying instruments, * * * and living organisms through space;

(4) The establishment of long-range studies of the potential benefits to be gained from, the opportunities for, and the problems involved in the utilization of aeronautical and space activities for peaceful and scientific purposes;

(5) The preservation of the role of the United States as a leader in aeronautical and space science and technology * * *;

(6) The making available to agencies directly concerned with national defense of discoveries that have military value or significance, and the furnishing by such agencies, to the civilian agency * * * of information as to discoveries which have value or significance to that agency;

(7) Cooperation by the United States with other nations and groups of nations * * * in * * * peaceful application * * *;

(8) * * * close cooperation among all interested agencies of the United States * * * to avoid unnecessary duplication. * * *

The crux of the current concerns regarding policy guidance in the 1958 act may be said to lie especially in the guiding request for "adequate provision" to be made for space activities, and in the simultaneous absence of any explicit indication of the priority even roughly assignable to such activities.

Nevertheless, it is not certain that a firmly stated, generally understood national space policy is essential to the effective conduct of a national space program. Nor is it apparent that the many elements included among the space agencies of the United States have ever acted in concert with some national space policy. The necessary instruments for shaping such a policy certainly exist in the White House, in the National Aeronautics and Space Council and the Aeronautics and Astronautics Coordinating Board, in the administrations of DOD and NASA, and in Congress. Authority for promulgation and implementation also exists, even if it has been relatively dormant. But it is not at all evident that a decision to proceed toward a manned lunar landing, or tacit acceptance of the habit of continued military space operations, has any of the attributes of a "policy." In any case, to the extent that a policy exists, it seems to have been persistently marred by an indifferent distinction between rationalization, and a true policy analysis identifying program goals and associating them with clearly defined benefits for the nation or some large segment of it.

Like many other aspects of Government, the definition of policy for the future is influenced, if not dominated, by pressures arising from existing establishments.¹ In its first (1958-1961) phase, NASA was only marginally susceptible to such influences because its constituent organizations were both too small and too slightly endowed with major facilities to develop much institutional inertia. Additionally, the NASA of 1958-1961 was governed by a White House philosophy and by a NASA administrator with a relatively dispassionate view of space program prospects and a candid appreciation of the probable costs and consequences of highly accelerated programs. With the Apollo decision of 1961, new national space objectives appeared and NASA shifted course, again largely unhampered by institutional inertia, because once more the existing institution was quite evidently too small to perform the new task now being required of it. Spectacular growth, driven by the scope of the Apollo task and by the energy and enthusiasm of the new administration, was characteristic of the next 5 years.

Although the growth and achievements of the military space program (1958-1968) were less visible than those of NASA, at the end of that period the military services owned a sizeable stable of vehicles and a large collection of space-focused facilities, with staff to match. The creation of an Apollo-oriented NASA was paralleled by a 1961 decision to assign substantially all military space responsibility to the

¹ Because of this, it seems pertinent to review the major aspects of the NASA and DOD space activity history in the last decade in sec. IV, and to make a few comments here.

Air Force, and by the subsequent accumulation of an impressively performing Air Force organization dealing with space matters.

One policy problem of 1969, only partly recognized or acknowledged by the several concerned Federal institutions, is that no widely acceptable space policy objectives have been stated to which any existing institutions may directly apply themselves. Goals, when defined, have been specific program goals.

The central difficulty is not that the existing space institutions, civil or military, have been unable to state space program objectives; but that those so stated have had relatively few long term policy implications and have indifferently influenced opinion shaping and enabling bodies.

Notwithstanding that questions of priorities for space activities have not been directly addressed, that there is less than complete agreement on what our national objectives in space ought to be (in the sense of objectives that can be compared with other defined national objectives); and that, in particular, neither the goals nor the accomplishments of the DOD space program are widely acknowledged, the funding of space activities in the last decade has been very substantial, especially in the period from 1962 on. The following table shows the space funding levels identified in the Federal budget. For NASA those reflect total outlays, including those for aircraft technology as appropriate; and for DOD, expenditures.

SPENDING (NASA, DOD) FOR SPACE ACTIVITIES

[In billions of dollars]

Fiscal year	NASA	DOD	Total, NASA plus DOD
1960.....	0.40	0.52	0.92
1961.....	.74	.71	1.45
1962.....	1.26	1.03	2.29
1963.....	2.55	1.37	3.92
1964.....	4.17	1.56	5.73
1965.....	5.09	1.59	6.68
1966.....	5.93	1.64	7.57
1967.....	5.42	1.67	7.09
1968.....	4.72	1.89	6.61
1969 (estimate).....	4.25	2.10	6.35
Total.....	34.53	14.08	48.61

During the period Fiscal Year 1960-1969, when the NASA expenditure appears to be about \$34.5 billion, and the DOD expenditure about \$14.1 billion, for a combined NASA plus DOD expenditure of about \$48.6 billion, there were also Atomic Energy Commission expenditures for space of about \$1.4 billion, and aggregated expenditures for space by all other organizations of about \$0.3 billion.

The United States as a whole therefore had a total space expenditure during Fiscal Year 1960-1969 of about \$50.3 billion.

Considerations of cost are particularly important; we have at present no formally recognized set of priorities for the totality of our national programs and objectives as identified explicitly in the line items of the Federal budget. There is at present no *a priori* way to rank the possible benefits which these diverse national programs might generate by the Federal allocation of given resources (funding levels); hence we are forced to use costs as one major gage of the space pro-

gram. Whether such a ranking of "benefits" would be possible, desirable, or even completely consistent with our pluralistic political and economic philosophy, is a separate question not further addressed here. Short of having generally available such quantitative measures of benefit by which to assess the external significance and content of our national space program vis-a-vis other national efforts, it nonetheless now appears possible and practical, for purposes of periodic review, to begin to consider the national space program, at any given time, as if it were composed internally of two kinds of individual programs.

(a.) Programs with identifiable benefits and ascertainable costs. So far as possible, space programs should ultimately produce genuine returns on investment. A maturing program should be expected to yield perceivable and explicitly identifiable benefits, related to the national well-being, or the national security, all broadly defined. Examples are satellite programs for communications, earth surveys, navigation, etc. Costs of obtaining such benefits via space programs can be compared with costs of other possible alternatives producing comparable benefits. Space programs in which such a cost comparison is favorable will clearly justify continued public support. Space programs in which the cost comparison is not yet favorable, but in which both the technological directions and costs of these are relatively clear and evident to realize a favorable cost comparison over alternatives, within some reasonable time frame, presumably also deserve support.

(b.) Programs whose returns are more qualitative or intangible, or whose benefits would be inappropriate or impractical to estimate quantitatively (a close photographic flyby of Mars, for example). Here a minimum goal should be to estimate the time-phased multiyear costs of achieving stated and explicitly identified time-phased program objectives, with periodic updating of the cost stream trends; NASA is beginning to develop such projections. Displaying these costs will at least assist in the informal and intuitive decisionmaking relevant to much of the allocation of Federal resources, even in choosing between widely disparate kinds of alternatives, since many otherwise desirable or attractive choices are simply not "reasonable" choices at every cost level.

Both kinds of programs require the same kind of rather sophisticated cost projections, based on explicitly identified criteria. Such projections are now called for by the government's PPB system, applicable to both NASA and DOD. Both kinds of programs probably deserve presentation of several alternative plans or options, so that a specific plan need not absorb all the crucial examination.

It is quite important to note that acceptance of a multiyear phased program implies a degree of commitment somewhat comparable to a commitment to accomplish a major mission objective; but a commitment to a multiyear phased program also implies a more complex review process because of greater program indivisibilities. The long leadtimes inherent in space programs make uncertain future funding an enormously complicating factor in planning, and therefore warrant a high degree of commitment. Somewhat surprisingly, predictability of program costs for major NASA space programs has generally been better than the predictability of costs or schedules in comparably large military system programs. It is likely that a congressional commit-

ment to a multiyear phased program would provide for the vitally important, essential continuity of effort in the space program, comparable to that afforded by a commitment to achieve a given very major mission goal (e.g., manned lunar landing), but without some of the inherent penalties of such a mission commitment (e.g., the necessity for freezes on design and technology many years prior to mission achievement).

In any case, the decision between these two kinds of commitment is a crucial and basic decision, one which will be a fundamental factor shaping the course of the next decade's space program.

IV. SOME POINTS ON THE GROWTH AND DEVELOPMENT OF THE NATIONAL SPACE PROGRAM—BRIEF HISTORIES OF THE NASA AND DOD PROGRAMS

Because of the impact of the development history of the NASA and DOD space programs on both the present U.S. potential and the future possibilities in space, it seems useful to trace through these developments briefly. Certain program and organizational aspects of the NASA and DOD programs may also hold some lessons for the future possible courses of the national space program.

Although some space work antedated the Space Act of 1958, it is useful to treat 1958 as the start of the national space program.

In July 1958 President Eisenhower signed the National Aeronautics and Space Act which created NASA. Basically, the administration and NASA management were committed to conduct a broadly based program in science and technology, pursued aggressively with the intent to extend the state of the art as rapidly as possible, but avoiding high-risk manned missions and launches purely for propaganda purposes. In August, the DOD agency, ARPA, authorized development of the Juno V, 1.5 million pound thrust booster (later known as Saturn I); authorized development of six Juno IV vehicles for the purpose of launching 500-pound earth orbiting payloads; and provided for development of the Juno IV upper stages and guidance system. In September, ARPA directed that the Thor based portion of the USAF Agena satellite program be separated and established as a separate project identified as Discoverer.

On October 1, the National Aeronautics and Space Administration (NASA) officially began operations. NASA absorbed the 43-year-old National Advisory Committee for Aeronautics (including its staff and five laboratories and field stations). On the same day President Eisenhower directed transfer of a number of ARPA-directed space flight projects to NASA.

In April 1959, the Tiros meteorological satellite program was transferred from DOD to NASA auspices. Organizational tools for handling the NASA and DOD space programs in the context of total national needs were provided by the creation of the Space Council in the 1958 Space Act, and by assignment of duties to the Civilian-Military Liaison Committee. During July 1959, President Eisenhower additionally revised the charter of the Civilian-Military Liaison Committee so that it could take the initiative in dealing with disputes between NASA and the DOD. Prior to this revision the committee handled only those problems that were brought before it by the respective agencies. In 1960, additional transfers of Army Ballistic Missile

Agency resources from the DOD to NASA were ordered, climaxing a very substantial shift of research and development capability from DOD to NASA.

In 1959 through 1961, a number of critical studies and reviews of national space programs and needs were underway. A national launch vehicle program study was underway in NASA and DOD, and post-Mercury manned space flight planning began. The guidelines for Apollo were developed by NASA by mid-1960; and, in July 1960, the House Committee on Science and Astronautics recommended that "a high priority program should be undertaken to place a manned expedition on the moon in this decade." Extensive planning for the manned lunar landing was undertaken at the request of NASA's first Administrator; and in March 1961 the Space Science Board of the National Academy of Sciences submitted to the President a generally favorable report on "Man's Role in the National Space Program."

A number of circumstances in early 1961 then led Mr. Kennedy to call, on May 26, 1961, for an all-out U.S. effort to land and return man from the moon. Undoubtedly, contributing factors were the far-reaching plans and reports just cited, taken in context with other significant reports such as the Gardner and Wiesner reports, as well as other intensive internal studies on a shorter time scale during the spring of 1961; the space plans developed by then Vice President Johnson via the Space Council mechanism; the successful orbiting of astronauts by the Soviet Union, and, perhaps, other international circumstances. Mr. Kennedy called for a debate in Congress—a debate which did not take place because of the high public and congressional interest (although it is of some historical interest that the first Gallup poll on the subject, on May 31, 1961, found that 33 percent voted yes, and 58 percent no, to the proposal to spend \$40 billion in a manned lunar mission). The immediate budgetary consequence of the commitment to the manned lunar mission goal was an increase in the near future budget allocation from the \$1.1 billion originally processed by the previous administration.

In the time following the decision to undertake the Apollo program, it is convenient to treat first the NASA and then the DOD programs, separately. The post-1961 NASA program was dominated, in both attention and allocation of resources, by the manned space flight programs (Mercury 1961-63, Gemini 1965-66, and Apollo 1968—).² Consequently, we will emphasize this portion of the NASA program because of its enormous impact on the character of the civil space program, and because it holds some lessons on the strategy of selecting mission or capability goals. Clearly, the other parts of the NASA program are intrinsically very important; they are commented on later.

Although the manned flight portions of the three programs fall into three neatly separated periods, the conceptual, design, research, and development phases are not neatly separated. Mercury was begun in the Eisenhower administration to be flown under Kennedy; Gemini begun under Kennedy and flown under Johnson; and Apollo begun under Kennedy-Johnson to be flown out under Nixon. The Mercury program, which had its origins in early study effort by both the Air

² Portions of this information were drawn from G. D. Putnam, Draft NASA/OMSF Historical Note, December 1968.

Force and NASA, was begun in 1958 with the limited objective of placing a man on earth orbital flight, observing his reactions, and recovering both man and spacecraft. The design capabilities of that system were severely limited in spacecraft weight by available booster thrust and were amenable to little expansion or elaboration.

In concept and design, the next step in manned spacecraft was Apollo and not, as is sometimes assumed, Gemini. Constrained by anticipated booster lifting capabilities, the unknowns of the cis-lunar space travel environment, and after May 1961, a definite time limitation, the Apollo spacecraft design competition was begun in 1960 and the command and service module configuration selected late in 1961. It was imperative to define the basic design of Apollo spacecraft many years before its first manned flight.

Gemini, begun 6 months after Apollo was approved, had the advantage of a spacecraft design more advanced in many ways than Apollo. Since Gemini was designed for earth orbital operating and reentry conditions, it was comparatively more flexible than Apollo, which was designed for the environment out to 250,000 miles from earth, reentry at much greater speeds, and which was essentially limited by 1961 technological state of the art.

The centrally important point of the necessary reliance in Apollo on early technology was reiterated by a PSAC report in February of 1967, on "The Space Program in the Post-Apollo Period," wherein it is observed that:

A major unified program such as Apollo with an explicit deadline for success compels a concerted effort toward developments leading to immediately usable technical results. The basis for those results is, unfortunately, quite often the state of the art of the technology at the beginning of the program.

The December 1961 decision to extend the manned space flight operations beyond Project Mercury, providing an interim program before the flights of Apollo hardware could begin, was the Gemini program. Gemini had several primary objectives. It was to be a followon program designed to subject two men and supporting equipment to long duration flights.

A parallel objective was to rendezvous and dock with another orbiting vehicle and to maneuver the combined spacecraft. Rendezvous and docking were key elements of the lunar orbit rendezvous mission mode to be followed in the Apollo program (and were also felt to be critical for DOD programs such as the satellite interceptor then under study).

Experiments were planned with astronauts leaving the spacecraft in orbit, to determine their ability to perform useful tasks; this extra-vehicular activity was another technique projected for more advanced missions.

Department of Defense support of the Gemini program was massive. Launch vehicles for both the manned spacecraft and the rendezvous target vehicle were modified Air Force ballistic missiles. The Titan II was converted into a man-rated booster for the Gemini spacecraft, and Atlas launch vehicles and Agena spacecraft modified to the Gemini target vehicle configuration. The Air Force also provided all launch operations functions and much range support as well as recovery operations and tracking and communications support. This very extensive participation by the DOD was effective preparation for the

later DOD manned orbiting program—the MOL—which exploits many of the Gemini developments.

Most of the major changes in design in going from the Mercury program to the Gemini program were incorporated to place increased emphasis on the role of the crew. Mercury was designed for completely automatic control from the ground (with backup provisions for pilot control); Gemini was designed from the beginning to be controlled by the astronauts, with ground control as the backup. Even today the U.S.S.R. philosophy on manned space flight resembles that of Mercury instead of Gemini, to a considerable extent.

Advanced techniques and equipment in the Gemini program made possible a large number of scientific and technological experiments on the 10 manned Gemini flights; 52 separate experiments were flown in the program. The 52 experiments included measurements of the crew and of the space environment as well as technological developments proving out new equipment and techniques for space flight.

Careful planning and attention on the part of the entire Government-industry team produced a phenomenon quite singular for an advanced research and development program—a schedule which slipped forward rather than backward: Gemini XII flew in November 1966 rather than early 1967, completing the program at least 2 months earlier than anticipated; and total program costs to completion, earlier estimated at \$1.35 billion, totaled out at approximately \$1.29 billion.

Concurrent with the Gemini activity, the Apollo program continued its first phase which consisted of initial studies and research and engineering efforts establishing the technological feasibility of a manned lunar landing program. Its second phase comprised detailed study, analysis, and preliminary design to identify the specific means of reaching the established goal; this phase ended in November 1962 with award of the development contract for the last major Apollo component, the lunar module, and lasted approximately 18 months. The third phase of Apollo was further definition and detailed design of critical systems and subsystems. By March 1964, quantities, schedules, and costs of the lunar landing could be estimated in detail.

A central decision affecting the scheduling of hardware production and test was announced on October 30, 1963—adoption of all-up flight testing for Apollo-Saturn space vehicles. Each flight of the Saturn I-B and Saturn V launch vehicle systems would be scheduled with complete space vehicles, using live stages and essentially complete spacecraft. Earlier Air Force successes with this technique, and the necessity to shorten some of the lengthy procedures involved in the step-by-step test approach, all contributed to the decision.

The total Apollo program proposed in 1964 called for flying 12 Saturn I-B and 15 Saturn V launch vehicles, with the associated Apollo spacecraft systems, before the end of the decade to provide reasonable assurance of achieving the manned lunar landing and return. The total cost estimate for this program, including Apollo research and development, construction of facilities, tracking and data acquisition, and operational costs, was estimated in March 1964 at \$19.5 billion.

During 1964-67 the Saturn I flight program was completed and major elements of the Apollo spacecraft were tested. By November

1967 the manned space flight network had been developed sufficiently to support the first Apollo-Saturn V mission.

On January 27, 1967, a tragic accident took the lives of the three-man Apollo crew during a ground test on the launch pad at Cape Kennedy. Astronauts Virgil I. Grissom, Edward H. White II, and Roger B. Chaffee died from inhalation of toxic gases inside the spacecraft, within seconds after a fire broke out.

NASA's recovery from this accident was painful but effective. It was completed by successful manned Apollo missions in 1968, including the successful manned circuits of the moon in December 1968. While the most complex parts of the operations are yet to come, the manned lunar landing attempt in 1969 appears on schedule, except for some residual concern with the lander.* A new funding estimate, assuming nine Saturn V launches before the end of 1969 and a program runout into 1971, with no costs assumed absorbed by a follow-on program, was generated in 1968, at \$23.9 billion, of which about \$19.6 billion has been obligated—a relatively modest increase over the estimate of 4 years earlier. In its major programs, in fact, NASA has a rather good record for estimating its costs.

In the meantime, a debate had begun over the future of NASA after the manned lunar landing. Questions concerning future goals in space exploration were first posed by President Johnson to Mr. Webb on January 30, 1964. The decisions on the fiscal year 1965 budget had already surfaced the difficulty of justifying advanced hardware development for programs not identified with specific missions.

These questions prompted sharply the continued discussion and controversy which still surround the future of NASA. Out of this situation, and under pressure from both the White House and the Congress to propose specific advanced missions, grew the post-Apollo manned space flight proposals, the Apollo applications program (AAP).

In response to President Johnson's January 1964 query, a February 1965 NASA report dealt with all areas of NASA's responsibility, aeronautics and unmanned space flight as well as manned space flight. For future manned operations, NASA projected a capacity of launch six Saturn I-B and six Saturn V vehicles annually, with eight spacecraft provided, and identified the necessary changes to Apollo hardware which would make these Apollo extensions possible.

By the late summer of 1965, planning for application of the Apollo capability to future manned missions was considering approximately 150 scientific experiments as candidate possibilities for manned flight; the scientific community had been consulted extensively.

Subsequent actions stretched out flight dates, and canceled or deferred missions. By early 1968 the use concept for post-Apollo manned missions (the Apollo applications program) had been reduced to two Saturn I-B and two Saturn V launch vehicles, using refurbished command and service modules. More recent actions indicate prospects of additional schedule slippage and mission curtailment.

In total, the NASA space program in the current fiscal decade (fiscal year 1960-69) will, with estimates for the later years, have a

* Since the time of writing this paper in January of 1969, the landing module has, of course, had extensive operational space testing in manned flight.

total budget outlay of about \$34.5 billion, divided into the following major categories of effort:

	<i>Billions</i>
Manned space flight.....	\$22.5
Space science and applications.....	5.8
Space technology.....	3.2
Aircraft technology.....	.6
Support activities.....	2.4
Fiscal year 1960-69 total.....	34.5

Of this total, there is a capital plant investment of about \$4.2 billion. A more detailed table, listing budget outlays from fiscal years 1960 to 1969, from the Federal budget follows (table 1).

TABLE 1.—NASA BUDGET OUTLAYS, FISCAL YEARS 1960-69

[In billions of dollars]

NASA programs	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969
Manned Space Flight.....	0.11	0.28	0.57	1.52	2.77	3.54	4.21	3.65	3.10	2.76
Space science, applications.....	.13	.25	.42	.58	.75	.75	.79	.80	.70	.60
Space technology.....	.05	.09	.16	.30	.43	.48	.44	.44	.41	.38
Aircraft technology.....	.07	.05	.03	.04	.04	.06	.08	.09	.13	.15
Supporting activities.....	.03	.08	.08	.12	.18	.26	.44	.45	.39	.37

Manned space flight, although the driving and dominant shaper of the overall NASA program, is not, of course, the totality of the NASA space effort. Programs in space science and applications, in space technology, and in aircraft technology have also been conducted, at varied levels of support. Levels of support for these programs are in general, however, rather small compared with the manned program support levels, as the previous table shows. These smaller programs have nonetheless made very major contributions to many of the purposes cited in the 1958 Space Act. In the purpose of scientific return, in the role of a technological stimulus, and in the promise of current and future social and economic benefits—benefits via the kinds of space applications discussed in NAS-NRC reports—the contributions of smaller unmanned space programs have been proportionately greater than those of the manned programs.

This imbalance of returns as between the manned and unmanned programs is sometimes taken to exemplify the current dilemma of NASA. The main thrusts of the NASA effort to date—in program orientation, in launch vehicle and space vehicle development, in facilities and laboratory equipment, and in the industrial base and production capability—have been keyed to the manned program. To keep this total resource active, and fully and beneficially occupied, has been a major NASA concern. It has encouraged a corresponding focus, in proposed immediate post-Apollo plans, on major manned undertakings. These plans involve earth-orbiting missions, and there is a strong temptation to include men in certain applications satellites even when it is clear that the man is redundant or that his value is marginal. Possibilities cited in such plans, singly and/or in sequence, have revolved around AAP activities; a Saturn V version of an orbiting manned “workshop” space station; and a more capable, multipurpose, semi-permanent orbiting, multiman space station, with shuttle support from earth, at a cost of \$1 to \$2 billion per year averaged over the estimated 5 years needed to develop the capability.

There is concern, however, about the relative inefficiency of a manned satellite as compared with the potential of an unmanned vehicle. There is some feeling that too much attention has been given to establishing how long man can safely stay in space, and what his capabilities are for useful work there, and too little attention to mission requirements: what missions really justify (or are incapable of accomplishment without) keeping a man in space, instead of relying on remote automated operation; what is the mission-essential duration; what are the relationships between task combinations and crew size implications, and so on. It is generally acknowledged that some very complex missions are flown unmanned, with complete success, even with current technology, by both NASA systems—for example, Surveyor and Orbiter—and DOD systems. In the words of the NAS-NRC 1967 report on Space Applications: “* * * systems * * * providing near term benefits * * * will be achieved more effectively and economically with automated devices and vehicles.” For NASA to concentrate relatively more emphasis on unmanned systems in this way, however, creates the real risk that the vast resources assembled to support the manned missions will then be grossly underutilized under tight budget constraints where primary choices of emphasis might have to be made.

The history of the DOD space program runs a rather different course, albeit necessarily publicly somewhat obscured by virtue of classification and sensitivity.

During the decade preceding the Kennedy administration, the United States had made good progress in observing and studying the space environment.

The Air Force began building a space technology satellite in 1958, using a Thor-Agena combination. An important side benefit of this satellite was the introduction of new engineering knowledge and techniques. It tested the Agena stage and exercised the Air Force's global Satellite Control Facility, Sunnyvale, Calif., for the first time.

The Air Force's Satellite Control Facility was a unique organization fundamental to DOD space operations: it provided for tracking, controlling, and command of satellites, some of which would require scores of separate commands on a single orbital pass.

By April 1961 the Air Force had organized the Space Systems Division at Los Angeles as a special management team to handle its space programs.

In 1959 the Air Force had been made responsible for furnishing space booster support to all the military services. In March 1961, Secretary of Defense McNamara resolved a drawn-out interservice conflict by assigning “research, development, test, and engineering of Department of Defense space development programs or projects, which are approved hereafter” to the Air Force. Again, during the same month, he assigned all DOD reconnaissance, mapping, and geodetic programs to the Air Force. These responsibilities aggregated into an Air Force space mission of significant scope and potential consistent with the 1958 Space Act:

(a) To conduct applied research and advanced technology to further the state of the space art.

(b) To manage the development and procurement of Department of Defense space systems.

(c) To launch, control, and recover DOD space vehicles.

(d) To support other Federal agencies as required in attaining national space objectives.

The military applications of space had been thoroughly studied, and answers as to how the Department of Defense can use space operations had focused on three categories of use: as an observation post, as a communication center, and as an arena for deterrence or general enhancement of military operations. All these applications utilize earth orbiting systems; some use synchronous altitudes or even more distant orbits for effective operation. The Air Force had arrived at these conclusions well before 1961 and by that time was developing space systems of each species. The nuclear detection satellite and the attack alarm satellite, for example, were designed to search space and earth for possible covert nuclear testing and ballistic missile launchings. By 1962 the Air Force was building communication satellites to furnish truly global information channels for military users. The Air Force had also begun work on a simple inspector satellite.

But in DOD space history, the administration in 1961-63 entered into a period of retrenchment and reevaluation. The OSD began to cancel or slow down a number of Air Force "pre-Kennedy" programs. In January-February 1961, the OSD canceled practically all funding for a spaceborne defense system. In July 1961 it organized a review task group to study the attack alarm system. In August 1961 OSD reduced the satellite inspector to a very modest research and development program; in April 1961 set a \$200,000 limit on individual Air Force space study programs; and in the summer of 1962 canceled the entire space system study program. Most of these actions clustered in 1961; some parallel actions extended into 1962. Cancellation of the Dyna-Soar program because of its uncertain value and high cost prospect followed in December 1963.

But 2 years earlier the OSD space team had essentially completed its initial review of the existing Air Force program and was sponsoring new objectives, as before largely under the stewardship of the Air Force. The new program was distinguished by new principles of management and space program evaluation. Henceforth, Air Force space programs, like other expensive military programs, would be disciplined in concept and scope by an external evaluation on OSD or national need, rather than service need; by system analysis considerations, capability needs studies, trade-offs; and by conservative, but continually evolving and flexible accommodation to, extensions of the technological state of the art. Before additional space programs were approved by DOD the services would have to demonstrate that all pertinent technical considerations had been adequately weighed. This DOD management philosophy, requiring that space programs be justified on basically the same terms as other national security programs (i.e., they must efficiently provide some desired operational capability and must be relevant to existing Department missions), has a major influence on the definition of criteria for space missions.*

Late in 1961, standardized Titan III space boosters emerged as the first output of this comprehensive screening process. In May 1962 OSD authorized the development of a communication satellite by the Air Force. Later that year, cooperative Air Force-NASA Gemini tests were approved. In March 1963 the OSD agreed to finance a new Air

* Further discussion of this issue is found in the paper by Enthoven & Smith in this volume.

Force satellite inspector. In December 1963 a Manned Orbiting Laboratory was started by the Air Force, a program powerfully bolstered by DOD's participation in Gemini.

Thus, years 1961-63 represented, for USAF, a period of adjustment to new management concept demands by OSD and reorientation of goals in conformity with broadened national space objectives. The management innovations of 1961-63 were subsequently institutionalized within the Air Force space organization.

From this summary one may deduce with reasonable confidence what may be the DOD-sponsored military spacecraft of the next decade, contrasting with the unsettled NASA case. Spacecraft will be assigned, as now, to provide observation, communication, and deterrent capabilities, completely centered around earth orbiting missions. The Manned Orbiting Laboratory will be operational during the 1970's, obtaining answers to a multitude of questions about military operations in space. The successful nuclear detection satellite will undoubtedly have descendants in orbit. Attack alarm satellites will be available to signal missile launchings over the entire globe. Communication satellites will be increasingly useful in both strategic and tactical assignments. Improved navigation satellites will continue to serve an important defense function. Inspector systems may be active. Cooperative programs using NASA-furnished systems (e.g., in weather satellites and geodetic satellite for common national purposes) will likely have counterparts. It is possible that a review or selected systems which in the last decade did not meet effectiveness standards, but which would otherwise provide attractive capabilities, might, on the basis of current and near-future technology, turn up one or two new contenders for development.

This cycle of DOD and USAF space history in the sixties may well be relevant to NASA's situation currently.

The current lack of approval for a post-lunar manned program may, for NASA, be reminiscent of the Air Force situation of 1961-62. The contrasts of NASA's affluence of 1961-66 with the comparative frugality of the present is turning NASA planners toward the question, "How do we fill the gap?" Some rather drastic proposals have been made: turn NASA back toward the model of the original National Advisory Committee for Aeronautics (NACA), or capitalize in a major way on the new interests such as oceanography or urban renewal. It is unlikely that the national interest would allow such retrenchment. But, like the Air Force of 1961-62, NASA perhaps may emerge from the on-going national review, and from its own self-analysis, with new concepts of priorities, choices, and management, and with a program more clearly responsive to the currently shifting national goals. The difficulty is that the relevance of intangible space program benefits to more matter-of-fact national goals has not been made sufficiently plain, either to Congress or to the American public. NASA has a more difficult task in this than DOD because national security is a historically evident national objective.

V. HOW WELL HAVE WE ACHIEVED THE OBJECTIVES OF THE 1958 SPACE ACT?—SOME CAVEATS AND AN EXTENDED DISCUSSION

The 1958 Space Act still provides the best extant guidelines for measuring the U.S. space achievement. To understand how well the

United States has actually done it seems advisable to evaluate that achievement against the goals of the Space Act. Obviously, the space program over the last decade has developed technologies that are now mature enough to be assessed in terms of their influence on the direction of the next decade's space efforts, the whole being reviewed against the background of updated national priorities and emphasis.

There is a need for appraisal of past achievement. Traditionally, Americans do not inquire closely into the causes of success. Only failures are carefully investigated, and the U.S. space program to date has been successful to an astonishing degree. Disappointing aspects of the space effort have been publicly aired and explained to the satisfaction of most. It may be a novel break with tradition, therefore, to suggest that the Nation inquire objectively, dispassionately, and in detail into the rationale and consequences of the decisions that shaped the present space program, however successful and above criticism these decisions may seem. This paper can only outline some personal views against a background of familiarity with the program. The object of such an inquiry would be to learn enough about the process of decisionmaking for the space program to improve the prospect that future decisions will be consciously as well matched as possible with current and emerging national needs and priorities. The postponed debate of 1961 on the policy aspects of what we want to do in space could fruitfully be held now. There seems in any case to be considerable advantage to creating an extended forum for consideration of space-related public policy issues by Congress and by the public, and for discussing fully the issues of "what to do" and "who should decide." The debates of the last few years have so far been somewhat aimless and unsatisfactory.

Whether concern for other pressing national needs will prevent such a debate is not clear. The future of our national space program is not the most urgent issue for national decision. But it is of special importance because questions about space activities intersect a surprising number of other issues—R. & D. strategy, the relations between technology and economic growth, the concepts of setting goals and priorities for national efforts, and so on.

One relevant aspect of the present civilian program is that very few really major policy or technical decisions had to be made once the basic Apollo schedules had been set and contracts let. The only unplanned event of any consequence was the 1967 capsule fire earlier mentioned, and here NASA recovered magnificently. So much having been done by NASA, and so well, there might be objection to an extended and objective policy inquiry, in consideration of the achievements of NASA. Nevertheless, such a policy inquiry would appear to be appropriate and fair to both NASA and the public, because of the continually increasing, but generally indecisive, controversy over Federal funding priorities—controversy which inevitably involves NASA.

There are evident dangers and shortcomings to reviewing the last decade in space against only the objectives cited in the 1958 Space Act, though this is inescapable in even a rudimentary policy discussion.

As one shortcoming, the legislative language is pedestrian. The arid, matter-of-fact words of the Space Act do not capture the excitement, pride, and vision which mark space exploration. Many Americans are

self-conscious about voicing emotional reactions to the space program. Our culture does not encourage displays of spontaneous enthusiasm and exuberance about new experiences and distant prospects, especially vicarious ones. Those with a high intellectual and social consciousness may find an emotional involvement with space exploration awkward and difficult to acknowledge, without evoking some feelings of guilt about ignoring social ills which also require emotional involvement for cure. But it would be a mistake to ignore or hide these visceral aspects of the space program, or to deny that in one of its intangible aspects space is still a unique frontier and an opportunity for satisfaction and meaningful completion of some of man's urges. The extent to which this quality alone of the space program constitutes an intrinsic worth (and therefore acceptable expenditure burden) of the program will be judged quite differently by different people, clearly. But at some level one should be able to accept this quality wholly and yet not feel derelict in other concerns.

For another thing, it is a continuing fact of life that there are still elements of competition between the U.S. and U.S.S.R. implicit in space activities. While we have undoubtedly matured in the last decade, to the extent that we can concede that we need not and cannot be first or foremost in everything, and that our activities should first and foremost be guided by our own ambitions, there nevertheless remains a background of technological competition in space. This background still implies that it would probably be wise to avoid a very highly visible foreclosure of capability by simple default. For example, if, as NASA officials have maintained, the U.S.S.R. is developing a booster comparable to the Saturn V in capability, it may be prudent to maintain Saturn V production lines open for this reason alone (at some modest level) to overlap by a continued U.S. capability the time of public display and operation of such boosters by the U.S.S.R.

Finally, the language of the 1958 Space Act did not take into account the incidental—but to many people quite important—consequences of the space program, particularly for NASA and the Nation after 1961. A new cadre of very highly skilled administrators and managers (both civilian and military) was demanded, and created. The requirements for very large system developments were successfully met, leading to the hope that dedicated groups working in concert could solve many complex problems considered intractable before. The notion of working on massive and extremely challenging problems with full public visibility has led to new standards (comparable to or exceeding those of the nuclear submarine program) of engineering competence and craftsmanship, reliability, and depth of inspection (the Apollo fire notwithstanding). A deliberate and conscious effort was made to contribute to, to broaden, and to enrich the educational base by enlarging student training, facilities, and research. [Continued shrinkage of some of this support in the future, as NASA budgets shrink (as has been the case in student training) would be attributable not so much to a change in attitude as to policy which emphasizes achieving scheduled program or mission goals, and would in this sense be one of the penalties for stating goals in terms of mission accomplishment rather than capability development. Actually, NASA's funding of research in educational and nonprofit organiza-

tions grew steadily from \$142 million in fiscal year 1964 to \$165 million in fiscal year 1968.] NASA has also made a conscious effort to improve the economic lot of various areas of the country by its allocation of contracts, and has sought to provide vital new stimuli by such allocations.

In any event, in all of these aspects the space program has, sometimes unexpectedly, made substantial and vital contributions. These consequences should also be considered as part of the capital developed through the space program. While not readily measurable against the objectives cited in the 1958 Space Act, they nevertheless represent some measure of intrinsic worth of the program.

In a formal review of the 1958 Space Act's guidance, it is important to remember that the 1958 Space Act is quite deliberate in directing only that "The aeronautical and space activities of the United States shall be conducted so as to contribute materially to one or more of the following objectives (our italic) * * *" The act then cites eight objectives. Reviewers of the space program do well to remember that criticism of the space program is sometimes based on unrealistic notions of what was formally expected from the program. The 1958 Space Act of course furnishes no notion of what would be regarded as "tolerable" expenditures to meet one or more of the eight objectives; the Act is in this sense deficient as a quantitative policy guide. But, in fact, the eight objectives cited in the 1958 Space Act have been met in the current decade, either wholly or essentially so, or in a significant way; an enormous amount has been accomplished in the national space program.

Responsibility for these accomplishments was largely assigned to NASA, and NASA was the primary force in achieving them. The DOD program was a powerful, complementary ally toward these accomplishments. The program outlined by the 1958 Space Act has met these objectives to a very high degree:

- A. Expansion of human knowledge.
- B. Improvement of aeronautical and space vehicles.
- C. The development and operation of vehicles capable of carrying instruments * * * and living organisms through space.
- D. The preservation of the role of the United States as a leader in aeronautical and space science and technology.

There could be some demurrers regarding B and D, in the context of aeronautical activities in such cases as the SST. But that extremely complex and demanding undertaking was not a NASA responsibility. There are certainly good reasons for NASA to expand its level of activities in aeronautics compared with its relative involvement during the current decade. NASA's aeronautics program has the characteristic that it is more clearly and immediately related to goals that are simpler to defend—i.e., the program includes projects such as aircraft noise reduction, increased operating safety, development of vehicles suited for interurban transportation, support of DOD aircraft programs, etc. This program is then a candidate for increased support. But the notion of NASA's returning to the NACA's really singular position of preeminence in aeronautics, prior to 1945-50, probably could not be realized now, because of the extraordinary investment of the aerospace industry in an aeronautical R. & D. capability during the last 20 years.

In addition to these aspects of the space program, there are a number of other identifiable considerations which can be thought of as public policy components which help shape a space program, and which are directly relevant to the remainder of the objectives defined by the broad guidelines of the 1958 Space Act. The discussion of these considerations is more complex and requires some elaboration, compared with the previous considerations. These remaining considerations can be aggregated into major categories of:

1. National security, and relationships of NASA and DOD programs.
2. Technological impact, exploration and scientific observation and experiment.
3. Practical civil applications, and opportunities for international cooperation.

These categories are of roughly equal importance for the purpose of assessing the degree to which the objectives of the 1958 Space Act have been met during the last decade.

Briefly, our assessments in these three categories are:

For one, the NASA and DOD programs have been relatively independent in the current decade. This is not incompatible with the guidance of the Space Act. But the next decade provides opportunities for some program intermingling which deserve careful study.

For two, emphasis in the current decade on achievement of major mission goals has acted to limit outputs of new technology and scientific discovery. But it will be possible to choose program directions in the next decade to enhance such outputs significantly.

For three, at the end of the current decade we have only begun to realize the potential for civil applications of space activities. More attention to this area, and appropriate R. & D. emphasis, will accelerate the realization of this potential—and can also provide new opportunities for international efforts.

1. NATIONAL SECURITY, AND RELATIONSHIPS OF NASA AND DOD PROGRAMS

The national security component of the national space program is regarded here as being composed of the efforts directly conducted by the DOD, together with the contributions made to the DOD programs by NASA/DOD interaction. This last contribution is the primary issue discussed here, but it is important to recall that there have been many two-way interactions between NASA and DOD.

The 1958 Space Act (see sec. III) stresses peaceful activities in space, to be directed by a civilian agency; but reserves to the DOD control and direction of activities (including R. & D.) "peculiar to" or "primarily associated with" defense needs. It further directs that, among the one or more of the eight objectives assigned to NASA as a focus for action, the NASA activities subsumed by the Space Act be so conducted as to make available to defense-concerned agencies discoveries of military relevance; and, reciprocally, that such agencies provide to NASA discoveries relevant to NASA. Finally, the act likewise requests "close cooperation * * * among all * * * agencies * * * to avoid unnecessary duplication." The act also directs NASA to make public all information obtained or developed, except for that information required by Federal statute, and for reasons of national security, to be withheld.

For the coordination of aeronautical and space activities the National Aeronautics and Space Council was established; after 1961 it was chaired by the Vice President and included the heads of the four relevant agencies concerned (in particular NASA and DOD). The Council was to advise and assist the President, via functions which include: development of a comprehensive U.S. program of aeronautical and space activities; designation and fixing of responsibility for direction of such activities; provision of effective cooperation among all U.S. organizations; and resolution of differences among organizations with respect to such activities.

The act and its subsequent implementations are not wholly unequivocal in spelling out the relations between NASA and DOD, a circumstance which is partially explained by the very wide range of responsibility assignments which were originally championed, but which were finally not included in the act's provisions. Proposed assignments ranged from execution of all U.S. space activities by USAF/DOD to the creation of a strong military support division in NASA. Strong proponents of such assignments can still be found. The reluctance of some scientists to engage in classified DOD programs, and the general desire for a peaceful image, made some separation of functions quite reasonable, for good political reasons at the time; and it was then natural that space-related national security activities be in fact assigned to the DOD. The authoritative step in this took place in March 1961. Thereafter, the basic NASA and DOD programs have generally run their separate ways, with the exception of the major cooperative activities and support activities noted elsewhere.³

Notwithstanding some of the inherent ambiguities of the 1958 Space Act, significant general support and exchanges of assistance between NASA and DOD have been constant since 1958.⁴ Large scale total transfers of personnel and facilities took place early, and major space programs initiated by DOD were transferred to NASA for completion. Until the Saturn series of boosters became available, NASA relied almost entirely on DOD-developed launch vehicles. Launch and range facilities were shared. Particularly through the Gemini program, DOD provided very large-scale support to NASA: launch and rendezvous vehicles, launch and range support, tracking and communications, and most importantly, recovery operations. DOD still provides such services to NASA, although NASA now has its own launch facilities and relies on its own manned space flight network for current manned operations. While attempts since the late 1950's to fully define a common stable of national launch vehicles have not been completely successful, NASA and DOD will continue in the future to use many of the same launch vehicles, up to and including versions of the USAF TITAN III.

DOD continues to be an important source of astronauts and managers for the NASA programs. In addition, NASA and DOD naturally utilize to a great extent a common industrial contracting, managerial and production base, which has been expanded in varying degrees by the NASA programs and by the DOD missile and space programs. Both organizations put the vast majority of their business with industry, for example NASA's direct and indirect diversion of its

³ See sec. IV.

⁴ *Ibid.*

funds to industry remains over 90 percent. NASA has adopted and enhanced many of the DOD management techniques, such as the systems development approach and the "all up" testing philosophy.

Common use of basic space vehicles (as distinct from launch vehicles) by DOD and NASA has lessened since 1961, although such utilization can be viewed as an objective of the 1958 Space Act. DOD has employed some of NASA's early communications satellites, and has adopted much of the Gemini system design and concept for its MOL program. DOD, primarily through the earlier experience of some of its contractors, has influenced the design and technology put into NASA's space programs; in addition, there have been major inputs of DOD concepts, techniques or technology into the spacecraft of those truly national space programs which currently are meeting the operational needs of several U.S. agencies and departments. In these programs—for weather satellites, and for geodetic satellites—NASA provides spacecraft, launch, and support responsive to the requirements stated by user organizations (DOD and Commerce). Some scientific satellite programs have also been jointly supported by NASA and DOD.

Both NASA and DOD have had space science programs (the DOD at a much lower funding level); and DOD experiments have been carried on NASA spacecraft, with some reciprocation, a practice which is expected to continue.

Finally, NASA continues to have an important role in DOD aircraft programs—for example, in an advisory capacity; in cooperative and joint research aircraft programs and lifting body programs; and in extensive support by NASA facilities (primarily wind tunnels) of DOD programs.

From 1958 to 1968 the DOD was developing its own space systems (consistent with the reservation of the 1958 Space Act), of the sort sketched briefly in sec. IV. The DOD program developed and operated those space systems determined to be essential to national security—that is, systems which met validated military operational requirements. The DOD program appears essentially independent of the NASA program, with the exceptions noted earlier, as is reflected in the detailed itemization of the NASA program and budget. From the generally demanding nature of the military requirements, one may surmise that the DOD space systems necessarily exploited state-of-the-art technology. This is also reflected in the PSAC report of 1967 on "The Space Program in the Post-Apollo Period," in the section discussing the direct application, in diverse areas, of satellite technology to the economic and social well-being of the country:

* * * Agencies other than NASA are actively investigating some of these areas. For example, DOD is developing its own communications system adapted to its special requirements, and is active in other applications of space. Thus, while development of space technology has already contributed substantially to our security in the direct military sense, DOD has also been involved in major new technological developments which can provide a wide range of civilian applications. An important problem for the next few years will be to identify, to declassify where necessary, and to encourage the transfer of all appropriate developments to the civilian community. A major fraction of the space applica-

tions program has, however, continued to be done under the aegis of NASA, * * *.

The NASA and DOD space programs should accordingly both be regarded as generally independent, technically advanced, and concurrent programs. There clearly are valid justifications for independence, including the major concern of both organizations for their own priority civilian and military needs, respectively, possible complications involved in NASA's undertaking of major classified programs, and the fact that the 1958 Space Act assigned defense-associated responsibilities to the DOD. Very major and pervasive contributions of the NASA program to national security needs should not be expected, nor does the 1958 Space Act direct this. These same circumstances also make it difficult, however, to accept statements that the civilian space program contributes more to national security interests than it costs, or that NASA technology lays the groundwork for future military hardware. Such statements are unjustifiable exaggerations.

Despite the relatively independent parallelism of the NASA and DOD space programs to date, there is scant evidence of any major "unnecessary duplication" so far. Of course avoiding such duplication is an explicit objective of the 1958 Space Act. The success arises from the extensive coordination and review mechanisms which protect against redundancy. The principal formal coordinating mechanisms for NASA and DOD are the Aeronautics and Astronautics Coordinating Board (AACB) and the Manned Space Flight Policy Committee (MSFPC), which provide opportunities for coordinated policy findings. The Office of the Special Assistant to the President for Science and Technology (OST) and the PSAC also provide for detailed reviews of major aspects of both space programs. Both PSAC and the National Academy of Sciences are additional entrees for scientific considerations which are ordinarily provided by several NASA advisory and review groups. The Space Council provides full opportunity to air both programs in detail. The Bureau of the Budget of course includes an authoritative recommending mechanism for both programs. Several congressional groups review each program and exercise ultimate authority by control of expenditure allocations. These bodies also provide ample opportunities for the senior NASA and DOD administrators and other decisionmakers and advisers to weigh both the open and sensitive portions of the national space program. The net impact of all these mechanisms has undoubtedly been to provide quite generally for the close cooperation and avoidance of unnecessary duplication which the 1958 Space Act calls for.

One should probably be cautious, however, in assuming that the good performance of these generally effective coordination mechanisms up to now fully insures comparable performance in the next decade. Instead, coordination in the next decade may well require greater care and attention. There are present several important circumstances during the last decade surrounding both the NASA and DOD programs which in fact then made coordination relatively easy and direct.

For one thing, the primary thrust and attention of NASA was focused on manned space flight, and in particular on the Apollo lunar program. The concurrent primary focus for the DOD was the development of automated space systems satisfying critical national security needs—needs which all involved earth orbiting systems for various

purposes of observation, communication, and general enhancement of military operations.

For another, both programs are well funded, each on its own merits and criteria. No serious questions arose regarding individual funding levels. The major thrusts of each program were sufficiently disparate in mission to make admittedly difficult judgments on any common basis unnecessary. There were present no hard questions prompting consideration of the total space funding level (NASA plus DOD) or of the appropriate relative funding level within that total. Little serious and concerted discussion arose in any major way on the purposefulness of the total space spending relative to other needs—for example, needs in other demanding societal areas; in federally supported R. & D. as a whole; or in other potentially exciting and rewarding major R. & D. areas such as development and exploitation of ocean resources.

In short, since the initial decision to create NASA and make it responsible for the Mercury program, it has not been necessary for the United States to make any really difficult choices and decisions regarding space programs. The situation has begun to change recently. We can anticipate that for the issues surrounding the national space program the period of relatively painless decisions is over.

Somewhat apart from, but nevertheless intimately related to, those questions external to the national space program, there are likely and possible courses internal to the national space program during the next decade which may well surface hard issues. These issues are raised by the possibility that the NASA and DOD programs in the next decade can involve some major, strong components which have, or appear to have, increased commonality of purpose and greater intersection of development paths. Such a possibility would sharpen the questions of coordination and avoidance of unnecessary duplication in the two programs, particularly if overall funding constraints are imposed.

For example, in the next decade both programs may involve these strong, technically related, components.

(a) Primary emphasis on earth orbital missions in general, and particularly on:

(b) major manned earth orbiting system developments, and

(c) continued development of earth orbiting systems for practical utilitarian social and economic benefits.

(a) might follow particularly from a plan to discontinue or severely limit manned lunar exploration following the fly out of the currently funded program, but will in any case be a major feature of the next decade's program. (b) might correspondingly follow from a plan to immediately concentrate on space station development, via the Apollo applications program (AAP) and follow-on programs, during the period in which the DOD Manned Orbiting Laboratory (MOL) is operational, after fly out of the current Apollo program. (c) will follow from a continuation of present development efforts, in which, as the 1967 PSAC report notes, both DOD and NASA are already active.

Conceivably, hard decisions may have to be made in the next decade's space programs in this context, if funding justification becomes more at issue. In this case, it is not clear that the coordination and review mechanisms described earlier have really been tested in this role. In a

sense, the situation has been one which more nearly allows a noninterference pact, rather than one requiring authoritative and explicit development of a comprehensive national space program. Such an authoritative program, to excerpt from the description of the role of the Space Council, would typically assign responsibilities, provide for effective cooperation and specify allowed concurrent activities, and resolve differences among all departments and agencies—all in the face of heightened budget scrutiny, or fixed, tight budget levels.

It should be emphasized that development of a program under these latter conditions is not easy, and is very often far more complex a process than generally appreciated. The process requires a painful accommodation by at least one institution and generally means postponing achievement of some otherwise desirable or attractive objectives and compromising on capabilities, for a time. Each institution naturally wants to preserve its own plans and programs intact.

In large programs, proposed changes which at first glance appear plausible often prove to be inefficient or ineffective once confronted by the hard realities of the present. For example, a proposal to fly a few spacecraft by a cheaper booster (e.g., TITAN III-class) than the one originally assigned (e.g., Saturn-class) is economically unrewarding—if it turns out that one needs to purchase the proposed (albeit cheaper) booster, while one has already paid for and stocked the original (costlier) booster. Unless one allows for such phenomena, intermingling of programs is not only difficult and inconvenient, but can prove to be more costly for a given compromise of objectives. Intermingling of major programs usually requires not only the decision to do so, but also, to be useful, a carefully laid-out, generally multi-year, plan describing how one reaches the end state by a phased transition. The decision to intermingle cannot be taken lightly; it is not a simple decision.

But the point nevertheless is that such a decision, and at least an exceedingly scrupulous examination of its merits and penalties, could be forced by further actual retrenchment or intensified questioning of space expenditures. Tighter space budgets can highlight the issue of whether a given level of total national capability in space can be reached more readily by some program mingling in the next decade. So far there has been no real test of an effective governmental mechanism to resolve major issues between the planning and programs of NASA and DOD. The difficult choices which might have to be made are neither purely scientific or purely budgetary. Whether any one, or all together, of the existing coordination mechanisms has the muscle to serve as penultimate umpire should be considered; if not, as one step, enhanced responsibilities, perhaps statutory changes, might be appropriate for existing groups, such as the Space Council and OST/PSAC.

In any case, it is improbable that the NASA and DOD programs could be completely intermingled. They would almost inevitably retain separate identities as largely "open-civilian" and "national-security oriented" programs. It would be difficult to conceive of a justification for a classified program of interplanetary probes, or, conversely, for releasing the products of some kinds of observation satellites mentioned earlier without preliminary screening for national security

information. The advantages of having both kinds of effort are apparent.

On the other hand, one could at least conceive of opportunities for intermingling of some programs based on earth orbital operations, or of shared or combined employment of some resources in those programs. These opportunities include, but are not limited to, the manned orbiting systems in the offing—the NASA AAP and the DOD MOL. Both problems and opportunities are abundant, including (typically) these examples, singly or in combination :

Common launch vehicles.

Common range employment.

Common spacecraft elements.

Adapters, allowing for use of one spacecraft on several boosters, or for one booster to be used with several spacecraft.

Common procurement cycle.

Common test facilities.

Common subsystems.

Common ground facilities and support (e.g., tracking and recovery systems; Houston and Sunnyvale facilities; astronaut training program; biomedical programs and facilities; etc.).

Common, agreed-to standards and procedures (where appropriate) for classification and declassification of hardware and results, and for appropriate partitioning of open and classified aspects of activities.

Appropriate methods for conducting open and classified operations in common facilities.

Shared management personnel.

Some of the questions here implied have been or are being asked, often with indecisive or negative results, principally because the necessary planning has not been sufficiently comprehensive. But not enough effort has been devoted to defining many such questions adequately, much less to soliciting answers for them in a context of severe fund shortages and many other claimants for support.

It is possible, of course, that a more careful examination of the questions could show benefits or opportunities for intermingling to be limited. Some U.S. space launches will remain classified as long as the Soviet Union maintains its private mystique of space operations, and that seems likely to be a considerable time. Other launches, particularly of manned missions, almost certainly will continue to attract a great deal of public attention. Simple prudence requires that some military options for space operations be maintained so long as there is reason to believe that another power could or would exploit space for military purposes. But it would be desirable to identify, to the extent possible, cases where it might be in the overall national interest to utilize for the whole space effort items from the military inventory. And might there not also be some merit, for example, to making the undoubtedly superb facilities at Houston responsive, to the extent possible, to the entire national space program—military as well as civilian?

There is one other motivation for considering some intermingling of a civil space program with a military program. Notwithstanding the risks and uncertainties of space science, NASA has established an excellent reputation for bringing in its major programs more or less on time and at pretty near the predicted cost. Some other U.S.

programs, for reasons that are not well understood partly because there has been a reluctance to investigate them in detail, seem consistently to understate technical difficulty, costs, and schedules. Whether NASA's unusual success is because of civilian management of individual programs, a greater organizational rapport, better personnel, or some more obscure factor remains to be determined. Nor is it evident that the military space program so largely conducted behind security blinds has the shortcomings of some of the more generally publicized military developments. There are indications that the classified Air Force space program is somehow more efficient or more effective than the usual run of military research and development. Still, the several possible advantages of some intermingling of portions of the military and civil space programs certainly include a possibility of more attractive project outcomes. In any event, the consequences of such a move certainly ought to be considered.

2. TECHNOLOGICAL IMPACT, EXPLORATION, AND SCIENTIFIC OBSERVATION AND EXPERIMENT

In large-scale programs with a mixed content of science and technology, two basic kinds of decisions can be made which relate to advancement of science and technology, and to exploitation of technology.

One kind of program is a broadly based program which tries to extend both science and the technological state of the art quickly, and which tries to achieve various benchmarks of capability and development in a schedule which is not rigidly fixed and which can accommodate new opportunities and maintain progress rapidly. Such a space program would be one which advances step by step, each step firmly based before undertaking the next. In such a program a relatively high premium would be placed on basic research, applied research, and advanced development efforts.

Another major kind of program is one which sets as a goal the achievement of a specific mission within a given time period. Such a program generally must rely on exploitation of available technology, rather than on exploitation of a number of serial developments. In such a program, a major, specific goal can be attained more rapidly, as a general rule.

Prior to undertaking the Apollo assignment, to be accomplished by a given time, the NASA program was generally of the first kind. With the Appollo decision and the commitment to a lunar landing by 1970, the preponderance of the NASA effort shifted to a program of the second kind, although significant portions of the effort remained of the first kind. But, by and large, the NASA program had its major technological impact and thrust in generally superb system engineering exploitation of technology, even in much of the unmanned space vehicle work. A concomitant result was the rather secondary emphasis on trying to maximize the scientific content of the program.

The Apollo mission, and its achievement within a given time frame, required very major exploitation of available technology for its achievement. In addition, the decision to adopt the all-up testing philosophy—which was required because of the extremely demanding schedule for Apollo within the allotted funds—also provided both op-

portunities for, as well as necessities for, exploitation of state-of-the-art technology. Consequently, as has been observed here and by groups elsewhere, such as the PSAC, the commitment to the Apollo mission required an early and irrevocable commitment to available technology.

Apollo technology is therefore, inevitably, largely pre-1962 technology. In consequence, the Apollo program could not capitalize on most post-1962 developments, and therefore placed less relative emphasis on basic research, applied research, or advanced development. Advanced development in the NASA program as a whole had to be a matter of secondary emphasis.

The fact that the largest single effort in the NASA program had to be based on exploitation of comparatively early technology had a number of consequences. It meant that the advancement of the technological state of the art could not be a predominant condition or major output in the NASA program. By and large, therefore, there were fewer opportunities to advance technology, and to diffuse this new technology into other areas, than would otherwise have been the case. Consequently, one should not have anticipated a great deal of new, innovative technological spinoff from the bulk of the NASA program. It should not be surprising, therefore, that it is difficult to cite the diffusion of new technology into other areas as a major return on the Apollo investment.

NASA has done a conscientious and thorough job of documenting its principal technologies for the use of others, via its technology utilization program, which provides summaries of new ideas, innovations, and new techniques. Applications useful to others have ranged over a wide spectrum, from employment of biomedical instrumentation to adoption of NASA manufacturing techniques. While there are a significant number of such application possibilities for NASA technology, it appears most difficult to justify claims which have sometimes been made that the development and diffusion of this NASA technology has in any major way, or entirely, justified the expenditures in the space program. Given that fact that the Apollo commitment required utilization of early technology, this should not have been expected, nor is it in fact the case. For example, emphasis in space program subsystem design on performance peaking for limited periods of time weighs against immediate civil applications, which typically require peaking in terms of low cost and long life.

The emphasis on the Apollo undertaking in the NASA program also implied that hardware off-shoots which had possible military-related use would be introduced at a slower pace, since for example, the development cycles of much military hardware would be mismatched with the development cycle of major Apollo equipments. Consequently, while the major thrust of the NASA program of the last decade, the Apollo undertaking, is a superb example of the most rapid exploitation of engineering knowledge possible, the selection of such a demanding mission, to be accomplished within the given time frame, also shows some of the dangers of such a primary goal orientation. In terms of the magnitude of the mission and the possibilities for realizing it, the Apollo commitment represents foresight of the highest degree, in that it undoubtedly represents a very narrow intersection of the most demanding mission, in every sense, and the most demanding time frame. It is scarcely conceivable that more could have been done in such a

giant step; in this sense, the goal and the schedule were ones which others would find most difficult to achieve. One consequence of the decision, however, was that rapid and broadly based development of many new technologies, as contrasted with the very powerful exploitation of an early stage of technology, was not to be expected.

For the next decade it would again be possible to select a very major mission goal and emphasize a program of the second kind. But there is also now an opportunity to make a new basic decision, to place more relative emphasis on advanced development programs, of a sequential nature, along a broad technology front. The probable implications of such a decision to shift more relative emphasis to advanced development would seem to be that more new technology would in fact be created and developed, and that one could anticipate greater spin-offs of this new technology.

A consequence of such a new decision might be that more relative emphasis on advanced development would possibly constrain the chances of accomplishing a very major new mission in the shortest possible time period. The analogy might be that if, at the start of the current decade, we had selected a program of the first kind, we might have, among other things, been capable of achieving the Apollo mission by the mid-1970's, but certainly not much earlier. Whether such an extended program, conducted as a series of sequential developments, would nevertheless produce more capabilities—in the long run—than the consecutive selection, decade-by-decade, of very major mission goals is arguable, but is not inherently implausible.

Another implication of greater relative emphasis on advanced development programs would probably be that more direct input into both new civil needs and military space needs might be provided.

Examples of advanced development objectives would include the development of a class of much lower cost boosters. While the present stable of launch vehicles in both NASA and DOD is adequate for most conceivable missions well into the 1970's, there are major implications for many kinds of programs to the development of much cheaper boosters. One can argue that, based on present projections of booster utilization, the development costs of a much cheaper class of boosters would not be recovered for a period of 5 to 10 years. But such a development program would at some period in time clearly begin to pay for itself. Also, because the projections of space traffic reflect at least an implicit accommodation to the currently high launch and vehicle costs, the development of a much cheaper class of vehicles could well foster far more traffic than it now seems reasonable to project. In any event, one attractive undertaking for the basic research and advanced development component of the NASA program in the next decade would be to build towards a new class of launch vehicles (and perhaps spacecraft), with emphasis on increased toughness to allow cruder launch operations (if necessary), but with primary emphasis on attaining much cheaper launch costs to get into orbit. A wide range of technical possibilities exists for such a development goal, including reusable boosters, atmospherically assisted vehicles, other propulsion developments, cheaper and cruder structures, etc. In the long run, such developments would be desirable for both NASA and DOD.

Another major development area is the building of spacecraft with much longer lifetimes. This implies new concepts of subsystem design

and construction, and new goals for reliability. Again, such developments would find civil and military use. Some important kinds of application satellites, in particular, would benefit from these developments in the direction of much greater cost effectiveness.

Another type of program which is beginning to provoke interest in many application areas, civil and military, may be the case of remotely manned systems. The possibilities of these have been dramatically exemplified by the success of NASA's Surveyor vehicle on the moon in successfully nudging a piece of hardware into action, in a controlled way, on command and with human direction, from the earth. These systems use a human operator to control, manipulate, and observe the action of remotely located automated devices via communication links between the human and the device. NASA has further strong potentials in this area via some of the developments involved in the lunar mission receiving and decontamination area. Progress in remotely manned automated hardware systems—"tele-effectors"—may be one of the most potentially useful products of the space program. Such devices could find major applications in many hazardous activities. These devices provide a very interesting intermediate alternative that falls between manned undertakings and unmanned, fully automated, undertakings, with some virtues of both manned and automated capabilities. It is conceivable that this kind of remotely controlled "effector" could find high utilization in the domestic sector as well as in the space and national security areas.

Finally, if the NASA space program in the next decade places more relative emphasis on a variety of advanced developments, and less relative emphasis on the accomplishment of a single major mission, there is less risk that in periods of tight budgets and unexpected demands for funds the smaller, but technologically very attractive programs, would starve for funds to maintain the overriding priority assigned to the single major mission goal.

The mission-oriented component development, system analysis, and system engineering expertise of NASA organizations naturally makes the organizations attractive candidates to work on other, non-space-related tasks requiring such expertise and a mission-oriented sense. Many such tasks are being undertaken by organizations with fewer capabilities. A complete or major assumption by a NASA organization of such a new mission would be contingent on the virtual disappearance of a current aeronautics- or space-related mission. Such a disappearance seems neither desirable or highly likely. On the other hand, some diversification can have attractions for both the organization and the new client. There are precedents for such diversification by contract assignments of research. For example, the Interior Department has contracted R & D work to the AEC, the NBS, and the Bureau of Reclamation and has been very pleased by the outcome. Opportunities certainly exist for NASA organizations to do similar work.

As in the case of technology, basic NASA program decisions have strongly influenced the scientific content of the program. In the general activities of exploration, scientific observation, and experiment, one can partition missions into two very general classes.

Earth orbit missions will continue to have the greatest relevance to national security, and will also tend to have the greatest chance of economic payoff. Deep space missions, conducted by lunar and plane-

tary exploration vehicles and interplanetary probes, would tend to have the greatest relevance to science and discovery.

This partitioning is insufficiently precise, of course. Orbital astronomy, for example, has great potential for basic findings in science and new discoveries; satellites also permit some other unique experiments and observations. It is possible that probes or vehicles which orbit bodies other than the earth might develop information which would have an ultimate economic payoff. But generally, the partitioning would be as indicated.

To the extent that the space program has produced significant new scientific findings so far, the deep space missions have been preponderantly responsible. This undoubtedly will hold true in the future. The major opportunities for enlargement of this effort are emphasized in several reports by the scientific community, most recently by the Space Science Board of the NAS. This area is also one which is both the subject of very active scientific competition by the U.S.S.R., and an opportunity for multi-national collaboration.

Because, in times of budget stringency, there might be a tendency to concentrate support on activities which have either the greatest major mission urgency or the greatest apparent chance of economic payoff, considerable extra attention and emphasis should be placed on support of deep space missions, because there the potential for new science and new exploration discoveries is high and will continue to be so. At a time when maintaining an adequate program of basic research is a part of a general national problem, it may be difficult to sustain such an emphasis on missions which tend to return abstract knowledge of no obvious relevance to the pressing problems of society. Maintaining this emphasis will require constant effort and should be a more significant consideration during the next decade than it has during this decade. These missions require continual new inputs of technology and relatively long lead times. They are therefore correspondingly good spurs for the development of new techniques, as well as being highly attractive and productive opportunities for important new scientific findings.

Greater emphasis on the deep space missions would also make it desirable to bolster the system engineering resources of the NASA spacecraft development organizations by enlarging the total capabilities for scientific research, planning, and analysis.

Numerous experiments have been carried out in the manned missions. But these missions have necessarily given primary attention to demonstrations of vehicle capability and physiological experiments to determine the ranges of human reactions and capabilities in the space environment. Other mission objectives have been secondary in priority.

However, it should be emphasized that a comprehensively designed space program in the future should encompass plans for both manned and automated experiments (and the intermediate case of remotely manned experiments) appropriate to the purposes, complexities, costs, and hazards of the experimental objectives. The situation is not one which now requires an either/or decision to be determined to as an enduring binary choice.

The experimental support of applications satellites, including those designed for national security needs, is probably most effectively done by mission-oriented programs in the context of the actual intended use.

As remarked earlier, the space program as a whole during the last decade has had a relatively small output of basic scientific knowledge considering the total space expenditures during the last decade, when compared with the basic science and research supported by the expenditures of agencies such as the NSF and the NIH. One of the cited extenuations is the emphasis accorded the manned lunar mission. Another is that there are relatively few opportunities for quick and flexible accommodation of experiments on spacecraft. If there were more possibilities for this, analogous to the quick reaction capabilities emphasized on occasion in DOD R. & D. programs, it is probable that scientific interest in the conduct of space experiments would promptly grow. In any case, opportunities certainly will arise during the next decade to increase the emphasis on exploration, and scientific observation and experiment, even in a context of reduced levels of overall expenditures for the space program as a whole.

3. ECONOMIC RETURNS FROM SPACE, AND POSSIBILITIES FOR INTERNATIONAL COOPERATION

Considering the fact that the space program has been in existence for only about a decade, it is both striking and reassuring that useful economic applications of space are already at hand, and that the future holds even greater promise. A semipublic Communications Satellite Corp. has been in existence for some time now. National programs in meteorology and geodesy serving the needs of many groups are in operation. In the civil sector, the programs in communications, meteorology, and geodesy are already beginning to benefit the public. The military impacts of these kinds of capabilities are also of major importance to effective operation of our national security forces. It is possible, even now, to make a clearly convincing case that applications in the three areas cited are already highly effective and justify the investments made in these programs.

A number of other application areas are rapidly coming into sight in which similar promise holds. The aggregate of the economic returns possible from these applications in the next decade is difficult to assess quantitatively. In some areas, we can already provide accurate estimates of the return on investment. In others, we can estimate within broad but conservative limits. In some, the returns are promises at the moment—but promises with a high possible payoff.

There are several ways to categorize space applications, depending either on mission or on requirements and techniques.

In terms of missions, there are two general classes of applications systems. One deals with survey and observation of the earth, its local environment, and its resources. Such systems would typically include applications to geodesy, meteorology, resource assessment, and utilization of the oceans. The second class would typically include applications to public communications, transmission of data, and command and control of both civil and military methods of transport.

But to categorize a system in terms of requirements and techniques permits a better appreciation of the economics of the technological capabilities which are available, or which are needed, to provide social benefits which are fiscally attractive, compared with alternative methods for accomplishing these objectives.

In general, the utility of satellite applications is proportional to the breadth of the phenomena being observed and to the need for frequently repeated observations. For example, in the field of meteorology, recurring global surveys of the earth's atmosphere, or continuous surveillance by synchronous satellites, provide data on the dynamic changes involved in weather observation and prediction. Such global, timely inputs will clearly also be essential in future attempts to modify the weather.

Observation of the oceans is another example in which the dynamic changes are relatively rapid and in which very large areas need to be observed continually or repeatedly. Satellite applications seem clearly to have promise of high usefulness.

Systems for observation of changes which take place over longer periods of time, or in more limited areas, require more careful consideration of estimated economic returns. For example, observations of forests and farmlands during the growing season, hydrologic observations which affect that growing season or which impact on power generation, etc., reflect changes occurring on time scales of perhaps weeks and months. In this case, other observation techniques could be and have been used (such as aircraft) and the determination of the comparative economic potential of satellites then requires more precise analysis.

Applications to geology and earth resource surveys, such as determination of the type, nature, and distribution of various soils over very wide areas, can be, and have been, conducted usefully over relatively long time periods. The phenomena to be observed change inappreciably with time. While there is merit to rapid observational results, the actual time urgency of the data is surely different from the previous cases. In this case, comparison between satellite observation and ground or aircraft surveys requires even more care: the potential returns from space vehicles relative to other methods of observation are a matter of some uncertainty and controversy. Here it seems safest to assume that several observational methods will remain in use. The complexity of comparisons in this kind of case is exemplified by the preparation of the detailed world soils map under United Nations auspices, representing some 3,000 man-years of work over about a 10-year period. Would a space program have done as well on an equivalent investment?

Nevertheless, several conclusions seem clear. In the aggregate, the returns on investment from space observations can total many billions of dollars, provided the user community for these observations is properly prepared to exploit the information from space, which is an extremely critical point. Second, many space applications are or shortly will become cost effective compared with other technical capabilities, particularly in those applications which, because of the dynamic phenomena to be observed, must be conducted over very large areas in very short times. The comparison becomes less clear cut as the allowable observation time becomes longer and as the area observed becomes smaller. Even in these cases, however, arguments for the efficiency of the space application become convincing as the lifetime or reliability of the spacecraft increases substantially beyond that now generally experienced. This, in turn, implies that an extraordinarily high return can be obtained from advanced development programs that increase the useful life of such observation vehicles from weeks or months to

years. Likewise, development of very cheap launch vehicles and payloads would also be a highly rewarding avenue for advanced development.

Third, one should expect that in some cases satellite applications will at best complement other observation options, and not replace them; but that even here the "tying together" capabilities of the satellite and its ability to fill in or update other observations may prove a valuable adjunct—provided the satellite is cheap to operate.

The unique advantage of satellites which are used as communications devices, and such applications as navigation, geodesy, and transport control, is simply its altitude, which allows it to be in simultaneous contact with many widely dispersed geographic points. That aspect of satellite operation is unique and such satellites provide essential services which can only with great difficulty, if at all, be provided by other techniques.

Here the advantages of the space system lie in the fact that for some cost, which we try to minimize, we can realize essentially unique highly useful services. Developments which emphasize very long lifetimes and/or inexpensive methods for launching the spacecraft again promise great benefits.

In the near future one can anticipate further exploitation of these satellite communications means, in concert with other communication means. An exciting area is the upcoming use of satellite communications (as one among other means for information exchange) to serve specific social ends, an example being the use of satellites to transmit instructional television and radio.

Another kind of program (again prospectively including satellite channels among other means for information exchange) specifically keyed to clearly apparent societal benefits is exemplified by the conceptual planning underway for a biomedical communication network as part of the development of the Lister Hill National Center for Biomedical Communications, under the aegis of the National Library of Medicine associated with the NIH and the DHEW. This network is to serve the medical community, broadly defined, with an ultimate goal of enhancing the health services available to society. This program also reflects the paramount importance of observing a fundamental principle of dual emphasis in developing innovative applications programs in general—the necessary, strong, interactive, and concurrent emphasis during development both on the component of technology exploitation, and on the component of preparation and participation of the user community. The technology component helps shape the realizable expectations of the user, and thereby bolsters his confidence in, and acceptance of, the innovations available through the technology. The simultaneous component of user participation helps select and focus on the preferred technology options which provide the most effective user service.

Application satellites also offer an excellent opportunity for broadening the areas of international cooperation in which the United States, through NASA, is already quite active. NASA has entered into cooperative arrangements with more than 80 nations, involving such activities as tracking of space vehicles, launching of international scientific satellites, flight of foreign experiments on NASA satellites, and data analysis. Proposals have been made to develop an interna-

tional applications satellite project, and to divide responsibility between the European Space Research Organization and NASA on major projects and scientific investigations.

Some work has been done on cooperative United States-U.S.S.R. space programs in the fields of meteorology, magnetic field mapping, communications, and space biology, and medicine. While the output has been small so far, it is a basis for new cooperative space programs with the U.S.S.R. as relationships between the two countries improve.

Cooperation with countries other than the Soviet Union has not eliminated unilateral space activities there. Europe maintains indigenous organizations for launcher development and space research, and a strong interest in telecommunications satellites, quite distinct from U.S. activities and the U.S. proposals to assist these countries. That NASA could not fully satisfy the needs or ambitions of friendly countries for space services was to be anticipated, because to those countries national pride and the belief in the value of indigenous space activities as spurs for developing research and technology are important factors, even though costs are threateningly high. In addition, some uneasiness about the predominant strength of the United States in activities such as those involving the Communications Satellite Corporation has led to new interests in developing unilateral capabilities in some countries and groups of countries.

Nevertheless, the international arrangements which NASA has made for space activities is an excellent and important example of the ability of NASA to meet the objectives of the 1958 Space Act.

International cooperation in application satellites conceivably could broaden these international cooperative arrangements much further. A number of applications discussions took place during the United Nations Conference on the Exploration and Peaceful Uses of Outer Space, held in Vienna in August 1968. The conference provided an excellent example of both the possibilities for, and some of the potential risks in, international space activities.

The possibilities inherent in the application of space systems to nations which are less developed than the United States and the Soviet Union are clearly great, and considerable interest was aroused in examining the benefits derivable from space programs and the availability of opportunities for international cooperation in space projects, particularly in such fields as communications, meteorological, and earth resource survey satellites.

The potential risks involved illustrate to a magnified degree some problems the United States faces in defining new applications for its own satellites. The benefits to be derived in many of these applications depend very sensitively on the ability of the final user to exploit the data from the space systems. This promises to be a significant problem for the United States itself in the near future. But for the United States there is clearly an excellent opportunity to resolve these problems during the next decade.

For many other nations, on the other hand, there is grave danger of promising more than can be delivered. Unless careful precautions are taken, an ironic situation could arise in which the relative benefits from application satellites were greater for the United States than for less developed countries—where the need was greater. But at the very least, there are now many nations aware of the basic potential for space applications. These nations, with U.S. assistance, can prepare

themselves to exploit such applications to understand how to evaluate such programs for their own particular case, and to determine where financial and training aid could be available.

Overall, the possibilities for international uses of application satellites as well as for U.S. domestic uses are certainly strong enough and attractive enough to warrant significant expansion of planned effort in these fields, with the exercise of particular attention to the user's requirements, so that he is able to exploit the space data. Attention to the exploitation needs seems to be lagging behind the current and anticipated availability of sensor hardware and results from the space vehicles, even in the United States. International cooperative arrangements are particularly sensitive to these exploitation needs, but NASA is completely prepared to cope with that circumstance. The prospects of multinational use of such satellites can also be expected to draw sharper attention to the political and legal sensitivities implicit in multinational global survey devices. In particular, the prospects could cause a closer examination of the international policy guidance which was only rather casually noted in the 1958 Space Act.

In any event, the development of applications satellite systems offers a highly promising and attractive area for substantially increased attention in the next decade of the national space program. The area therefore warrants a significant increase in relative emphasis. Depending on the development progress, this could become a major component of the space program.

Finally, there is one other major possibility for international cooperative arrangements which could be both an exciting and desirable employment of space capabilities by NASA. The entire range of advanced developments implicit in a serious program for application satellites would also be directly useful in the development of satellite systems to assist in arms control arrangements.

This is a longer range possibility which could be contemplated only if and when there were major progress toward an international agreement on arms moderation. But should that happen, it appears that NASA could find a highly rewarding mission in developing and operating arms control satellites under the appropriate international agreements. Such a mission would be a singularly fruitful combination of the basic technological capabilities of NASA and its vigorous attempts to develop international cooperative ventures in space.

To recapitulate this extended discussion of how well we have achieved the objectives of the 1958 Space Act in the three major categories noted, it seems safe to conclude that these objectives of the act have been met to only a partial or rather limited extent in the current decade. However, the next decade provides clearly identifiable opportunities for much fuller realization of those objectives—objectives relevant to national security, new technology, science and discovery, practical civil applications, and international cooperative efforts.

Realization of those objectives will require careful and deliberate choices from among all the new program options now open to us. But the central point is that our space-associated technology is now rich enough to permit a conscious selection of programs able, in the next decade, to achieve those objectives to a far greater extent.

VI. A SUMMARY APPRECIATION : SPACE PROGRAM POLICY

With a manned lunar landing attempt imminent, an era is ending. It is time to discuss what the next decade of space enterprise should seek to accomplish. We have the occasion to conduct the postponed debate of the early 1960's, on the purposes and goals of the national space program. Whatever implicit national priorities the space program had in its formative years, national concerns, national goals, and national emphasis have all changed significantly during the last decade—and the space program needs to be reviewed against these changes. Extended discussion by the Congress and by the public, objectively reassessing and reevaluating the purposes of and possible returns from space activities, are warranted. Preliminary but detailed analysis of programs and resources should be stressed, even while inevitably emotional aspects of space activities are present. The backdrop for the discussions should be the recognition, as a plain and hard fact, of the continuing national commitment to space activities for both civilian and national security purposes.

Whether concerns for other pressing national issues will obscure the need for this debate, or limit its scope, is not clear. But whereas there have been extended and evident discussions of national security issues during the last decade, there has been no correspondingly painstaking discussion of the national space program. Now is the time to begin it. The coupling between the expectations of the public concerning the space program and congressional decisions by fund allocation needs to be improved and strengthened.

The 1958 Space Act still provides general guidance for our space activities; it is not clear that substantial reshaping of the act is either desirable or necessary. But keeping in mind the implications of that act, we must, as a minimum, acknowledge that the next decade will probably force harder decisions on the space program than in the past. Some partial intermingling of NASA and DOD space activities may well be desirable, or even inevitable, and its probable consequences should be very carefully evaluated; and the implications of space activities for international cooperative arrangements during the next decade could force more attention to those international aspects.

The central issue at stake in any debate on future U.S. space activities arises from the implications of the two most critical alternative decisions. One such decision would be to reestablish a very major mission goal as a focus for the next decade, something comparable to that provided by the Apollo mission during the current decade. The other such decision (one favored in this paper) would be to focus on a more balanced program, for at least the early years of the next decade, thus initially emphasizing multiyear plans for a number of advanced developments encouraging extensions of science and technology along many fronts. Highly visible appeal characterizes the first choice, with a concomitant need for an overriding priority to reach the goal. The second choice may, in the long run, be of considerably more benefit to the Nation because it would probably create a new fund of valuable scientific and technological knowledge. But to adopt it, and carry it to success, would require correspondingly more careful planning and more detailed control.

At the least, a debate on our space goals for the next decade would very likely produce a generally acceptable description of the broad

priority to be placed on space activities as compared to other national priorities. The informal decisionmaking mechanism which establishes priorities by the end process of funding allocations will still make the final choice. For the space program this decisionmaking mechanism has operated in a more desultory and narrow way than it need. The budget review processes and the fund allocation processes now view the space program in unnecessarily disparate ways. But that decision-making mechanism can be enhanced by consideration of alternate space program plans feasible of accomplishment within a range of space expenditures, and with the run-out cost implications fully displayed by multiyear cost projections. These several plans could then be weighed by extended public discussions, including the voices of Congress, the voices of the scientific community, and the appropriate managerial and administrative voices, in an iterative process which relates plans, goals, and expenditures—and other national needs.

If the decision is made to select some goal other than a single major mission as a focus for the next decade's space activities, then a cohesive multiyear plan relating a number of distinct space activities, with their proposed goals and cost streams explicitly spelled out, would logically become the vehicle by which Congress could periodically review and update its space priorities and emphasis. Presentation to and acceptance by the Congress of such multiyear, phased plans would probably require more care and precision on the part of all the involved institutions, and perhaps heightened organizational attention to plan preparation and plan approval than has been required in the past.

The overall funding levels resulting from reevaluation of the space program cannot realistically be governed by simplistic general rules. The lessons of the spending projections at the start of this decade should convince us of that. Then, informed public and private estimates of total space spending projected for this decade varied by an order of magnitude, as did projections for annual spending rate at the end of the current decade. Simple formulas for setting spending levels would not seem to have much application to a space program defined in terms of desired results. Thus, it is unreasonable to arbitrarily set space spending at some prespecified fraction of the gross national product; nor can we set some preemptory funding level and assert that it is warranted by the essentiality of retaining preeminence and leadership in space activities.

The funding levels for the national space program, including both the civil and national security components, should be those established by careful analysis of needs and goals, by careful consideration of individual activities and their costs, and by the outcome of an informed discussion of the implications of the resulting program—implications for science and discovery, for costs and resource expenditures, for applications, for national security, and for prospective but genuine technological gains. Underlying this debate should be the understanding that, whatever its other aspects, spending for a space program is already an effective mechanism for wide diffusion of economic benefits.

A great variation in the range of average annual expenditure for space activities during the next decade is unlikely, whatever the outcome of a national debate on space program goals and resources. A proposal to limit NASA to a modest unmanned satellite and space probe program, achievable at levels of perhaps \$1–\$1.5 billion a year,

would be as unlikely of acceptance as an early commitment to manned interplanetary exploration that implied an average annual expenditure of \$7-\$10 billion during the next decade.⁵ Lying between these extremes are many significant and important alternative civil space program missions that could be carried out at average annual expenditure rates of between \$2.5 and \$5 billion. A program operated at the upper level, for example, would permit the concurrent rapid conduct of both manned lunar explorations and major earth space station programs. The lower level of annual spending on space would force much harder choices.

In the end, it would not be too surprising if a base program of diverse activities in the civil space sector, averaging over the next decade annual expenditures in a range of somewhere between \$2.5 and \$4 billion, turned out to be a quite readily justified, publicly acceptable compromise between cost and comprehensiveness of the civil space program, in the context of other national needs, and considering the existence of a continuing military space program. While expenditures anywhere in this range would strike some as disappointingly low, such steady-state expenditures are very substantial now that major capital facilities and other resources have largely been provided for. The upper end of this expenditure range would reflect the attainment of a high degree of public commitment, mirroring a carefully reasoned plan prepared with a unified organizational dedication.

In addition, this base program would desirably be supplemented by other space activities whose justification is of a different kind. One such activity would be a dedicated prosecution of applications systems (not only spacecraft), drawing on all appropriate national resources. Expenditures on programs that plausibly promise an economic return on investment by socially beneficial applications appear justifiable as add-ons, increasing the average annual expenditure rate for the base program. Another special kind of add-on possibility (for the largest launch vehicles) is mentioned later.

The military space program would continue to be judged, as in the past, largely on the basis of its support of military operations and well-recognized national security objectives; its funding for this support would not be expected to depart in any very major way from the recent trend. But it seems timely to consider the possibilities of some intermingling of NASA and DOD activities, of the kind and for the reasons discussed in section V. Otherwise, it may turn out that the resources of one or both programs are not adequately utilized.

The dominant general considerations, then, appear to be:

No major legislative revision of existing space legislation seems necessary or is clearly desirable.

Interest in the definition of a space policy for the next decade should be high enough to support a public clarification of the issues through a broad general discussion of goals, but there is no assurance that such a debate will occur automatically. Some stimulation of such a debate may be necessary.

⁵ For the sake of specificity, all funding levels in this and subsequent discussions are assumed to be in terms of 1969 dollars. Year-by-year, actual spending in the next decade would reflect slight progressive changes due to inflation under this assumption.

The present space program, in both its civil and military aspects, is being capably and effectively managed, and is producing precisely those results demanded a decade ago.

In terms of fully definable assignments for the foreseeable future, one or both of the civil and the military space program resource base levels may be somewhat larger than is entirely necessary, encouraging some intermingling of these programs.

The need for maintaining as a continuing, viable national resource a space program base on which any of several conceivable future major enterprises may be built is self-apparent.

Beyond these general considerations lies the problem of defining some specific activities which might be included in a national space program during the next decade.

Elaborate discussion of specific program features, options, and time phasing is not the purpose of this paper. The composition of the national space program as a whole, considering both its military and civil aspects, would be highly complex in its technical details, and would be very sensitive to total funding allocations and to the amount of intermingling of some aspects of the military and civil programs. Existing space organizations are best equipped to treat these matters. But a desirable general program could include the following considerations:

1. A decision to select several mission-relevant, advanced-capability development activities in the next decade as focuses for emphasis, instead of focusing in a single mission goal of overriding priority.

2. Within that program, placing relatively more emphasis on applications system development and exploitation; emphasizing the scientific content of the program by providing for proportionately greater support of deep-space exploration and discovery missions; placing additional emphasis on development of long-life subsystems; and placing greater emphasis on a specific, large-scale, major capability development—production of a class of cheap boosters applicable to several uses. There are enough technological avenues for developing cheap boosters to make a hardware prototype competition among the several development possibilities an interesting strategy. Shared sponsoring of the competition and development by NASA and DOD would be plausible.

Such activities, which can usefully be accorded relatively more emphasis in the next decade, might require accommodation with any new manned follow-ons not already underway and/or readily accessible within the next few years. The accommodation should probably be made at the expense of a major new start on a manned follow-on, if the budget constraints are such as to require this in the near term.

3. In manned follow-ons to Apollo, several options can be analyzed now. The long-term implications of such options clearly require very careful and detailed technical assessment; a single option, or several options together, could be pursued. These options include extension of the manned lunar exploration capabilities; continuation or expansion of the Apollo applications programs as now conceived; a follow-on major space station undertaking, leading to a large, multiman, semipermanently operable and inhabitable station; and some intermingling, for at least a significant part of the next decade, of manned orbiting capabilities developments already underway in NASA and

DOD. This last option would, of course, still allow separate and distinct NASA and DOD flights.

One relatively simple decision might be to undertake no new major space station commitments beyond those available via the Apollo application program (AAP) plans and Manned Orbiting Laboratory (MOL) plans, or through some combination of objectives and operations in these two development paths, until one or both of two critical factors become clearer. One factor is the prospective availability of cheap launch systems which also have alternative uses as shuttle, fueling, assembly, and replenishment craft for very large space stations or departure stations. A second factor would be acquisition of more objective knowledge about the true dimensions of the necessity for manned involvement in a number of space activities (recognizing, of course, that some possible manned space activities can involve man's presence for extended times). The first factor would be illuminated by the advanced development program for cheap launch vehicles. The second factor can be illuminated adequately, if not to everyone's complete satisfaction, by experimental possibilities already inherent and exploitable in AAP plans and MOL plans, or in an intermingling of these plans. By the second half of the next decade, these factors could lead to a program encompassing major, manned space station developments, or to one of several other possibilities.

It is already clear that there are good reasons for continuing a substantial program of manned space activities into the foreseeable future. Therefore, it is useful to note that not all of the options mentioned above would be foreclosed even at the lowest probable support level for the civil space program, although some capabilities would have to be forgone.

4. A fourth feature of a reasonable and responsible program for the next decade would be to maintain production of the very largest NASA booster by either continuing production at a modest level, following on after completion of the currently funded program, or by a surge of more rapid production and stocking during the next few years. The availability of some limited number of these boosters (and some associated spacecraft) would provide a continuing and enduring capability to undertake the manned follow-on options just noted, as well as some possible unmanned space missions demanding unusual performance. Stockpiling would also protect against the undesirable aspects of giving up an existing capability by default as a new Soviet superbooster—said by NASA to be under development—becomes available and begins to achieve missions. It would seem sensible and reasonable to decide now to continue production of the largest NASA booster until perhaps 10 to 20 of them became available, in addition to those already funded, as a capability reserve.

This decision should be frankly viewed as a primary hedge against the possibility of major destabilizing Soviet undertakings, even if few specific plans for using the NASA booster can yet be detailed. In any case, the decision would lessen the possibility of an unduly chaotic transition period as the reassessment of the national space program progressed and its future course emerged.

5. In addition to some of the intermingling possibilities for the NASA and DOD space programs already suggested, a general review could profitably be conducted along additional lines noted in section

V. This review would have to consider the consequences of time-phased program intermingling over an extended period. That is, reasonable intermingling would almost certainly require a transition period spanning several years if it is to be a responsible strategy.

The implications of a general program encompassing some of the suggestions of the five previous items would make it highly desirable to take some actions and make some decisions in time for fiscal year 1971 program reviews (for example, in the case of continuing launch vehicle production); in addition, should a program of this sort be seriously considered, it would probably be useful to review the compatibility of such a general program with the range and types of options associated with the already disclosed fiscal year 1970 NASA program and budget proposals.

VII. NEXT STEPS FOR ANALYSIS IN DETERMINING SPACE PROGRAM POLICY

The previous discussion of this paper has concentrated on some of the decisions needed to validate or reformulate our national space policy for the next decade. Two general aspects of the decision process have already been emphasized: (a) the need for continued development of cost and schedule estimation procedures, to display multiyear cost streams for accomplishment of individual space projects proposed to be undertaken. Availability of improved procedures will be highly useful and important, no matter what the character or composition of the national space program is. (b) The desirability of an extended public discussion to help shape space program policy for the next decade. As has been noted previously, key issues in such a discussion are neither purely scientific nor purely budgetary; the space program is of particular importance precisely because it intersects so many nationally important issues and interests. To lend focus for this effort, starting points for such a discussion could be consideration of a range of alternative proposals for space programs, formulated by those key organizations and individuals responsible for planning, execution, or review of various portions of the national space program.

To support and complement such a public discussion (a discussion which, as observed previously, may require some stimulation), concurrent analysis efforts could be highly useful. Some of these analyses would be more qualitative ones, providing background and context for consideration of the national space program; other analyses would be quite quantitative, and in the spirit of the PPB system within which NASA and DOD are now asked to plan and propose their programs. We can order these analyses in several categories:

A. National policy issue questions, in which questions of space policy play an important role.

Here it is probably useful to begin with a note of caution on the intensely interesting question of space versus nonspace programs. One can ask: "Judged by some priority ranking of national efforts, and constrained by fixed budget levels, could some of the current expenditures for space be more beneficially assigned elsewhere?" The quick and honest answer to this is: "Possibly—but we can't really be sure." One reason for this is that there is no such national priority ranking, as remarked earlier, and no evident way of generating one. Hence there

is no readily apparent public policy mechanism which would permit such a judgment to be quantitatively and unequivocally made to everyone's satisfaction. In the absence of such a ranking, it is necessarily uncertain, for example, that \$1 billion transferred from space to urban renewal produces a net societal "gain" for the United States as a whole, any more than it is certain that a transfer of \$1 billion from farm income stabilization to education, or a transfer of \$1 billion from ground transportation to economic opportunity programs, or a transfer of one-half the funding of high-energy physics to ocean farming, would produce a net societal "gain." All present claimants for Federal funds can aver some urgent legitimacy to their claims; in each case, a substantial public constituency is involved. In fact, neither economic theory, nor any other technique for making value judgments, is currently capable of measuring net societal "gains" in such transfers, although such "gains" would be claimed to be obvious by some public sector in each case (these same circumstances make it equally, if not more, difficult, of course, to argue that transfers *to* the space program will produce net societal "gains").

Accordingly, it would probably be unwise to expect that, as a general rule, one could bolster the qualitative and informal judgments, provided through informed intuition and experience and already at work in comparing space and nonspace expenditures, by significant or wholly convincing quantitative arguments. That is, one would likely have to be content with plausibility arguments in answer to questions of whether there is a "natural" budget level for the space program, or a budget level for the space program which does not "unduly compromise" attacks on more clearly socially relevant national concerns, etc.

There is, however, one question of considerable potential background interest in any discussion of space versus nonspace programs which is susceptible to rather quantitative analysis. The space industry is highly labor intensive—there is a heavy use of manpower relative to the cost of the product, and there are undoubtedly rather substantial secondary employment effects on other industries. A ratio of the order of \$13,000 to \$15,000 in governmental spending per employed worker, and in turn the secondary impacts of the subsequent spending, would seem already to represent a rather good diffusion of economic benefits throughout the civil sector—an economic flow which it might be difficult to improve on. It appears possible to trace through this flow in some detail and to compare it with several other spending inputs into the national economy, to get a measure of the relative performance of space spending vis-a-vis other governmental expenditures in terms of the breadth of the impact of such expenditures in the national economy.

B. *General questions of space policy.*

Here there are at least three questions of choice which have both a strong operational flavor and a substantial economic impact. In these three questions a significant amount of quantitative analysis can be undertaken.

1. We have argued for a long-term commitment to a national space program, once the basic character of the program has been decided on. A long-term commitment not only permits much more effective planning, but is also an explicit recognition that enhancing the returns, in

an economic sense, from the space program requires a protracted and broadly based effort extending over many years which considers many factors not directly related to the space program per se.

Many of the most difficult problems involved in making this come about lie not so much in the aerospace devices and techniques themselves, as in insuring that the relevant civil or governmental sector can capitalize on or effectively exploit the available knowledge and information. This implies that studies of the economic impact of the space program must consider the *total mechanism* by which a potential user of space derived information can directly and efficiently relate the information to his needs, and in turn implies a considerable broadening of the economic return studies performed to date.

Also, whether or not technological fallout can have been a central justification for investment in space enterprises in the past, there seems every reason to believe that careful studies will indicate that a different and more directly useful technological fallout would likely follow after development of space systems that more powerfully emphasized long-term cost-effectiveness considerations, rather than performance effectiveness alone (as has generally been the case in the past).

In short, support for a long-term commitment to a national space program could be powerfully influenced by the availability of studies such as those indicated.

2. We have emphasized previously at some length that study of the operational and economic aspects of some degree of program mingling of the NASA and DOD space programs seems highly desirable. Here we will merely reiterate that such a study is both highly important and largely susceptible to quantitative analysis.

3. It is obviously difficult to judge the probability of some active space program cooperation of significant magnitude between the United States and the U.S.S.R. during the next decade. Nevertheless, it would seem useful to study in broad outline the possibilities for enlargement of space experiment capabilities and/or cost reductions inherent in several different assumptions of the level of such a shared space effort.

C. Questions of space policy which impact strongly on science and technology, or on general R&D strategy.

Here we are considering primarily two major questions internal to the space program. These questions will necessarily involve considerable components of qualitative judgments, and are not fully susceptible to rigorous quantitative argument. Further, there will be many shades of opinion on these questions.

We have argued two points in this paper:

1. That there would most likely be a net long-term advantage, in the breadth and scope of the space capabilities ultimately achieved, as well as in the indirect technological impacts of the space program, to a space program structured around several major development activities (but developments which are mission relevant), rather than one which chooses as a focus a single major mission. We have noted earlier some of the development courses which appear attractive. It would seem appropriate to study in some depth the advanced developments now accessible to our space program, since it appears that our technology is sufficiently mature to provide many interesting future development opportunities.

2. That choice of a program putting more emphasis on selection of advanced development activities will also enhance the overall scientific and exploration returns from the space program. This seems intuitively eminently plausible (if for no other reason than that such a strategy decreases the pressures to compensate for the inevitable cost and schedule perturbations experienced by most major programs by conversion of "secondary" program resources to support the "primary" program), but this conclusion could be analyzed in more convincing detail by studies drawing on other historical experience.

It would perhaps be an interesting study in this area to explore the possibility of structuring a total program which has a mix of both advanced development activities and a major mission goal (but with considerably more relative emphasis on the former and substantially less relative emphasis on the latter, contrasted with the comparable non-Apollo and Apollo-oriented programs of the current decade), since the proponents of the single major mission approach will argue, with some justice, that a single major mission goal provides a strong focus for a concerted organization thrust.

D. Questions regarding the direct economic returns from space activities, and the developmental and organizational implications of steps to realize such returns most effectively.

Many of the important questions in this area are even now susceptible to rather quantitative analyses via techniques already employed in PPB studies, and the attainable precision of such analyses will surely increase pronouncedly in the near future. Several kinds of specific studies seem desirable:

1. Studies which determine how best to enhance further those space applications which have already achieved some significant operational capability. For example, if agriculture is to benefit in an important way from the global meteorology provided by satellites, an expanded meteorological satellite program will be essential; and other vital elements—not yet provided for—include the ability, using the satellite inputs, to furnish accurate multiweek forecasts by a suitable prediction model, and the capability to produce and disseminate such forecasts to the user, the farmer, quickly and effectively—perhaps on a daily basis. Such considerations suggest that, among other things, an important part of each and all of the economic benefit studies will be to determine to what extent the space program should subsidize the development of the user's exploitation capabilities.

2. Studies which will help choose among the potential future applications activities in some order of priority. One method for doing this is that described in section V—to assess possible space systems in terms of user requirements, and to place priority on those systems which either meet such requirements in a unique way, or which promise "crossovers" (in terms of costs to achieve given performance or benefits compared with alternative techniques to meet the user's requirements) in the relatively near term or within relatively accessible technology.

3. Studies which identify and relate both the user's detailed requirements and the cost (in a general sense, the present user resources which the user would be willing to transfer to space applications, or the additional resources which the user would be willing to utilize to exploit new capabilities from space applications) to meet those re-

quirements. Requirements need always to be measured against the costs to meet them. For the space systems themselves, these studies would provide guidelines for the technical boundary conditions (e.g., in earth survey systems studies one would produce guidelines on resolution, timeliness, lifetime, etc.), as well as for the R. & D. costs and schedule.

4. Studies which describe the mechanism by which the user's steady state demands can best be satisfied. The preceding three kinds of studies will define and identify, at least in a preliminary way, the following:

(a) An ordered list of space applications which it makes sense to develop.

(b) The *total system* involved (e.g., including that part of the system which permits the user to apply the space-derived information to his direct requirements).

(c) The costs which must be borne by R. & D. programs, and the costs which the user should be willing to assume, based on either replacement at a lower cost of existing user techniques or the user willingness to assume new costs to achieve hitherto unavailable capabilities.

(d) The schedule of effort required to make the space applications self-sustaining (i.e., what has to be done to induce identifiable users to assume the steady state operating costs of the particular space systems involved).

With the information of (a) through (d) available, it will be possible to consider the operating arrangements preferred to manage any particular space applications program. It would be assumed that the necessary space R. & D. would in general be undertaken by the existing space organizations. Differences in operating arrangements could well result from different kinds of users (e.g., arrangements by civil airlines to utilize air traffic control and navigation satellites could presumably substantially differ from those employed by ACDA to utilize any prospective satellite arms monitoring systems).

Several kinds of space applications operating arrangements are conceivable:

Operation by an existing governmental agency (e.g., as the weather bureau now does).

Operation by a semipublic organization (e.g., COMSAT).

Operation by an existing commercial contractor (as is in effect done in some cases where the Government is the user).

A flexible operating arrangement, where the system is developed in a turnkey fashion, with the intention of transferring operating responsibility to an organization other than the R. & D. organization, but where the selection of the operating organization can be deferred initially.

In general, there would seem to be merit in ultimately maximizing to the practicable, legally allowable extent the role of the private sector in operating applications systems for the benefit of the private and governmental sectors. Space applications systems can most plausibly be considered to have come of age, in an economic sense, when they can be viewed more as commercial enterprises displaying sound returns on investment rather than primarily as exotic and glamorous technical tours-de-force.

SOME POLICY ISSUES IN THE ANALYSIS OF RESEARCH AND DEVELOPMENT PROGRAMS

BY WADE P. SEWELL

Wade P. Sewell is an Economic Consultant with Econometric Associates of Alexandria, Va.

Meaningful analysis of research and development (R. & D.) programs is more difficult than that for other types of programs mainly because of the larger role played by uncertainty. Indeed, Mr. Sewell argues that R. & D. program analysis must view the program output as information useful for reducing the uncertainty prior to a procurement decision. He concludes that " * * * the usual arsenal of cost-benefit or systems analysis tools are applicable to R. & D. programs, subject to perhaps more than the customary difficulties and caveats." and also that " * * * the planning philosophy underlying PPB is, with appropriate modifications, nicely suited to the planning and management of R. & D. but current modes of implementation of a PPB system pose a very real possibility of introducing further undesirable rigidities into R. & D. management."

Following a brief discussion and categorization of R. & D. expenditures, Mr. Sewell discusses the sources of uncertainty in R. & D. programs and rational approaches to managing it. Then, largely abstracting from inter-program and interagency effects, he discusses the kind of analysis appropriate to decisions about allocations between R. & D. and procurement and allocations within R. & D. In the same context he proposes a form of incentive contracting that has apparently not been used and argues that it could be helpful in bringing R. & D. contractors' objectives into a closer relationship with the Government's.

Finally, Mr. Sewell notes that retrospective economic analyses of R. & D. have produced illuminating insights but not the beneficial effects resulting from the analyses underlying procurement decisions " * * * among the alternatives cast up by the development process. A similar salutary effect should result from the introduction of systematic economic analysis [of R. & D.] as a component of analyses affecting the menu of alternatives cast up and the methods of producing them;"

Introduction

A reasonable amount of economic research on Federal research and development has been carried out in the past.¹ Although much of it has been policy oriented the impact on policy issues has been small. This is partly due to the fact that the intrinsic uncertainties characterizing research and development make convincing results difficult to obtain from retrospective, comparative studies. Another contributing factor is that little economic analysis has been applied, at least by economists, to the actual choice problems of the development process. Although economic aspects of weapons selection and acquisition prob-

¹ See, for example, T. K. Glennan, Jr., "Research and Development," ch. 15 of *Defense Management*, edited by S. Enke, Englewood Cliffs, N.J., Prentice-Hall, 1967; Carl Kaysen, "Improving the Efficiency of Military Research and Development," *Public Policy*, XII, Graduate School of Public Administration, Harvard University, Boston, Mass., 1964; B. H. Klein, "The Decision Making Problem in Development," and A. W. Marshall and W. H. Meckling, "Predictability of the Cost, Time, and Success of Development," both in *The Rate and Direction of Inventive Activity*, Princeton, N.J., Princeton University Press, 1962; Peck, M. J., and F. M. Sherer, *The Weapons Acquisition Process: An Economic Analysis*, Boston, Division of Research, Graduate School of Business Administration, Harvard University Press, 1962.

lems have been intensively studied in Defense by economists,* there has been little opportunity to conduct current or prospective analyses of the choice of development programs and development program strategies.

The immediate outputs of R. & D. programs are usually public goods (in the technical sense), in large part not appropriable as private property and are produced in response to demands for knowledge, that is, for information usable in reducing and managing uncertainty. That these problems, as well as the more standard economic allocation problems appear, suggests that economic analysis could prove a valuable tool in this area of large Federal expenditures.

Following some introductory background material, the sources and management of uncertainty in R. & D. are briefly analyzed. Then, some fragmentary evidence and analysis, suggestive of trends in some major allocation issues, is presented. Lastly, two recommendations, one cautionary and one positive, in keeping with the modest and tentative character of the paper are made.

BACKGROUND

Federal expenditures for research and development (R. & D.) rose from \$3.15 billion in fiscal year 1954 to about \$17.25 billion in 1969. In 1969, research expenditures totaled about \$6 billion. During the same period, R. & D. expenditures by the Department of Defense rose from \$2.5 billion to \$8.3 billion while its share of total R. & D. outlays declined almost uninterruptedly from 80 percent to less than 50 percent. The most dramatic change, of course, was in NASA's increase from \$90 million (3 percent) to \$4.6 billion (27 percent).²

Although the purposes and scope of R. & D. vary greatly among the departments, there are substantial similarities between them in the issues presented by R. & D. program analysis. At least the characteristics that differentiate R. & D. from other programs are essentially the same. For this reason, and because Defense presents the whole spectrum of issues and the richest body of experience (or lore), most of the discussion will concern R. & D. in a Defense context.

One convenient way of characterizing the spectrum of R. & D. activities is by remoteness from final product. At one extreme the dominant feature is the search for knowledge; that is, research or the attempt to enlarge the scientific base from which it is hoped useful applications will eventually flow. At the other, the aim is to complete the final steps that will permit production of a rather well-specified product for which an actual demand is felt or anticipated shortly.

² These expenditure data appear, for example, in *Budget of the United States, Fiscal Year 1969: Special Analyses*, app. J.

*Further discussion of this issue is found in the papers by Enthoven, and Enthoven & Smith in this volume.

Within this spectrum the following categories are specified in Defense.³

1. *Research*.—Investigation of basic physical or behavioral phenomena, usually not involving extensive experimental hardware.

2. *Exploratory Development*.—Sophisticated applied research and the design of experimental hardware to test new principles and ideas.

3. *Advanced Development*.—Development of hardware for experimental and developmental tests to explore the practicability of component designs.

4. *Engineering Development*.—Design and test components and systems for service use prior to a production decision.

5. *Operational-systems development*.—Production planning, designs for operational systems products support, et cetera, for system approved for production.

As development passes through these stages it progresses through a sequence of intermediate product toward the final product. At each successive stage more information is available about the desirability and feasibility of achieving a final product with a specified benefit. Of course, this is the distinguishing feature of R. & D. programs; they provide, at a cost, information making the benefits and costs of prospective operational programs more predictable. In this respect, R. & D. programs reduce uncertainty sequentially in a fashion essentially like that of sequential inspection of commodity lots where the decision to make further inspections can be better and better guided as more inspections are made. Indeed, in a benefit-cost framework the benefit from R. & D. is the value of the information obtained or, alternatively, of the reduction in uncertainty; or, in still another equivalent formulation, the benefit is the value of the increased predictability. It is not the value of the final product, although they are derivatively related. After all, decisions to proceed to the final product can be made with more or less information and, consequently, more or less chance of success. *Ex post*, of course, the value of particular development programs can range from very great to very small but these outcomes are not known with certainty when *ex ante* plans are formulated. In other words, while most program policy analysis must deal with some degree of uncertainty, in R. & D. programs the uncertainty is so great that its reduction can be regarded as the program objective.

These considerations indicate the usual arsenal of cost-benefit or systems analysis tools are applicable to R. & D. programs, subject to perhaps more than the customary difficulties and caveats. What remains is to identify the important kinds of tradeoffs or allocation problems and to inquire whether a PPB milieu is a congenial one for

³ The descriptions are only intended to be suggestive sketches for present purposes and are not the more complete descriptions actually employed.

their resolution. Of course, the predominant role played by uncertainty and the function of R. & D. in reducing uncertainty provide the principal features differentiating analysis in this field from that elsewhere. As a first step it will be convenient to survey somewhat systematically the sources of uncertainty that can lead to the initiation of R. & D. programs.

UNCERTAINTY ABOUT WHAT?*

The production of any commodity or service is affected by uncertainty or unpredictability, to a greater or lesser degree. In some instances, for example, those entailing minor design changes, a modest amount of production planning can yield the necessary information to resolve the uncertainty in choosing the appropriate (least-cost) method of implementing the change. In agricultural production, however, there seems to be an irreducible unpredictability, due to the vagaries of weather conditions, that does not change from year to year. Between these extremes are a wide variety of cases in which the elements of uncertainty can be reduced but only with significant expenditures, over substantial periods of time, in the sequential acquisition, organization and utilization of information. These are the cases involving research and development activities. Such activities range, of course, from product improvement programs through the extensive engineering development necessary for some complex projects that are very close to immediate application and, from that point, shade off through development programs leading to prototypes quite far from immediate application to research programs that add to the stock of knowledge in areas of prospective utility so distant that predictions about concrete applications cannot confidently be made.

There are three major matters that can be surrounded with uncertainty⁴ in a development program: (1) the feasibility of the object of the program; (2) what it will cost; and (3) the uses that will be realized; that is, the actual demands for or benefits from a future operational program's output.⁵

Feasibility.—Naturally, feasibility is a principal *ex ante* concern in development programs. If it were not, costs could be estimated on the basis of feasible methods and the time to operational use would frequently be short enough that the value in use could be estimated well enough that a decision to proceed or not could be made without a development program. *Ex post*, however, it seems that major programs rarely fail on feasibility grounds alone. In major program terminations feasibility considerations are compounded with those of cost and of changes in prospective use. On the other hand, developments that seem infeasible, at some stages, because of difficulties with one component, can become feasible through developments in another. For example, the early ICBM development seemed infeasible at one point

⁴ The traditional distinction between uncertainty and risk is not very useful for the purposes of this paper. Uncertainty will be used as a generic term meaning absence of certainty (which encompasses both of the usual terms, uncertainty and risk) and more precise distinctions drawn as necessary.

⁵ A fuller discussion of these topics can be found in Marshall, A. W. and V. H. Meckling, Predictability of the Cost, Time and Success of Development in *The Rate and Direction of Inventive Activity*, Princeton University Press, Princeton, N.J., 1962.

*Further discussion of this issue is found in the paper by Hirshleifer & Shapiro in vol. 1 of this collection.

because of the guidance accuracy necessary with atomic warheads, but this difficulty was obviated by the development of hydrogen warheads.

Feasibility can, in the later development stages, be usefully thought of in terms of the possibility of achieving, at some cost, an output meeting prescribed specifications. In this sense, the relevant physical and/or social relationships are presumably not changing over time although the specifications can, sometimes should, and, indeed, frequently do change over the course of a development program. Of course, the chances associated with the successful demonstration of feasibility and the development costs depend upon the strategies selected within a program. If a development program is thought of as a sequence of experiments, steps in the development strategy (including termination) can be chosen in the light of accumulating evidence about the relative promise of alternative lines of investigation, the overall chance of success and the costs associated with various strategies.

Feasibility estimates prior to initiation have, of course, to be based on the stock of knowledge then available, that is, on the judgment of those who possess that knowledge. There seems to be some agreement, although solid evidence is scanty, that competent technical people will, at least in some fields, typically prove too optimistic in estimates of feasibility or the time (and cost) to demonstrate feasibility when it is thought that the underlying principles are well understood and overly pessimistic in these respects when more remote possibilities are considered. It is not at all clear whether these consistent patterns in failures of judgment, if they exist, are technical or psychological facts of life, or are influenced by the prevailing institutional milieu and incentive structure or result from a combination of such factors.

Costs. By definition, costs are not completely known when the decision whether or not to undertake a development program is being considered. One object of such a research and development program is the acquisition of cost information and the successive improvement of cost estimates—for the balance of the development program, the investment in a subsequent operating program and operating costs over the life of that program. It is well known that, in the Department of Defense experience, initial costs estimates have often been wide of the mark—and inevitably so. Not quite so understandable is the predominance of underestimates of costs. In fact, in the surveys of programs that have been made average (over programs) realized (usually production) costs have been about three times the average of the initial estimates. There is some difficulty in making a case for a balancing of under- and over-estimates, that is, average realized costs approximately equal to the average of the original estimates, but there are grave difficulties in rationalizing, on uncertainty grounds alone, a bias as large as that observed.

There have been substantial advances in cost-estimating techniques in recent years, at least in the form of more sophisticated methodology. There seems to be little evidence, however, on whether periodic program cost estimates based on the experience accumulated in development programs have improved as well.

Benefit or Utility. The prospective benefits realizable from a development program, if successful, can range from the almost certain to the highly uncertain. For example, the benefits from organ trans-

plant programs or an air pollution abatement program are presumably rather predictable although they do present conceptual difficulties of measurement. However, in the supersonic transport development program there apparently is considerable uncertainty about the magnitude of the demand to be realized as well as the timing of both the demands and the appearance of potential competitors. In this instance, the uncertainty about the time-profile of demand can have, for some discount rates, a significant impact on the value of the benefits to be compared with the time-profile of costs. Lengthy military development programs, like that for an antiballistic missile system, often entail substantial uncertainties about eventual effectiveness because of possible changes in the tactics that may be employed against it, the alterations in other nations' force structure that its existence may induce and changes in the political environment that may be realized before it is available for deployment.*

In a sense, then, benefit uncertainty arises in some instances simply because the future unfolds in an unpredictable way, although the chances associated with any particular realization do not change. Benefit uncertainties are largely independent of feasibility and cost uncertainties in these cases.⁶ In other situations, typified by the ABM example, the probabilities of various future outcomes can change and, indeed, may shift in response to either the existence of the development program or the particular course that it takes.⁷

It should not be forgotten that, although uncertainty about the benefit from a final product can be a motivating factor in the initiation of an R. & D. program, the benefit from R. & D. is not the benefit from the final product. Rather, it is the value of the reduction in uncertainty.

THE MANAGEMENT OF UNCERTAINTY

Uncertainty can be dealt with in two rather different ways. One already stressed is the sequential acquisition and use of information to reduce the degree of uncertainty; that is, to increase predictability. It is also possible, of course, to insure against uncertainty. The way in which these are combined in a given R. & D. program should depend largely on the certainty with which the final product benefits are perceived and the urgency with which they are demanded. For example, in the Polaris and atomic bomb programs there was considerable uncertainty about the success of various technical alternatives but little doubt regarding the existence and urgency of the demand. Insurance against the technical uncertainties was bought by proceeding in key problem areas with the simultaneous exploration of several independent alternatives. Because the demand was relatively certain and urgent, each alternative was pursued on a scale that assured timely success of the program if any one alternative approach succeeded. As an example of insurance against demand uncertainties consider complementing an ABM deployment with the development of systems

⁶ The benefits can depend on the outcomes of other programs that lead to more efficient ways of doing the same job. The displacement of Navaho by the ICBM is an example. But this may have been a case of insurance against uncertainty. Insurance of this kind entails, indeed plans on, a dependence of benefit uncertainty among programs.

⁷ The first situation corresponds approximately to the idealization of a game against nature and the second to a game against an intelligent opponent.

*Further discussion of this issue is found in the papers by Enthoven, and Enthoven & Smith in this volume.

for further defenses against other types of airborne attacks. The demand for additional defense systems would arise, if at all, mainly from shifts in other nations' force structures in response to the ABM deployment. Because these shifts could take a number of forms the nature of the prospective demand is uncertain. In such a case simultaneous development, at a pace just sufficient to offset the defense's leadtime disadvantages, of several alternative types of defense appears to be the most that is justified. This sort of procedure amounts to acquisition, at a cost, of information about the future threat or source of demand. The cost can be chosen as any desired mixture of delay in meeting the threat, if it emerges, and the dollar cost of simultaneous developments to offset the delay. At one extreme no delay maximizes the dollar cost of development. At the other extreme no development maximizes the uncertain cost of delay if it materializes.

From a broader standpoint, the relative importance of the various elements of uncertainty, and therefore of the tactics for managing uncertainty, shift over time. That is to say, new technological developments, generating new demands for additional development, and externally generated demands for developments based on more or less existent technology usually do not appear in a coordinated fashion. Examples, again from defense, are that the major innovations represented by the development of the atomic bomb and the ICBM initiated bursts of related development. On the other hand, external demands for developments related to hardening of military and civilian targets, ABM defenses, warning systems, and the like were generated by ICBM developments in other nations and by a deeper understanding of strategic issues. In a more recent example of the latter kind, international political events made the deterrent effect of strategic missile forces appear less than entirely adequate in dealing with peripheral confrontations. This situation, in turn, led to demands for development of modernized transport and weapons appropriate to nonnuclear wars.

An elementary requirement for dealing with uncertainty is the recognition that failure probabilities are important and that variances and covariances as well as expected values need to be considered. If the chances of failure are ignored, one alternative, indeed any alternative, line of development will usually appear superior to several. When the probabilities of failure, and the costs of failure, are explicitly accounted for, however, the perspective can change. Of course, the value of alternative approaches is diminished if their chances of success are positively correlated.

It does not follow, of course, that development programs cannot be, or should not be, planned. Rather that such a plan is a map for exploration of the unknown rather than a chart of known territory. Indeed, it is possible, in principle, to lay out a plan with many branches at each successive stage with contingent decisions about which branch to choose at each point. The contingent decisions will, naturally, specify the choice to be made given the detailed information accumulated to that point on feasibility, costs, and benefits. Completely comprehensive planning of this sort is costly too, of course, and should be regarded typically as a useful idealization rather than a goal to be realized. Indeed, since an idealized plan requires anticipation of all possible outcomes at each step there often would be uncertainty about the comprehensiveness of the plan.

Therefore, effective planning and management of development requires alertness to the inherent riskiness of the enterprise; that is, to the sources, magnitudes, and relative importance of the possible variances in outcomes. The costs of unsuccessful as well as successful outcomes must be taken into account and ingenuity exercised in devising strategies for insuring against failure. Of course, insurance is not free either nor is the protection provided independent of the comprehensiveness of the insuring strategies and the correlations among them. Also, an environment conducive to appropriate changes in the course of development programs is desirable. Otherwise the effectiveness of the best organization and planning for the use of accumulating information and exploitation of the unexpected will be blunted. These considerations suggest that systematic analysis of choices within R. & D. should prove as fruitful as have analyses of the investment alternatives offered by the results of R. & D. programs.

SOME R. & D. ALLOCATION ISSUES

Allocation issues in R. & D. differ from those in other fields only because of the pervasiveness of uncertainty and the fact that the outputs are intermediate products. In particular, there are external effects of various kinds as is often the case in Federal expenditure programs.

Noteworthy among these are the problems, due partly to the decentralization of R. & D. among Federal agencies, connected with the assessment of the benefits that may accrue from any one program beyond those of value to its own objectives and the comparison of payoff across agency boundaries. These problems are largely the responsibility of the Office of Science and Technology, the President's Scientific Advisory Committee and the Bureau of the Budget. The associated evaluations require substantial elements of political judgment. For that, and other reasons, they receive little attention in this paper.

More technical issues of this nature are also present. Two possibly important ones are the extent of economies of scale in R. & D. and the effects of different institutional arrangements on the value and costs of exchanges of information among projects, agencies and contractors. On these issues only abstract, speculative discussions are currently possible. Issues that can be addressed with somewhat more confidence are those concerning allocation between R. & D. and procurement, between various phases of R. & D. and between Government-conducted and contracted R. & D. These receive the bulk of the attention in what follows.

Allocation Between R. & D. and Procurement.—Consider a case where a more or less well-defined demand is perceived and a development program that may lead to production to fill the demand is considered. How much development, if any, should be undertaken? It is tempting to say, "just enough, and no more, so that the total of development, production and operating costs are minimized." But this is trite, since a major reason for considering a development program is that the way to accomplish this laudable goal is unknown. It does, however, capture the essentials of the desired ex post result although some care in interpretation is necessary. As a highly simplified example suppose that the benefits, if realized, are confidently estimated at the value

6, in money terms, while cost outcomes of 2 or 10 are projected.⁸ If initial estimates for the chance that the benefits and costs will be realized are $6/7$ for both the benefits and the cost of 2 and these probabilities are independent, there are the following possibilities:

<i>Outcome</i>	<i>Probability</i>
-10	1/49
-4	6/49
-2	6/49
4	36/49

If a development program costing 1 unit completely clarifies the costs the postdevelopment possibilities are:

Cost=2		Cost=10
Outcome	Probability	Outcome
3	6/7	-1
-3	1/7	---

The principal effect is that the development program eliminates, at a cost of 1, the uncertainty associated with extremely unfavorable outcomes. There is little change in the average net outcomes.⁹ In the course of development the estimates can be improved by increased understanding of which cost outcome is most likely or by technical advances making one outcome more likely. These effects are difficult to separate, of course, but both contribute to eliminating uncertainty.

An example isolating the effects of feasibility or technical uncertainty is the following. Suppose the known benefits are 10 and the certain costs of two methods of achieving the benefit are 2 and 4 but each have a probability of $1/2$ of proving feasible. Three possibilities are to proceed to production with one or the other or both with the following results:

Cost=2		Cost=4		Both	
Outcome	Probability	Outcome	Probability	Outcome	Probability
-2	1/2	-4	1/2	-6	1/4
8	1/2	6	1/2	4	3/4

On an expected value basis, the alternative with cost equals 2 is superior to the other two, but there is often no reason to combine costs and benefits linearly; indeed, to do so implies that insurance will never be worth while. Consider, then, the possibility of putting both in development, at a cost of 1 each, with the possible outcome that the chance that the alternative with cost equals 4 is raised to three-fourths. The postdevelopment production alternatives are:

⁸ Units can be supplied to produce any scale of project the reader finds convenient.

⁹ Without development the average is 2. With development the average is $2 \frac{1}{7}$ if $C=2$ results and -1 if $C=10$. If these are weighted according to the initial estimates the average outcome is $82/49$ with development. Of course, this isn't the whole story. The figures, before and after development, are estimates and the postdevelopment estimates are likely to be more accurate.

Cost=2		Cost=4		Both	
Outcome	Probability	Outcome	Probability	Outcome	Probability
-2	1/2	-4	1/4	-6	1/8
8	1/2	6	3/4	4	7/8

In this example, none of the alternatives with development exhibits an expected value, when development costs are taken into account, as great as the alternatives without development, although this can happen. Notice, however, that the successful outcome with a three-fourths chance is available at the same expected value in both cases while a higher success probability is attainable with development, although at a lower expected value.¹⁰ But expected values are not the sole criteria for choice. For example, perhaps the Congress and the Department of Defense, and certainly the Navy, would have paid a substantial price, at least on ex post grounds, for a parallel development program to develop an aircraft with F-111B performance.

Of course, anything can be illustrated, and nothing proved, by contrived examples. The above examples only illustrate, on an *ex post* basis, some of the trade-offs between development and production. Decisions, however, are made *ex ante*. But a decision can often be taken to postpone action and collect information, that is one function of R. & D. The other is to hedge, or insure, against uncertainty. Thus, decisions about the allocation between R. & D. and procurement are, ideally, made successively through the development process. In the balance of this section, some bits of evidence on changes in the allocation between R. & D. and procurement are examined.

It is widely believed that there has been a trend in Defense toward fewer and bigger research and development projects. Some fragmentary evidence on this score is at hand.¹¹ For example, the ratio of R. & D. expenditures to procurement expenditures for aircraft rose from roughly 30 percent in the 1950's to nearly 50 percent in the 1960-66 period while the appropriate manufacturing price index increased about 20 percent.¹² This growth trend has recently abated because of the large procurements for the Vietnam war. At the same time, the number of aircraft development program initiations declined from 82 in the 1950-59 period to 23 in 1960-69. Moreover, new programs in the 1960's have more often, relative to the 1950's, been major modifications of existing designs rather than new aircraft developments. The same trends are evident in the separate services.

During roughly the same period the rate of abandonment of major programs declined and the average investment in terminated programs rose. Specifically in 1953-59, 41 major projects, or about 6 per year, were canceled with an average investment of \$96 million while in 1960-65, 20, or three per year, were terminated with an average funding of \$157 million.¹³

¹⁰ This contrast between parallel production, single-item production, and parallel development is a bit different from the usual discussion of parallel development of sub-components within an overall development program. The basic ideas are similar, however.

¹¹ Most of the data in this section are available through the courtesy of Mr. C. J. DiBona and Dr. Arnold Moore of the Center for Naval Analyses. The author, however, assumes all responsibility for interpretation.

¹² These data are adjusted for changes in accounting procedures that took place in 1959.

¹³ Projects with large investments, on the order of \$700 million, were abandoned in both periods.

These facts suggests that in the latter period fewer alternatives have gone into development as hedges against uncertainties about program success and future defense requirements. However, failure costs have apparently not declined although investment in terminated programs is not a completely adequate measure. Casual observation and these facts indicate that it may have become more difficult both to initiate and to cancel programs.

Several hypotheses can be advanced to explain these observations:

(1) Increasing complexity, relative to the state of the art, in development programs.

(2) An increasing ability to make good predictions.

(3) Less specialization in individual weapon systems. This could result in either or both of fewer starts and more procurement per successful development.

(4) The increase in development costs has caused more projects and more redundancy within projects to become less attractive.

(5) A shift from final-product orientation to the search for knowledge or technology oriented in the demand for development.

(6) Less risk aversion in Defense programing than was the case in the 1950's.

There are enough counterexamples to cast substantial doubt on (1) and (2). The third hypothesis fits the observations only if there is a dominant effect of larger development expenditures per project for less specialized systems, or of a downward trend in procurement quantities or prices offsetting the capability for satisfying more varied requirements. Systematic evidence not at hand is necessary to assess the validity of this hypothesis.

Regarding hypothesis (4) it isn't possible, of course, to calculate a unit price for development in a way that makes a direct confrontation of the hypothesis possible. However, experience with the C-5A, F111B and the supersonic transport does not suggest that this hypothesis runs from cause to effect.

The fifth hypothesis is best split in two parts. There almost certainly has been a shift of R. & D. emphasis toward the application-oriented end of the spectrum and, at least in the evidence considered here, a decline in procurement quantity demanded per successful program. The first part will appear again in the next section.

Hypothesis (6) is the sort of motivational-shift hypothesis whose explanatory power in most uses is directly related to the almost impossible difficulties inherent in constructing a test, even in principle. However, it is difficult for a reasonably close observer to be convinced that a shift toward risk preference in this respect took place by conscious design. If it did, it should be a proper subject for public policy discussion.

Another possibility exists. The fact that these trends coincided with the rise of somewhat mechanistic methods, like PERT, for planning development and the later advent of PPB may not be entirely fortuitous. Proposed programs are now often planned and scheduled in considerable detail from the initiation of development through full operational capability at specified levels in the future force structure program. The plans are replete with interlocking completion dates on tasks within the development plan and complex, interdependent, test schedules. If initial predictions prove incorrect revisions are costly

and involve major planning dislocations, and the opportunities for exploiting accumulated information are restricted. Planning of this deterministic nature is indeed admirable in laying out an assembly plant but of dubious value for even the modest ventures into the unknown that most development programs represent. There must be some degree of freedom, of course, in deterministic plans to deal with the unexpected when it occurs; and there is, in cost and schedule slippages.

This state of affairs has a built-in tendency toward heavy advance commitments in funding and to completion schedules. Both probably operate to inhibit changes that would otherwise be desirable in the course of development and to militate against program cancellations by magnifying the consequences of admission of failure.

Allocations Within R. & D. Allocations to activities within the R. & D. spectrum can, of course, be regarded as part of the allocation between R. & D. and procurement but they are worthy of separate discussion. Allocations to technology-oriented activities have outputs of an intermediate nature that are even more difficult to value than application-oriented outputs. Current practice gives recognition to this fact in the form of allocations to technology-oriented projects more on the basis of prospects and with less detailed and less centralized review and supervision.

This practice accords with the belief that informed professionals are more likely in these instances to judge correctly the value of the intermediate output when realized, and of its prospects for realization, than are public administrators. This is almost certainly correct. Even so, professionals exhibit less consensus about prospects than about realized value.

The overall allocation between application and technology-oriented R. & D. should, and probably does, respond to shifts in final-product demands and to greater demands, at the other end, as a consequence of the funding necessary to exploit breakthroughs. This effect is a bit difficult to assess as more costly projects tend to be shifted into funding categories where more detailed review is customary.

Two conflicting tendencies have been at work in affecting this allocation. One is based on the fact that retrospective studies like Project Hindsight tend to find, because of their methodology, that there would have been less waste and more expeditious proceeding to applications if exploratory development had been more application-oriented. The other proceeds from the observation that canceled programs like the B-70 were rich in technical advance and costly in unused production planning, design of supporting systems and the like. The one leads to more combination and centralized direction of exploratory development, the other to procedures designed to insure a high degree of predictability prior to the passage of a systems development into the engineering development and operational systems development stages. There is little evidence that the latter has substantially inhibited the incentives present to shift knotty problems forward in one guise or another. On balance the net result of the two effects has likely been to shift allocations, in fact, toward the applications end of the spectrum.

Relationships Between Government and Private Development Agencies. By and large research is carried out on a grant basis, exploratory development in government laboratories and the more advanced stages in development by contract although there are, natu-

rally, exceptions to this generalization. Such an arrangement is rationalized on the grounds (1) that research talent is found in universities, and (2) that, because of the constraints they face in adjusting to changing levels of effort, Government laboratories should be most heavily involved in work with substantial funding stability.

This sort of allocation of development funds also involves issues of the distribution of risk bearing and of the relative efficiencies of public and private organizations. Consider efficiency and risk considerations as they bear on contractual relationships. Now, there is no way of separating the contribution to a development program failure of the inherent uncertainty effects from those of the contractor's ability and efficiency. This fact suffices to explain the absence of private markets for exchanging development risks. Owing to these capital market imperfections a contractor must rely on his internal resources for self insurance or shift some of the risk to the Government. In the latter case the contracting agency inevitably acquires some of the responsibility for judging the intertwined effects of efficiency and uncertainty. In addition, it is often difficult, even in privately financed ventures, to appropriate the results of development as private property. In the private sector these two effects, capital market imperfections and lack of appropriability, tend to drive the private marginal rate of return on development below both the social rate on development and the private return on alternative expenditures.¹⁴ Typically, the same considerations have produced similar effects in government-sponsored development as well. Thus, for a contractor to take on development there must be some incentive in addition to the return on development. Frequently an important additional return is, of course, the prospect of the higher returns from a production contract. Indeed, if a production contract follows development it is often virtually guaranteed to the winner of the development award.

This means of closing the gap between the private and social marginal rate of return on Government-contracted development has some unattractive features. For example, the contractor's development incentives depend upon his expectations regarding the volume of a follow-on production contract. Clearly this can cause his interests to diverge unnecessarily from those of the contracting agency. It is also in the contractor's interest to shift problems forward to the higher return production phase. A better arrangement would provide a positive motivation for the contractor to contribute constructively to the allocation between R. & D. and procurement. Furthermore, a contractor may not wish to jeopardize a production award by giving full emphasis to all the relevant information resulting from his development efforts. This pernicious effect to have been involved in a recent important example of program difficulties. Indeed, it may be a factor in the observed bias, noted earlier, in cost estimates.

These and similar difficulties could be largely circumvented by a form of incentive contracting that has apparently not been used. Consider, for example, a prototype development competition among qualified concerns for a lump-sum award on the basis of accomplishment of well-defined objectives weighted by known criteria. Suppose also, that losers

¹⁴ Patent arrangements are, of course, a response to a long-standing recognition of these facts. It does not seem so widely recognized that they are an important factor in sensible consideration of alternatives to marketing practices that have been questioned in industries, e.g., pharmaceuticals, that engage in private development.

are to be compensated according to a schedule, up to a fixed amount, based on their own outlays. The award and the compensation schedule would, of course, reflect the sponsoring Government agency's assessment of the uncertainties and the prospective benefits. Contractors' behavior would be governed by their own perception of the risks, and by the completely appropriable award.

Carrying out contracted development in this way eliminates one difficulty by making the return on development depend on the completely appropriable award. The capital market imperfection effects are addressed directly, and made more predictably responsive to Government policy, through the guarantees offered by the compensation schedule. Also important in this connection is the clear establishment, through the outlay choices in relation to the award, of the responsibility for successfully judging and dealing with the mixture of efficiency and uncertainty effects as an internal matter for each of the competing firms. The removal of the appropriability and capital rationing impediments should result in a choice of development outlays so that the return on development would approximate that on production, at least after adjustment for valuable side benefits from development in the form of learning and experience as well as for any remaining differences in risk. This also eliminates, of course, the divergence of private and social return on development. In addition, more competition should result from the removal of financial restrictions on entry for firms not engaged in production. Indeed, this effect could result in a desirable increase in private specialization in development.

It is often argued, with some merit that production and development should be lodged in the same organization because, for example, development should take some account of ease of production and some development experience is valuable in subsequent production. Of course, to the extent that this is true a firm engaged in development should also have a competitive edge in bidding for a subsequent production contract. Additional arguments for joining production and development, and restricting entry into development, are that the government may pay unnecessarily for learning by new entrants and that an increased capital utilization results. Paying for learning could still be at the Government's option, however, in the proposed scheme. In any event some learning is necessary for new employees in old firms and the mobility of development skills partially vitiates the argument as it applies to firms in any case. The capital utilization argument is, of course, dependent on the nature of the economies of scale operating in production. In any case, it is an optimal arrangement for production and development, not just production, that is sought so that advantages and disadvantages to both must be balanced. Naturally, then, no sweeping *a priori* case for the universal applicability or inapplicability of the proposal offered above can be made. Rather, that it presents important advantages that should not be overlooked.

In the arrangement described, the contracting agency's tasks include defining objectives in a fashion that does not inhibit ingenuity in development and carrying out the appropriate trade-off analyses for criteria determination. Clearly, this job is not trivial but it should be performed in any case if the government's objectives are to be controlling. A more subtle analysis than is now required for setting the terms of incentive contracts would almost certainly be necessary. But

this is likely true of any plan to improve the efficacy of incentive contracting.

Quite possibly an arrangement of this nature would introduce enough duplication to increase the cost of successful programs. However, the duplication and the additional competition should reduce the number of failures while the improvement of the incentive structure can contribute as noted to increased predictability about operating program costs and performance. Indeed, the net effect could be a reduction in the overall costs of development and production. Moreover, modifications could be introduced in the form of proposal requirements for entry or allowing firms to select among award and compensation packages or limiting total Government outlays to the competitors as a group. All of these and undoubtedly other modifications could be used in various combinations to control the costs of duplicate developments while retaining the essential incentive features. In any event consideration of contractual development arrangements of this nature is merited by the very real possibility that overall development and production costs could be significantly reduced and the predictability of operating program characteristics increased.

None of the discussion above touches on the appropriate allocation between Government-conducted and contracted development or the related issue of efficiency in Government laboratories. Efficiency is difficult to measure because laboratories are largely sheltered from competitive comparisons, a fact that may also affect efficiency. Real consideration is due, too, to the constraints on laboratory adjustments and to the value of a Government R. & D. capability in some fields. These sorts of questions make a direct confrontation of the Government versus contractor allocation issue difficult. However, an indirect approach is possible. Consider putting Government laboratories on the same footing as contractors in competition under the incentive arrangement sketched above. Contracting agencies could allow different awards and/or compensation schedules to laboratories. Agencies would then have to address directly the worth to them of a Government capability and perhaps pay a subsidy for it. The combined effect on costs of laboratories' efficiencies and their constraints would be revealed. Awards and compensation to laboratories might, unfortunately, have to be returned to the Treasury rather than used for expansion of those Government facilities that proved most valuable. Even in this eventuality the purpose would be partially served if it were known that these measures of competitive performance would be used as guidelines for future appropriations.

RECOMMENDATIONS

Clearly the combination of bits of analysis, warnings of pitfalls, speculation, fragments of evidence, and *obiter dicta* contained in this paper do not constitute the basis of solid analysis necessary for confidently laying down specific recommendations on concrete issues. Two broad conclusions or recommendations do emerge, however.

The first is that a PPB context may not be very suitable for the planning and management of R. & D. To be sure, the suggestion was made that a development plan could usefully be thought of as a contingent plan; that is, with the choice among specified alternative

activities at each stage dependent on the outcomes at prior stages. Any particular realization resulting from decisions about the choices could be cast in a program budget format with time-phased costs and activity levels for program elements. In this sense the plan might be regarded as a set of possible program budgets. The rub is that a major object of development, it has been stressed, is production and timely use of information to reduce uncertainty about appropriate activity levels at successive stages. This means, of course, that advance commitments to a single plan for periods well into the future are undesirable and that procedures for program change approval should be flexible. Experience with PPB to date does not suggest that these desiderata can easily be introduced. In other words, the planning philosophy underlying PPB is, with appropriate modifications, nicely suited to the planning and management of R. & D. but current modes of implementation of a PPB system pose a very real possibility of introducing further undesirable rigidities into R. & D. management.

A second recommendation might consist of compiling a bill of fare for further economic research from the suggestions scattered through this paper and from other sources. This is not a promising exercise, however, since considerable effort has gone into research on research and development. That research has produced illuminating insights but useful concrete results, at least if measured by impact on R. & D. planning and management, have not been forthcoming. One reason for this is clear. Namely, that economists have been very little involved in those analyses that do underly R. & D. planning. Indeed, economic analysis had little impact on cost-benefit studies and what is now called systems analysis either until economists began actively participating in the studies. It can hardly be denied, however, that the introduction of economic analysis of professional quality has had a beneficial effect on the choice among alternatives cast up by the development process. A similar salutary effect should result from the introduction of systematic economic analysis as a component of analyses affecting the menu of alternatives cast up and the methods of producing them; that is to say, the introduction of economic analysis in the choice of development programs and development program strategies.

SECTION C

SOCIAL OVERHEAD EXPENDITURES

ECONOMIC ANALYSIS IN NATURAL RESOURCE PROGRAMS

BY JACK L. KNETSCH

Jack L. Knetsch is Director of the Natural Resources Policy Center and Professor of Economics at The George Washington University.

Benefit-cost and similar analyses have been applied to proposed public expenditures in the natural resources area for over 30 years. It has been argued that the openness and explicitness with which economic analysis has been applied in this area has accounted for the advanced state of methods for measuring economic impacts in this area. In this paper, Professor Knetsch applies the concepts and techniques of benefit-cost analysis to additional public expenditure issues in natural resources programs.

While analysis has been applied to traditional functions of the natural resource agencies, a new set of issues relating to recreation, pollution control, and other environmental concerns is evolving to which explicit economic criteria have not yet been applied. These new areas present a major challenge to policy analysts. Although most public sector action in the natural resources field is a result of market failure, Professor Knetsch asserts that market criteria can be developed and applied to expenditure policy in this area. "Even if the private market is not used to make allocation decisions, it may still be possible to sometimes use the market mechanism to allocate resources, and thus still achieve gains of efficiency inherent in private market transactions."

One of the major problems in undertaking comprehensive analysis of proposed expenditures is the inadequacy of information on the demands for natural resource services. Professor Knetsch appraises the demand studies typically done in the natural resource area and makes suggestions for improving procedures. He points out the problems related to those outputs of public projects which are difficult to quantify and measure. Here, too, he suggests some principles for improving the estimates of economic demands.

Finally, Professor Knetsch addresses himself to the important question of "divergencies between social costs and benefits and those which fall on a single individual." He argues that the purely local or secondary benefits of public investment projects should not be counted in a national efficiency criterion. "To include them * * * is to seriously blunt the basic choice rationalizing purpose of benefit-cost analysis."

Introduction

The public expenditure category encompassing natural resources spending derives its importance not alone by virtue of the large sums of funds devoted to it, but also because of the large market and non-market economic values associated with natural resource development activities.

It is also the case that explicit analysis of the consequences of public actions has probably the most successful history in the natural resources area. Such analysis has without doubt proven to be of immense usefulness in improving public expenditure decisionmaking by directing resources toward efficient uses and eliminating some of the most inefficient. This has particularly been the case in the more traditional natural resources activities such as water development projects to provide flood control, hydropower, navigation, and irrigation.

Whatever problems remain, the demonstrated utility of formal specification of project effects cannot be minimized. Further, the application of analyses and development of techniques and methodology in the natural resources field can provide valuable insights into the issues and policies in other expenditure areas where such techniques have not been as rigorously applied.

It is not the intent here to catalog the myriad of problems relating to natural resources, nor to tabulate the successes and failures of applying program planning and budgeting systems and benefit-cost analyses to natural resource expenditures. Instead, the aim is to focus on but a few of the items which appear to be germane to efforts to improve public expenditure decisions in natural resources programs, especially as related to relevant information and decision tools. More specifically, discussion will deal with: (1) some notions about the changing concerns of natural resources programs; (2) a sketch of how analytical guides for public expenditures bear a resemblance to how markets allocate resources, and how perhaps there may be gains in a greater use of market incentives to set priorities in public provision of resource services; (3) the weakness of much demand information and the consequent effects on rationalizing expenditure decisions; (4) an outline of some problems posed by the increasingly important values associated with natural resources which are largely beyond present means to quantify; and (5) a caveat concerning the varied viewpoints of the results of natural resource programs and the assessment of their effects.

ALTERED CONCERNS FOR RESOURCES POLICY

While expenditure analysis, particularly in the form of benefit-cost analysis, has shown merit in many natural resource areas, the potential for more extensive and serious analysis is even greater. In natural resources, the focus of analysis has been on traditional development activities—construction of dams for irrigation and levees for alleviating flooding, for example. While marked improvements can be made in these areas, others, often involving large values and rapidly changing demands, are becoming equal or even of more pressing importance.* Attention is increasingly called for in analyzing areas where public awareness and activity has, until recently, lagged—such as environmental quality and the provision of more outdoor recreational opportunities to larger numbers of urban residents.

The newer areas of concern in natural resources are far ranging, not limited to resources as traditionally defined, and involve issues often differing from a straightforward Federal investment in construction projects. These include, for example, the large and growing demands for recreational opportunities, municipal and industrial water supplies, waste disposal, pollution control and other environmental concerns, preservation of complex ecosystems, and the minimization of dangers from pesticides. In all of these, each growing in importance, indications are that the issues will not alone involve a simple Federal expenditure to provide the various resource services. Such expenditures will no doubt remain important, but increasingly

*Further discussion of this issue is found in the paper by Krutilla in vol. 1 of this collection.

more attention will be given to other institutional arrangements where traditional benefit-cost analysis will need to be cast in other roles and a greater use made of other explicit decision criteria.

Evidence of change, and a significant opportunity for program budgeting and benefit-cost analysis, is provided by the growth of State and regional expenditures on natural resource projects. While much of this is directly tied to Federal activity and programs, there is little doubt but that the trend of State and local involvement will continue, especially as meaningful State and regional planning activities increase.

Further, in many of the new areas of concern in natural resources, public involvement and spending will include areas in which the consequences and values are difficult to measure; for wilderness preservation, for example. Also, many of the concerns involve, quite apart from projects and their direct effects, questions of spillover or external effects, such as air or water pollution and the impacts of urban sewer and water decisions. In other areas natural resource programs encounter questions of serious uncertainties, as for example in programs of oceanography and desalination.

Another set of issues which is becoming increasingly important encompasses the uses of natural resources in urban and metropolitan areas. The traditional rural bias in natural resource development activities, and in the natural resource literature, is beginning to yield to concern for the very real problems of making urban areas more inhabitable. Natural resource related programs and projects can deal not only with questions of providing open spaces and water and sewer services, but also with shaping the type of urban development that takes place and providing more attractive and healthful environments. It is becoming clear that, while now largely ignored, the principles of economics as applied in other natural resource situations are applicable to metropolitan resource problems. The application may not be as straightforward as in, for example, problems of river basin development. Urban planning decisions usually take place incrementally with each contemplated change or addition influenced by the presence of large fixed assets such as piping systems and treatment plants, rather than in a context of relatively undeveloped river basin. Characteristically, urban activities are also carried out with the existence of many involved agencies, each of which has its own geographic and/or service jurisdiction.

MARKET INDICATORS AND ANALYTICAL ANALYSIS

Most public activity in the natural resource field takes place because of a rejection of the outcome of private market decisions. There may well be good and sufficient reason for such judgment.

Public provision of natural resources services has usually meant that we lose the restraints and incentives provided by a market, substituting instead public decisions to set policies, to design plans and programs, and to allocate resources. Benefit-cost analysis and program budgeting, as well as other devices, have usefulness for these public decisions as substitutes for market indicators.

Even though the public provision of natural resources services does not often use market prices to allocate resources, national economic efficiency objectives remain the announced primary goal of most such

programs. It is usually the failures of various kinds in the private markets that provide main rationale for public involvement to assure a greater efficiency in the use of resources. While keeping national efficiency objectives in project and program expenditures, public policies have enlarged the purposes to be served to include concern for other national objectives such as income redistribution. In some cases such redistributions are disguised in the language of efficiency, although the usual policy to generally not expect beneficiaries to pay the costs of providing the services would often run counter to this.

Putting allocation decisions into a system that is expected to accomplish most of the same objectives of the market, but without the incentives and restraints provided by that institution, imposes considerable strain on the methods of determining which choices best serve the public interest.* Benefit-cost analysis and other techniques are an immense aid, and a greater use of better analysis can more nearly assure better choices, but they are not a complete substitute.

The current provision of natural resource development activities and services has been alleged to have built-in biases, as interest groups surrounding proposals to undertake certain types of natural resource development projects often systematically favor such decisions.¹ These biases stem in large part from the disassociation of the incidence of benefits from that of the cost of the projects and further from the fact that the magnitude of the gain to those individuals favoring the project is large relative to the magnitude of the loss falling on individual taxpayers. It is also generally conceded that the law under which most agencies operate, together with clientele interests, which are often aided by elected officials, nearly assures that a fairly narrow range of alternatives are considered.²

The need, it would seem, is to adhere strictly to improved analytical devices to insure that decisions are more nearly consistent with national welfare and do not sacrifice this for the welfare of individual regions or groups. This need for rigorous analysis to offset demands for projects is increased with most present cost-sharing policies that favor low non-Federal contributions, making these attractive expenditures for local areas. The need is still more the case owing to the organization of most resource development agencies, which encourage many decisions to be made in regional offices. There is, of course, much to be gained with such decentralization and the opportunities for familiarization with local problems. However, part of this gain is offset by the strong tendency for regional officials to identify themselves with local interests and consequently promote projects of sometimes questionable desirability to the nation.**

¹ Emery N. Castle, "Conceptual Issues in the Conduct of Regional Research on the Economics of Water", in *Opportunities for Regional Research on Water Resources Problems* (Agriculture Law Center, Iowa City, Iowa 1968).

² Robert K. Davis, *The Range of Choice in Water Management* (Johns Hopkins Press, Baltimore, 1968). Interests of the development agencies are sometimes indicated, for example, in such things as the award of the Army's outstanding civilian service award for, "recognition of sustained and outstanding contributions to the expansion and improvement of the inland waterways system". National Waterways Conference, Inc., *Newsletter*, Jan. 24, 1969.

*Further discussion of this issue is found in the paper by Freeman in vol. 1 of this collection.

**Further discussion of this issue is found in the paper by Schultze in vol. 1 of this collection.

The absence of any, or of any realistic or fair cost sharing, particularly when this absence serves little other purpose, places a great burden on objective analysis.* A disequilibrium between supply and demand is imposed by greatly underpricing the outputs of resource development projects, giving rise to a large demand relative to limited availability or supply of such projects brought about by constrained budgets. A public agency is, therefore, dependent upon a substitute mechanism and is called on to parallel the allocation objectives of the market, where limited goods with numerous applications are allocated, but in this case where demand far exceeds the supply.³

Even if the private market is not used to make allocation decisions, it may still be possible to sometimes use the market mechanism to allocate resources and thus still achieve gains of efficiency inherent in private market transactions. An example may illustrate both the difficulties and burdens placed on analysis and also the possibilities which may remain for using the market to allocate natural resource activities even though these are provided publicly. Such a situation exists in the current problem involving the selection of ports to take advantage of what may well be significant economies of large cargo vessels and tankers.⁴

If present ports are to accommodate large ships, the facilities will need to be improved dramatically, principally by dredging very deep ship channels. In most, if not all, cases, this can be done only at very large cost, probably dictating that choices for at least initial improvement will need to be made among alternative locations. If alternatives to port improvement are not deemed appropriate, and costs are not borne by the beneficiaries, the ports selected will reap a large windfall benefit.

Consistent with goals and objectives of our economy, the port offering the greatest economic advantage should logically be improved to accommodate the new ships. The selection can be based on careful planning considerations and detailed benefit-cost analysis. While this can result in the most appropriate, that is the most efficient, selection, it places a great burden on planners and their techniques and furthermore encourages other noneconomic and other nonrelevant considerations which may mitigate against the best solution. Even if the best selection is made, the issue of economic equity or fairness remains in that the beneficiaries do not in fact bear much of the cost.

It would seem that these problems may be resolved by basing the selection on market preference, such as might be given by an open bidding scheme involving the potential recipients. Each port community desiring port improvement could compete with the others by pledging payment from the local area to the National Government. The payment would, of course, reflect both the anticipated gain or benefit which each community would receive from the improvement as well as the relative cost that would need to be incurred. In this way, the port which stands to gain the greatest net benefit from the improvement would also be able to bid the highest price.

³ Robert O. Tillman, "Emergence of Black-Market Bureaucracy: Administration Development in the New States," *Public Administration Review*, September/October 1968.

⁴ An informative summary of many of the facets of such port developments is contained in, U.S. Army Corps of Engineers, *Harbor and Port Development, A Problem and An Opportunity*, July 1968.

*Further discussion of this issue is found in the papers by Krutilla and Milliman in vol. 1 of this collection.

The criteria of selection among the ports could be that which minimizes the difference between the actual cost of port improvement and the bid received from the city. As the cost of the construction of the port would be reflected in the criteria, a great incentive is provided for the port to be located in the area in which the greatest increase in national economic benefits is achieved. A port requiring more costly improvements would need to reflect far greater benefits in its bid before it could successfully compete with ports where the cost of improvement is less.

The benefits of the bid approach are that national economic welfare consideration are more nearly assured than by alternative means and that the economic equity or fairness problem is far better handled because the beneficiaries receiving the gains would bear more substantial portions of the costs.

While the example of the port improvement is merely illustrative, other opportunities for improving allocation and establishing development priorities may be provided by the wider use of market indicators. Pricing of water in water deficient regions is surely another case.⁵ Gains in the efficiency of natural resource services provision in this country could no doubt be greatly enhanced by a closer association of benefits received and costs incurred by the beneficiaries of public programs and projects. I would agree with the observation of Fox and Herfindahl that:

"If the direct beneficiaries were required to pay for the services they receive, political support for projects would more accurately reflect their social value. Probably no other single measure would contribute more to the attainment of efficiency in satisfying demands for water services and in decisions such as location that are presently distorted by subsidized prices."⁶

DEMAND INFORMATION*

In spite of the significant progress that has been made in the application of formal analysis to investment policy and management choices in natural resources, a major limitation continues to be the inadequacy of information relating to the demands for natural resource services. This remains the case even in those areas of natural resources development, such as water, where analyses have met with the most success.

Demand information is immensely important as a guide for planning and carrying out natural resource programs. However, all too often the meagerness of meaningful available data and the interpretations made of them give rise to assessments which—when converted into decisions—have needlessly all but guaranteed results that are far from satisfactory.

A primary weakness stems from the predominantly single-purpose nature of demand studies in the natural resources field. Demand studies commonly precede investment projects and programs, but are

⁵ Joe S. Bain, Richard E. Caves, and Julius Margolis, *Northern California's Water Industry* (John Hopkins Press for Resources for the Future, Baltimore, 1966).

⁶ Irving K. Fox and Orris C. Herfindahl, "Attainment of Efficiency in Satisfying Demands for Water Resources", *American Economic Review*, May 1964, p. 205.

*Further discussion of this issue is found in the paper by Margolis in vol. 1 of this collection.

nearly always an assessment of the demand for a single means of accomplishing an objective or providing a service. Thus an assessment of reservoir recreation demand is made rather than that for water related recreation or outdoor recreation; demands for low-flow augmentation are estimated with little attention given to the myriad of substitute measures for quality control; and demands for future acreages of timber become invariant national needs rather than some among other demands to be considered for these resources.

Without making relevant comparisons, alternative means of obtaining objectives and goals are often overlooked, and alternative uses of resources are seldom realistically examined and compared. Consequently, a great deal of uneconomic development may well take place. The procedures tend to assure a minimum of attention to a search for alternative ways of dealing with problems. Examples of attention to but a small segment of the range of choice abound, for instance, in the area of seeking efficient means for dealing with water pollution or ways of providing recreation opportunities. These alternatives are seldom examined when the planning mechanism is based on single-purpose demand projections.

There are often important trade-offs which can be made in attaining various natural resource related purposes. There may, for example, be ranges of substitutions that can be made in coping with outdoor recreation demands—substitutions that could result in more economical use of resources. Developing utility rights-of-ways, or flood planning areas for recreation in urban areas is, for example, often a better alternative than development of a single large unitary park area in the region. There are likewise alternative means for dealing with problems of air and water pollution.⁷

All too often single-purpose projections are made to legitimize increases in the current means of providing a resource service. This is reinforced by interpretation of demand data which almost invariably show that more of the program in question is needed. This outcome tends to become locked into the planning procedure as a result of the constraints on the demand studies which are undertaken. Much of this particular difficulty stems from the single purpose or interest advocacy built into the agency structure that deals with natural resources problems. Single agencies usually provide only a single or but a narrow range of means for dealing with problems, and consequently single-purpose demand studies result which combine both poor data and information with the narrowness of interests of the agency and lead to poor investment planning. Surely, the enthusiasm for providing reservoir-based water recreation is an instance of this.

The problems resulting from the single-purpose nature of most demand assessments are compounded by the naive and mechanical nature of most demand projections. For example, open space, recreation, sewer and water supply planning all make extensive use of nearly invariant standards of one sort or another, usually stated in terms of acres, gallons, or other physical unit per capita.⁸ Typically, demand projections are made by estimating current and future populations, the

⁷ John Haldi, "Applications of Program Budgeting to Environmental Problems", in Morris E. Garnsey and James R. Hibbs (eds.), *Social Sciences and the Environment* (University of Colorado Press, Boulder, 1967).

⁸ This reaches some extreme with at least one instance of a planning study that sets out the number of acres of historic sites to be provided per population unit.

current and future use rates for this population, with the increased use rate multiplied by the increased population taken to be the future demand. Such procedures effectively foreclose many of the options that should be open in planning our environment. Few degrees of freedom remain after given use rates are applied as coefficients to given population projections and a minimum cost engineering solution applied.

Too often the myth persists that we are able to multiply population figures by use rates, call it demand, and use the figures to justify doing about anything we care to in the name of satisfying a "need." While much of such number manipulation occurs, it is economic and planning nonsense to treat the resulting magnitudes seriously as guides for improving the provision of natural resource services.

The commonly followed procedure has the apparent advantages of: appearing correct, with some of the appropriate terms used; it is straightforward and can be easily institutionalized; and almost invariably it yields large numbers. The reality seems to be, however, that the procedures give erroneous planning guides; are largely a waste of effort that preempts the opportunity to undertake more useful studies; and that many alternative ways of dealing with the problem are effectively locked out of consideration.

Too many of the factors which are in reality variable, are assumed to be fixed. Consequently, many of the decisions which we may want to make dealing with problems of natural resources are all but ruled out of possible consideration before the planning effort is really allowed to begin. These procedures not only lock us into continuing the kind of provisions we have made in the past, but effectively ignore the alternative means for dealing with the problem. Further, by making critical decisions on these supposed standards and needs, planners implicitly are with little choice at the end point where they have the maximum information—the procedures are rigged to avoid the decisions we most want to make.

The use of such mechanical projections is particularly pervasive in the provision of urban services. They are also common in river basin plans that are intended to take account of a range of water related services, and in supposedly comprehensive outdoor recreation plans where future recreation activities continue to be projected, almost totally ignoring many of the most important recreational and environmental demands of large segments of the population. Many demand studies are, on the whole, of little value and some are of negative value. Most of the profusion of comprehensive plans and demand appraisals, which abound in the natural resources area, are little more than collections of single-purpose assessments, with a minimum of attention given to the relationships among them or to demands not included.

To do any sort of reasonable planning in many of the resource areas it is simply not enough to know that the demand for the services is increasing. There is usually sufficient evidence to make this kind of growth abundantly clear to all. The important question is, what is to be done about it. For this we need to know far more about the nature of the various kinds of demands. Further, we need to establish far better links between the results of improved demand studies and the investment, management, and policy decisions implied by them. For example, if we knew how use of alternative recreation areas, by num-

ber and by population segment, would respond to varied investment opportunities—that is, if we knew more about the demand—we could presumably choose a more appropriate mix of recreation expenditures, one that would more nearly meet all of the objectives of such programs. Public bodies—at National, State, and local levels—and private individuals need to have the nature of demand for activities and facilities defined in such a way that rational policy and investment decisions can be made.

Demand in natural resources activities has often been used in a somewhat special and, in terms of planning and guidance, fairly misleading way. One example has certainly been provided by the planning efforts in the various river basin plans. Ambiguity enters principally at two points.

The first is that the basic premise of a price-quantity relationship which lies behind the concepts of demand and supply is ignored. This effectively rules out a great deal of flexibility in dealing with various demands; for many of our seemingly unlimited demands for certain resource services are largely just a function of unrealistic pricing and repayment policies.

A further persistent difficulty in demand analysis has been a continued confusion between demand and use or consumption. Use rates are dependent upon both supply and demand factors—that is, the result of prevailing supply and demand conditions. For example, outdoor recreation participation or attendance is determined or influenced by both demand and the availability of supply. The data commonly referred to as demand are rather consumption figures, simply the use of given existing facilities with existing prices.

We should expect that the availability of opportunities has as much to do with certain kinds of natural resource use rates as does demand. This is more than a simple semantic problem. It can cause severe difficulty in dealing with ways to meet the demand. Improper accounting of supply consideration leads, for example, to the assumption that people demand only increasing quantities of what they now have and therefore can perpetuate present imbalances in certain kinds of resource provision. This is very much the case, for instance, in some forms of outdoor recreation. Some areas of the country show far greater population participation rates for given activities, and if this is taken as a demand statement without consideration of the availability of opportunities it could lead to decisions to build even more facilities in areas most adequately served rather than attempting to provide opportunities in deficient areas. Thus as facilities are developed and used, new studies report that more of the same should be built in these same places. Nearly any project or investment may then be “justified,” and investment decisions can be severely warped. An equally serious error is that we may miss completely many important demands for important natural resource products and values.

Without an explicit account taken of the effect of availability of natural resource supplies on the amount or level of use that we observe, these studies can direct planning efforts to wrong conclusions or to irrelevancies and blunt plans and investment policies. Most demand surveys and studies do not provide any means of determining how resource use will respond to changes in supply—and that after

all is the portion on which guidance is needed. As Professor Wantrup has warned:

“Existing projections of land and water use are neither conceptually nor empirically identical with projections of land and water demand. In the first place, use projections do not separate demand and supply conceptually nor statistically. If demand is to serve as a principal of orientation for public land and water policy—that is to help in planning on the supply side—problems of demand and supply need to be separated conceptually and in empirical investigation, variables pertaining to demand must be differentiated from those pertaining to supply.”⁹

NONMARKET VALUES AND MEASUREMENT*

Analysis—program planning, budgeting, and benefit-cost—is essentially an aid in determining efficient allocation and investment of public funds. Comparison of alternative means of achieving given ends is an integral part of such analyses. While a great deal of success has been achieved in certain areas of public expenditure, certain shortcomings remain with respect to others. In many cases the principals for determining gains and losses have not been correct nor defined in as meaningful terms as might be possible. In others the estimates have remained poor.

The problems in the natural resources field have been particularly acute with respect to the management of resources yielding products or gains which have thus far not been susceptible to measurement. Among the difficulties posed by such incommensurables is a danger that the focus in many resource programs may be prejudiced in favor of those products which are more quantifiable at the expense of those which may be as meaningful but less easily measured. Problems both of measurement of value and of making provision for their inclusion in natural resource development considerations are posed in these cases.

In certain instances the market can be used to advantage to aid allocation decisions and overcome some inequities as well. One example is a suggestion to deal with the problem of airport noise and the deleterious effect on urban residential areas.¹⁰ Homeowners suffer losses from noise created by airplanes, and as the courts have been a poor resort for relief to homeowners, the market has been advanced as a mechanism for both quantification of the effect and redress for losses sustained. It was suggested that actual loss to the value of property suffered by homeowners as a result of the location under flight paths of urban airports, fall not on the homeowner but on the airport and air passengers. This could be accomplished by a system of payments from the beneficiaries of air travel to those suffering losses. While compensating losers, such a scheme would provide incentives for better locations of facilities and less noisy operations.

⁹ S. V. Ciriacy-Wantrup, “Conceptual Problems in Projecting the Demand for Land and Water”, Land Economics Institute, *Modern Land Policy* (Urbana, University of Illinois Press, 1960), pp. 41–68.

¹⁰ Charles M. Haar, “Airport Noise and the Suburban Dweller: A Proposed Solution,” *The Appraisal Journal*, October 1968.

*Further discussion of this issue is found in the paper by Margolis in vol. 1 of this collection.

One of the more common means for dealing with the measurement problem is to estimate those effects that lend themselves to quantification and to submit an exhibit of the best definition of other effects, either separately or as a direct portion of project formulation and justification. There is a danger that even though such effects are called to attention, they may receive little weight in comparison to project effects for which more readily calculable values are exhibited. An opposite danger is, of course, that basically unsound projects may be justified on the grounds of "overriding social benefits." There is considerable opportunity in this procedure to substitute vague opinion for fact, and sufficient examples exist to raise serious questions regarding such judgments. Indeed, the current response to some of the Nation's concern with urban problems, poverty, and regional development, has provided handy crutches for supporting natural resource development projects which may be of marginal or no value in dealing with these issues. Opinions of project formulators on the impact of various natural resource development projects and activities on distantly related national issues, though well intended, carry the serious possibility of being highly biased and an excuse for justifying basically unsound projects.

While direct and adequate quantification of the full values associated with many resource uses is currently not feasible, some principals can still be applied directly to the question of relative values in certain cases. Current procedures in some instances, for example, introduce a systematic bias for given types of development, notably in the recreation and environmental fields. Such a predilection arises in connection with the use of Supplement 1 to Senate Document 97 to determine the values of alternative forms of outdoor recreation.¹¹

The procedure currently in use by Federal agencies essentially take the recreation benefit to be the product of the total number of recreation days estimated to occur at a site, multiplied by an unvarying unit recreation-day value of from \$.50 to \$1.50 per day for most forms of recreation or from \$2 to \$6 for specialized forms.

The major difficulty is that this procedure is simply inadequate to reflect major differences in the economic value of alternative recreation opportunities or alternative development of recreation resources. The efficiency criteria for evaluation of the benefits of recreation afforded by alternative development of natural resources is given by the willingness of users to pay for the alternative opportunity rather than do without it and is measured by the area under the appropriate demand curve. The concept of willingness to pay measured by the area under the demand curve provides value data comparable to other price values in the economy and in terms of economic efficiency is an appropriate guide for social choice.

Current practice of benefit analysis deviates significantly from this principal imparting a severe bias in evaluation of alternative kinds of recreation development. This is particularly the case where, for example, comparisons are made between development of flat water recreation as opposed to maintenance of free-flowing streams or in cases of mass use versus wilderness use of recreation areas. The point

¹¹ *Policies, Standards and Procedures in the Formulation, Evaluation, and Review of Plans for Use in Development of Water and Related Land Resources*, 87th Congress, 2d sess., Senate Document 97, approved May 1962; *Evaluation Standards for Primary Outdoor Recreation Benefits*, Supplement Number 1, June 1964.

is that the criteria outlined in supplement 1 to Senate Document 97 does not allow for the vast differences that exist in the shapes of the appropriate demand curves which reflect the differences in willingness to pay on the part of users for different kinds of recreational opportunity.

When the same or even similar unit values are used to estimate the value of recreation development alternatives the official procedure is effectively rigged, for the greatest value among the alternatives must be shown to be associated with the greatest number of people attracted. The evaluation simply reduces to a head count (which is itself usually a poor estimate), whereas quite a different result may be obtained when using the willingness to pay measure based on varying shaped demand curves.

The benefits of alternatives depend upon the shape of the relevant demand curves and cannot accurately be estimated in ignorance of the shape. It may be the case, as has been demonstrated by economists, market researchers, and merchants in many cases, that the demand curves for some forms of recreation are likely to be very flat (elastic over much of its range) especially if many ready substitutes are available, and for other forms of recreation, particularly those without substitutes, demand curves are likely to be far more vertical (inelastic over much of its range).¹² The implication for evaluating alternatives is that forms of recreation which attract smaller numbers of people than other forms may still have an economic value that may even exceed that associated with recreation that caters to larger total numbers of participants. Official methods for evaluating recreation benefits currently in use are simply incapable of indicating this important difference. A more realistic examination of the relative economic values must go beyond the total number of visits that can be expected from the alternatives and examine the likely elasticities or slopes of the demand curves associated with each type of use.

Using basic economic principles, some realistic speculation can be made about the situation surrounding, for example, flat water and free-flowing stream alternatives. If it is established that the recreational opportunities provided by the free-flowing alternatives are relatively unique and rare, that is, that they have few close substitutes, then we can expect that even though the total numbers of visitors making use of this resource for recreational purposes may be quite small, the total value may be relatively large for this number of visitors; alternatively, if substitutes are available for the flat water alternative the opposite would be true. Even though the total visitor use of an area may be numerically greater under a flat water alternative this alone does not insure that the total benefits are greater. Indeed, there is strong reason to suggest that the relative difference in elasticities of the demand curves may often more than compensate for the possible greater use.

It may also be the case in such instances that great differential exists in the changes in demands for each over time. There is, for example, considerable reason to expect that shifts in the demand for more remote forms of recreation may be taking place at a differentially greater rate than for other forms. If such is the case, then resources

¹² See any standard economics text, for example, Donald S. Watson, *Price Theory and Its Uses* (Houghton-Mifflin Co., Boston, Second Edition, 1968), p. 46.

useful for more esoteric pursuits would take on relatively greater value.

These problems of measurement and of how resource services are to be provided are particularly acute in problems of the quality of the natural environment. Such issues are commanding increasing attention because of a heightening awareness of the values involved. Examples of the concerns include increasing use of pesticides; air and water pollution; landscape deterioration; lack of open areas, particularly in older parts of metropolitan regions; and destructions of scenic amenities and estuarine areas.

Marked changes in our society have established new values and patterns for use of natural resources. Complete protection of natural biological communities as well as open space for active outdoor recreation have taken on new importance. An awareness of environmental problems is reflected in general public support for legislation and programs to dedicate wilderness, attack water pollution, develop recreation areas, and reduce taxes on farmland to relieve urban spread. Some support emanates from a concern for health, but much more can be traced simply to the desire to live, work, and play in pleasant surroundings. The result of the changes in our society and in our environment where technological, economic, political, and social changes have all had an accelerating effect has been a rapidly increasing awareness of environmental amenities, their destruction, lack of availability, and a consequent increase in the demands for them.

Most of these environmental goods are not marketed and have no retail price, but they are just as much economic goods because of their scarcity and value as those regularly produced, purchased, and consumed in our economy. As with market goods, the values attached to better environments—pleasant urban and agricultural landscapes, undisturbed bogs, mass recreation beaches—are related to demand and supply.

While the economic values of different uses of important natural resources change, it is not clear that our usual reliance on the market and other institutions is very effective in bringing about corresponding changes in resource use. Environmental quality values are very real, but our society seems poorly prepared to inject these values into the social and economic calculus in ways which are effective in resolving the conflicts of divergent interests centering on these values. There are many reasons for this, principally those relating to externalities, or spillover effects, of resource use and to the nonmarket nature of most of these demands.¹³

Current efforts to deal with these problems have not all been particularly rewarding. Reliance primarily on such devices as total Government purchase in the case of certain types of land areas, zoning to prohibit nonconforming uses, and administrative edicts to prohibit certain types of activities, have a certain number of disadvantages which lead to far less in social payoff than may be obtained from other alternatives. It is in areas such as these that analytical aids will find both difficulties and potentials.

Realistic approaches to many environmental problems take into account the relative supply and demand for the resource products and the incidence of gains and losses resulting from alternative responses.

¹³ See for example: John V. Krutilla, "Conservation Reconsidered," *American Economic Review*, September 1967; Jack L. Knetsch, "Economic Aspects of Environmental Pollution," *Journal of Farm Economics*, December 1966; Henry Jarrett, editor, *Environmental Quality in a Growing Economy* (Johns Hopkins Press, Baltimore, 1966).

Far more information needs to be developed on causes and effects of activities relating to varying aspects of the quality of the natural environment. Public involvement would seem to call for actions differing from traditional resource development roles to a range of programs and policies, and to administrative devices ranging from tax adjustments, installation of public utilities, effluent charges, subsidies and partial purchases to outright acquisition of fee simple titles. There is, in other words, a range of incentives and restraints which can be utilized to encourage resource utilization which more closely approximates the social optimum.

THE VIEWPOINT OF BENEFIT MEASURES

A persistent problem in the rationalization of public expenditures in the natural resources field stems from the varied objectives of the different interests surrounding natural resource expenditures. A primary reason for this is that the cost and gains of contemplated actions are perceived differently depending on the viewpoint of the individual involved.

Each individual and each community reacts very much in accord with the gains and losses as they themselves contemplate them. Questions of projects being in the interest of the general public are often secondary to affected individuals if losers are not compensated nor gainers required to make payment.¹⁴

There may indeed be important divergencies between social costs and benefits and those which fall on a single individual. These discrepancies between the interests of individuals or groups and those of the Nation as a whole can pose limitation on the implementation of natural resource development activities and often place burdens on the analysis of the desirability of such activities. Those who suffer losses as a result of resource development activities have every reason to object to such activities unless adequately compensated. Similarly, gainers who achieve benefit without payment have every reason to favor projects whether the projects are in the national interest or not.

A similar divergence of interest, together with renewed enthusiasm to use natural resource development activities to aid in the solution of a range of social problems, has brought the issue of secondary benefits to renewed importance. This interest has been further stimulated by the recent increase in the discount rate applicable to Federal water resource development projects.* The general assumption among resource development agencies and most economists has been that, to the extent that the primary objective of these expenditures is national economic efficiency, by and large secondary impacts associated with development activities are simply transfers of economic activity from one part of the economy to another, and therefore cancel out in terms of the national accounts. To be sure, an individual region stands to gain a great deal from such economic activity generated as a direct result of a project undertaking. However, the entire economy is not likely to gain at all or possibly only to a very slight extent. While direct evidence is not complete, it overwhelmingly appears that the

¹⁴ Roland N. McKean, "Costs and Benefits From Different Viewpoints," in Howard G. Schaller (ed.), *Public Expenditure Decisions in the Urban Community* (Resources for the Future, Inc., Washington, D.C., 1963).

*Further discussion of this issue is found in the paper by Baumol in vol. 1 of this collection.

great bulk of the secondary impacts are gains which are only regional benefits that "wash out" from a national point of view because of the loss of benefits elsewhere. There may well be reason to secure information on such impacts, but these cannot be construed as national gains. To include them or to associate them with other projected gains from an investment is to seriously blunt the basic choice rationalizing purpose of benefit-cost analysis.

Certain so-called secondary benefits may well represent a form of legitimate benefit to the national economy. For such gains to accrue, however, special circumstances need to be satisfied, such as increased efficiency as a result of economies in a region or employment of previously unemployed and immobile resources. Even when such conditions prevail, there is reason to suggest that the proportion of secondary benefits which are in fact national efficiency gains, is very small.¹⁵

It has often been proposed that secondary benefits, even if only regional gains, ought to be counted because the goal of national economic efficiency is only one of those to be pursued by programs of natural resource development. Others, such as income redistribution and balanced regional growth, are also of concern. Therefore, in the interest of dealing with chronically depressed areas, unemployment and "other social objectives" pressures have increased for other water resource agencies to propose counting secondary impacts or "redevelopment benefits" in project justifications. In the main, such calculations impose gross harm to the objective of increasing national economic well-being. While not denying the relevance of other goals, it appears to be highly questionable whether in fact most natural resource development projects contribute significantly to them, or whether they are efficient means for society to go about dealing with them. Lacking much meaningful evidence in support of the efficiency of such projects to aid in the attainment of these other goals, and the competing demands on public expenditures, it would appear that the possibilities for grossly misallocating resources are large.

CONCLUDING COMMENTS

In general, the role of explicit analysis of the effects of public expenditures in natural resources programs has been impressive. There has been sharp criticism of many of the applications. However, much of this can be expected when analysis has been made as explicit as it has, for example, in determining the costs and benefits of water development expenditures.

There is need for improvement in present applications, but probably even more desirable is the extension of analysis to other natural resources programs and expenditures. This is particularly the case with areas of increasing concern that may well involve ever greater spending.

The information flow is far from that needed—with perhaps as much to be gained from redirecting current efforts as initiating new ones. Though not all nor even many value questions can be completely settled, more careful analysis and dependence on adherence to fairly rigid investment and allocation guides appears to remain immensely useful in the natural resources area.

¹⁵ Evidence on the impact of unemployment is contained in Robert H. Haveman and John V. Krutilla, *Unemployment, Idle Capacity and the Evaluation of Public Expenditures* (Johns Hopkins Press, Baltimore, 1968). See also the paper by Haveman in volume 1, in this collection.

POLICY ANALYSIS IN TRANSPORTATION PROGRAMS

BY JAMES R. NELSON

James R. Nelson is Professor of Economics at Amherst College. Until September 1968, he was Director of the Office of Economics, Office of the Secretary, at the Department of Transportation.

Without question, the comprehensive application of economic policy analysis to transportation policy is a difficult undertaking. Investments in transportation facilities appear to be little different from other private and public investments in that they involve large-scale physical facilities, require valuable inputs, and produce outputs for long periods into the future. However, analysis of alternative investments in this area is especially complicated by the fact that a large portion of the output entails the saving of human time and reduction in the loss of human life. Both of these are notoriously hard outputs to value. More significantly, transportation facilities are intimately related to the environment in which they are placed—not only does the environment determine the demand for transportation services, but the existence of transportation facilities alters the environment and in turn, alters the demand for its own output. Finally, transportation policy in the United States is affected by many institutional and economic constraints which make policy analysis both highly important and most difficult.

In this paper, Professor Nelson deals with the full range of analytical and policy issues in the transportation area. He traces the history of American transportation and demonstrates that it is only in recent decades "that transportation has become * * * amenable to program budgeting." Although the empirical evaluation of investment benefits and costs is now appropriately applied to transportation policy, there are serious problems relating to demand forecasting, the discounting of future effects, the financing of transportation investment, and the evaluation of reductions in travel time and increases in safety.

In dealing with the application of discounting analysis to highway investments, Professor Nelson emphasizes the legislative constraints which affect current Department of Transportation practice.

"This system contains several built-in economic irrationalities. * * * The Department of Transportation appears to be under congressional mandate *not* to attempt the introduction of economic rationality into this and certain other programs. * * * The prohibition on use of economic criteria on grant-in-aid programs immediately rules out their use with respect to two-thirds of the entire budget of the Department of Transportation."

In his paper, Professor Nelson cites those issues on which attention should be focused in developing and improving the economic analysis of policy alternatives in the transportation area. These include demand forecasting and a data base necessary for implementing competent forecasts, multiyear planning and budgeting, and the establishment of schedules of values for travel time, human life and health, and other transportation benefits.

Introduction

This discussion will start with the Pioneer Age of American transportation programs, because transportation is to a special degree the captive of its own history. It will then move on to the changing attributes of the supply of and demand for transportation, and of the environment in which transportation modes provide their services, to show how the idea of program budgeting has become steadily more

relevant to the field of transportation because of the movement of the field toward the concepts of program budgeting as well as because of the development of program budgeting in ways of use for transportation analysis. It will then examine certain general economic problems of policy analysis in transportation—with special emphasis on demand forecasting, choice of appropriate interest rates, and evaluation of human time, human safety, and the interaction between transportation and its environment—and continue on to two problems which are of special importance for transportation policy: Federalism in American Government, and the use of user charges to cover differential fractions of the cost of transportation. Finally, it will relate these to the existence, present budget, and most pressing policy problems of the Department of Transportation.

I. THE PIONEER AGE

Two entirely different sources may be used to illustrate the difficulties which modern policy analysis would have had in the field of transportation if it had been born ahead of its time. The first illustration, in the form of a rhetorical question, comes from two outstanding authorities in the field of program budgeting theory:

... we should recognize that the *attempt* to make decisions more rational and less responsive to bargaining pressures might introduce a conservative bias. Would the transcontinental railroad or the Panama Canal have made the grade in a regime of long-term program budgeting? ¹

The very expression "made the grade" is itself a testimonial to the penetration of the railroad into our general culture in a way that no cost-benefit analysis could measure. And, for inland waterways, the National Waterways Conference, Inc., launches a very similar argument from a very different position:

... A nation of continental expanse required cheap transportation; preservation of the Union and the economic welfare and growth of the Nation demanded an end to sectional rivalries with their attendant burdens on commerce and trade; facility of communication and ease and economy of transport were prerequisites for holding and defending the Trans-Appalachian West against the depredations of foreign powers and the splintering tendencies of the remote frontier. Much the same combination of influences operated to forge the principle of Federal responsibility for waterway improvements.²

Against this soaring rhetoric, an economist's mention of "allocation of resources" would sound mundane indeed. Nor does this passage simply represent an *ex parte* reading of history by the National Waterways Conference; instead it provides a reasonable summary of the views of Henry Clay and Daniel Webster and John C. Calhoun, and of many eminent economic historians. One of the most significant recent contributions to American economic history argues that, even in the granger States which have always been associated most closely

¹ Roland N. McKean and Melvin Anshen, "Limitations, Risks, and Problems," ch. 10 in David Novick (ed.), *Program Budgeting*, Harvard University Press, Cambridge, Mass., 1965, p. 299. Italics in the original.

² William J. and Robert W. Hull, *The Origin and Development of the Waterways Policy of the United States*, National Waterways Conference, Inc., Washington, D.C., 1967, p. 8.

with the importance of railroad transportation and its regulation, natural waterways or canals could have provided outbound routes for most of the agricultural commodities which were in fact largely shipped by rail.³

It will help us to determine when, and why, policy analysis based on modern techniques is relevant if we can first determine why such methods would have been anachronistic in principle as well as in practice at the dawn of modern American transportation.

1. *There is no entirely rational method of forecasting the timing or the impact of a really new technique*—nor the damping or reinforcing waves in the rest of the economy which may be produced by it. This is true even if the technique itself does not change fundamentally: the canal was “invented” in pre-historic times, and the steam train of the 1830’s was not essentially different, in its basic appearance or application of scientific principles, from its descendants of quite recent date. What cannot be predicted with assurance is the response of demand to a really new supply stimulus. To this day, with all the advantages of hindsight, students of transportation tend to under-rate the flexibility of demand response to service improvements—a tendency which may appear in the form of measuring the potential services of minor mid-western rivers only in costs per ton, or in the form of “proof” that trucks cannot compete effectively with railroads beyond a certain limited radius even though the most profitable trucking firms do, in fact, tend to specialize in shipments going far beyond the radius which supposedly represents their outer limit of efficiency. And a really novel form of transportation can scarcely avoid major service improvements while it is still very young.

2. *Transportation in the 19th century was regarded, not just as a handmaiden of the economy, but as a prime instrument of economic development.* The first reason for this was that even men of affairs who would not have understood the analytical concept of costs decreasing with density were perfectly aware of its main implications: (1) transportation contained decreasing-cost elements; (2) these were concentrated, then as now, in connective infrastructure (turnpikes, canals, improved waterways, railroads—beginning late in the 19th century, pipelines); (3) a rapidly expanding economy placed particular strategic value on this one decreasing-cost economic sector which did or could spread across the entire national map; (4) areas which were particularly blessed, geographically or politically, could combine the technical advantages of good transportation with the economic advantages of competitive transportation.

In Europe, in much of Asia, and even in Africa and Latin America, transportation change often reflected the previous relative distribution of population and economic activity at the same time as it stimulated enormous absolute growth. In the United States, transportation change was responsible for both relative and absolute economic growth of particular centers and particular areas.

3. *Transportation, in the United States, was the prime instrument of economic development subject to political control.* Many States had ruined both their treasuries and their credit by premature attempts to capitalize on this fact just before the panic in 1837. The Federal Gov-

³ Robert William Fogel, *Railroads and American Economic Growth*, Johns Hopkins Press, Baltimore, Md., 1964, especially pp. 208–219 and app. A.

ernment had been introduced into the effort with the national road very early in the 19th century, and was to assume major responsibilities in connection with railroad construction in the two decades ending in 1876. Before 1861, American politics was ideological largely to the extent that it was sectional; and even after 1865, when it was not sectional it was often geographical. Appointments to the Interstate Commerce Commission are still judged more in terms of geography than of ideology.

4. *But transportation was viewed as a uniting as well as a divisive factor.* The first important railroad in the United States named its first locomotive the "Best Friend of Charleston"; but the eastern link of the first transcontinental railroad was named the "Union Pacific."

Its construction was in large part a response to the desire to bind the Far West to the rest of the country, and hence to politics.

This type of rationale for Federal encouragement of transportation was by no means confined to the mid-19th century. Right up to the establishment of the Department of Transportation, disinterested students of Government were still talking about the need for a promotional as distinct from a regulatory agency for transportation—as if the main advantage of concentrating Federal responsibility would consist of more transportation, or more demands on the Treasury, or both. And the Interstate Highway program which began in the mid-1950's and is still underway is a far more widespread and generalized monument to national unification than the Union Pacific, or the Panama Canal, or the inland waterways program ever were. As an economist with outstanding qualifications in transportation matters has commented:

... It was obviously highly desirable, if not imperative, that the Interstate Highway program be planned at the Federal level in cooperation with local and State governments to insure that the individual State programs were consistent with one another and created a comprehensive highway system that served interstate and national interests as well as local or intrastate objectives. * * * For this particular transport investment decision, it is difficult to see how cost-utility analyses would have made a major contribution, though more extensive application of such analyses during the preliminary design stages could have been quite productive. Given the complexity of the political and economic decisions involved, and the emphasis on designing a geographically consistent system, it probably would be difficult to improve on the congressional process as a means of developing such a program in an orderly and systematic fashion.⁴

Revolutionary new technologies might revive one phase or another of this fourfold—technological, developmental, sectional, and national—case for elevating transportation problems from the tactical level of policy analysis to the strategic level of national policy debate. But in the meantime it can be asserted that the national significance of *transportation analysis* has increased precisely because the national importance of *new transportation decisions* has in many ways decreased.

⁴ John R. Meyer, "Transportation in the Program Budget," ch. 6 in *Program Budgeting*, *op. cit.*, p. 170.

II. TRANSPORTATION IN ITS PRESENT ECONOMIC FRAMEWORK

The first step in attempting to prove that transportation has become more amenable to program budgeting, and to the kinds of policy analysis related thereto, is to summarize the relevant features of program budgeting. These include: a structure which is functional and "end-product oriented" and related to time periods including the traditional budgetary year but also stretching as far beyond it as is appropriate to the specific program; an analytical process, which is known by various names but which will be described here as cost-benefit analysis; and data systems to provide the information needed to accomplish these functions.⁶

Of the three attributes of end-product orientation, cost-benefit calculability, and data adequacy, the most important at this stage of policy analysis in transportation is that of cost-benefit calculability. The basic change in this area may be inferred from the historical developments sketched in the previous section. But it may be helpful to supplement this chronological treatment:

1. *Creative and reactive approaches to demand.*—The word "need," although often used to describe the wants to which public expenditures are expected to minister, has two defects as an analytical tool. It assumes that there exists some rigid requirement for a minimum quantum of a certain service; and it assumes also that this "need" can be "satisfied." Both of these defects may be summed up in the proposition that an approach to benefits via the concept of "need" is an attempt to apply an absolute standard to an idea which is essentially relative. Above a basic minimum supply, no need is overriding; indeed, it may even be possible in some cases to have too much of a good thing. These comments can be summed up in the familiar economic concept of the demand *curve*, depicting the various quantities of a given good or service that one, many, or all individuals would like to buy at each of all possible relevant prices. The same idea of relativity can be carried over to "benefits." The differences between "demand," in the technical economic sense, and "benefits," in the sense appropriate for Government policy analysis, are twofold. First, the economist's meaning of "demand" assumes the existence of the modifier "effective"—i.e., desire reinforced by purchasing power—while the point to Government provision of certain benefits to some citizens may be precisely the need for redistribution of command over some or all goods to assist those who could not otherwise buy them. Second, even the idea of a market, in which either actual prices are charged or shadow prices are introduced into the calculations, may do violence to the nature of the benefit-conferring process. Defense analysis, for example, has proceeded very far along the lines of cost *effectiveness*, defined to mean ability to achieve a given overall objective, or objectives, at minimum cost. But no level of defense analysis can place a very meaningful price tag on each of all possible degrees of security.

The Government's role in supplying transportation service is generally a good deal closer to the familiar idea of supplying a market demand than is the Government's role in many other sectors of the econ-

⁶ Both the general comments and the specific quotations in this sentence are taken from George A. Steiner, "Problems in Implementing Program Budgeting," ch. 11 of *Program Budgeting*, pp. 310-312. See also the paper by Jack Carlson in vol. 2 of this collection.

omy. With exceptions to be noted in the next section, governments are not expected to engage in massive redistribution of income from rich to poor, or conversely, by transportation taxes or expenditures—although such redistribution may be the more or less inadvertent result of actual government programs. Nor are governments expected to be willing or able to recast the economy, or major segments thereof, on either new geographical or new functional lines by manipulation of transportation budgets.

This last observation must, of course, be surrounded with a number of qualifications. Advocates of rivers and harbors expenditures still paint glowing pictures of the economic changes that improvements in navigability will produce. Moreover, in the localized area of *urban* transportation, talk of “externalities” and “feedbacks” expresses the fact that transportation may have marked effects on its environment and the further fact that some of these effects may interact with the transportation itself to create an interdependence of supply and demand. But the *regional* claims associated with waterways improvements tend to be concentrated on the specialized transport economics of a short list of bulk commodities. In local urban transportation, the emphasis is not on the traditional national goal of transportation promotion to speed and facilitate indefinite expansion, but rather on the problems of harmonization raised by the interplay between transportation and the demand for it.

Thus it is fair to conclude that present emphasis on transportation is no longer in the direction of such transcendent goals as creating a new country, or binding its parts more closely together politically, or facilitating permanent population movements and resettlements. These essentially *creative* functions were those which followed from the 19th-century conditions sketched in the previous section. Their primary objective may not have been the *satisfaction* of demand for transportation so much as its conscious *stimulation*. Today’s general transportation goals are, in the main, a good deal more modest: they involve essentially adaptive reactions to more nearly predictable and more nearly externally conditioned shifts in demand, as well as efforts to exploit each demand curve more fully by lower price or to affect it more or less marginally by alterations in service. Present transportation expenditures may be viewed much more appropriately as responses to exogenous conditions of demand for transportation services, and much less appropriately as the causal factor in economic change throughout the economy, than was true for transportation expenditures a century or more ago.

2. *Effective relationship to a common denominator.*—The source for our discussion of the program budgeting concept in the first paragraph of this section comments, in connection with the analytical process involved (cost-benefit analysis, or its equivalent) that: “It makes comparisons of alternatives from measurements of a common denominator, usually money.”⁶ Conversion of apples and oranges, or of intangibles, into monetary terms must always be a difficult and treacherous operation. But it can only be more so if the original units which must be converted are very dissimilar, and still more so if the various modes of transportation actually or potentially generating these units are growing or declining at very different rates. The problem becomes still

⁶ Steiner, *op. cit.*, p. 311.

more complicated if the Government expenditure which provides the denominator for the cost-benefit equation takes the form of an indirect or concealed subsidy, or even a hypothetical subsidy in the form of a loan guarantee. Finally, to render the problem still more confused, it may not always be possible even to apply the correct algebraic sign to "benefits" for the "public" derived from Government transport regulation.

Many of these confusions are straightening themselves out. The private automobile is overwhelmingly dominant in passenger transportation, with up to 90 percent of all movements except for very long distances. Its leading and most widespread urban competitor, the bus, uses about the same main routes during about the same peak hours. For long-distance transportation, the airplane is moving into a position of almost comparable strength. For freight movements, the motortruck now has a lead in value of transport services rendered over all other modes combined—although this lead is not as great as that enjoyed by the automobile in passenger transportation. So any monetization of benefits must weight benefits to passenger cars, buses, and trucks very heavily. From the cost side, highways and the services related to them account for the heaviest government outlays, by far, of any form of transportation.

The difficult question of setting off the costs and benefits of governments as investors against similar comparisons for government as regulators is also being gradually simplified by the relative decline of regulated transportation. Railroads, which involve intensive Government regulation and very little Government investment, have been losing relative position to all other intercity carriers. Although commercial air transport has one of the highest growth rates of any transport mode, the critical cost-benefit questions involving the Government role in commercial air transport are not those stemming from regulation. The relationship of commercial to general aviation in the use of publicly financed airways and airports, and the proper investment and financing policy for these facilities, are thrusting aside traditional regulatory problems.

So the automobile and the highway provide the greatest weight in the cost-benefit scales, and public investment steadily gains importance as compared with public regulation. Each tendency helps to simplify the basic assumptions required to obtain comparability in transportation cost-benefit analysis.

III. SOME GENERAL ECONOMIC PROBLEMS OF POLICY ANALYSIS IN TRANSPORTATION

The Department of Transportation has summed up its own conception of its mission in a list of four objectives: (1) Economic efficiency; (2) optimal use of environmental resources; (3) safety; and (4) support of other national interests.⁷ The ambitious word "optimal" is later elaborated: "To increase the benefits derived from the preservation and enhancement of the environmental, esthetic, and social factors of transportation."⁸

This concern with the *external relationships* of transportation is

⁷ Department of Transportation, *Goals and Objectives*, May 1968, p. v.

⁸ *Ibid.*, p. 4.

now facing its crucial test in urban areas. So the special problems of urban transport investment will be considered first in this section. The section will then continue to the more general question of demand forecasting for all modes of transportation, for all purposes, in all areas, and for all types of movement. It will then proceed to the special problems of discounting future benefits, together with their relationship to the finance of transport investment, and conclude with a survey of such criteria for transport benefits as the value of travel time and the value of human life and health.

1. *Urban transportation: The interaction between transportation and its environment.*—The special problems of urban transportation can be summed up in the following general observations: (a) Transportation investments not only facilitate movement; they also occupy space and affect the characteristics of the surrounding space; (b) specifically, urban transport facilities do not just “meet needs” or “satisfy demands”; they also create demands at the same time as they redistribute population and economic activity; and (c) cost or benefit measures which may not be inappropriate for intercity transport facilities may be much too general and rough hewn for use within cities; vehicle-miles, passenger-miles, and ton-miles may describe both long-distance transport demands and the general characteristics of the cost of meeting them, but these shorthand measures may conceal enormous differences in urban conditions created by the time, the place, and the quality of transport service.

These points will be amplified in order:

a. *Transportation as a user and reshaper of urban space*

In the United States, railroad transportation often got there first, and may have provided the initial reason for the very existence of a city. Therefore, the prodigal railroad use of urban space could largely be written off as preurban, or even as the precondition for urbanism. In Western Europe and other more densely settled areas, railroads were often not allowed into the central cities as they existed a century ago.

Until after World War II, the use of automobiles in American cities created new congestion without creating any startling new spatial patterns except in areas of exceptional growth, such as southern California. Streets which were already there in horse-and-buggy days could at least accommodate more traffic in a given period of time because the traffic moved faster. The addition of through traffic might depreciate the value of frontages along main thoroughfares for residential purposes; but the new traffic was also likely, at first, to increase the value of these frontages for commercial purposes. The radial traffic patterns set up by public transport before the proliferation of the automobile were preserved more or less intact. The automobile exerted increasing pressure for more urban land. But differences in land use due to the automobile were differences in degree, not in kind.

Urban freeways and expressways have substituted revolution for this evolution. The throughput of cars they permit is vastly greater than anything possible on city streets. So they may actually be economical users of land per vehicle-mile of transport service rendered. But, to be effective, they must constitute a new, superimposed, trans-

portation system, with high minimum width and high minimum distances between entrances and exits. The effect is almost as if railroads had been substituted for streetcar lines. The scarcest of all urban assets—space—is consumed in amounts which may be trivial when compared with the entire area but which require indivisible slices of land, cut off from surroundings, and therefore completely change urban geography. The inevitable negative external effects of transportation—noise, fumes, and so forth—have no offsetting advantages for those who do not have long distances to go to work, do not own automobiles, or do not live near expressway entrances and exits.

Thus the new urban transport cost-benefit comparison cannot be confined to traffic flow. Geographical externalities must be allowed for in any assessment of costs and benefits. The measurement of these externalities is in its infancy. A first major chore for program budgeting in the urban highway area is to develop not only the methodology but the grasp of the urban spatial problem without which the siting of new urban expressways must lead to blind metropolitan power struggles.

b. *Urban transportation as a creator of transportation demand*

For interurban transportation, the old frontier is gone. The main function of new transport facilities serving rural areas or connecting cities through rural areas is to satisfy demands for transportation. Urban transportation, however, is at least as dynamic in its impacts as ever. Perhaps its most important aspects relate to *feedbacks* and *indirect effects*, with special reference to *redistributional effects* in both a geographical and an economic sense.

The first aspect of this dynamic effect of metropolitan transportation change can, in principle, be managed by resort to familiar cost-benefit and forecasting concepts. This aspect has to do with the *service elasticity of demand* for new urban transport facilities. Expressways divert traffic; they also create traffic. In the second case, comparison of new with old transport capacities is not adequate to explain the new supply-demand equilibrium. Until it reaches a fairly advanced level of congestion, the new facility provides better transportation as well as more transportation.

But once this response to better facilities has passed a critical point, the mutual interaction of transport investment and transport use becomes irreversible. In responding to new service possibilities, users of transport services may change the whole pattern of their residences, their work places, and their preferred mode of commutation. Relationships to commercial and shopping centers may be completely re-oriented. The familiar role of mass transportation was to be concentrated along axes of maximum density of industrial, commercial, or residential occupancy, and to increase this very concentration. So pre-automobile facilities tended to accentuate, rather than to modify, the salient characteristics of the area served. But facilities designed to cater to the passenger vehicle may alter the geographical distribution of transportation demand as well as increase its quantitative impact.

In terms of *Federal* transportation planning, this shift has three important corollaries.

The first is that central city and suburbs may have fewer common interests in transportation than ever. Expressways which are designed to provide ready access to central business districts for outer suburbs

may do less than nothing for the residents of center cities; yet the city government may find itself facing two ways because outright opposition to expressway construction might do more to doom the central business district than to provide any kind of assistance for residents in the surrounding slums.

The second is that the possibilities for interaction between the passenger automobile and mass transportation are increased, but without any mechanism for rational planning of the characteristics of this interrelationship. Highway taxes do not usually accrue to local government units. And no State has a mass transportation administration, much less an organization for mass transportation which might be a match for the State highway department in financial and political strength and general influence on State transportation policies. Mass transit authorities do exist which transcend city political boundaries. But they are unusual; they are *ad hoc*; and they are necessarily remote from the interlock between all urban transportation problems and associated problems of urban renewal and the spatial distribution of urban economic activity.

The third influence arises from the fact that metropolitan areas often spread across State lines. The New York-New Jersey and Chicago-northwestern Indiana standard consolidated areas had a 1960 population of 21,553,889, and five additional standard metropolitan statistical areas which extended over State lines and contained over 1 million inhabitants each added a further 10,885,200, for a total population of interstate metropolitan areas with over 1 million inhabitants each of more than half of the grand total population in metropolitan areas of over 1 million.⁹

Thus the need for *Federal* participation and programing in urban transportation resembles the rationale for the original Federal interest in the interstate highway program. Without some outside referee or catalyst, local and State government units may not be able to work out cooperative arrangements for rational areawide programs of transportation investment and interconnection of mass transportation facilities with the private passenger automobile.

This Federal role need not, of course, imply a continuation of the present arrangements for financing different forms of urban transportation. The 90-percent Federal share of the interstate highway program is an open invitation to heavy expenditure on expressways when compared with the Federal share of 50 percent even in other Federal programs. And the present rate of accumulation of the highway trust fund completely dwarfs the amount of Federal funds available for assistance to mass transportation. So the future Federal program with respect to urban transportation involves two separate issues: First, how best to deploy Federal efforts to act as mediator and catalyst? Second, how to obtain and use Federal funds to permit an evenhanded approach to all urban transportation problems, without differential subsidy for any form of transportation—or at least without *accidental* subsidy for any form?

Specific criteria for transportation benefits will be reserved for later consideration. This section is designed to clear the ground by pointing out the *inappropriateness* of global or "averaged" criteria.

⁹ Calculations are from Bureau of the Budget, Office of Statistical Standards, *Standard Metropolitan Statistical Areas*, 1967. The five large interstate metropolitan areas in addition to New York and Chicago were those centered on Philadelphia, St. Louis, Washington, Cincinnati, and Kansas City.

c. *Appropriate measures of urban transport costs and benefits*

The most obvious criterion is far from perfect. This criterion is the passenger-mile or the passenger-vehicle-mile. In urban areas, the *time, direction, place, rapidity, continuity, and external effects* of movement have a great deal more effect on transport investment needed and on the character and quality of metropolitan living than the bare fact of the movement itself. Parking may create constraints, and affect locations and land uses, more than all attributes of movement combined. Vehicle movements, which pay their own way, on the average, throughout a metropolitan area, may be heavily taxed when they occur offpeak or on the outskirts or on conventional city streets, and heavily subsidized when they occur onpeak, in the direction of heavy movement, over the most expensive facilities, and followed by a demand for scarce and expensive parking facilities. Programs for urban transport development cannot rely on averages. They must face the specific supply-and-demand conditions, and the attendant presence or absence of need for subsidy, of the transportation modes for which new Government funds are requested.

All trends in planning for transport investment now point in the same directions: out of the country, into the city; out of the purely esthetic or scenic environmental concern into concern for the total environment; out of stimulation of forms of urban transportation investment which are very demanding of land into forms of investment, or operating practices, which may economize land as well as time; out of self-sufficient transportation cost-benefit forecasting into open-end forecasting which must rely, for its completion, on the pluses and minuses to be contributed from other Government agencies and from other sources, not to mention the reaction between these contributions and that made on behalf of transportation.

To satisfy the new budgeting requirements demanded by this enlargement of emphasis will require not just a more distant budgetary horizon, but a combination of a budgetary horizon which extends far beyond 1 year, a capacity for introducing flexibility into advance planning to allow rapid adaptation to what will always be manifold and shifting problems, and a width of planning effort which transcends any one transport mode or even the needs of transportation itself.

For the first time, planning efforts are now universal in all the larger communities in the country:

By provision of the 1962 Federal Highway Act, we are required to develop in every city of more than 50,000 population, and there are 233 of these at the present time, a comprehensive continuing transportation planning process. This is done cooperatively with the local governments. This process, which is underway in every one of these 233 urban areas at the present time, involves the development of a land-use plan and from that a projection of existing and probably existing future traffic loads. From that we go to a program of projects aimed at satisfying that need * * *.¹⁰

¹⁰ *Department of Transportation Appropriations for 1969, Hearings* * * * Subcommittee of the Committee on Appropriations, House of Representatives, 90th Cong., 2d sess., Washington, D.C., 1968, testimony of Francis C. Turner, Director, Bureau of Public Roads, p. 395.

Planning elements which are not yet universal, or even firmly established, include uniformity in basic methodologies, development of time series of key indicators of transportation demand, postauditing to determine the accuracy of previous forecasting, and the actual versus the anticipated effects of previous planning, the feedback effect on transportation via altered land uses of particular transportation investments, and other aspects of the interaction between transportation and its environment.

The first step in relating local planning activity to the Federal transportation budget has already been taken, with the 1968 request of the Department of Transportation for funds to begin a program of advanced land acquisition for which it already had legal powers.¹¹ The major step, of relating advance planning for transportation land use and advance planning for other land uses, still lies in the future. And it clearly involves solutions more complicated than the old-fashioned one of simply zoning all land abutting on the railroad tracks for industrial and commercial uses, and all land lying along streetcar lines for commercial and multifamily residential use.

2. Transportation demand forecasting

The preceding section purposely avoided a most important question: How might methods now used to forecast urban transportation demands be improved? This avoidance was necessary because of the necessity to consider the problem of urban demand forecasting along with the related but quite different problem of interurban forecasting.

Although urban demand forecasting probably involves the most complicated problems, it has nevertheless been much more generously funded than interurban forecasting, and therefore has made greater progress in several respects. So the main outlines of present forecasting methods for urban demand forecasting will be sketched in, and criticized, before any comments are made about interurban transportation demand.

a. Urban transportation demand forecasting

The first stage of present forecasting methods is to determine what might be termed the macroeconomic parameters conditioning urban transportation demand: economic growth and decline factors, and a population forecast. Then there intervene a number of elements which form the immediate environment of urban transportation: land use, as conditioned by such institutional factors as zoning ordinances; terminal and transfer facilities; transportation facilities; travel patterns; traffic control procedures; and, in the broadest sense, social value systems. The final stage proceeds to the technique of transportation planning: trip generation; modal split; trip distribution; network development, evaluation, and selection; and community value factors.

A critique of this system must concede, at the outset, that it provides a relatively advanced technique both for raising, and for answering, the kinds of questions that must be faced by any forecast, such as: What are the independent variables on which the forecast depends? What are the quantitative relationships between the independent variables and the dependent variables? Its particular virtues are its em-

¹¹ *Ibid.*, Testimony of Francis C. Turner, p. 375.

phasis on the role of demand for transportation as the ultimate justification for all planning, its reliance on information with respect to *origins* and *destinations*, and its connection of this origin-and-destination information with the configuration of economic activity in the area. Many other areas of government would doubtless be overjoyed if they could approach their planning problems with the methodology, or the data, available to urban transportation planners.

The first defect of this approach stems from a familiar defect of the data, which in no case go back very far and in very many cases are collected once only. A forecast is essentially a projected time series; and a first approximation to any such projection ought at least to start with an actual time series extending as far back as similarity of causal factors permits. Therefore the land use and planning studies which have been undertaken in all of the metropolitan areas with over 50,000 population could not, in any case, provide a very extended basis for prediction. This defect could be gradually overcome if the planning process included provision for the generation of certain essential statistics on a recurrent basis. But this has not been done so far. A partial corrective for this absence of time-series data might be cross sectional planning and traffic comparisons. But, in the absence of standard methodology, standard data requirements, standard collection techniques, and standard statistical evaluation, these comparisons would be very hard to make whether or not they were of great probative value.

The second problem with the present approach to urban transportation demand forecasting is not a positive defect but an omission, and an omission, moreover, which cannot be remedied simply by collecting more data more systematically.

This omission has two aspects.

The first is analytical. To assume that the growth in population and economic activity of a metropolitan area has no connection with transportation investment in the area may go a long way toward undermining economic impact studies which purport to measure economic gains from specific transportation investments. For, if the area as a whole gains nothing from the improvement, many of the apparent internal gains may be compensated by dispersed and therefore hidden losses. But, even if population growth and economic activity are assumed to be independent variables in order to simplify the later analysis, it obviously does not follow that the volume of trip generation is independent of transportation facilities available. The same comment can be made of the other sequential stages which lead finally to a transportation demand forecast. *Not only modal split, but total traffic, depend on the quality of transportation available and therefore also, in part, on the quantity of transportation investment.*

The second omission is at a level which combines analysis, policy decision, and basic attributes of citizenship. The problem of feedbacks which has just been discussed is one which can be whittled away by the standard modern recipe: better models, bigger and faster computers, iteration, iteration, and iteration. But the problem of the kind of urban environment we really want cannot be established quantitatively. At this point, we revert to the whole function of PPB as an adjunct to public decisionmaking. The model city, as visualized by each of its citizens, is one which cannot be approached by successive marginal adjustments. *To determine what they want, the citizens must have some rea-*

sonably clear idea of what their options would look like and how they would operate. This, in turn, involves the preparation of a minimum number of feasible alternatives, with the maximum amount of differentiation to allow for the kind of compromise likely to be involved in a final solution, and maximum attention to internal consistency.

b. Interurban transportation demand forecasting

This type of forecasting presents fewer inherent problems than its local or urban counterpart. But it suffers from greater data weaknesses; it is, at present, considerably less glamorous and therefore less likely to receive adequate funds and professional attention to remedy manifest weaknesses; and the *transportation* (as opposed to environmental, or transportation-cum-environmental) target is harder to hit.

The last point is the least obvious. Therefore it will be discussed first.

Vehicle-miles, or some weighted measure for vehicle-miles that allows for the greater size of trucks and buses, cannot directly provide the kind of demand information needed for transportation forecasting which is to be used to establish criteria for *investment*. The important determinant of demand for new investment is peak, or "rush hour," demand. Within urban areas this will generally be determined by the journey to and especially from work. The latter demand, especially, is reinforced by shopping and other nonwork demand for transport facilities. It does not follow, of course, that the peak hours of travel movement and from work are matched by comparable peaks in vehicle movement. The discrepancy is greatest for rush-hour trips to and from the central business district, and in cities large enough to possess reasonably elaborate public transportation systems.¹² Transit systems account for a much large percentage of trips during rush hours than at other times. But passenger automobile commutation still occurs at the automobile peak even to and from most central business districts, transit enterprises are generally themselves in need of some form of public assistance if they are to expand or even to maintain their investment, and the passenger automobile is overwhelmingly important in commutation to and from the rapidly-expanding places of employment on the periphery of most cities. Moreover, both central and peripheral employment tend to be concentrated because of zoning ordinances, large size of certain firms, or for general business convenience. Therefore a forecast of major employment areas will go a long way to determine trip distribution and to provide the preliminary skeleton to which transportation expenditures (on infrastructure or operations) are to be applied.

Interurban movements are quite another matter, and movements between metropolis and country are something else again. The extreme weekend peaking on the latter is notorious, but quite predictable and sometimes so concentrated on a few holidays that there may be little economic justification for expanding facilities to meet exceptional demands. But, aside from certain favorite vacation routes, the "when" of interurban travel involves more complexities than it typically does within cities.

¹² On this point, see J. R. Meyer, J. F. Kain, and M. Wohl. *The Urban Transportation Problem*, Cambridge, Mass., 1966, pp. 84-99 especially table 26, p. 89.

It is at this point that present data fail most signally to provide the help needed for rational interurban demand forecasting. Systematic information on origins and destinations is available only for commercial air line passengers (but not for air freight or general aviation), and, via the 1 percent waybill sample, for rail freight (but not for rail passengers). A great deal of interstate common carrier trucking information which can be made to yield origins and destinations is submitted by individual trucking firms to their rate bureaus, and much of this finds its way into Interstate Commerce Commission dockets. But there is no systematic national collection of common carrier trucking data. For trucking in general, including private and contract carriage, movement of exempt commodities, and intrastate trucking, there is no information available from the carriers covering either origins and destinations or the nature of the commodities transported. The nearest approximation to these data is the information on ton-miles compiled and published by the Bureau of Public Roads.¹³ Since the basic information is obtained at highway weighing stations, the result consists of *flow* data which may be of limited use for investment planning because the data yield only actual movements rather than preferred paths. Finally, with respect to the ubiquitous passenger automobile which is easily dominant in both interurban *passenger* mileage and interurban *road* mileage, information as to origins and destinations is patchy indeed. Here again the data are flow data.

The first broad-gauged attack on these problems was in the Census of Transportation of 1963. But this was conducted with a very limited budget; therefore, unlike other censuses, it involved only very small samples. Additionally, the freight movements recorded were confined to manufactured commodities, and surveyed from the standpoint of shippers rather than carriers.

All of these points reduce to the proposition that the present data base is inadequate to determine the dimensions of actual use of the American transportation system—something which can only be established by systematic information on origins and destinations. For passenger transport, at least some of this empirical inadequacy may be avoided by taking refuge in the formal structure of a gravity model, which can be used to infer origins and destinations from a relatively limited set of observations. But gravity models may be unworkable, or even misleading, for estimation of freight movement. In terms of the total Department of Transportation budget, a reservation of only one-tenth of 1 percent, or the price of between 5 and 10 miles of interurban four-lane superhighway, would revolutionize the whole environment of transportation demand forecasting and hence the degree of rationality in present transport planning and budgeting.¹⁴

3. *Choice of appropriate interest rates in transportation cost-benefit analysis* *

The question of whether to discount future costs and benefits to be anticipated as a result of Government programs is, of course, not a question which is significant for transportation alone. Any Govern-

¹³ For a description of the methods employed, see Alexander French, "Highway Ton-Miles," *Highway Research Record*, No. 82 (1965), pp. 77-93.

¹⁴ For a detailed discussion of the various aspects of the problems involved in interurban freight data, see Herbert O. Whitten (ed.), *Transport Flow Data*, Transportation and Logistics Institutes, School of Business Administration, American University, 1968.

*Further discussion of this issue is found in the paper by Baumol in vol. 1 of this collection.

ment program with a substantial investment component is an obvious candidate for the application of discounting, because the very word "investment" necessarily implies a hiatus in time between outgo or cost and income or benefit. There may be some question as to the application of an overall, standard governmental discount rate in transportation projects; for Government transport investment is closer to the private economy, in nature of benefits conferred and in methods of financing (e.g., user charges instead of general tax receipts) than Government programs for activities such as defense or regulation. But questions of *adjustment* of a general rate to arrive at some special transportation rate are minor compared to the *similarity* of transport investment to other Government (and private) investment with respect to the rationality of discounting procedures.

Therefore it is disconcerting to find that a modal administration of the Department of Transportation, the Bureau of Public Roads, is the first of all Government agencies to be criticized by the Subcommittee on Economy in Government of the Joint Economic Committee:

While substantial progress has been made in instituting analysis in the agencies, some agency personnel resist the application of economic criteria to programs in their departments. The record of the hearings shows, for example, the statement of the Director of the Bureau of Public Roads claiming that the "Bureau of Public Roads does not use discounting techniques in administering the Federal aid and direct Federal highway construction programs. In addition, we do not plan to use discounting techniques in the future." This * * * contradicts the testimony of the other experts that discounting analysis applied to the highway program would be especially useful.¹⁵

This section will attempt to demonstrate that this position of the Director of the Bureau of Public Roads may be attributed, in part, to the present system of Federal aid for highway construction. This system contains several built-in economic irrationalities. Moreover, the Department of Transportation appears to be under Congressional mandate *not* to attempt the introduction of economic rationality into this and certain other programs (e.g., the FAA grant-in-aid program for airports,) whether through the employment of discounting or the use of any other standard economic criteria. Section 7(a) of the act establishing the Department of Transportation instructs the Secretary to "develop and * * * revise standards and criteria consistent with national transportation policies, for the formulation and economic evaluation of all proposals for the investment of Federal funds in transportation facilities or equipment * * *" But section 7 (a) also contains a list of six exceptions to this instruction, the most important of which not only keep the Department of Transportation out of the area of establishing criteria for water resource projects, but also inhibit it from establishing such criteria for grant-in-aid programs.* The highway trust fund is by a very wide margin the

¹⁵ *Economic Analysis of Public Investment Decisions: Interest Rate Policy and Discounting Analysis*, Report of the Subcommittee on Economy in Government, Joint Economic Committee, 90th Cong., 2d sess., Washington, D.C., 1968, p. 8.

*Further discussion of this issue is found in the paper by Achinstein in vol. 1 of this collection.

most important of these programs. The great importance of this exception for the rational conduct of the Department of Transportation is apparent from this summary of the President's budget proposals for the Department for fiscal 1969:¹⁶

Federal funds:	<i>Millions of dollars</i>
Office of the Secretary-----	\$19.3
Coast Guard-----	608.1
Federal Aviation Administration-----	1,227.9
Federal Highway Administration-----	195.9
St. Lawrence Seaway Development Corporation-----	8.6
National Transportation Safety Board-----	4.7
 Total Federal funds-----	 2,092.7
Trust funds-----	4,209.0
 Total DOT expenditure-----	 6,301.7

Thus the prohibition on use of economic criteria on grant-in-aid programs immediately rules out their use with respect to two-thirds of the entire budget of the Department of Transportation.

The use of discounting to assign present values to future costs and benefits is, of course, only one of many criteria to be employed in testing for economic rationality. But, since discounting is absolutely central to a market-oriented, capitalist economy, and since it is also absolutely central to any investment program, this section is not an unreasonable place to sum up the underlying reasons for the irrationality of our present federally aided highway program. These include, first of all, the irrationality with respect to demand forecasting which was discussed in the previous section. They also include the irrationality in division of labor between Federal, State, and local government which has been discussed above and will be discussed again. Moreover, they include possible irrationality in pricing, which is related both to irrationality in demand forecasting and to an imprecise or misleading interpretation of the meaning of user charges. This latter defect will also be discussed later. The present catalog of irrationalities, however, still includes the following:

a. *Trust fund financing*

The trust fund approach to Government finance has one, and only one, clear advantage: it provides an institutional means of assuring users of a governmentally provided service that any user charges they pay for the service will be dedicated to continuing or improving it, and not diverted to general budgetary purposes. This one advantage may be critical in obtaining legislative agreement to user charges in the first place; therefore anyone who believes user charges to be necessary and appropriate in the field of transportation—as the vast majority of economists do—must grant the trust fund technique a high grade at the outset.

The trust fund approach may also have a conditional advantage as a method of financing programs whose needs are bound to grow very rapidly in the foreseeable future (as is true, for example, of most investments and operating expenses relating to air transportation).

¹⁶ *Department of Transportation Appropriations for 1969, Hearings* * * * Subcommittee of the Committee on Appropriations, House of Representatives, 90th Cong., 2d sess., p. 1052.

If the rate of growth of the financial needs of the service is in excess of the interest rate to be used to relate present values to future benefits, the annual receipts of a trust fund could either be hypothecated for future debt service—in which case the required facilities could be expanded at a constant marginal cost-benefit ratio—or the receipts would have to be supplemented from other sources in a pay-as-you-go system. In either case, there could be no danger of overinvestment. The expansion would also be under constant surveillance by the Nation's financial institutions, if expansion were financed through bonds, or by Congress, if the final increment of each year's expansion were financed through the budget. Rational standards could be applied in both situations.

But as an industry approaches maturity, receipts from a trust fund will continue to rise as a function of larger *total* use of facilities while investment needs will rise less rapidly, or even drop, as a function of incremental use. All that is needed to set off a continuing decline in the marginal ratio of benefits to costs is a combination of decelerating growth and growing receipts. At some point in this process, the dead weight of earmarking required by a trust fund will push the benefits from a dollar of new investment below the yields obtainable elsewhere; and every year's over-investment will make the next year's problem of overinvestment even worse.¹⁷

The two transportation modes to which this argument applies are road and air. The former already has a very large trust fund, which is being poured into a maturing industry. If proper data were available, the point at which the Highway Trust Fund would represent an uneconomic diversion from valuable uses of tax receipts (including repeal of the taxes feeding the trust fund) could be calculated directly. In the absence of proper data, it can still be said that the point of low returns is at least being approached. For airway and airport expenditure, however, the outlook well into the future is for a continuation of rapid and dynamic growth. Even here, a trust fund can achieve nothing economically—except to console those who, quite understandably, would like to receive all the airway and airport services they think they need free of charge.

The pay-as-you-go trust fund, of the highway type, is even more inflexible than the trust fund which has the option of incurring debt. If the immediate need for a service is great, pay-as-you-go yields too little. As the growth in use of the service tapers off with approaching maturity, pay-as-you-go yields too much. No business corporation would dream of equating its annual net investment and its undistributed profits. Yet that is the principle underlying a pay-as-you-go trust fund.

¹⁷ A simple illustration may help to clarify the argument developed in the last two paragraphs:

Suppose that both demand for the services of a given type of transportation facilities, and annual investment in these facilities, had been growing at a steady rate of 7 percent per year. Any user charge directly related to demand would also produce revenues growing at 7 percent a year. If the charge was adequate to pay for the first year's expansion, it would be adequate to pay for the last. If it was inadequate to pay for the first year's expansion, its growth would still be rapid enough to pay interest charges on any money borrowed to finance part or even all of the first year's expansion.

Now suppose that this growth abruptly stops at the end of year (Y+10). User charges are still yielding twice as much revenue as they yielded in year Y, because use of the service is not twice as great (any quantity growing at a compound annual rate of 7 percent doubles every 10 years). But the need for *investment*, or new *transportation facilities*, has now fallen to zero. If such investment is made anyway, simply in response to the current availability of the proceeds of user charges, it must steadily yield lower and lower benefits per dollar spent.

b. *Rigid allocation formulas*

The present highway trust fund is distributed in two different ways: just over three-quarters of it, or \$3 billion-plus, goes to finance the Interstate Highway System, and just under one-quarter, or \$1 billion, goes for the so-called A-B-C program of Federal support to rural primary roads, rural secondary roads, and urban routes. This latter program existed before the trust fund was set up in 1956.

The formulas for allocation of the trust fund are rigid in three different ways: (i) The division of funds between the Interstate Highway System, and the A-B-C program, and within categories of the A-B-C program, is determined without any comparison of relative investment benefits or productivities. (ii) The required division of contributions—90 percent Federal and 10 percent State for the Interstate Highway System, and generally 50-50 for the A-B-C program—is identical for all States regardless of the ratios of marginal benefit to marginal cost in each State, and regardless of the ratio of marginal benefit to marginal cost in each type of program. Since the State is spending 10-cent dollars in one case and 50-cent dollars in the other, there may also be an incentive to expand the State's total highway program beyond the limits it would have reached without Federal matching assistance. (iii) The distribution of A-B-C funds among the States is based on formulas including such ingredients as land area, road mileage, and population (rural highways) and towns of over 5,000 population (urban highways) which only indirectly affect *total* road use—and, of course, reflect even more indirectly, if at all, the kind of *incremental* road use which should presumably be the most significant single factor in requiring the kind of expansion of capital plant involved in the process of investment.

Allocation of the trust fund has one very important nonrigid feature, which is the annual allocation of funds to the Interstate Highway System on the basis of the ratio of work yet to be done on the program to the total. When combined with the 90-percent Federal contribution to total costs and in the absence of maximum design standards, this flexible feature provided an invitation to a competitive race for higher cost and quality which was put under only partial control by the introduction of maximum standards in 1960.¹⁸ Some of the increase in expense may be justified by the increased stress on safety and esthetic considerations since the trust fund was set up in 1956. A relatively small part—\$8.25 billion out of a \$28.9 billion increase in the estimated cost between 1955 and 1968—may be accounted for by price increases. A practically identical sum—\$8.36 billion—is charged to mileage increases and increased traffic. The remaining \$12.3 billion includes everything from increased local needs to landscape features.¹⁹

The basic difficulty with the highway trust fund formulas is that they are either *inflexible* (as is true of the A-B-C formulas), or *too flexible at the margin* (as is true of the interstate highway program). This difficulty is compounded by the fact that the program which is too flexible at the margin—the interstate highway program—also pro-

¹⁸ Phillip H. Burch, Jr., *Highway Revenue and Expenditure Policy in the United States*, New Brunswick, N.J., 1962, pp. 246-250.

¹⁹ *Department of Transportation Appropriations Fiscal Year 1969*, H.R. 18188. Subcommittee of the Committee on Appropriations, U.S. Senate, 90th Cong., second sess., Washington, D.C., 1968, testimony of Francis C. Turner, p. 299.

vides 90 cents of Federal money to match every 10 cents of State and local money, whereas the A-B-C matching is normally 50-50. *The combined effect of these weaknesses is to encourage a highway program involving expenditures in the wrong places, at the wrong times, in the wrong quantities, for the wrong purposes, and in the wrong forms.*

Against all this, it has been argued that one of the primary objectives of PPB—planning for periods well beyond the single budgetary years—cannot be achieved without the highway trust fund:

I think the experience that we have had with the highway trust fund since 1956 demonstrates very clearly the advantages of establishing a program, knowing the resources that will be available for it, being able to schedule it several years into the future so that all of the various participants in the program know what is to be expected and they can align their resources accordingly. * * * 20

If this is true, then the PPB cause in the U.S. Government looks hopeless indeed, for it would seem to depend on the establishment of a separate trust fund for every functional category of Government expenditure. But the quotation does underscore the need for careful development of programs requiring far more than a year to carry out, and some method of presenting them for initial discussion and periodic review, *without* being forced into the straitjacket of a trust fund.

4. *The value of travel time, value of human life and health, and other transportation benefit criteria.*

Benefits derivable from improved transportation could be listed to any desired length. This section will not try to be inclusive, or conclusive. It will merely sketch in certain characteristics of a very few important benefit concepts.

But before any detail is attempted, it should be pointed out that transportation capacity in any mode is a term whose meaning may be both flexible and misleading. Airway and airport capacity is ultimately based on safety. Since increasing the throughput obtainable from given physical facilities tends to be associated with increased congestion and hence increased danger, the safety problem usually shows up in the empirical disguise of congestion or waiting time. If investment in airports and in traffic control facilities were freely flexible and freely divisible, four different criteria would yield equal results at the margin. First and second: total value of time lost through stacking or holding, at departure points or elsewhere, should equal the value for time that would have been lost if the flight patterns were compressed, plus the actuarial value of the additional accident danger from this marginal degree of speedup in flight control; conversely, of course, the same rule could be expressed in terms of stretching out total holding time and reducing the value of the actuarial expectation of accidents. In both cases, two different values are being traded off at the margin. Third, each of these marginal values should equal the marginal cost of an additional unit of traffic control—assuming, of course, that traffic control contains adequate technical flexibility to permit continuous marginal changes. Fourth, each of these quantities in turn should equal the marginal cost of providing new airport runway capacity. Thus, if the public is skittish about direct discussion of

²⁰ Department of Transportation Appropriations for 1969, hearings * * * House of Representatives, 90th Cong., second sess., testimony of Lowell K. Bridwell, Federal Highway Administrator, p. 376.

air safety, an optimum program of airports and air control can be *explained* (but not *determined*) in terms of reaching an equality between marginal loss of travel time from addition of another plane to a stack and marginal control costs or marginal airport addition costs, whichever is relevant.

The same general reasoning can be applied to highways. Here the statistical problems are simpler, because accidents come in very large numbers of small-scale episodes in varying degree of severity from mishap to calamity. But the interrelationships between capacity, quality of service, and safety are more complicated. The Interstate Highway System has simultaneously accomplished three things: it has greatly enhanced highway capacity available for interurban trips between cities; it has permitted substantial increases in both maximum and average speed; and it has significantly lowered accidents and deaths per million passenger miles. Once it was decided to build a higher quality national highway system, no widely separated options among these three attributes of capacity, speed, and safety were available.

As the end of the first interstate highway program draws near, the outlook is for less complementarity among the programming goals of highway construction. A new national highway system, enabling proportional speed increases equal to those achieved by the present one, would probably require radical redesign of cars and certainly require radical redesign of their drivers. The once-for-all improvements in safety resulting from highway design features such as controlled access cannot be carried to a still higher power in a new program without altering the whole relationship of the vehicle and its driver to the highway. This leaves straightforward additions to capacity as the major claimant for new express highway funds. And here it should be noted that the correlation between safety and congestion is rather loose. Night traffic accident rates are double those of daytime rates for the same miles of travel even at moderate speeds, and relatively even higher for speeds above 55 miles per hour; the most hazardous period in terms of accidents relative to exposure comes, in fact, between 2 and 4 a.m.²¹ "Alcohol is known to be a factor in at least 50 percent of all fatal crashes, and . . . in 70 percent and more of single vehicle fatal crashes."²² Granted that two inebriates on a highway increase the chances that each will be involved in a collision, most of the accident factors cited above are inversely related, or unrelated, to congestion. More patrolmen or a strict curfew might be a more efficient way to aim directly at safety than the kinds of addition to capacity which in the past allowed much higher speed as a second extra dividend.

a. *The value of travel time* *

Congestion *requires* mutual interference, actual or potential. So, if congestion increases and recklessness does not, the result will be either a decline in maximum speeds or a decline in average speeds. Thus any

²¹ Secretary of Commerce. *The Federal Role in Highway Safety*, 86th Cong., first sess., H. Doc. 93. Washington, 1959, p. 5.

²² *Department of Transportation Appropriations for 1968*, hearings * * * Subcommittee of the Committee on Appropriations, House of Representatives, 90th Cong., first sess., p. 566.

*Further discussion of this issue is found in the paper by Margolis in vol. 1 of this collection.

given trip will take longer, and time will have been lost to congestion. The cost of the congestion can be measured by the value of the time.

The "time" being discussed in this section is not the homogeneous "waiting" of classical interest theory. Every use of time has its own pleasurable or disliked characteristics. The individual is willing to pay to have the pleasurable use of time extended (as in many recreations), or demands payment to stay at a disagreeable chore. Thus the use of wage or salary rates to measure the value of time throughout the day or week is misleading even for those who work for money. If rush hours always came within working hours, then it might plausibly be argued that the value of travel time saved by reducing congestion would equal the summed rates of pay of all those caught in the congestion. But the worst congestion usually occurs out of the working hours of those caught in it, for the obvious reason that the congestion is associated with moves to and from work.

Therefore the easy approach of measuring lost time by the minute and applying a standard money value to each of these minutes is not even a very good first approximation for transportation planning purposes. It skips right over the possibility that the highway or airport system of the future will move toward a characteristic of the automobile or the private airplane of the present—different qualities of service at different prices. (The difference, of course, would be that these different prices would be designed to sort out those who place high values on their travel time from those who "have all the time in the world.") Moreover, the worst congestion from the standpoint of *value* of time lost may not even coincide with the worst congestion from the standpoint of *minutes* lost. Sunday afternoon traffic jams may not provide the bottleneck which justifies expansion of highway or airport capacity, if each person held up in the jam is really not in much of a hurry. In the absence of any uniform rate which can be applied to all situations or even to each level of income, the only answer is to continue the study of traveler behavior. And for this purpose, even if for no other, *tolls* on at least an experimental basis are indispensable.

b. *The value of safety*

This is now being approached within the Department of Transportation from two entirely different directions.

By far the major effort involves most of the budget of the Federal Aviation Administration, as well as an appreciable fraction of the budget of the Federal Highway Administration (part of the budget of the Bureau of Public Roads, plus all the budget of the Bureaus of National Highway Safety and National Traffic Safety). Much of the Coast Guard budget is also allocable to safety—*a priori*, or accident prevention, as well as *a posteriori*, or rescue. The railroad safety program accounts for most of the direct employment of the Federal Railroad Administration. In addition to all this, there is the National Transportation Safety Board. A casual glance at an organization chart of the Department would lead one to believe that safety was its primary, if not practically its only, function.

The other direction of approach to safety does not dominate the organization chart, and has a total budget of well under \$2 million. This is the Department's study of automobile insurance. The particular im-

portance of this is that automobile accidents are the outstanding blot on the transportation safety record, just as automobiles themselves are the dominant form of passenger transportation. The National Safety Council estimates losses of over \$6 billion from automobile accidents in 1966.²³ This is 50 percent higher than the annual receipts for that year of the Highway Trust Fund, and about equal to the entire Department of Transportation budget. Moreover, no other mode of transportation is so dominated by amateur, part-time "pilots," who have no direct job incentives for safety.

The field of automobile insurance, and of automobile accident liability, would seem to be complicated enough without trying to establish relationships between the safety aspects of insurance and the safety aspects of highway expenditures. But the record seems to indicate that the incentive aspects of insurance leave much to be desired; insurance driver classification schemes which try to bring incentives into liability insurance rates may create new social problems; and the present trend toward "no fault" liability insurance clearly requires careful examination with respect to its impact on safety incentives. It may be that any attempt to enforce such incentives against individual drivers would have undesirable byproducts which would rule it out as a socially efficient solution. In that case, or for that matter in any other based on radical alteration of present automobile liability insurance systems, the interaction between the new solution and investment and operating programs to improve highway safety will require careful examination and planning.

IV. SOME PLANNING PECULIARITIES OF TRANSPORTATION

Special planning problems for a Federal Department of Transportation focus on intergovernmental relations, and the use of prices to allocate present facilities and to determine the need and provide the revenue for the construction of new facilities.

1. *Transport planning and intergovernmental relations* *

This is an all-pervasive problem. Therefore it has already cropped up on page after page, and need only be summarized here.

In principle, intergovernmental relations in transportation have three aspects: To what extent should each level of government raise its own revenues and take sole responsibility for its own sphere of transportation investment and operating costs? To the extent that the Federal Government contributes large percentages of the cost of transportation programs—90 percent, for example, in the case of the interstate highway program—how long can present quasivoluntary arrangements continue to circle around the fact that he who pays the piper calls the tune? And to what extent do present systems for distributing Federal funds by mode represent an obsolete method of trying to solve problems that might better be approached through block transportation grants, or even by general plans for sharing of Federal receipts with State and local governments such as the Heller-Pechman plan?

²³ *Investigation of Auto Insurance*, hearings before the Consumer Subcommittee of the Committee on Commerce, U.S. Senate, 90th Cong., second sess., on S.J. Res. 129, Washington, D.C., 1968, testimony of Secretary of Transportation Alan S. Boyd, p. 12.

*Further discussion of this issue is found in the papers by Mushkin & Cotton, and Olson in vol. 1 of this collection.

The second of these questions must, in the long run, be rhetorical. In view of Federal Government involvement in more and more aspects of urban living, it would be impossible for reasons of rational budgeting at the Washington level to continue the degree of freedom enjoyed in the past by State highway departments in the spending of Federal money. This would be true without reference to the desires of the Bureau of Public Roads, the Federal Highway Administration, or the Department of Transportation.

This leaves the first and third open for consideration. An answer to these questions will depend, in part, on the answer to two further questions: To what extent does the past developmental role of the Federal Government need to be continued? To what extent does it need to be replaced by coordination?

As for transportation and economic development: there are no present transportation modes which are, in themselves, infant industries; nor are there any transportation modes which provide levers which can be used to raise the whole economy. The thorniest modal question at the moment, the future of American railroads, does not involve policy toward development but policy toward continuation and modernization. Moreover, Government policy cannot operate through PPB techniques in this case because the railroad industry is privately owned and in the hands of a number of separate corporations. In other modes, the decreasing cost aspects of interurban movement are often being overshadowed by the increasing costs—for transport, or for its environment—of urban and other terminal operations.

If the development role of the Federal Government in transportation is losing importance, its integrative and coordinating role must steadily grow. This has double significance for the organization of the Federal role in transportation because, even when development was still regarded as the principal function, it was pointed out that the “* * * lack of a major center of responsibility is almost unique to the United States; no other major western industrialized country follows this practice to the same degree.”²⁴ If the world regards centralization of government transport responsibility as essential for transport development, even the United States must adopt this opinion with the present shift in emphasis toward choice, opportunity costs, externalities, and intermodal and intergovernmental relations.

2. *Transport pricing* *

This is the second all-pervasive problem, which has also permeated this discussion. But a final point needs to be underscored. This is that the implications of the phrase “user charge” must be explored much more carefully, at both the legislative and the planning level, if the United States is ever to have rational transportation planning.

The *user charge* question now rests as follows:

(a) In practice, highway investments (although not all costs of highway operation) are expected to be financed by general user charges; commercial airline passengers pay taxes which fall not far short of covering the user charges of the airway system and cover the applicable costs of many metropolitan airports; general aviation makes

²⁴ *Program Budgeting*, John R. Meyer, “Transportation in the Program Budget,” p. 146.

*Further discussion of this issue is found in the papers by Schultze, Krutilla, and Milliman in vol. 1 of this collection.

only a small contribution, and general aviation using jet planes makes no contribution, to user costs; waterway users make no contribution to either investment or operating costs.

(b) Congressional opinion has been consistently opposed to *specific* user charges in the form of tolls.²⁵ But professional economists practically always examine problems of transport routes and terminals in terms of *specific* user charges. The economic theory of the optimum use of roads or airports begins with the question of how to alleviate or charge for congestion, and how to relate both the alleviation and the charge to the need for and financing of new transport facilities. Every assumption of these economic discussions is more or less diametrically opposed to the idea that tolls are a nuisance which should be eliminated as soon as possible.

The difference in positions is especially marked on the occasion of celebrations marking the removal of tolls due to the rapid increase of use of the facilities. An economist would say that the tolls should be *imposed*, or *increased*, as use increases.

A partial explanation for this paradox is the difficulty and expense of levying tolls on some types of transport. For certain highways, tolls may cause more congestion than they alleviate; and on all highways, tolls require changes in design and additions to operating costs which might not always seem economically justifiable even to an economist. But the paradox runs deeper than that, and it does not all originate from the fact that economists are often working from simplified assumptions. For example, the idea of a spacious and unfilled country in which maximum movement must be encouraged for both political and economic reasons is an idea with firm roots in American history, which still made sense when the Interstate Highway System was started in 1956. But, as an approach to the location and financing of new urban expressways or new metropolitan airports, this idea is wrenched from its appropriate context and thrust into an environment where its results can be downright pernicious. For example: as long as land taken for transportation purposes goes off local tax rolls, and as long as immediate neighbors receive no compensation for noise and fumes, the airport problems of the largest American cities are going to continue to get worse before they can possibly get better. And as long as an attempt is made to subsidize urban freeways first,²⁶ and then to subsidize mass transportation, fringe parking, and other alternative methods of transportation, the final result could be to drown aid to cities in aid to uneconomic utilization of metropolitan space.

²⁵ For example:

We consider most unfortunate this evidence of a wide extension of toll facilities across our Nation.

We would like to see movement not in the direction of more but of fewer and fewer toll facilities.

We take this position because it is clear, and conceded by almost all, that the nontoll facility serves the better public interest.

These statements are taken from Relationship of Toll Facilities to the Federal-Aid Highway Program, Report of the Special Subcommittee on the Federal-Aid Highway Program and the Subcommittee on Roads to the Committee on Public Works, House of Representatives, 90th Congress, first sess., H.R. No. 597, Washington, D.C., 1967, p. 175.

²⁶ Possibly more through not charging them the local taxes on the value of the land they use than through not covering direct investment costs of the freeway through fuel taxes.

V. THE ROLE OF THE DEPARTMENT OF TRANSPORTATION

Where does all this leave the Department of Transportation?

First, it most certainly justifies its existence. As was pointed out before the Department was created:

. . . Perhaps the best argument for creating such a new department is that it could not possibly result in worse coordination of different Federal transportation activities than now occurs.²⁷

Second, it shifts the emphasis *toward* the Department's main job, which is that of budgeting for present and future transportation demands and making due allowance for their interactions with the environment in which transportation facilities exist and operate. Promotional activities, in the sense of regional development or national prestige, require detailed, as well as ultimate, decisions at the political level. But coordination involves both analysis and policy. It cannot even be debated properly in terms of national goals and aspirations until the concepts are clarified and the options are described.

Third, it shifts Government emphasis from regulation to planning. The great age of transport regulation was after the railroad system was largely developed, and before railroads became subject to strong, many-sided competition in whose costs and efficiencies Government budgets had a large part to play. The regulatory problem, then, is twofold: the railroad precedents may not be appropriate for other transportation modes; and a thousand dollars of Government investment may be more important than a day of Government hearings.

Finally, it is the only way to shift from the era of major concern with the wide open spaces to major concern with congestion. Much of the history of American governmental concern with transportation, from the Articles of Confederation to 1956, may be summed up in one question: what to do when density of use of transportation service is inadequate to support a privately owned system which is competitive and yet meets legislative standards of national need? The new history of governmental concern, to be written in a different context, must ask the question: what to do when density of use is too great, and at the same time may not support privately owned systems which meet Government standards of local or metropolitan as well as national need?

²⁷ John R. Meyer, *Program Budgeting*, p. 174.

PROGRAM ANALYSIS AND AGRICULTURAL POLICY *

BY VERNON W. RUTTAN

Vernon W. Ruttan is Professor and Head of the Department of Agricultural Economics at the University of Minnesota.

The fundamental restructuring of our economy in this century has led to vast changes in the agricultural sector—once an industry which closely approximated classical competition. Dr. Ruttan examines these changes in terms of the five market channels which connect agriculture to the rest of the economy: the product market, the current input and capital markets, the labor market, the land market, and the market for consumer goods. "During the last several decades these linkages have been modified by powerful forces of economic and political change. The failure of these market relationships to generate either effective resource allocation or equity in income distribution has led to a set of policies designed to modify market behavior. As a result commercial agriculture has emerged over the last several decades as a 'public utility' like industry characterized by substantial public intervention in the markets which link agriculture to the rest of the economy. More recently these policies are being supplemented by a new set of programs designed to affect directly the welfare of rural people outside of the market relationships."

Dr. Ruttan outlines the major problems of the agricultural sector, which might be summarized under the headings of instability in commercial agriculture and poverty among the members of the rural population. He describes current agricultural policy, and examines the trends and developments which are likely to influence future policy.

Various studies undertaken to reexamine the public programs that serve agriculture have suffered from the fact that there has been little recognition of the connection between program activities and program goals. Although the Department of Agriculture has attempted to recast its program activities and budget in a more mission-oriented framework, so as to improve its ability to evaluate the relationship between programs and goals, several obstacles remain. "First, there exists in agricultural programs substantial ambiguity with respect to the relationship between program instruments and objectives * * *. A second major difficulty * * * stems from the close integration of commercial agriculture with other sectors of the economy * * *. A third major limitation stems from limitations in analytical capacity."

Dr. Ruttan suggests that the first two difficulties could be reduced substantially if the major responsibility for program analysis were shifted from the Department of Agriculture to the Executive Office of the President. Only then would the integration of the agricultural sector into the national economy be properly reflected in agricultural policies. "At a very minimum the Executive Office of the President should be staffed to interact more formally with the several departments and agencies in establishing program objectives and in coordinating the program analysis for activities which impinge on the agricultural industry and on rural communities."

Introduction

In this paper I would like first to step back and review, in broad strokes, some of the major forces which have lead, in the 20th century,

*This statement draws very heavily on an earlier paper by the author previously published in the *Journal of Farm Economics*, (Ruttan, 1966). The author is indebted to Martin Abel and Wilbur Maki for review and comment on an earlier draft of this paper.

to (a) a fundamental restructuring of the relationship between the agricultural and nonagricultural sectors of the American economy, and (b) the emergence of a set of agricultural policies which have transformed agriculture from an industry which closely approximated the classical competitive model to a "public utility" like industry characterized by substantial public intervention in input and product markets.

I will then turn to a consideration of number of the issues that must be faced if the policies and programs which have been designed for agriculture are to be evaluated in terms of the effectiveness of program expenditures in relation to direct program and broad social objectives. No attempt is made to specify a research agenda for agricultural program analysis.

From the time of the Plymouth and Jamestown settlements until the closing years of the 19th century, America's 300-year encounter with the frontier was the dominant theme in the Nation's agricultural development. This long encounter created an opportunity for the evolution of an agriculture based on abundant land and scarce labor. This in turn stimulated the development of an agricultural technology that was primarily directed toward achieving gains in labor productivity rather than gains in land productivity.

Since the closing of the frontier, agriculture's encounter with an increasingly dominant urban-industrial sector has emerged as the major theme in American agricultural development. In 1880 non-agricultural employment first exceeded agricultural employment and by 1929 manufacturing employment alone exceeded agricultural employment. By 1980 it seems likely that agricultural employment will be less than total unemployment in the United States even during periods of high level economic activity (table 1). This development alone opens up entirely new dimensions in the evolution of agricultural policy which could scarcely have been considered when agriculture represented an important share of the Nation's labor force.

The fundamental restructuring of the relationship between the farm and the non-farm sectors of the American economy can best be understood by examining five sets of market linkages by which U.S. agriculture is joined to the rest of the national economy and to the world economy. These linkages include the *product market*, through which agricultural output is transmitted to the nonagricultural sector and through which agricultural incomes are generated; the *current input and capital markets*, through which the manufactured capital equipment and other operating inputs used in agricultural production move; the *labor market*, through which manpower is allocated between the agricultural and nonagricultural sectors and among firms in each sector; the *land market*, through which land is allocated among firms in the farm sector and between the agricultural and nonagricultural sectors; and finally, the market for *consumer goods*, through which farm families achieve or are denied access to the patterns of consumption now identified with the American standard of living.

TABLE 1.—EMPLOYMENT BY SECTOR IN THE UNITED STATES, 1880-1968, AND PROJECTIONS TO 1980
[In thousands of workers]

	Agriculture	Total nonagriculture	Manufacturing	Unemployment
1880.....	8,585	8,087	(¹)	(¹)
1929.....	10,450	37,180	10,534	1,550
1964.....	4,523	64,782	17,274	3,786
1968.....	3,817	72,103	1,9734	2,817
1980.....	2,800	97,500	-----	-----

¹ Not available.

Sources: 1880 and 1929: U.S. Bureau of the Census, "Historical Statistics of the United States, Colonial Times to 1957," Washington, D.C., 1960; 1964 and 1968: Economic Report of the President, January 1969, pp. 252, 258; 1980 projections: Rex F. Daly, "Agriculture: Prospective Growth and Structural Change," in President's National Advisory Commission on Rural Poverty, "Rural Poverty in the United States," Washington, U.S. Government Printing Office, May 1968, pp. 415-420.

During the last several decades these linkages have been modified by powerful forces of economic and political change. The failure of these market relationships to generate either effective resource allocation or equity in income distribution has led to a set of policies designed to modify market behavior. As a result commercial agriculture has emerged over the last several decades as a "public utility" like industry characterized by substantial public intervention in the markets which link agriculture to the rest of the economy. More recently these policies are being supplemented by a new set of programs designed to affect directly the welfare of rural people outside of the structure of market relationships.

THE PRODUCT MARKET

Through most of American economic history the product market—the market for things farmers sell—represented primary link between the farm and the nonfarm sectors of the economy. It was the dominant channel through which shifts in the international terms of trade, national fluctuations in nonfarm income, and local variations in nonfarm demand have been channeled into the agricultural sector.¹

In most low-income countries, where a substantial share of increases in per capita income are devoted to dietary improvement, the product market is still the main link between the peasant and the urban-industrial sector of the economy. As income per person rises consumption of agricultural products expands less rapidly. At very high income levels there may be no additional farm level food consumed as income continues to rise (Stevens, 1965).

In the United States, the declining response in consumption of farm level food and fiber to increases in nonfarm income has sharply reduced the commodity market effects of both fluctuations and growth in national economic activity in the nonfarm sector (Bryant, 1962). Monetary and fiscal policy measures have tended to produce rather stable rates of growth in per capita income. Agricultural trade and commodity policies have tended to insulate agricultural commodity prices, particularly crop prices, from normal trade and market fluctuations. Participation in the growth of foreign demand, except for

¹ The importance of interaction between the farm and nonfarm sectors of the economy represented the major thesis of Schultz' classic study of *Agriculture in an Unstable Economy* (Schultz, 1945). Major attention was focused on product market interactions. A decade and a half later Hathaway argued that there had been a major shift in this pattern of interaction with the input markets having much greater significance (Hathaway, 1959).

a few products such as soybeans and some feed grain exports, has been heavily dependent on export subsidies or food aid throughout most of the postwar period.²

Changes in the product markets which link the agricultural to the urban-industrial sector have also modified the local market effects of urban-industrial development. The production of some products has become essentially industrialized. Broiler and turkey "factories" have almost entirely replaced farm production of poultry meat. Commercial production of fruits and vegetables is becoming highly concentrated. Regional specialization in production of fruit, vegetable, and animal products, resulting from technological and organizational changes in processing, transportation, and distribution have reduced the impact of local urban-industrial development on the demand for locally produced farm products. Although there are a few minor exceptions, fluid milk, which is protected by a series of local market trade barriers, remains one of the few major farm products whose demand remains responsive to local urban-industrial growth, and even this linkage is under pressure from a merger movement designed to achieve greater bargaining power for dairy producers and processing cooperatives.

The decline in the significance of the product market as a generator of either dynamic growth or instability in the agricultural sector is a consequence of both (a) the economic development of the U.S. economy and (b) the monetary, fiscal and commodity policies that have been implemented since World War II (Firsch, 1964). As a result it is no longer reasonable to suggest that the farm problem is primarily a product of "business fluctuations and unbalanced expansion of the (national) economy" (Schultz, 1945, p. 2). There is, however, a very substantial possibility that instability in growth in the international markets for agricultural products associated with commercial policy in Western Europe and U.S. policy with respect to food aid for the developing countries may become an important source of instability in the produce market during the next decade.

THE MARKET FOR PURCHASED INPUTS

The markets for manufactured capital equipment and current inputs have become increasingly important in transmitting the effects of changes in the nonfarm economy to agriculture (Bryant, 1962; Hathaway, 1959). Much of the new agricultural technology is embodied in the form of new capital equipment or more efficient fertilizer, insecticides, and other manufactured inputs. In 1870 the typical American farm was still in some respects a subsistence unit—with the value of intermediate products supplied by the nonfarm sector amounting to less than 9 percent of the value of gross farm product. By 1900 intermediate inputs still amounted to only about 13 percent of gross farm product (Towne and Rasmussen, 1960, p. 265). In recent years nonfarm inputs have exceeded 60 percent of the value of farm output (MacEachern and Ruttan, 1964).

Growth in the use of purchased inputs has been closely related to developments in the labor market. The demand for labor, resulting

² Throughout most of the 1950's assisted exports (subsidized sales for dollars on exports under ICA & Public Law 480) accounted for between one-half and two-thirds of U.S. exports of farm products. It is only since 1961-62 that assisted exports have fallen below 50 percent of all agricultural exports. (USDA, FAT).

from rapid urban-industrial development, reinforced the economic pressure for substitution of capital equipment for labor in American agriculture at precisely that period when the frontier was disappearing as a major factor in agricultural development (Edwards, 1940).

The rapid growth of labor productivity in American agriculture was not, at first, accompanied by parallel changes in land productivity. Aggregate gain yield per acre in American agriculture remained essentially unchanged between the end of the Civil War until well into the 20th century (Johnson and Gustafson, 1962).

By the mid-1920's, however, a new biological and chemical technology was beginning to emerge. This combination of rapid advance in biological research plus a high volume of relatively inexpensive agricultural chemicals created a new dimension in agricultural productivity in U.S. Agriculture. Land productivity, which had experienced no real growth between 1900 and 1925, rose by 1.4 percent per year for the period 1925-50 and by 2.2 percent per year between 1950 and 1967. This higher output per acre combined with continued mechanization to produce a rate of growth of labor productivity of 6.2 percent per year between 1950 and 1967 (table 2).

TABLE 2.—ANNUAL AVERAGE RATES OF CHANGE IN TOTAL OUTPUTS, INPUTS AND PRODUCTIVITY IN U.S. AGRICULTURE, 1870-1967

Item	[Percent per year]			
	1870-1900	1900-25	1925-50	1950-67
Farm output.....	2.9	0.9	1.5	1.9
Total inputs.....	1.9	1.1	.3	.4
Total productivity.....	1.0	-.2	1.2	1.4
Labor inputs ¹	1.6	.5	-1.8	-4.1
Labor productivity.....	1.3	.4	3.3	6.2
Land inputs ²	3.1	.8	.1	-.6
Land productivity.....	-.2	0	1.4	2.2

¹ Number of workers, 1870-1910; man-hour basis, 1910-67.

² Cropland used for crops, including crop failure and cultivated summer fallow.

Sources: Computed from U.S. Department of Agriculture, "Changes in Farm Production and Efficiency," Statistical Bulletin 223 (revised), Washington, July 1965; and D. D. Durost and G. R. Barton, "Changing Sources of Farm Output," U.S. Department of Agriculture, Production Research Report No. 36, Washington, February 1960; "Agricultural Statistics, 1967," pp. 540-547; Agricultural Statistics, 1968," pp. 456-460.

The productivity gains in agriculture have been achieved, to a substantial degree, through advances in technology that are embodied in inputs produced by the farm machinery, chemical, feed processing and related industrial sectors. These industries have increased the quality of the technology embodied in their products and have achieved rapid productivity gains in the use of factor inputs. The results have been transmitted to agriculture through improvements in the physical productivity of factor inputs and reductions in factor-product price ratios.

As purchased inputs have risen relative to the value of farm output, however, the market for purchased inputs has become not only a source of productivity gains for the agricultural sector but a source of instability as well. Agriculture is no longer an industry with a vested interest in inflation. The monetary gains resulting from the higher product prices associated with rises in the general price level tend to be rapidly converted into net reductions in real purchasing power by inflation in the prices of purchased inputs (Hathaway, 1963,

Bryant, 1962). In the 7 years 1955 through 1961, when the consumer price index rose by 11.7 percent, prices received by farmers rose by 3.1 percent, while prices paid by farmers for production items rose by 9.6 percent. In the 7 years 1961-68, when the consumer price index rose by 16.0 percent prices received by farmers rose by 8.1 percent, while prices paid by farmers for production items rose by 17.5 percent.

THE LABOR MARKET

The labor market has become an increasingly important channel of interaction between the farm and nonfarm sectors. Technical and economic developments have made it increasingly profitable to substitute inputs purchased from the industrial sector for farm labor. The slow growth in domestic demand for farm products, the insulation of domestic markets from changes in demand in other countries, the rapid growth in labor productivity have all combined to place the major burden of balancing the rate of growth of agricultural output with the rate of growth in demand for agricultural products on the labor market.

With the demand for agricultural output expanding at less than 2 percent per year and labor productivity rising by more than 6 percent per year, the burden of adjustment in the labor market has been extremely heavy. The rate at which labor leaves the agricultural sector, either through migration or local off-farm or part-time employment is highly responsive to the level of unemployment in the nonfarm labor force (Sjaastad, 1961).

Labor market adjustments have also been particularly difficult in the low income agricultural regions where local nonfarm employment has not expanded at a sufficiently rapid rate to absorb both the excess agricultural labor force and the new entrants to the labor force from rural areas. The labor surplus has been so large, and the obstacles to migration for the older and less well educated members of rural communities have been so great, that migration has generally not been sufficient to induce convergence of wage and income levels between high and low income regions or to narrow income differentials between farm and nonfarm workers, except where there has been substantial growth in local nonfarm labor markets (Borts, 1960; Borts and Stein, 1962; Hathaway, 1960; Ruttan, 1954).

Even in areas where the intersector labor market has functioned effectively it has also served as a channel for transferring capital from the farm to the nonfarm sector. The capital invested in the education of farm youth, while low relative to investment in education in the nonfarm sector, has been large relative to aggregate net farm income (Long and Dorner, 1954; Owen, 1966). Clearly lower birth rates in rural areas could reduce the magnitude of the capital transfer of capital invested in education associated with migration. And greater reliance on State and Federal rather than local revenue sources to finance educational services could partially compensate rural areas for the capital drain through investment in education.

THE LAND MARKET

The land market has, throughout American agricultural history, played a major role in facilitating the redistribution of the owner-

ship of both land and nonland assets between the farm and nonfarm sectors. Yet there is remarkably little quantitative evidence on the magnitude of intersector shifts in asset flows and ownership.

The success of land speculators in attempting to preempt the productivity gains from the opening up of new land in the west appears to have been greatly overemphasized in the Populist literature (Bogue, 1955). The extent to which economic rents resulting from national economic growth and local urban industrial development have contributed to rising land prices is also a question that appears to generate more heat than light (Scharlach and Schuh, 1962; Schofield, 1965).

Nevertheless, it seems reasonable to hypothesize that the system of owner-operatorship of land which requires that farm property be re-financed each generation has served as an effective channel for the transfer of the ownership of capital accumulated in the farm sector to the nonfarm sector at least since the beginning of the decline in farm population after 1910.

In recent years the transfer of the ownership of capital accumulated in the agricultural sector to the nonfarm sector through the land market has been modified by (a) rapid technological change coupled with (b) the institutional devices designed to maintain farm prices (Herdt and Cochrane, 1966). In the absence of farm price support and production controls the productivity gains in the agricultural sector would have been transferred directly to the nonfarm sector through the product market as a result of lower prices for farm commodities.

Price support and production controls have, except for short periods such as 1952-55, effectively dampened such tendencies. During the last several decades agriculture has been operating in an environment in which (a) technological changes has drastically increased both land productivity and the area that could be efficiently combined in one operating unit and (b) price supports made effective by the willingness of the Government to stockpile the major agricultural commodities. In addition the Government itself has entered the land market in an effort to bid land away from crop production.

In this environment farmers have apparently utilized both the actual and anticipated gains from higher productivity to bid against each other and the Government for the reduced acreage available to the agricultural sector for farm production. The effect has been to capitalize the benefit of technological change and commodity programs into land values.³ As farms have changed hands at higher and higher prices a substantial share of the realized capital gains have been channeled into the nonfarm sector through inheritance by nonfarm family members. The result has been an increase in fixed costs and a reduction in net income to the new owner.

THE MARKET FOR CONSUMER GOODS

The market for consumer goods represents the channel through which farm families achieve access to, or are excluded from full participation in the patterns of consumption that are identified with the American standard of living. In the past it also represented an impor-

³The capitalization of program benefits into land values has in recent years been carefully documented (Gibson, *et al.* 1962; Herdt and Cochrane, 1966; Hendrick, 1962; Maier, *et al.*, 1960).

tant source of demand for the products of an expanding urban-industrial sector. With the reduction in farm numbers, and the decline in consumption expenditures relative to the value of farm output the consumption expenditures of farm families no longer represent an independent dynamic factor in aggregate consumer demand.

Curiously enough economists have devoted relatively little attention to the economic behavior of farmers as consumers. In an earlier era it was assumed that farmers, and other rural residents enjoyed a substantial advantage in real purchasing power as compared to urban families with similar money incomes. This was apparently confirmed, by Koffsky's classic study of nearly 20 years ago (Koffsky, 1949). Although this work has not been repeated in comparable detail there is evidence, from later estimates, that the purchasing power advantage of rural residents, measured in terms of a market basket concept, which included items consumed in both urban and rural areas, has declined sharply (Puterbaugh, 1961; Hathaway, 1963).

In contrast to these estimates I would personally hypothesize, generalizing from the very inadequate data available on which to make such comparisons (Murphy, 1965), that a valid analysis covering the broad range of commodity, social and cultural components of consumption, with appropriate corrections for quality differences and transportation costs, would indicate substantially lower real purchasing power for rural than for urban families with similar money incomes.

Furthermore, I would hypothesize that purchasing power advantages in rural areas that have been postulated in the past were based largely on the fact that baskets of goods consumed in rural and urban areas were not really comparable. The rural basket was inferior in terms of both the quality and range of items included, not only in the areas of health, education, housing and cultural amenities but in terms of the commodity components of food and clothing.

In the past these disparities were disguised by what purported to be a distinct rural and urban culture—by a distinct rural economy and rural society—in which the rural and urban sectors presumably gave different weights to the elements entering into consumption patterns. This rural-urban cultural distinction is disappearing (Nelson and Donahue, 1966). There has emerged today a high-income commercial agricultural sector which does participate fully, both culturally and economically, in the consumer markets of a dynamic urban-industrial society. In most areas the families who operate the Nation's high income commercial farms do their shopping at the same supermarkets and suburban shopping centers as the families of urban workers and professionals. They share the same cultural values and aspirations. But the real cost of full participation, I would argue, remains substantially greater for rural families than for the families that reside in or near the Nation's standards metropolitan areas.

POVERTY IN AMERICAN AGRICULTURE

An unanticipated byproduct of the effective economic and cultural integration of the commercial agriculture into the national society is the emergence of a dual structure in the well-being of rural families. There is no sector of American agriculture that can be properly classed as a peasant sector. There is, however, substantial poverty in

rural areas. Rural income is far less equitably distributed than urban income (Boyne, 1965). The poverty problem has in the past been reflected primarily in terms of occupational, age, racial, and regional dimensions (Ruttan, 1962; Bird, 1964).

First, consider the occupational dimension (Fuller, 1968; Larson, 1968). Incomes of hired farmworkers are substantially lower than incomes of farm operators. The hired farm labor force is the most heterogeneous employee group in the American economy. Incomes of full-time hired workers on commercial farms have increased rapidly. Incomes of part-time and migrant farmworkers, however, have not kept pace with the income of either full-time farmworkers or farm operators. Mechanization of operations formerly performed by hand has actually reduced the number of days worked per year by some categories of hired farmworkers (Ruttan, 1962).

The rapid growth in the size of commercial farms has also helped create an age dimension to the poverty problem (Kreps, 1968). Many older farm operators have been caught in a situation where they have neither the financial resources to expand their farm operations nor the labor skills necessary to find remunerative off-farm employment.

There also is a racial dimension to the poverty problem (Price, 1968). Roughly half of the farm families that fall in the poverty class are located in the South. Although the median income of white farm families in the South is only about half that of white urban families in the South it is almost twice as high as the median income of nonwhite farm families. (Table 3.)

TABLE 3.—MEDIAN MONEY INCOME OF FARM AND NONFARM FAMILIES, BY REGION AND COLOR, 1964

	United States	Northeast	North-central	South		West
				White	Nonwhite	
1. Median income:						
Nonfarm.....	\$6,755	\$7,277	\$7,101	\$6,136	\$3,112	\$7,378
Farm.....	3,558	4,804	4,160	3,168	1,721	5,248
Farm median income as a percent of nonfarm.....	53	66	59	52	55	71
2. Families with incomes of \$5,000 or more (percent):						
Nonfarm.....	68	74	72	62	24	72
Farm.....	36	48	42	31	4	53
3. Families with incomes under \$2,500 (percent):						
Nonfarm.....	12	8	10	14	40	10
Farm.....	35	23	30	39	77	21

Sources: U.S. Bureau of the Census, "Income in 1964 of Families and Persons in the United States," Current Population Reports, series P-60, No. 47, and unpublished data.

The regional dimension has, in the past, been closely interrelated with the racial dimension. Regionally, approximately half of the Nation's low productivity and low-income farm families are located in the South. However, nonwhites have accounted for one-third of the total decline in farm population in recent years (CEA, 1966). This rapid decline in nonwhite farm operators since World War II means that the racial dimension is becoming less important in explaining poverty in rural areas in the South. There are also substantial pockets of poverty in certain peripheral areas such as the Ozark Mountains area in Missouri and Oklahoma; parts of southern Ohio, Indiana, and Illinois; certain cutover areas in the Northern Lake States of Michi-

gan, Wisconsin, and Minnesota; and the areas of Spanish and Indian concentration in the Southwest and in other scattered areas throughout the Nation.

It cannot be stressed too strongly that rapid growth in the nonfarm labor market is necessary for any successful effort to overcome the regional, occupational, age, and racial dimensions of poverty in American agriculture. However, neither off-farm migration nor local industrial development are capable by themselves of fully overcoming the poverty problem in American agriculture (Ruttan, 1958; Ruttan and Wallace, 1962). Substantial numbers of the least mobile portion of the farm labor force remain stranded in rural underdeployment—on small farms, in part-time employment, and in barely remunerative nonfarm employment.

NEW DIMENSIONS IN AGRICULTURAL POLICY

American agricultural development policies have been uniquely successful in meeting national farm output and productivity objectives. These policies have clearly been less successful in meeting the income objectives of all of the families engaged in the production of agricultural commodities. One observer has pointed out that "behavior of rural people, their representatives, and their institutions implies a materialistic bias in favor of plants, land, and animals and against people" (Schultz, 1965). While this is perhaps overdrawn it is true that the policies of the past were designed primarily to solve technological and commodity problems of rural people.

This was a valid choice at the time these policies were established. It was important for U.S. economic development that the agricultural sector achieved a sufficiently high rate of output and productivity growth to simultaneously meet national food and fiber requirements and release substantial numbers of farmworkers for nonfarm employment. If technological change and farm output growth had continued at the 1900-25 rate during 1925-65 the United States would today (a) be importing as much as one-third of its total food and fiber consumption, and/or (b) paying substantially higher prices for food. Instead the United States is exporting approximately one-fourth of total agricultural production and enjoying relatively low food prices.

There is no economic reason, however, for the continuation of a dual structure in American agriculture. And, in fact, a new set of agricultural policies that are less commodity oriented are now emerging. There is a shift away from the relatively unsuccessful attempts to improve the income distribution within agriculture through policies designed to maintain or improve commodity prices.

EMPLOYMENT PROGRAMS

Since the mid-1950's the traditional commodity policies have been supplemented by a new set of employment-oriented rural development policies. Initiation of the rural development program in 1956 represented belated recognition that the Employment Act of 1946 applied to rural as well as urban areas. The rural (later area) development pro-

gram was followed by a number of other efforts designed to expand farm and nonfarm employment opportunities in rural areas.⁴

A basic limitation of the rural employment programs is that they have not departed sufficiently from the production-oriented policies of the past. They continue to assume that the way to help rural people is to help them produce something of value to the rest of society—be it agricultural commodities, recreation services, or automobile parts—without changing their geographic location. The employment-oriented programs are failing to make a significant aggregate impact on underemployment in rural areas simply because the resources in rural areas in which major effort is being concentrated (the Appalachians, the Ozarks, the Upper Midwest and others) are not underdeveloped—they are redundant. The employment programs may have a modest impact on location and investment decisions. They have little prospects of reversing the trend for greater centralization of industrial activity within standard metropolitan areas in much of the Nation outside of the midcontinent and southern crescent manufacturing belts. For most counties the contribution of the employment programs toward solution of the problem of rural poverty will be marginal at best (Ruttan, 1958; Ruttan and Wallace, 1962; Tang, 1965; Beale, 1968).

INCOME TRANSFER PROGRAMS*

The commodity policies have also been supplemented by a new set of policies designed to separate income support payments from either commodity prices or direct participation in the production process. Extension of social security to farmers in 1955 was the first and remains the most important step in the separation of income support payments from commodity prices. This action was by itself responsible for a sharp reduction in the disparity between the incomes of older farm and nonfarm workers (Ruttan, 1962, p. 88). Land retirement programs, which make rental payments to farmers for removing marginal land from intensive crop production, also operated as a mechanism to transfer income to farmers without directly affecting market prices. The Food and Agriculture Act of 1965 goes farther than earlier legislation in separating income support payment from production incentive and price stabilization payment (UM AE, 1965).

The use of effective income protection programs, employing some criteria of a socially acceptable minimum standard of living and protection against the risks imposed on individuals through product, inputs, land and labor market instability, and trends is particularly important in the short run (Swerling, 1959). Such payments permit the program participants to achieve a level of consumption more nearly in line with American standards. But they typically do not meet the additional objective of enabling the participant to contribute effectively to the further growth of the American economy.

⁴The literature on the Rural Development Program (RDP), the Rural Area Development (RAD), the Area Development Act (ADA), the Public Works and Area Development Act (EDA), and related employment-oriented programs such as the Appalachian Regional Development Act is too extensive to list in detail. The rural development program was outlined in *Development of Agriculture's Human Resources* (USDA, 1955). For a discussion of the more recent programs see the discussion at the 1965 winter meeting of the AFEA-AEA meetings (Houston and Tiebout, 1966; Booth, 1966; Bonnen, 1966; Moore, 1966). See also the paper by McGuire in vol. 1 of this collection.

*Further discussion of this issue is found in the paper by Bonnen in vol. 1 of this collection.

EDUCATION, TRAINING AND HEALTH PROGRAMS*

To meet this latter objective, a stronger emphasis must be placed on investment in the human agent of production—in man. The current pattern of underinvestment in rural health, rural education and training and other rural social services must be corrected.

New steps in this direction are now being taken (Bonnen, Apr. 1966; Cochrane, 1965; NACRP, 1967, 1968). Government programs involving substantial investments in education, training and health are being developed to assist low income families to increase both their incomes and their contribution to national economic growth by improving their capacity for fuller participation in an urban-industrial society. In education (*a*) the Elementary and Secondary Education Act of 1965, (*b*) the Adult Basic Education programs of the Economic Opportunity (Poverty) Act of 1964 and (*c*) the Operation Headstart program of the Office of Economic Opportunity are potentially of great significance. In the field of training (*a*) the post high school vocational training possibilities opened up by the Vocational Education Act of 1963 (*b*) the Work Experience Program under the Economic Opportunity Act of 1964, (*c*) the Job Corps and Neighborhood Youth Corps programs under the same act, and (*d*) the training and retraining provisions of the Manpower Development and Training Act of 1962 are all of potential significance (Bachmura, 1963). In the area of health (*a*) the Hill-Burton Act which gives grants and loans for hospital and medical care facilities and (*b*) the 1965 Social Security Act amendments which provides limited medical and substantial hospital care benefits for the aged (medicare) and low-income (medicaid) families are of great significance.

Most of these new programs, except medicare and medicaid, must still be classified at present, as having potential rather than present significance for solution of the rural poverty problem. Most are inadequately funded. Among those that have been operating for some time, evidence is accumulating that they may be most effective for the lower middle class rather than for the very poorest families.⁶

AGRICULTURAL POLICY FOR THE FUTURE

What kind of agriculture will emerge out of the technological and economic forces that currently impinge on the rural sector of the American economy? And what should be the orientation of the agricultural policies that can best serve (*a*) the people who earn their living producing the Nation's food and fiber, (*b*) the rural society that emerges out of the changes that are underway, and (*c*) the American economy in general? Let me attempt to respond to these questions, first with reference to commercial agriculture, and then with reference to the problem of poverty in rural areas. Many of the conclusions that will be drawn in this section should be treated as hypothesis for further

⁶ Lampman (1965) points out that " * * * our system of public income maintenance * * * is aimed more at the problem of income insecurity of the middle class and at blocking returns to poverty than in facilitating exits from poverty" (p. 526). This is also true for the older farm production and employment programs as well (U.S. Commission on Civil Rights (1965), Wadsworth and Conrad, 1965).

*Further discussion of this issue is found in the paper by Grosse, Brandl, Mangum, and Levine in this volume.

program and policy analysis. Many of the trends and developments can and may be modified by the implementation of alternative policies and programs.

COMMERCIAL AGRICULTURE

In commercial agriculture the decline in farm employment and farm numbers certainly will continue. By 1980 farm employment will decline to well below 3 million workers. This may be less than the number of unemployed workers even in times of high level economic activity. In 1966 there were approximately 1 million farms with sales of above \$10,000 and less than half a million farms with sales of \$20,000 or above (table 4). This latter group alone produced almost 70 percent of U.S. farm output. With family median incomes in metropolitan areas now well above \$7,500 per year, it is clearly only farms with sales of \$20,000 or more that can come near providing family incomes sufficient to permit a level of participation in the market for consumer goods that is consistent with American standards (Tweeten, 1965).

If total production were to be concentrated on farms such as those with sales of \$20,000 or more, the total U.S. farm output could be produced on 750,000 farms. If production were concentrated entirely on farms such as those with sales of \$40,000 or more, the total U.S. farm output could be produced on less than 400,000 farms. It seems apparent that the technological capacity already exists that would permit production of 80 to 90 percent of the value of total U.S. farm output on 50,000 to 100,000 production units.

It is unlikely that continued decline in farm employment and farm numbers will be accompanied by a withering away of farm price and income programs. Even a highly concentrated food and fiber production industry would be subject to great price and output instability in the absence of public intervention (Breimyer, 1965, p. 206). The rationale for public intervention in agricultural commodity markets is, and will continue to be, essentially similar to the rationale for setting rates and regulating output in the public utility and transportation industries, that is, to lend stability to an industry which technological and economic forces would render chronically unstable in the absence of such intervention (Kolko, 1965).

While the production and marketing of agricultural commodities will continue to be regulated through a combination of marketing allotments and quotas, multiple price systems, marketing orders and agreements, and land use contracts, it seems likely that these programs will be less oriented to achieving income goals in the agricultural sector than in the past. They will be directed to a greater extent toward the protection of urban consumers from undue price fluctuations and to the achievement of international trade and development policy objectives.

It seems reasonable to expect that the programs designed to lend stability in product markets will increasingly be supplemented by efforts to dampen the new instability that is now channeled through the markets for labor and purchased inputs. Minimum wage legislation covering a substantial share of the hired farm labor force can certainly be anticipated in the next several years. Efforts to organize agricultural labor are likely to receive more effective Government support

TABLE 4.—ESTIMATED NUMBER OF FARMS, PERCENTAGE OF SALES, PARITY INCOME GOALS, AND NECESSARY PRODUCT PRICE CHANGES TO ACHIEVE THESE INCOME GOALS, 1966

	Farms with sales of—							All farms	
	\$40,000 and over	\$20,000 to \$39,999	\$10,000 to \$19,999	\$5,000 to \$9,999	\$2,500 to \$4,999	Less than \$2,500	Part-time farms		Part retirement farm
1. Number of farms (thousands).....	193	334	510	446	356	114	911	388	3,252
(a) Percentage distribution.....	5.9	10.3	15.7	13.7	10.9	3.5	28.0	11.9	100
2. Cash receipts percentage distribution.....	48.7	21.1	16.7	7.5	3.0	.3	1.6	1.1	100
3. Parity income goal in 1966 dollars ¹	22,283	11,140	4,497	8,497	4,425		(?)	(?)	-----
4. Necessary change in product prices to achieve parity income (percent).....	-9		+10	+38	170		(?)	(?)	11

¹ Parity income includes the returns to capital and labor and the capital gains. An 11.8 percent rate of return was considered for the combination of return on capital and capital gains. Wage rates were determined from estimates of the wages in the nonfarm sector with the same characteristics adjusted upward to reach parity incomes. They ranged from \$1.98 to \$2.84 per hour.

² Not available.

Source: U.S. Department of Agriculture, "Parity Returns Position of Farmers," (S. Doc. 44, 90th Cong., 1st sess.), Washington, U.S. Government Printing Office, August 1967.

than in the past. There will be increased pressure for legislation regulating bargaining and other relationships between suppliers, growers, processors, and retailers participating in vertically integrated systems.

The situation I have characterized opens up entirely new possibilities in the evolution of agricultural policy. With the level of farm employment less than unemployment in the rest of the economy even during periods of high level economic activity, there is no longer any question of the capacity of the nonfarm sector to absorb displaced farmworkers. A substantial share of the entire farm labor force could be absorbed into the nonfarm labor force in a relatively short time if it were deemed important for the national economy.⁶ It is now both technically and economically possible to develop policies designed to organize a food and fiber production industry capable of permitting that part of the population engaged in food and fiber production to participate fully in the level of material and cultural consumption that is available only in a modern urban-industrial society.

RURAL POVERTY

A first step in a realistic effort to deal with rural poverty as it is now emerging is to recognize that it is no longer possible to make a significant aggregate impact on rural poverty by programs designed primarily to (a) increase agricultural production or (b) increase non-farm employment in rural areas. Production policy is an important element in the creation of a modern agriculture described above but production expansion will not provide new jobs in rural areas. Employment policy oriented around regional development efforts can be important in achieving the efficient use of labor both nationally and regionally. However, the potential number of new jobs created by such activities in the remaining poverty pockets will at best be extremely limited relative to the size of the poverty problem.

Furthermore, with continued outmigration the absolute number of families in the poverty class living in the traditional depressed areas appear to be declining relative to the number of poor families in the areas that have traditionally been classified as commercial farming areas. Northeast Minnesota is classified as a depressed area but the number of rural poor is greater in the commercial farming areas of southwestern Minnesota. Iowa has more poor families in rural areas than many Southern States. This means that we must move beyond the charge that Schultz gave us to consider the regional and community dimensions of poverty in his classic "Reflections on Poverty Within Agriculture" (Schultz, 1950) and consider the psychological and cultural dimensions of poverty, a society dominated by urban values.

A second major element in the design of an effective attack on poverty in rural areas is recognition of the changing relationship between commercial agriculture and the rural community. The economic relationships that previously existed between commercial agriculture and

⁶Dernberg and Strand (1966) point out that the recorded rates of unemployment understate the true level of unemployment by substantial margins. Both recorded unemployment and the gap between recorded and actual unemployment has been heavily concentrated in the 14 to 24 and over 65 age groups. During the next decade this will pose a much more difficult problem for the U.S. economy than absorption of displaced farmworkers.

the local community through the product market and the market for purchased inputs are rapidly disappearing. A prosperous agriculture no longer implies a prosperous rural community. The rural community and commercial agriculture are no longer joined in a mutuality of interest stemming from the possibility of a common solution to their economic problems.

A third major element in the design of an effective attack on poverty in rural areas is to reject the assumption that it is rural poverty in any significant respect except location. More positively it is to recognize that the problems of both rural and urban poverty are essentially similar in their psychological, sociological and economic dimensions and that the agencies that develop the most effective capacity to deal with these problems in urban areas will be the agencies best equipped to deal with them in rural areas.

The implication is that the problem of rural poverty is growing progressively less amenable to solution through use of the program instruments available to the USDA or to the colleges of agriculture. Indeed, to attempt to bend the program instruments available to these agencies into the direct service of antipoverty efforts would be to blunt their usefulness for agricultural production and employment policy.

PROGRAM ANALYSIS

The analysis presented in the previous sections of this paper have identified two particularly significant structural changes in the relationship between the agricultural and nonagricultural sectors of the economy. One is the emergence of commercial agriculture as a public utility like industry in which the market relationships which link commercial agriculture to the rest of the economy are strongly identified with the public interest. A second is the emergence of a dual structure in rural communities resulting from the disintegration of the economic linkages that previously existed between commercial agricultural and other economic activity in rural areas.

Concern with these changes and their implications for agricultural programs and policies have resulted in a number of attempts to re-examine the public programs that serve agriculture. These efforts include (a) the joint study of the Association of State Universities and Land-Grant Colleges and the U.S. Department of Agriculture, *A National Program of Research for Agriculture* (1966); and the reports of the National Commission on Food Marketing (1966); the National Advisory Commission on Food and Fiber (1967); and the National Advisory Commission on Rural Poverty (1967).

The report of the National Advisory Commission on Food and Fiber calls for a more market oriented agriculture, continued market expansion, less Government intervention, and freer international trade. The report of the National Commission on Food Marketing emphasizes the tendency for increased concentration in agricultural product markets and calls for public intervention to reduce the arbitrary exercise of private market power. The National Advisory Commission on Rural Poverty recommends a program of action to end the neglect of the rural poor by both the commodity oriented agricultural programs and the urban-oriented poverty programs that have been developed in the mid-1960's. The *National Program of Research*

for *Agriculture* recommended increased research expenditures in each of 91 research categories.

A common deficiency of the program proposals that have emerged from the three commission studies is that there is no clear-cut linkage between program activities and program goals. It has not been possible to arrive meaningful estimates of program costs nor has it been possible to specify how far implementation of the program recommendations would move the economy toward achieving program objectives.

The policy reviews occurred simultaneously with the emergence of a new set of "third generation" program planning oriented budgeting techniques (PPB).⁸ The U.S. Department of Agriculture has, during the last several years, undertaken a substantial effort to recast its program activities and budget within a mission oriented framework in order to permit a more precise evaluation of the relationship between program activities and the several missions of the Department of Agriculture (Table 5).

There are, however, a number of difficulties that face any attempt to achieve greater precision in program analysis.

First, there exists in agricultural programs substantial ambiguity with respect to the relationships between program instruments and objectives. A higher value is placed on achievement of income objectives if the income goals are achieved through higher prices "in the market place" than through direct income transfers; a higher value is placed on achieving price objectives through voluntary restrictions on acreage planted than on mandatory marketing quotas. A higher value is placed on domestic and international programs of food aid which make use of surplus commodities than those which require programs which relate production targets to specific nutritional goals. Programs to increase nonfarm employment opportunities for rural people are more acceptable if jobs can be provided in a way that does not induce migration. Programs to relieve poverty by increased employment or through education and training are more acceptable than programs involving direct income transfers.

This ambiguity is reflected in the new budget classifications of the USDA. Both the annual and long term acreage diversion programs designed to balance production and utilization (classified under the heading of Farm Income) and the agricultural research programs designed to maintain and improve the rate of growth of agricultural productivity (Agricultural Production Capacity) are classified under the same general heading—"Income and Abundance." Programs to increase timber production (Timber) are, in contrast, classified under the heading of "Communities of Tomorrow". These difficulties of classification illustrate the ultimate difficulty of relating program instruments to objectives in evaluating the effectiveness of agricultural commodity programs—what weight should be given to farm income objectives relative to consumer welfare objectives in the analysis of any particular activity.

⁸ For a review of the evolution of public sector budgeting techniques from the limited objectives of expenditure control to the more recent policy orientation see Allen Schick (1966).

TABLE 5.—U.S. DEPARTMENT OF AGRICULTURE BUDGET OUTLAYS (FEDERAL FUNDS)

[In thousands of dollars]

Program category and subcategory	Fiscal year 1967 actual	Fiscal year 1968 actual	Fiscal year 1969 esti- mate	Fiscal year 1970 budget	Change, fis- cal year 1969/1970
Income and abundance:					
Farm income.....	1,882,885	3,350,206	3,599,339	2,816,568	-782,771
Agricultural production capacity.....	582,339	600,343	599,702	594,618	-5,084
Agricultural marketing and distribution system..	76,302	91,479	83,179	78,818	-4,361
Total, income and abundance.....	2,541,526	4,042,028	4,282,220	3,490,004	-792,216
Growing nations—new markets:					
Food for freedom.....	1,458,487	1,208,523	1,040,735	928,800	-111,935
Export market development.....	357,576	165,816	140,154	74,782	-65,372
Agricultural development.....	5,309	4,498	5,701	10,461	+4,760
International agricultural services.....	5,887	6,951	7,426	7,541	+115
Total, growing nations—new markets.....	1,827,259	1,385,788	1,194,016	1,021,584	-172,432
Dimensions for living:					
Diets and nutrition.....	705,908	914,193	1,236,565	1,469,165	+232,600
Health and safety.....	71,914	83,418	110,412	133,355	+22,943
Education and training.....	21,070	20,731	22,421	23,366	+945
Services for living.....	45,535	49,517	47,290	46,455	-835
Total, dimensions for living.....	844,427	1,067,859	1,416,688	1,672,341	+255,653
Communities of tomorrow:					
Community development services.....	21,480	29,416	33,996	42,347	+8,351
Housing.....	14,704	33,540	-71,105	65,120	+136,225
Public facility and business expansion ¹	327,640	507,463	548,208	521,749	-26,459
Resource protection and environmental im- provement.....	227,057	234,341	245,837	214,678	-33,159
Recreation, wildlife, and natural beauty.....	48,463	61,034	56,926	68,834	+11,908
Timber.....	232,394	232,490	243,412	243,754	+342
Total, communities of tomorrow.....	871,738	1,098,284	1,057,274	1,156,482	+99,208
General support:					
General administration.....	3,728	4,335	4,722	5,052	+330
Program support.....	36,153	42,744	45,254	44,397	-857
Total, general support.....	39,881	47,079	49,976	49,449	-527
Total.....	6,124,831	7,641,038	8,000,174	7,389,860	-610,314

¹ Total budget outlay is as follows:

	Fiscal year 1968 actual	Fiscal year 1969 estimate	Fiscal year 1970 budget
Total budget outlay (Federal funds).....	\$7,640,836	\$8,000,249	\$7,389,934
General administration working capital fund.....	204	-75	-64
RCDS advances and reimbursements.....	-2	-	-10
Total.....	7,641,038	8,000,174	7,389,860

A second major difficulty in agricultural program analysis stems from the close integration of commercial agriculture with other sectors of the U.S. and world economies. The program instruments available to the U.S. Department of Agriculture and to the House Committee on Agriculture and Forestry and the Senate Committee on Agriculture have significantly less "leverage" on the behavior of the agricultural sector than formerly. More than half of research related to agricultural productivity is now conducted outside the public sector. It seems likely that by 1980 less than 25 percent of production related agricultural research will be conducted by the public sector. One result of this shift is that the rate of productivity growth in agriculture is becoming only marginally sensitive to changes in the rate of public

sector investment in agricultural research. The program instruments available to the Department of Agriculture to match agricultural production and utilization are also becoming less effective. The accelerated depreciation and capital gains provisions embodied in tax legislation has a substantial effect on the rate of capital investment in agriculture and the distribution of ownership of farm assets. Most of the legislation concerned with rural education, rural development policy, and with poverty in rural areas outlined earlier in this paper originates outside the Agriculture Committees and is administered by agencies other than the Department of Agriculture.

A third major limitation in attempts to achieve greater precision in the relationship between program instruments and objectives stems from limitations in analytical capacity. Any attempt to achieve a completely consistent evaluation of the relative effectiveness of program activity in relation to a broadly defined set of national goals would place an impossible overload on present analytical capacity.

It is possible to visualize a model, or a series of interrelated models, which would fully incorporate the behavior of the product, current input and capital, labor, land, and consumer goods markets at a sufficiently disaggregated level to permit the analyst to trace the effect of changes in direct public expenditures or program decisions at any point in the system on the flow of commodities and the distribution of income among firms and individuals in both the agricultural and nonagricultural sectors of the economy. There is no prospect that such a system will become operational in the near future.

Economic research can, however, make important contributions to program analysis at two interrelated levels. At one level are efforts to understand and to quantify economic behavior of particular markets—the market for soybeans, cotton, or hired farm labor, for example. At a second level efforts can be made to relate program activities and objectives at a sufficiently low level of aggregation to permit fairly broad agreement on program objectives. Efforts to analyze the impact of alternative schedules of payments limitations under the commodity stabilization programs on (a) income distribution and (b) on compliance with commodity program production targets or to estimate the effects of size and organization of experiment stations on the productivity of research investment are examples. By and large such research would focus directly on the individual program activities which make up a particular program subcategory (table 5). The accuracy and precision of program analysis for particular activities will depend on the extent to which the structural relationships describing the behavior of the markets which through the impacts of the program are transmitted have been investigated.

The first two limitations on the effectiveness of program analysis within the U.S. Department of Agriculture—(a) ambiguity with respect to the relationship between program instruments and objectives and (b) interdependence between the agricultural and nonagricultural sectors of the economy would become less restrictive if the focus of responsibility for program analysis were shifted, to a greater degree than now, from the Department of Agriculture to the Executive Office of the President. The integration of the agricultural sector into the national economy has been emphasized throughout this paper. The organization and responsibilities of the Department of Agriculture have

not yet fully reflected these economic changes. As a result program analysis conducted primarily within the Department will in some cases result in greater efficiency in the conduct of activities which should not be performed at all. At a very minimum the Executive Office of the President should be staffed to interact more formally with the several departments and agencies in establishing program objectives and in coordinating the program analysis for activities which impinge on the agricultural industry and on rural communities.

REFERENCES

1. Association of State Universities and Land Grant Colleges and U.S. Department of Agriculture, *A National Program of Research for Agriculture*, U.S. Department of Agriculture, October 1966.
2. Bachmura, F. T., "The Manpower Development and Training of 1962—Its Significance for Rural Areas," *Journal of Farm Economics*, 45:61-72, February 1963.
3. Beale, Calvin L., "Demographic Dimensions of U.S. Rural Economic Policy," paper presented at the annual meeting of the American Economics Association, Chicago, December 1968.
4. Bird, A. R., *Poverty in Rural Areas of the United States*, USDA ERS AE Report No. 63, November 1964.
5. Bogue, A. G., *Money at Interest: The Farm Mortgage on the Middle Border*, Cornell University Press, Ithaca, 1955.
6. Bonnen, J. T., "Emerging Public Policy Orientations and New Programs in Rural Life" in *Policies Affecting Rural People*, North Carolina Agricultural Policy Institute, Raleigh, April 1966, pp. 59-76 (also abstracted in *Agricultural Policy Review*, 6:10-12, January-March 1966).
7. Bonnen, J. T., "Rural Poverty: Programs and Problems," *Journal of Farm Economics*, 48:452-465, May 1966.
8. Booth, E. J. R., "Discussion: Economic Impacts of Development Policies and Programs," *Journal of Farm Economics*, 48:449-451, May 1966.
9. Borts, G. H., "Returns Equalization and Regional Economic Growth," *American Economic Review*, 50:319-347, June 1960.
10. Borts, G. H. and J. C. Stein, "Regional Growth and Mobility in the United States," *Schweizerische Zeitschrift für Volkswirtschaft und Statistik*, 98:290-321, 1962.
11. Boyne, David, "Changes in the Income Distribution in Agriculture," *Journal of Farm Economics*, 47:1213-1224, December 1965.
12. Breimyer, H. F., *Individual Freedom and Economic Organization of Agriculture*, University of Illinois Press, Urbana, 1965, pp. 115-135.
13. Bryant, W. K., "The Changing Effects of Nonfarm Business Expansion on Farm Costs," *Journal of Farm Economics*, 44:1077-1084, November 1962.
14. Cochrane, W. W., *The City Man's Guide to the Farm Problem*, University of Minnesota Press, Minneapolis, 1965, pp. 194-226.
15. Council of Economic Advisors, *Economic Report of the President and Annual Report of the Council of Economic Advisors*, U.S. Government Printing Office, Washington, January 1966, pp. 134, 135.
16. Dernberg, Thomas and Kenneth Strand, "Hidden Unemployment 1953-62, A Quantitative Analysis by Age and Sex," *American Economic Review*, 54:71-94, March 1966.
17. Edwards, E. E., "American Agriculture—The First 300 Years," *Farmers in a Changing World*, (1940 Yearbook of Agriculture) U.S. Government Printing Office, Washington, 1940, pp. 171-276.
18. Firch, R. S., "Stability of Farm Income in a Stabilizing Economy," *Journal of Farm Economics*, 46:323-340, May 1964.
19. Fuller, Varden, "Hired Farm Labor in the West" in NACRP, *Rural Poverty in the United States*, U.S. Government Printing Office, May 1968, pp. 421-441.

20. Gibson, W. L., C. J. Arnold, and F. D. Aigner, *The Marginal Value of Flue-Cured Tobacco Allotments*, Virginia Agricultural Experiment Station Technical Bulletin 156, January 1962.
21. Hathaway, D. E., "Agriculture in an Unstable Economy Revisited," *Journal of Farm Economics*, 41:487-499, August 1959.
22. ———, "Migration from Agriculture: The Historical Record and Its Meaning," *American Economic Review*, 49:379-391, May 1960.
23. ———, *Government and Agriculture: Public Policy in a Democratic Society*, Macmillan, New York, 1963, pp. 32-39.
24. Hendrick, J. L., "Effects of the Price-Support Program for Peanuts on the Sale of Farms," *Journal of Farm Economics*, 44:1749-53, December 1962.
25. Herdt, R. W., and W. W. Cochrane, "Farm Land Prices and Farm Technological Advance," *Journal of Farm Economics*, 48:243-264, May 1966.
26. Houston, D. B., and C. M. Tiebout, "Economic Implications of Regional Development Policies and Programs," *Journal of Farm Economics*, 48:440-449, May 1966.
27. Johnson, D. G., and R. L. Gustafson, *Grain Yields and the American Food Supply, An Analysis of Yield Changes and Possibilities*, University of Chicago Press, Chicago, 1962.
28. Koffsky, Nathan, "Farm and Urban Purchasing Power," *Studies in Income and Wealth*, vol. 11, National Bureau of Economic Research, New York, 1949, pp. 151-178.
29. Kolko, Gabriel, *Railroads and Regulation, 1887-1916*, Princeton University Press, 1965.
30. Kreps, J. M., "Rural Poverty: The Special Case of the Aged," in NACRP, *Rural Poverty in the United States*, U.S. Government Printing Office, May 1968, pp. 40-50.
31. Lampman, R. J., "Approaches to the Reduction of Poverty," *American Economic Review*, 55: 521-529, May 1965.
32. Larson, O. F., "Migratory Agricultural Workers in the Eastern Seaboard States," in NACRP, *Rural Poverty in the United States*, U.S. Government Printing Office, May 1968, pp. 442-460.
33. Long, E. J., and P. Dorner, "Excess Farm Population and the Loss of Agricultural Capital," *Land Economics*, 30:363-68, November 1954.
34. MacEachern, G. A. and V. W. Ruttan, "Determining Factor Shares" in L. B. Fletcher (ed.), *Market Power in the U.S. Economy*, Iowa State University Press, Ames, 1964, pp. 190-213.
35. Maier, F. H., J. L. Hendrick, and W. L. Gibson, *The Sale Value of Flue-Cured Tobacco Allotments*, Technical Bulletin No. 148, Virginia Agricultural Experiment Station 1960.
36. Moore, E. J., "Discussion: Agriculture's Programs and Rural Poverty," *Journal of Farm Economics*, 48:465, 466, May 1966.
37. Murphy, K. R., "Spending and Saving in Urban and Rural Areas," *Monthly Labor Review*, vol. 88, No. 10, October 1965, pp. 1169-1176.
38. National Advisory Commission on Food and Fiber (NACFF), *Food and Fiber for the Future*, U.S. Government Printing Office, July 1967.
39. National Advisory Commission on Rural Poverty (NACRP), *Rural Poverty in the United States*, U.S. Government Printing Office, May 1968.
40. National Advisory Commission on Rural Poverty (NACRP), *The People Left Behind*, U.S. Government Printing Office, 1967.
41. National Commission on Food Marketing (NCFM), *Food from Farmer to Consumer*, U.S. Government Printing Office, 1966 (Also Technical Studies, No. 1-10).
42. Nelson, Lowry and George Donohue, "Social Change in Goodhue County, 1940-65," University of Minnesota Agricultural Experiment Station Bulletin 482, 1966.
43. Owen, W. F., "The Double Developmental Squeeze on Agriculture," *American Economic Review*, 56:43-70.

44. Price, Daniel O., "The Negro Population of the South," in NACRP, *Rural Poverty in the United States*, U.S. Government Printing Office, May 1968, pp. 13-39.
45. Puterbaugh, H. L., "Purchasing Power of Urban, Rural Non-Farm, and Rural Farm Income, 1955," *Agricultural Economics Research*, 13:89-94, July 1961.
46. Ruttan, V. W., "Discussion of Development and Adjustment," *Journal of Farm Economics*, 36:1158-1160, December 1954.
47. ———, "The Potential in Rural Industrialization and Local Economic Development," in E.O. Heady, et al. (eds.). *Agricultural Adjustment Problems in a Growing Economy*, Iowa State College Press, Ames, 1958, pp. 185-197.
48. ———, "The Human Resource Problem in American Agriculture," in Fox, K. A., et al. *Farming, Farmers and Markets for Farm Goods*, Committee for Economic Development, New York, 1962, pp. 73-116.
49. ———, and L. T. Wallace, "The Effectiveness of Location Incentives on Local Economic Development," *Journal of Farm Economics*, 44:968-978, November 1962.
50. ———, E. M. Babb, and L. T. Wallace, "Evaluation of Area Redevelopment Loans to Agricultural Firms," *Journal of Farm Economics*, 45:1022-1026, December 1963.
51. ———, "Agricultural Policy in an Affluent Society," *Journal of Farm Economics*, vol. 48, No. 5, December 1966, pp. 1100-1120.
52. Scharlach, W. C., and G. E. Schuh, "The Land Market as a Link Between the Rural and Urban Sectors of the Economy," *Journal of Farm Economics*, 44:1406-1411, December 1962.
53. Schick, Allen, "The Road to PPB: The Stages of Budget Reform," *Public Administration Review*, December 1966, pp. 243-258.
54. Schultz, T. W., *Agriculture in an Unstable Economy*, McGraw-Hill, New York, 1945.
55. ———, "Reflections on Poverty Within Agriculture," *Journal of Political Economy*, vol. 58, No. 1, February 1950, pp. 1-15.
56. ———, "Underinvestment in the Quality of Schooling: The Rural Farm Areas," *Increasing Understanding of Public Problems and Policies: 1965*, Farm Foundation, Chicago, 1965.
57. Scofield, W. H., "Dominant Forces and Emerging Trends in the Farm Real Estate Market," in *Factors in the Land Market*, University of Illinois Agricultural Experiment Station Special Publication No. 8, October 1965.
58. Sjaastad, L. A., "Occupational Structure and Migration Patterns," in *Labor Mobility and Population in Agriculture*, Ames, Iowa State University Press, 1961, pp. 8-28.
59. Stevens, R. D., *Elasticity of Food Consumption Associated with Changes in Income in Developing Countries*, USDA ERS, Washington, 1965 (Foreign Agricultural Economics Report No. 23).
60. Swerling, B. C., "Income Protection to Farmers: A Possible Approach," *Journal of Political Economy*, vol. 67 (April 1959), pp. 173-186.
61. Tang, A. M., "A Critical Analysis of the Area Redevelopment Act and Local Subsidies to Industry" in *Problems of Chronically Depressed Rural Areas*, Agricultural Policy Institute, North Carolina State University, Raleigh, 1965, pp. 85-99.
62. Towne, Marvin and Rasmusson, Wayne, "Farm Gross Product and Gross Investment in the 19th Century" NBER, SIW, *Trends in the American Economy in the Nineteenth Century*, Princeton, 1960, pp. 255-315.
63. Tweeten, L. G., "The Income Structure of Farms by Economic Class," *Journal of Farm Economics*, 47:207-221, May 1965.
64. University of Minnesota, Department of Agricultural Economics, "The Food and Agriculture Act of 1965—Its Implications for Minnesota," *Minnesota Farm Business Notes*, special issues, 1965.
65. USDA, *Development of Agriculture's Human Resources, A Report on Problems of Low-Income Farmers*. A report prepared for the Secretary of Agriculture, Washington, April 1955.

66. USDA, *Foreign Agricultural Trade of the United States*, February, March 1964.
67. U.S. Commission on Civil Rights, *Equal Opportunity in Farm Programs: An Appraisal of Services Rendered by Agencies of the U.S. Department of Agriculture*, U.S. Government Printing Office, Washington, 1965.
68. Wadsworth, H. A. and J. M. Conrad, "Leakages Reducing Employment and Income Multipliers on Labor Surplus Rural Areas," *Journal of Farm Economics*, 44 :1197-1262, December 1965.

POLICY ANALYSIS IN THE POST OFFICE

BY JOHN HALDI*

John Haldi is president of Haldi Associates, Inc., New York City. Following the Report of the President's Commission on Organization of the Post Office, there has been much discussion of issues concerned with the fundamental reorganization of the postal system. In this paper, Dr. Haldi discusses the needs of policy analysis relating to reorganization of the postal system, the chronic shortage of capital equipment with which it is afflicted, and the directions for revision in the postal rate structure.

In regard to postal system reorganization, Dr. Haldi presents the range of options available to the Congress and suggests that "it would not be inappropriate for Congress to request in-depth studies on any or all of them."

One of the most serious problems in the Post Office relates to its failure to replace equipment and facilities as these have become outmoded or obsolete. "Cumulative underinvestment in facilities since the end of World War II is approximately \$840 million." Dr. Haldi examines the feasible alternative courses of action to close this investment gap: in financing additional investment, determining the best investment strategy, and organizing responsibility for investment planning. He points out the relevance of careful analysis in choosing the best options in these areas.

Finally, Dr. Haldi cites the need for additional economic analysis to guide policy decisions in the pricing of postal system outputs. He judges that substantial innovation would result from careful study and consideration of volume discounts, incremental pricing, delivery service pricing, and the development of new postal products.

Introduction

Until quite recently long-range planning and the systematic analysis of alternative programs within the Post Office Department were conspicuous primarily by their absence. Planning future postal development was limited in the main to design and engineering of new facilities. Since 1966, however, the level of analysis and the outlook for better and more intensive long-range planning has been significantly improved by substantial research emanating from three sources:

- The President's Commission on Postal Organization, better known as the Kappel Commission.
- The newly created Bureau of Research and Engineering.¹
- The Office of Planning and Systems Analysis.*

¹ Established in 1966 as a Bureau; formerly the Office of Research and Engineering. The proposed 1970 budget for the Bureau of Research and Engineering is over 200 percent larger than the 1966 budget, an indication of the exponential growth in the Department's research capability.

*This paper was prepared prior to the recent formation of the Bureau of Planning, Marketing, and Systems Analysis.

Despite the work done by these three groups, a notable accumulation of worthwhile ideas remain for future intensive study, research, and subsequent implementation. It is natural, of course, for any organization as large as the Post Office to have several major policy issues deserving of study at any one time, particularly after so long a period of general neglect. Many of these can wait, however. In consequence of the wide-ranging studies and research done over the past 3 years we have now reached a point where the pressing need is to move toward decisions and action. Discussion in this paper is therefore limited to three major issues on which Congress, the administration, and the Post Office should focus their immediate attention. They are—

—Reorganization.

—Reform of the postal rate structure.

—The chronic shortage of capital equipment and facilities.

These are not new issues. They have all been discussed at length in the report of the Kappel Commission. The problems of under investment and the need for rate reform have received substantial analytic attention both within the Post Office and from concerned outside groups. Reorganization of the Post Office was recommended by former President Johnson both in his final state of the Union address and in his fiscal 1970 budget message. In the unlikely event that any of these issues require still further analysis, then all available analytic efforts should be focused on them in order to speed their resolution.

II. REORGANIZATION OF THE POST OFFICE

A. THE KAPPEL REPORT

The most important postal policy issue, in my opinion, is the Kappel Commission proposal to reorganize the Post Office into a Government-owned corporation.² This is the most sweeping organizational change in the Post Office ever officially endorsed by the executive branch and makes the Kappel report one of the most significant Post Office documents of the 20th century. This proposal deserves an immediate, impartial public hearing before Congress, even though certain influential groups would like to see it quietly shelved.

By way of historical perspective, it should be noted that recommendations to make the Post Office less subject to political influence are not new. For example, the former Republican Postmaster General, Arthur Summerfield, recommended such a change after leaving office, and there have been many similar suggestions from nonofficial sources. It was not, however, until April 4, 1967, when Postmaster General Lawrence O'Brien publicly advocated far-reaching reorganization of the Post Office that such a radical departure became official policy. Mr. O'Brien's recommendation to President Johnson led directly to the establishment of the Kappel Commission. Subsequently, in January of this year, President Johnson's state of the Union message said, "I believe that we should reorganize our postal system along the lines

² "Towards Postal Excellence," the report of the President's Commission on Postal Organization (the Chairman of which was Mr. Frederick R. Kappel), June 1968; Government Printing Office, \$1.25.

of the Kappel report." When such high-level sources recommend sweeping reorganization it should not be dismissed without a fair and extensive public hearing.

Turning now to the reorganization proposal itself, the principal issue boils down to deciding whether one considers the Post Office (a) a business which should respond to market forces and be managed on a businesslike basis; or (b) a Government agency, whose principal purpose is to execute governmental programs, with questions of economic efficiency held subservient to desired political goals.

Clearly, Mr. Summerfield, Mr. O'Brien, Mr. Johnson, and Mr. Kappel are on record as favoring a businesslike, nonpolitical approach. In support of this position the Kappel report noted that "the mail is used primarily as a commercial medium; transactions and advertising together constitute two-thirds of all mail volume."³ The report also noted that the job of processing and delivering the mail is essentially a business operation with no inherent political overtones.

It is widely recognized, however, that the proposed reorganization runs counter to some vested interests (primarily mail-user groups and postal labor unions), bureaucratic inertia, precedent, and a long and generally proud tradition. The motivation behind some of this opposition seems fairly obvious. For example, for many years Congress has used the postal rate structure as a means of implicitly subsidizing many groups of mailers. The Kappel Commission recognized the strong political appeal possessed by these subsidies when it specifically advocated continuing them under the new organization.⁴ However, the Commission also recommended that (1) the amount of subsidy be calculated according to sounder economic principles, (2) the total of all recognized subsidies be limited to 3 percent of total cost, and (3) all subsidies be paid by other mailers. In other words, the Kappel report would shift the ground rules which apply to ratemaking, and would shift the burden of current subsidies from the general taxpayer to various mailers. Presumably at some unforeseeable time in the future certain subsidized mailers might find their postal rates different from what they would be under direct congressional supervision. For these and other reasons, political reality may differ radically from what otherwise appears to be a sound proposal. If the Kappel recommendation does not coincide with political reality, then the political process will have to find other means of coming to grips with the well-known, but basic and serious economic problems of the Post Office. Briefly, these are:

- *Continuing deficits.*—The recent series of rate increases do not cover the existing postal deficit. This deficit will continue to grow each year, unless Congress enacts further substantial increases.
- *Extreme capital shortage.*—Many major postal facilities are obsolete and inadequate, and new construction is not sufficient to replace old facilities, much less provide for the increasing mail volume.
- *Stagnant productivity.*—Overall productivity is not increasing, and it may actually be declining.

³ Kappel Report, p. 73.

⁴ Kappel Report, p. 62.

• *Lack of managerial incentive for increased efficiency.*— Needed postal investments in facilities and equipment, even when made, do not yield expected savings in money or manpower because managers lack the proper incentives and managerial tools to capture such savings.

B. ALTERNATIVES

In addition to the specific course of action recommended by the Kappel Commission, other courses of action also may be worth considering. Congressional options include: (a) other organizational forms; (b) changes and improvements within the framework of the existing organization, and (c) increased ability of private enterprise to compete with the Post Office.

Each of these options is discussed briefly below. It would not be inappropriate for Congress to request in-depth studies on any or all of them.

1. *Other organizational forms.*—The various organizational possibilities available to Congress range from (a) sale of the entire Post Office to the public, with the Post Office thereafter functioning as a regulated private utility, to (b) establishment of the Post Office as a quasi-Government corporation with considerably less autonomy from political forces than the Kappel Commission recommended. Within this spectrum of reorganization possibilities, the Commission's recommendations seem to fall about some place in the middle.

2. *Change within the existing framework.*—It is my opinion that if the Kappel recommendations were carried out as proposed, this would probably be one satisfactory way of effecting radical improvement in the postal service. A sweeping restructuring may well be the minimum requirement for overcoming the problems created by tradition, inertia, and lack of adequate managerial incentives. However, if Congress is unwilling to authorize such a reorganization, then many piecemeal improvements can and should be made.⁵ Along this line, let me cite four specific examples which Congress might consider.

i. Remove all restrictions on the competitive procurement of transportation by the Post Office. Strong competition is still the best known method for protecting the public interest and, wherever possible, Government agencies should encourage and benefit from such competition, not stifle or suppress it.

ii. Abolish the 15,000 to 25,000 small post offices which almost certainly are no longer necessary for efficient operations. These are now being phased out gradually at a rate of about 500–600 per year, but the process can be greatly accelerated by replacing many of these unnecessary post offices with new and improved self-service installations. This step would save tens of millions of dollars annually.

iii. Make every postal supervisor, including postmasters, career civil servants free from any geographical restrictions or political qualifications. Clearly it is not necessary to reorganize the entire Post Office in order to achieve this one highly desirable change.⁶

iv. Change the existing ratemaking procedure. Establish an independent body to review and pass on rate proposals which would then

⁵ According to the Wall Street Journal (Jan. 15, 1969, p. 3) Postmaster General Marvin Watson proposed this policy to President Johnson prior to the State of the Union Message but Mr. Johnson rejected it in favor of the Kappel report.

take effect unless Congress specifically objected. In other words, move the ratemaking process one step away from the political arena by establishing a procedure more or less similar to the one recently proposed for congressional and executive salaries.

3. *Competition from private firms.*—Most discussions of potential solutions to postal problems tend to overlook or lightly dismiss the service improvement which could come from competition among independent private firms. Provided the preconditions for effective competition exist, the competitive solution supplies the best safeguards to the consumer and the best incentives for the producer to operate efficiently.

At present the so-called private express statutes prohibit anyone from carrying first-class mail in competition with the Post Office. No other class of mail has such a blanket prohibition of competition, however, and all other classes of mail do face some form of competition from the private sector. To cite two specific examples:

i. Since the recent postal rate increase various firms in several parts of the country have been established to accept, distribute, and deliver direct mail advertising to households.

ii. United Parcel Service serves an increasingly large part of the country. This firm is generally agreed to be far ahead of the Post Office in operating efficiently, reducing damage and speeding delivery of small packages.⁷

Some may differ as to whether the postal system meets the preconditions for effective competition.⁸ However, most observers believe that some competition would arise under the existing rate structure, if it were permitted. In fact, the assertion is usually made that repeal of the private express statutes would likely result in "cream skinning" by new entrants. This means, in other words, that the Post Office enjoys a surplus or "profit" from delivery to high-density business areas of major cities, and new entrants would probably concentrate their efforts in these areas (to their profit and at the expense of the Post Office). The defenders of postal monopoly seem to accept without doubt or hesitation the "principle" that certain mailers should be forced to pay a higher than competitive price in order to subsidize other groups of mailers and parts of the system. But they offer no real excuse as to why certain mailers should be "taxed" in order to contribute to deficits which arise from low-density, high-cost elements of the system which

⁶ Postmaster General Blount has recently moved to make postal appointments non-political. In addition, however, the Post Office needs authority to recognize and reward (via increased responsibility and compensation) those managers who exhibit better than average executive and supervisory ability.

⁷ It should be noted that on the subject of competition I differ from the generally excellent studies done for the Kappel Commission. One study (Arthur D. Little, "The Market for Postal Services," May 1968 Report to the President's Commission on Postal Organization, pp. 6-11) concluded that since the Post Office has not adopted the obviously superior methods of United Parcel Service, competition has therefore not been effective in leading the Post Office to adopt more productive methods. The study then proceeds to dismiss (lightly, I would say) the potential benefits from competition. I contend that if anything the study has proven that competition both is and can be effective, and that what the situation really requires is more competition, not less.

⁸ The argument is frequently heard that the postal system is subject to significant economies of scale. I have treated this at greater length in my paper "The Value of Output of the Post Office Department" in *Economics of Public Output* (forthcoming). National Bureau of Economic Research. I argue there (a) that for much of the system the existence of economies of scale is not obvious and such economies may be virtually nonexistent, and (b) that those who would suppress competition in favor of monopoly on the grounds that economies of scale are widespread should bear the burden of proof. That is, some documented evidence on economies of scale should be offered, but to the best of my knowledge not one authoritative study has as yet been published on the subject. The fact is, most people simply assume that economies of scale exist, without any worthwhile evidence to back up their assumption.

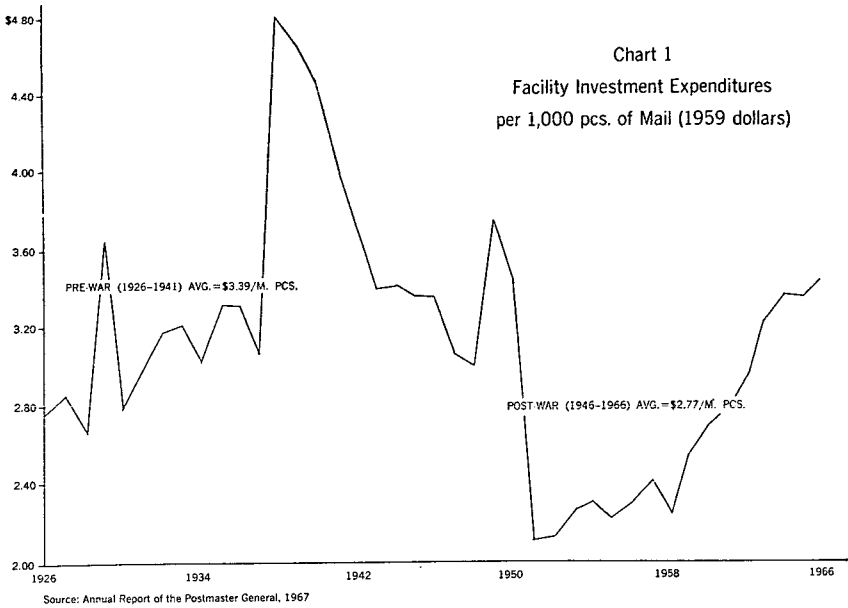
are not offset by adequate revenues, nor do they present strong arguments for depriving the public of the benefits of competition. Refutation of this sort of monopolistic reasoning is the major assignment of both the Antitrust Division of the Department of Justice and the Federal Trade Commission. With competition from private firms I would expect to observe the introduction of many cost-cutting innovations because such firms would have maximum incentive to experiment and cut costs. Considering the extremely slow pace at which innovation has been introduced by the Post Office, the potential gain seems well worth any attendant risk.

To conclude this entire discussion on reorganization, let me say that I join those who urge Congress to consider reorganizing the Post Office along the lines recommended by the Kappel Commission. The Kappel report is now before us, and it deserves top congressional priority in postal matters. Congress should hold public hearings and dispose of the issue which it poses in one way or another. The public interest demands that such an important recommendation not be left in limbo; this course would be far more harmful than either outright acceptance or rejection. If reorganization is not possible, then I recommend that Congress move quickly to repeal the private express statutes and approve legislation to effect other desirable changes and improvements within the existing postal organization. However, only by sweeping reorganization can we hope to have tomorrow's mail moved by economically motivated managers.

III. CAPITAL INVESTMENT

For the last 30 years the Post Office has not spent enough on its facilities to meet the needs of normal replacement. Expansion to accommodate the burgeoning mail volume and modernization necessary to keep abreast of the improving technology have been even more deficient. As a result, postal officials have at times estimated that as much as \$5 billion is needed to meet new investment required by increasing mail volume and to replace and expand currently outmoded and inadequate equipment and facilities. My own rough estimates tend to confirm this order of magnitude.

The long-term trend in expenditures on facilities (in constant 1959 dollars) is shown as a percentage of mail volume in chart I. On this basis spending for facilities reached its peak in 1939 and, despite our general affluence and economic growth, such expenditures since World War II have average only 58 percent of the 1939 peak. Assuming that 30 to 40 years is the maximum feasible economic life of a major postal facility, replacement needs over the next decade will become critical, particularly as the 1930 vintage post offices become untenable for continued operations. A good example is the main Chicago Post Office, built in the early 1930's and one of the largest structures of its kind in the world; it is now generally acknowledged to be inadequate for the demands placed upon it. If we use the 1926-41 average as a norm for an adequate facility investment program, cumulative underinvestment in facilities since the end of World War II is approximately \$840 million, in 1968 dollars. However, recognizing the ambitious facility construction programs of the late 1930's and early 1940's—facilities which are now presumably either fast approaching or at the end



of their economic efficiency—I would conservatively estimate the current underinvestment at over \$1 billion. Remember, too, that this amount is for facilities alone and does not include required investment in equipment and mechanization, nor new spending requirements.

Appropriations for postal equipment have risen sharply in recent years. Here too, however, a serious investment gap was allowed to develop in the years immediately following World War II. Considering this gap plus technological improvements in recent decades, I doubt that current equipment funding levels are exploiting all potentially profitable investment opportunities. The roots of this under-investment problem⁹ were critically examined in the Kappel report. Primarily, the Commission found two major problem areas:

The appropriations process, through which most capital outlays must be financed.

The planning process, which is unduly complex for new facilities.

Since both capital and operating expenditures are part of the same general appropriations bill, all too frequently the seemingly more deferrable capital outlays yield priority to the less easily postponed operating expenses and to national programs of seemingly higher urgency. Even though we all realize that the Post Office cannot live on its capital base forever, all too often we fail to read the subtle economic signals which indicate the cost of failing to act.¹⁰ We have already begun to pay a high price for past neglect in this area.

⁹ Circumstantial evidence also reinforces the overall conclusion that the Post Office suffers from serious underinvestment. Net assets per telephone employee currently averages \$36,000, while the average for each postal employee is only \$1,000.

¹⁰ Obsolete buildings mean poor working conditions, which in turn leads to poor morale of the work force, bad labor relations and high turnover, with subsequent high cost. Inadequate mechanization can and is being overcome, in the case of the Post Office, by uneconomic expansion of the work force.

The planning process for a new facility has been detailed in the Kappel report, and there is no need to repeat it here. Suffice it to say that the process is intricate and time-consuming. Many projects are subject to long intervals, of as much as 10 years, between project initiation and completion. Given current staffing levels and the fact that virtually every major project is designed by Post Office staff, the total number of projects can be contained in the "pipeline" is severely limited.

In order to significantly speed up the rate of investment and substantially reduce the shortage of equipment and facilities, decisions and action are needed in three critical areas:

- *Financial*.—provide the necessary funds.
- *Strategic*.—determine the best investment strategy.
- *Organizational*.—reorganize responsibility for investment planning within the department to facilitate the most efficient implementation of the selected investment program.

Now we examine briefly several of the more feasible alternative courses of action open in each of these three areas.

A. FINANCIAL

Failure by Congress to adopt the full set of Kappel Commission recommendations will require that the administration and Congress come to grips with the problem of providing adequate funds for postal modernization. One obvious solution, of course, is for Congress simply to appropriate the required money, as it did during the 1930's. However, given the continuing pressure on the Federal budget and the funds required, the direct appropriations route may be as inadequate in the future as in the past. Should this prove to be the case, we should then examine the alternative funding schemes that are available and consider them on their merits. To illustrate the sort of alternatives which are available, I will touch upon three proposals which have been suggested from time to time. These are: (1) Direct issuance of postal bonds; (2) A postal revolving fund, and (3) Increased leasing authority.

Note that I have not fully analyzed the merits of any of these proposals. Hence their inclusion here is for illustrative purposes only, and is not a recommendation for any of them.

1. *Postal Bonds*

This proposal would empower the Department to sell debentures directly to the general public, with annual sinking fund and interest payments being met from postal revenues. A variant of this plan is to establish a Government-owned postal modernization authority empowered to take title to all postal properties, which would then be leased back to the Department on an annual basis. This corporation would have authority to float its own debt issues secured by the annual rental income received from its postal properties. In effect, this scheme would allow the Post Office to lease its facilities and equipment and annualize the initial construction cost. The front-end money would be borrowed at interest rates close to those prevailing for U.S. Treasury bonds.

2. *Revolving Fund*

It also has been proposed that a portion of postal revenues be diverted for several years into a postal modernization fund. The Department would then be allowed to draw on these funds for its modernization program over and above the annual appropriation for this purpose. These special drawings would be repaid, with interest, over the life of the projects which they financed. In this way the fund would eventually be placed on a self-sustaining basis and this lending-repayment mechanism would conceivably provide some extra oversight or economic discipline on future postal investment. However, siphoning off revenues for this purpose will increase the postal deficit by the amount allocated to the revolving fund.

3. *Leasing*

The Department has increasingly been led to lease its facilities from the private sector because at present this is the only allowable method of annualizing facility payments; and it is also the fastest way to acquire new space. Leasing authority is now limited, but it could be increased substantially. In passing, note that with the two alternative financing plans discussed above—postal bonds and revolving fund—the Government would still have to raise all money for immediate capital expenditures on a current basis. Under the recently adopted “comprehensive” method of presenting the budget, any such borrowing or appropriations would be reflected in the current budget and would thus reduce any overall surplus or increase any overall deficit. With a lease plan the private sector will initially provide all necessary capital; however, this seeming advantage is offset to some degree by higher implicit costs.¹¹

B. INVESTMENT STRATEGY

Assuming that sufficient funds are somehow made available for an adequate capital improvement program, a major challenge is selection of the optimum strategy to follow over the next 15 to 25 years. This choice depends essentially upon one's assessment of future improvements in postal technology. Basically, the choice is between three broad alternative programs: (1) automation and mechanization, (2) modular post offices, and (3) a new postal system. All three of these alternatives, of course, contain many common elements, but each also represents a different emphasis in approach.

1. *Automation/mechanization*

The major emphasis in this program is on efficient utilization of existing floor space and substitution of capital for labor, with less emphasis on the addition of net floor space. Under this program increased attention will be given to the postal R. & D. program. In addition, the standardization of envelope sizes, address locations, and other inputs to the mail flow will quickly become matters of prime importance.

2. *Modular Post Offices*

The emphasis here, instead, is on an ambitious construction program which utilizes existing types of equipment and procedures to the maximum extent possible. The difference between this program

¹¹ Principally local taxes, which are not a “cost” to Government as a whole since they are a Federal-local transfer payment.

and the preceding one is strictly a matter of degree. In this program major attention is given to construction, engineering, and facility procurement, with less emphasis on research and development. Given the existing state of postal technology, this program would probably have the least effect on existing postal procedures and, over the long run, it would result in less substitution of capital for labor. Because of this last factor, the modular post office concept also stresses locating these new facilities so as to take full advantage of labor force concentrations.

3. *New Postal System*

The most radical approach of all is to allow the current system to continue to operate "as is" and concentrate instead on a massive systems study which

- Projects all postal communication requirements of the post-1980 economy;

- Designs a product line to meet those requirements;

- Handles these product lines with a newly designed, fully integrated transportation and processing *system* which includes new machinery, facilities, and procedures; and

- Lays concrete plans for the gradual introduction of the system.

This system might represent a complete break from established postal practices as if, for example, it were designed around a satellite-linked, computer-directed series of ground stations for facsimile transmission and automatic routing of written messages.

No final recommendation on these alternative strategies can be made here. Postal management will have to apply its own criteria in order to determine the optimal strategy. However, my own guess is that if the criterion is economic efficiency, as measured by return on investment, the choice will center on some variant of program No. 1, automation/mechanization. Given the current shortage of facilities, my principal recommendation is that we choose and follow through to completion one coherent logical program rather than continue to vacillate between competing programs of the type described above.

C. ORGANIZATION

Looked at from overall perspective, past management of the on-going investment program of about \$400 million a year leaves much to be desired. Any expanded investment program, no matter how well conceived or financed, is doomed if handled by traditional postal methods. The diffusion of responsibility, redundancy, and lack of analysis and systems planning in the present set-up have all been well documented in the Kappel report. Within the Department itself, this situation is generally acknowledged for what it is and the resulting inertia generally frustrates innovation and risk taking, and is responsible for the major delays between initiation and completion of construction, mentioned previously. Regardless of whether Congress adopts the Kappel recommendations, and regardless of whether Congress agrees to expand significantly total capital spending, postal management should, with all deliberate speed, take the following three actions to invest whatever capital funds it receives more wisely and expeditiously.

- Centralize authority and responsibility for design, construction and modernization of all facilities in one assistant Postmaster General and in one bureau.
- Establish a formal departmental capital budgeting system to determine investment priorities and ration available capital among competing projects.
- Introduce modern investment planning techniques to insure that systemwide effects of alternative capital programs are explicitly considered before funds are committed.

1. *Centralized Responsibility and Authority*

One Assistant Postmaster General should be given the authority to direct and control the capital investment program from preliminary planning and initial selection down through project completion. He should also have the responsibility for meeting scheduled deadlines and staying within target budgets. Some authority for small projects could be delegated to regional and field personnel, but only within carefully prescribed and limited budgets.

Facility planning responsibilities are now divided among four Bureaus (*Facilities; Finance and Administration; Operations; and Research and Engineering*), plus the Office of Planning and Systems Analysis, and the Chief Postal Inspector. None of the Bureau chiefs, however, has the authority to cut across Bureau lines when necessary to accelerate steps in the process or insure that deadlines are met. Since no one office has sole responsibility for meeting schedules, there can be little question why the schedule for most projects slips continually.

Viewing the organization from the outside, it is my guess that the Bureau of Facilities might be the appropriate place for such centralization of authority. The question of where authority should be centered, however, is less important than that it be centered somewhere before any new and ambitious programs are authorized. It is true that giving centralized authority for all major equipment and facility decisions to a staff organization can lead to occasional costly mistakes. But excessive delay on a majority of all major projects is also extremely costly. The Post Office can no longer afford the luxury of an operational veto over capital spending decisions or the delays inherent in present procedures. The viewpoint of those with experience in postal operations can be given adequate consideration by supplying an "investment Czar" with a cadre of experienced postal operations experts.

Within the centralized organizational responsibility advocated here, it is not unlikely that an ambitious construction program would be considerably more productive through extensive use of a "turn-key" construction *program* in which full authority for design and construction of facilities is placed in the hands of private contractors. A properly designed turn-key program could, by encouraging competition among individual contractors, markedly increase innovation in the design of postal facilities and sharply reduce the time between project initiation and completion.

Let me emphasize that this turn-key approach, if it is to be tried, needs to be a *program*—not simply one or two construction contracts for single facilities—and the program must, of course, be properly designed and executed. The Post Office has experimented with an

occasional turn-key project, but to my knowledge a turn-key program such as I have in mind has never been contemplated, much less attempted. To initiate such a program the Post Office should select 5-15 major facility projects and sponsor a design competition, as the Defense Department frequently does, covering the entire group of projects. The winner of the design competition should then be awarded one "turn-key" contract for construction of all the facilities. Moreover, in order to make this an ongoing program, this first contract should be followed in due course by successive similar contracts. With a major program of this sort the Post Office could probably interest major industrial firms in working on various systems approaches to design new postal facilities.

2. *Capital Budgeting System*

Despite a great deal of discussion and exploratory studies during the past several years, the Post Office still lacks an explicit capital budgeting system which systematically

- Identifies potential capital spending projects.
- Submits these projects to an economically sound screening process.
- Assigns a priority to feasible projects on the basis of a well-defined economic criterion such as the discounted cash flow rate of return.
- Conducts postinvestment audits which check on the assumptions made in the original analyses and uncover weaknesses in order to improve future studies.

Capital budgeting procedures such as these are established and essential features of today's well-run corporations. Indeed, they are taught routinely in all business schools. But such procedures are not found in the Post Office. For example, investment planning, as reflected in the capital budget, is typically a dominant concern of a headquarters staff organization and is a familiar part of the headquarters landscape in most large corporations. However, on the basis of my observations I venture to guess that less than 5 percent of the present Post Office Department headquarters personnel are now engaged in active support of the investment planning function. In-depth analysis of alternative projects and programs is impossible given the current level of effort.

It is my opinion that a complete capital budgeting system should be given top priority over all other "investment" problems. In fact, it would not be altogether unreasonable to suggest that capital spending be virtually halted pending development and installation of such a system. In today's times it is generally accepted as an article of faith that mechanization and spending on capital improvement is "good" and will result in increased efficiency. In the case of the Post Office, however, this is less than obvious. It is not evident that Post Office spending on mechanization has saved manpower or money. In fact, hundreds of millions of dollars spent on mechanization may have been wasted inasmuch as the Post Office has not captured any increased efficiency from such spending. The Post Office should be challenged to produce economically valid before-and-after studies showing productivity and return on investment of both individual installations

and the overall system, and these studies should be examined critically until it is fully established that postal investments are yielding an adequate return on each dollar invested.

3. *Systems Analysis*

Ideally the various Post Office activities should be studied and managed together as one fully interdependent system with one set of system objectives. But in practice the operation's sheer size, as well as the tradition of decentralization, has precluded an overall systems viewpoint. With modern management methods and tools, however, there is no necessity for this situation to persist. Current efforts to introduce the systems approach, via the planning-programing-budgeting (PPB) system, and otherwise, need to be encouraged and strengthened.

IV. POSTAL PRODUCT PLANNING

The Post Office offers the public a number of different services or "products," such as special delivery, money orders, registered mail, parcel post, or rental lock boxes at post offices. Also, within the scope of the more familiar "mail" service, such widely different types and levels of service are offered to various groups of postal customers that it is not unreasonable to think of separate "mail" and "delivery" products. For example, correspondence, advertising, merchandise, rural free delivery, once-a-day home delivery, or multiple business delivery can all be considered products of the Post Office Department.

Despite this wide variety of products and its avowed dedication of service to the public, the postal organization is neither product—nor market—oriented. The Kappel report indicated concern over the almost complete lack of routine marketing information on such vital questions as:

- Who uses postal services?
- What reasons do they have for using postal services?
- What are the probable consumer responses to changes in service?

Such information is easily garnered. For example, the market study which Arthur D. Little did for the Kappel Commission is a solid first step towards accumulation of a bank of market statistics and shows what can be accomplished. However, systematic gathering of market information can be virtually useless unless the organization makes decisions along service or product lines. This section of my paper examines the product-line concept as it applies to the Post Office and indicates several types of innovative changes which should be studied for possible introduction into the postal product line.

A. PRODUCT-LINE MANAGEMENT

The Post Office has no process either for periodically reviewing and evaluating existing postal products or for planning new postal products. Product decisions typically fall out of other processes, such as the annual budget cycle. For example: (a) Postal rate increases are usually an attempt to reduce the deficit to an acceptable level; and (b) Reductions in service, such as elimination of twice-a-day delivery, have been in response to budgetary pressures.

In essence, then, postal product decisions are primarily reactive. Change usually comes only as the result of external pressure, and the introduction of new products becomes increasingly rare—the last wholly new product, certified mail, was introduced in 1955. Unfortunately, raising postal rates or dropping postal products in response to budgetary pressures gives no assurance that the most effective product-line changes are being made. Suppose, for example, that everyone agreed the budget deficit should be reduced by “*x*” dollars. Traditionally this cut has been accomplished by eliminating some feature of an existing service. Careful analysis, on the other hand, could well lead to the conclusion that the addition of a new product or new feature to an existing service would improve the product line, increase revenues and thereby reduce the deficit by an equal amount. There will, however, be no incentive for such analysis until managers are assigned full-time responsibility for discovering postal needs and designing the most economically efficient package to meet those needs. Managers for groups of postal products should be created and given equal prestige with such managers as those now in the Bureau of Operations who are responsible for clerks, carriers, and so forth.

B. POTENTIAL INNOVATIONS

The latter part of the 19th and the early 20th century saw a spate of innovative postal marketing activity, such as the introduction of parcel post, special delivery, rural free delivery, and other products. In the last two decades, however, changes have usually been in the direction of eliminating or reducing products. For example, twice-a-day delivery in residential areas and the postal savings system were eliminated, and other products, such as postal money orders and parcel post insurance, are slowly but surely pricing themselves out of the market. Such innovations as have been made are mostly of the type that will save the Department money and/or shift part of the burden to the consumer. ZIP code, the VIM (vertical improved mail) program, and presort regulations for bulk mail are all innovations of this general type.

It should be clearly understood that I am not advocating the abolition of ZIP code. Quite the contrary—each of the above-mentioned examples represents the results of good, imaginative thinking about postal problems. However, virtually all innovations have been restricted to an unenlightened cost accountant's or production manager's approach to budgetary problems, which is to cut costs, lower quality, or increase rates. Changes only of this type will, however, be self-defeating in the long run. Their cumulative effect will be to drive people into the use of competing communication services even though an efficient mail type of service would, if offered, better satisfy their needs.

As mentioned, there is no shortage of innovative ideas which deserve attention and careful study by the Post Office. I will briefly expound on a few such ideas here in order to illustrate, hopefully, what an imaginative and exciting organization the Post Office could become. By way of disclaimer, please note that I have not conducted any studies on the cost/revenue effects or other effects which these proposals might have, nor am I aware of any recent in-depth studies to which I might refer. Consequently I am in no way recommending these proposals for adoption. But I do strongly advocate that they be

given intensive study and consideration. For purposes of discussion these innovations can be grouped in four areas: (1) volume discounts; (2) incremental pricing; (3) delivery service pricing; and (4) new postal products.

1. *Volume Discounts*

Third-class bulk mail rates are substantially less than first class mail rates (about 35 percent) in return for which third class mail is required to be presorted and bundled before it arrives at the Post Office. The fact that virtually all advertising is presorted and sent at third rather than first class rates is strong evidence that the discount provides mailers with strong incentives to engage in economically efficient activity. Except for this one example, however, there has been resistance to any attempt to introduce the fairly standard business practice of granting discounts for large purchases which result in economies or savings to the "seller." Yet compelling arguments might be made for just this type of practice in many postal transactions. For example, it is generally agreed that the cost of providing window service in post office lobbies is quite high. Public dissatisfaction with the high prices charged by privately-owned stamp dispensers is also apparent. Nevertheless, these private stamp dispensers provide the public with a convenience which the Post Office cannot supply. If the concept of volume discounts could be made acceptable, an arrangement might be made between the Post Office and the owner-operators of vending machines whereby privately owned machines would dispense postage at face value. Such an arrangement could work to the mutual benefit of the Post Office, the vendors, and the consuming public—simply because it is an economically more efficient way of doing business.

Nor need the idea stop at just discounts for dollar volume. There are numerous incentive pricing arrangements that might be examined. For example, with first class mail, (i) the use of standard envelope sizes, (ii) presort by ZIP code, or (iii) deposit at the post office at specified times convenient to the department, all represent possible ways by which the incentive pricing concept might stimulate desirable mailing practices.¹²

2. *Incremental Pricing* *

There has been excessive reliance on published figures of the Cost Ascertainment System. This system, which allocates the full cost of operating the entire Post Office Department to all postal products, is inappropriate for use in pricing decisions. For example, the full-cost philosophy of ratemaking is clearly working to price postal money orders out of the market. This once-important product, which previously made a substantial contribution to postal overhead, has been subjected to a steady series of price increases, each of which has eventually lead to a further decline in total money order revenue.

As Foster Associates noted in its report to the Kappel Commission, ratemaking is an art in which it is impossible to apply unequivocal decisionmaking formulae. The Department should move away from

¹² For further elaboration, see my paper "The Value of Output of the Post Office Department", op. cit.

*Further discussion of this issue is found in the paper by Vickrey in vol. 1 of this collection.

the rigid application of full-cost pricing techniques, and in so doing it will become increasingly important to have product-oriented managers become responsible for monitoring and recommending changes in rates of individual postal products (subject, of course, to some type of regulatory or legislative oversight). If knowledgeable managers were freed from the restrictions of the full-cost pricing system and allowed to experiment, I expect that the pricing of postal services would begin to reflect the application of basic economic principles such as marginal cost constraints as well as consumers' service preferences. It might well benefit both the public and the Post Office if many of the special, nonpostal services such as money orders were priced more competitively.

3. *Pricing of Delivery Services**

Traditionally all charges for postal services fall exclusively on the sender.¹³ However, there appears to be little economic justification for this policy, especially in view of the rather wide differences in the amount of actual delivery services which various patrons receive. For example, firms in the central business districts usually receive several deliveries a day, and all business routes receive at least two deliveries a day. However, business firms which happen to be located along residential routes receive only one delivery per day. Along residential routes some patrons have their mail delivered at curbside, whereas other houses receive door delivery—which costs the Post Office two to five times as much. It seems somewhat incongruous that the Post Office and the Cost Ascertainment System try to have all cost differences reflected in the rates charged senders while cavalierly ignoring these significant and expensive differences in delivery standards and costs. When this basic realization sinks home, one quickly discovers why there is so much continual disagreement over the Cost Ascertainment System and the procedure of allocating all costs to senders.

To illustrate this point by analogy, the telephone company has discovered a market for services above the standard "black box" receiver, multiple extensions, color, touch-tone dials, et cetera, and it charges on an individual basis for each service received. In this way, one user doesn't pay for services received exclusively by another customer.¹⁴ Similarly, there is probably a market for delivery services beyond a minimum free service—such as curb delivery—as in twice-a-day delivery or in delivery direct to the door rather than to a box at the curb. There would seem to be wide areas for experimentation and innovation open to a product-oriented manager charged with the definition and pricing of delivery services. Widespread application of this pricing approach might even enable the Post Office to reduce first-class letter rates to 4 or 5 cents.

4. *New Postal Products*

Volume or performance discounts, incremental pricing, and charges for extra delivery services all represent changes in the price or service characteristics of *existing* products. Even more interesting to speculate

¹³ Except for the very small amount of C.O.D. mail.

¹⁴ And any customer can avoid the additional charge by electing not to receive the service.

*Further discussion of this issue is found in the paper by Vickrey in vol. 1 of this collection.

on are possible *new* postal products that the Post Office could introduce. Among these might be:

- An electronic service for long-distance transmission and same-day delivery of facsimile documents.
- Cooperation with the telegraph company on a merger of special delivery with their public message service into a new high-priority written message system.
- Domestic air lettergrams.
- Special tape cartridges for recorded voice messages sold and transported by the Post Office. Input/output terminals for these messages might also be supplied on a sale or lease basis.

Naturally, I have no firm idea as to the eventual profitability of these or any other potential new products. However, some degree of risk taking is absolutely necessary if there is to be progress. I am convinced that the Nation would, on balance, benefit from a higher level of product innovation in the Post Office.

SECTION D

SOCIAL DEVELOPMENT AND HUMAN RESOURCE PROGRAMS

DETERMINING THE RESULTS OF MANPOWER AND ANTIPOVERTY PROGRAMS

BY GARTH L. MANGUM

Garth L. Mangum is McGraw Professor of Economics and Director of the Human Resources Institute at the University of Utah. He is also affiliated with the Center for Manpower Policy Studies at the George Washington University.

Dr. Mangum begins his paper by asserting that no manpower or anti-poverty program has ever been evaluated. In his paper, he elaborates this proposition, pointing out that while a number of public administration studies have been done on manpower programs, no meaningful benefit-cost type analysis has yet been performed. He suggests four steps which are necessary for quantitative benefit-cost or cost effectiveness studies of manpower programs and discusses the benefit concepts appropriate to manpower programs. While the problems confronted in quantifying benefits are substantial, Dr. Mangum proposes some potentially fruitful methodologies for overcoming these problems. He asserts that accurate program analysis requires the quantification of both the direct and indirect costs entailed by an expenditure.

The major emphasis of Dr. Mangum's paper concerns the need to implement followup studies in order to do competent economic analysis of program effects. He states, "Persistent reiteration over several years of the need for followup data has not broken through its low priority among the pressures confronting program operators." In the last year or two, however, he notes that increased efforts to obtain followup data on the participants in governmental programs have been made. A number of practical steps which must be taken in increasing the amount and quality of followup data are described. "The ultimate success or failure of evaluation rests upon the availability of solid data on the post-enrollment employment and earnings experiences of participants. That requires followup, intensive in detail and extensive in time. Grubby as the task is, there is no substitute."

Introduction

No manpower or antipoverty program has ever been evaluated.* No one really knows what has been accomplished and whether any of these programs has been worth the cost. Considering the growing congressional agitation for evaluation, the rising Federal expenditures (in-house and contracted), the modest amounts spent by Ford and other foundations and increasing interest by academic researchers, the opening statement appears extreme. Yet, if by evaluation one means identification of results, measurement of costs, and determination whether the former justifies the latter, the statement is factual.

Many good public administration studies have been made leading to useful recommendations for more efficient operation of the programs. Some, including the author of this lament, have pieced together the fragments of evidence available and have published their own opinions, but these reflected unabashed personal judgments supported by no "scientific" methodology. Informed judgment is to be preferred to uninformed or no judgment, but it is constantly vulnerable to counter judgments. It is no substitute for statistical demonstration.

*Further discussion of this issue is found in the paper by Levine in this volume.

Based on available studies, one can say a great deal about the administrative structure and problems of the manpower and antipoverty programs, their community institutions, the competence of their staffs, and a little about the content of programs and the reaction of employers. Such studies have contributed significantly to effective administration. However, little, if anything, can be dependably said about the subsequent employment and income experience of the enrollees. And that, after all, is the objective and the relevant measure of a program's worth.

The embarrassing discovery in the midst of urban riots, that no one knew how many Federal dollars and enrollees were involved in Federal manpower and antipoverty programs in any city, brought improvements in management information data. However, the cardinal sin of agencies and the lament of evaluators is that no program has been accompanied by any significant amount of consistent and dependable followup. Persistent reiteration over several years of the need for followup data has not broken through its low priority among the pressures confronting program operators. Without tracing postenrollment employment and income patterns for some substantial period of time, evaluative judgments can be based on no more than guesswork and heroic "extrapolation" from limited data. Benefits must be determined and costs measured. Technical decisions are required concerning discount rates and statistical techniques. The nonquantifiable must still be subjected to judgment, but no more than an academic exercise can emerge until quantifiable results are attempted. Examples are internal, Department of Health, Education, and Welfare, Labor Department, and Bureau of the Budget calculations, which demonstrate highly sophisticated techniques but are used only as tentative indications for each lacks trustworthy data to feed into the models.

Because follow is the beginning, rather than the end, of evaluation, it is useful to discuss the concepts underlying a practical evaluation system, including its necessary data base. One can then compare the existing data base with it, identify the obstacles to adequate evaluation and make recommendations for a useful system for evaluating employment-related public programs.

SOME CONCEPTS OF EVALUATION

The only valid measure of a program's worth is its ability to achieve its objectives at costs which are less than the benefits achieved. This suggests four steps, none of which has ever been satisfactorily achieved in practice: (1) practical objectives, not political rhetoric, must be clearly stated; (2) the extent to which those objectives are being achieved must be determined; (3) all the direct and indirect costs must be measured; and (4) a measure must be developed of all direct and indirect benefits, recognizing the nonquantifiable nature of many of the latter, yet guarding against the tendency to use the nonquantifiable as a justification for any difference between costs and benefits.

OBJECTIVES AND MEASURES OF ACHIEVEMENT

The primary objective of the manpower and antipoverty programs of the 1960's has clearly been improvement in the employment and earnings experience of the participants. This has ranged by program

from temporary employment and income to see the enrollees through a difficult age or economic situation to a permanent improvement in employability, employment and income. However, pursuit of this straightforward objective may ignore possible offsetting external costs imposed by displacing or blocking those who might otherwise have obtained the jobs and received the income, as well as possible external benefits. On a more sophisticated level, then, there are three valid objectives and three corresponding measures of benefit: (1) increase in gross national product; (2) external benefits—social benefits (or reduction in social costs) to those other than the direct beneficiaries; and (3) deliberate redistribution of jobs and income. The first would measure the net increase in employment and income, taking into account any possible displacement effect. The second would measure such benefits as reduction in welfare costs, crime rates, and so forth, not noted in GNP measures. The third would recognize explicitly a set of beneficiaries in whom society has taken a special interest and whose employment and incomes merit expansion regardless of the incidence of displacement.

The GNP test is almost impossible to administer for manpower programs. For each person hired there must be assurance that first, his job would not have been obtained without enrollment in the program; second, the production would not have occurred for lack of an incumbent to do the job; and third, the expenditures to purchase the product resulting from the job did not result in a decreased expenditure for some other product, reducing GNP to the same degree. The first two conditions can be ascertained only under conditions of a near absolute shortage of an essential and irreplaceable skill. The flexibility of people, employers, and production processes makes this circumstance highly unlikely. Higher costs, delayed production, or reduced quality are the most probable impacts. These first two conditions can also be met in situations where the manpower program enhances the quality of the labor force without promoting offsetting wage increases, increasing the marginal revenue product of the firm and encouraging expansion of production and employment to a new position of marginal cost equal to marginal revenue—a situation certainly conceptually possible but unlikely to be identifiable. It is doubtful that the third condition could ever be measured but, considering the expansion potential of a credit-based economy, the occurrence may be so unlikely as to be safely ignored.

Some externalities are easily identifiable, such as the removal of individuals from welfare or unemployment insurance roles. Reduction in crime rates are more difficult to identify, though social groups could be chosen and comparisons made with averages of the same demographic groupings and locations. The longrun gains of a better-prepared labor force, whether defined in GNP terms or as externalities, are unlikely to be measurable because the question to be answered must always begin, "What would have happened in the absence of * * * ?" Improvement in such institutions as the public employment services or the vocational schools as a result of involvement in manpower programs is already apparent, though its value is not quantifiable. The same could be said of the influence of community action agencies on the political structure.

Thus the direct beneficiary or income redistribution approach becomes the practical one for evaluating manpower and antipoverty programs. It is necessary only (1) to assure that the immediate beneficiary of the program was a member of a category that public policy had chosen, through accepted political processes, to favor by redistributing jobs and/or income in their favor; (2) to ascertain that the enrollee did not displace another member of the same or an equally favored category; (3) to determine that the desired redistribution did in fact occur through an improved employment and income experience for the beneficiary; (4) measure the cost of the redistribution to allow the body politic to determine whether the objective was worth the price; and (5) explore the alternative approaches which might have accomplished the same objective at lower cost.

Assuming that measurement of each requirement is technically achievable, evaluation of program results becomes conceptually possible as long as the objective of manpower programs remains improvement in the employment and income status of those facing observable disadvantages in the competition for jobs and singled out by public policy for special treatment. Under present Labor Department definitions these people are those who are both poor and without a satisfactory job and in addition fit into one of the following categories: (1) under 22 years of age, (2) over 44, (3) member of a minority group, (4) welfare recipient, (5) physically or mentally handicapped, or (6) have less than a high school education. Given the relatively firm political commitment to improving the welfare of this group, program evaluation can be initiated demonstrating that enrollees were indeed members of target groups and coupled with a comparison of their characteristics with those of the incumbents of any resulting employment, give some assurance that the jobs would not have gone to those of like characteristics in the absence of the program. When the target of manpower programs shifts, as it gives evidence of doing, to upgrading the nondisadvantaged labor force, the income redistribution test will no longer hold and it will be necessary to return to the GNP and externalities arguments with all their pitfalls. Until then, proving that the disadvantaged were successfully aided at acceptable costs, though still difficult is within reach.

COST-BENEFIT VERSUS COST-EFFECTIVENESS

Conceptually, there can be no argument with the premise that benefits-in-excess-of-costs is the valid justification for any public program. How could anyone justify an expenditure for anything which costs more than it is worth? The objections to the cost-benefit approach are practical problems of defining and measuring costs and benefits.

Cost-benefit analysis as ordinarily applied measures the relationship only for a particular program or project. It is perfectly conceivable that one project or program might have direct costs less than its benefits, yet be less effective in achieving its objectives than some alternative project or program aimed at the same objective. The economist escapes the problem in concept by including the "opportunity cost" of alternative uses of the same resources along with direct costs. Displacement of other actual or potential employees could also be included among the costs in the same way. In practice, however, the alternatives are considered only in cost-effectiveness analysis which

asks, "What is the least-cost method of achieving any specified result?" Cost-effectiveness alone can answer whether the least cost method has been identified but not whether the task was worth doing, while cost-benefit studies normally determine only whether the effort was worthwhile and not whether there was a better way. Obviously both questions must be asked.

Cost-effectiveness analysis requires specifying the objectives being pursued. Cost-benefit allows summing all benefits, no matter how diverse and even unintended. Both assume that costs and benefits can be identified and measured.

Determination and quantification of costs is not easy. Moving from the less to the more difficult, there are (1) technical accounting problems of actually identifying the direct monetary costs involved in establishing and running a program, many of which are embedded in a variety of joint costs of various social products; (2) the personal costs to the worker such as estimates of wages foregone to participate in a training program; (3) the possible infringement on other social programs by absorbing the scarce supply of "human service" manpower—over and above the resource allocation costs represented by the wage and salary payments; (4) possible displacement costs to other workers; and (5) any undesirable impacts on social institutions. Since objectives are achievable only over extended periods of time, an imputed rate of capital cost consisting of the interest rate available on the next most profitable available use of the same funds must be added to cost, or conversely the value of any benefits must be discounted by the same factor.

The benefit side is even more troublesome. Conceptually, an increase in GNP is an easily definable and measurable magnitude, but the problems of that approach have been noted. Externalities can often be only conceptualized and rarely measured. The beneficiary approach is deceptive in its simplicity: find the additional total income accruing to each individual as a result of his participation. Leaving aside for the moment the practical difficulties of determining what the impact of a current event on a future income stream may be, it is conceivable that redistribution in itself may be considered to have value over and above that of the incremental income. Clearly, given current goals, an incremental income to a disadvantaged person has a social value greater than that of a similar increase for the nondisadvantaged—but how much greater?*

It is possible that redistribution might be considered so important as to justify a program with monetary costs in excess of monetary benefits.

Conceivably, the cost-effectiveness approach could offer a solution. If income is the objective, anytime the same income could accrue to a target individual at less cost by a simple income transfer, the manpower program could be considered unjustified. But work may well be considered a benefit in itself and society may be willing to pay a premium to encourage the recipient to earn his own way. In the end, only a value judgment can determine which is best. However, the monetary costs and benefits must be measured and the nonquantifiable ones estimated to the extent possible. If benefits clearly exceed costs, worthwhileness is unquestioned. If quantifiable costs exceed quantifi-

*Further discussion of this issue is found in the papers by Weisbrod, Bonnen, and Freeman in vol. 1 of this collection.

able benefits the justification for the program is doubtful, though extenuating circumstances may justify continuance. The task of quantitative analysis is to expose the discrepancy so that a political decision can be explicitly made and priced in the political marketplace.

STATE OF THE FOLLOWUP ART

All these are conceptual problems, however. If reasonably accurate measurements or estimates of costs and benefits cannot be produced, the issues are academic. Most discussions of program evaluations have stopped at the conceptual level—what is the objective, the appropriate yardstick, and the proper discount rate? These can be solved to some reasonably satisfactory degree as concepts, but they are useless without actual measurements. It is in this latter area that most evaluation efforts have failed. As stated at the beginning of this paper, the great unknown for every manpower and antipoverty program is: what happened to the people as a result of their participation? Leaving aside a few graduate-thesis studies of fragmentary populations, only MDTA and Job Corps among manpower and antipoverty programs have produced significant amounts of followup and these have been burdened by serious technical weaknesses.

Only for MDTA is a followup requirement built into the program data reporting procedure. Local employment service offices are directed to contact each MDTA completer 3, 6, and 12 months following completion. As of mid-1967, followup reports of any kind were available for only 56 percent of institutional completers and 38 percent of those trained on the job. However, sufficiently detailed characteristics to identify them as members of appropriate target groups was available for only 34 and 16 percent, respectively. All of the reports tailed off rapidly with most limited to the 3-month followup and few having been contacted at the 12th month interval. There was no way of judging the biases built in to the underreporting. Since telephone and mail seem to have been heavily relied upon, one would expect the employed to be found easier, biasing the data in a favorable direction. No tests have been made to assess the accuracy of reporting and there have been unsubstantiated stories of blanks being filled in to avoid the necessity of followup. It is clear that the employment services have not placed a high priority on MDTA followup. Employers and administrators of on-the-job training have been disinterested if not opposed. Even at best, the MDTA followup is only 1 year and one can only guess at its longrun impact. Nevertheless, the data is all there is and it has been used as a basis for the only overall evaluation of the program available, relying upon the consistency of fragmentary and shaky evidence.¹ Cure of the followup ills is essential to a meaningful test of the program's worth.

Examples of effective followup are available from one large and a number of small sample followup studies of MDTA completers. The small ones can be disposed of easily. Most are sound studies of the benefits to those in the sample but, given their size and their limitation to particular locations and projects, their results are not generaliz-

¹ Garth L. Mangum, *MDTA: Foundation of Federal Manpower Policy* (Baltimore, Md.: The Johns Hopkins Press, 1968).

able.² One nationwide sample study was large enough and sound enough in their technique to support generalizations.³ However, it was limited to following up trainees completing MDTA courses in the early phases of the program. Both the clientele and the economic environment have changed radically since. Continuous followup is necessary in evaluating experimental and unproven programs during their formative stages when they must adapt to new forces and policy directions.

Job Corps inaugurated no official followup system but instead contracted with a private survey firm to followup a sample of Job Corps centers during 1966. The product has been made doubtful by rapid fallout. Given the youth of the corpsmen and the lack of contiguity between centers and homes, mobility is high and followup difficult. Of the original sample drawn in August 1966 for 6-, 12-, and 18-month followups, less than one-quarter were interviewed a year and a half later and these were of doubtful representativeness. Neighborhood Youth Corps can claim only a simple one-time nationwide sample to test its economic benefits and a few fragmentary looks at its social impact.⁴ There has never been an evaluation of the results of work experience and training, new careers or Operation Mainstream based on followup of a substantial sample and measurement of the economic benefits.

PRACTICAL STEPS TO FOLLOWUP

The need to evaluate the results of programs expending nearly \$2 billion of public funds each year for the benefit of particular groups of citizens should not be challenged. One can argue truthfully that much larger expenditures are made regularly on veteran's benefits, agricultural price subsidies, highways and space travel, to name a few, with never a thought of relating benefits to costs. Even the Defense Department, despite its formidable reputation for evaluation rarely indulges in cost-benefit analysis. Their questions appear to be, "Now that we have decided upon a particular objective, what is the most cost-effective way to achieve it?" Nevertheless, if only because funds

² David A. Page, *Retraining Under the Manpower Development Act: A Cost-Benefit Analysis*, Brookings Institution Studies of Government Finance, reprint 86 (Washington, D.C.: by the Institution, 1964).

U.S. Department of Health, Education, and Welfare, Office of Education, *Education and Training—Third Annual Report on Training Activities* (Washington, D.C.: U.S. Government Printing Office, 1965).

Gerald C. Somers and Ernst Stromsdorfer, "A Benefit-Cost Analysis of Manpower Retraining," *Proceedings of the Industrial Relations Research Association*, December 1964; Glenn G. Cain and Ernst Stromsdorfer, "An Economic Evaluation of the Government Retraining of the Unemployed in West Virginia, 1965," mimeographed. See Gerald C. Somers, ed., *Retraining the Unemployed* (Madison: University of Wisconsin Press, 1968), for summaries of these and a number of related cost-benefit studies of retraining programs.

Michael E. Borus, "The Economic Effectiveness of Retraining the Unemployed," *Yale Economic Essays*, 4, No. 2 (fall 1964): 371-429.

M. Borus, "Time Trends in Benefits From Retraining in Connecticut," and Gerald G. Somers and Graehme H. McKechnie, "Vocational Retraining Programs for the Unemployed," in *Twentieth Annual Winter Proceedings of the Industrial Relations Research Association, Washington, D.C., December 1967* (Madison: University of Wisconsin Press, 1968).

³ Earl D. Main, *A Nationwide Evaluation of MDTA Institutional Job Training Programs*. Report of a research project done by the National Opinion Research Center, University of Chicago for the Department of Labor (Chicago: the University, October 1966).

⁴ Robert J. McNamara and Charles S. Kamen, *Characteristics of Neighborhood Youth Corps In-School Projects: An Analysis for the Year 1966-67*. A Report by the National Opinion Research Center, University of Chicago for the Department of Labor. April 1967. (Mimeographed.)

See also the various reports of a study done by Dunlap and Associates, Inc., of Darien, Conn., for the Office of Economic Opportunity in 1966 and 1967 and another series of reports done by the social research group of the George Washington University for the Department of Labor in 1967 and 1968.

are limited and must be used efficiently; or because the programs are new and need to be tested and redirected where desirable; or simply because, as unpopular programs for an unpopular and politically weak clientele, manpower and antipoverty programs must constantly defend themselves, the demand for dependable evaluation is a growing one.

Determination of results as the vital first step to evaluation of manpower and antipoverty programs has rarely been attempted on a consistent and substantial basis because the task is difficult, expensive and not glamorous. It consists of: (1) tracing the postenrollment experience of program participants, and (2) estimating in what way those results differ from the situation which would have prevailed in absence of the program. The experience with the MDTA Employment Service followup suggests that those with operating responsibilities, left to their own preferences, will give it low priority. Certainly the administrators of all programs have shown little interest in followup, even in programs like CEP where the purpose of followup is supposedly service rather than evaluation.

Part of the reluctance apparent in the MDTA followup may be in part explained by its ambition to include every MDTA completer. Following up the complete universe is not only burdensome and expensive, it is also inefficient. A small sample with a high rate of return is a more dependable guide than an underreported and uncertain universal survey.

The few attempts that have been made through contractors or in-house research to trace samples of participants have also proven difficult, expensive and uncertain, as the Job Corps example suggests. The life styles, housing arrangements, legal problems and employment experience of the disadvantaged clientele appear to be characterized by high mobility and avoidance of forwarding addresses. The contemplated OEO-funded 3-year longitudinal cross program comparison may improve upon past experiences by establishing contact with the sample immediately upon completion of program involvement.* Rather than wait upon the results of this lengthy followup, it would be well to experiment with sample selection during rather than upon completion of enrollment. The enrollee's background and pre-enrollment experiences could then be explored more thoroughly and confidence gained to win postenrollment cooperation. Since the primary purpose of this initial exercise would be to perfect followup methods, different approaches should be taken with a number of subsamples. For instance, mail followup using instruments furnished the participant during the enrollment period might be used with some. Others might be promised a substantial reward for periodic self-reporting. Other samples would rely on telephone and personal followup by interviewers.

Long-term followup such as that contemplated in the OEO longitudinal study is likely to confront a decreasing return, rising costs and increased uncertainty. At the same time, work and living patterns may emerge early and give decreasing value to the increasingly expensive information. Yet one of the critical questions is the durability of any employment and income advantages appearing after participation in a manpower program. An alternative to be experimented with could be intensive followup during the critical first year or so followed by

*Further discussion of this issue is found in the paper by Levine in this volume.

reliance on social security data through subsequent years. The Social Security Administration seems at last resigned to use of its data as a major research tool. Currently, periods of employment and nonemployment, the employer and industry, and covered earnings per quarter can be easily obtained. Over time it is not inconceivable that other data could be added. The introduction of automatic data processing into State employment services may ultimately offer a similar tool with greater detail available.

The combination of sample selection and initial contact during enrollment, 1 year personal followup, supplemented by continuing monitoring of social security covered earnings should allow adequate tracing of postenrollment employment and income experience. The problem would remain of determining the net influence of program participation in contrast to other influences improving employment and incomes. For adults, a comparison of pre- and post-enrollment employment and earnings, discounted by general wage trends for the area and occupation would be a useful guide. For youth there is little preenrollment experience with which to compare. Use of control groups is preferable but really comparable controls are difficult to find, whether selected in advance or after the fact. Reaching them periodically is even more difficult and expensive than following up those who because of their participation have some reason to cooperate. For long-range comparison, the social security covered earnings experience of a control group of like initial characteristics should suffice without further detail.

THE FOCUS OF EVALUATION

To date evaluation has been approached program by program. The OEO longitudinal study will attempt to compare the results of various programs to determine which is the most effective. If the question is the survival of particular programs as presently constituted this is a valid approach. However, there is currently discussion of a movement to a single comprehensive manpower program. Whether that emerges or not, the question of program justification is relevant primarily to Congressional decisionmaking. The appropriate question for the administrator is what services in what combinations were most effective in aiding the clients? This requires a functional assessment (basic education, skill training, etc.,) rather than a programmatic one. It also requires going beyond discovering that a program paid or did not pay, cost-benefit wise, to determining why or why not? If the move is to be toward an expanded State and local role in the planning and administration of manpower programs and if monitoring those activities is to be a federal responsibility, evaluation will have to be structured along a combination of functional and community lines: What has been the total impact of all manpower programs in city x and which of the various services have been the most effective for whom?

SUMMARY

Day-to-day pressures of administering programs and the embarrassment of not being able to answer Presidential queries about programs in rioting cities has led to development of improved management information systems for manpower programs. Yet despite

congressional pressure, little has been done to make systematic and meaningful evaluation possible. It is still true that no manpower program has ever been adequately evaluated on the basis of its results. Evaluation of public programs aimed at changing the individual and his employment prospects is inherently difficult. Yet effective programs will never emerge unless administrators have "their feet put to the fire" by being required to produce results and achieve objectives. Evaluation's purpose is to determine whether objectives have been achieved and at what costs. A great deal of thought and study have gone into cost-benefit, cost-effectiveness and PPB approaches. A body of theory and good practice is emerging but the ultimate success or failure of evaluation rests upon the availability of solid data on the post-enrollment employment and earnings experiences of participants. That requires followup, intensive in detail and extensive in time. Grubby as the task is, there is no substitute.

POLICY ANALYSIS AND ECONOMIC OPPORTUNITY PROGRAMS

BY ROBERT A. LEVINE

Robert A. Levine, until recently Assistant Director for Research, Plans, Programs, and Evaluation at the Office of Economic Opportunity, is now at the Urban Institute, doing research on poverty and related matters, under a grant from the Ford Foundation.

This discussion by Mr. Levine focuses on the application of economic analysis to Government economic opportunity programs. Even though the benefits of these programs are ultimately economic, Mr. Levine argues that because of the existence of an undeveloped conceptual framework and unavailable data "the economist spends most of his time dealing directly with noneconomic benefits." In reviewing the progress made in the Office of Economic Opportunity in implementing the PPB system, he describes the threefold emphasis of policy analysis in this agency as consisting of general systems analysis, the application of an economic style of analysis to "noneconomic" variables, and finally, the sponsoring of substantial analyses of economic variables. He emphasizes, however, that "analysis does not make decisions."

In discussing the general systems analysis for policy choice in OEO, Mr. Levine describes the program budget for that agency. He notes that the structure focuses the attention of policymakers on the choices which have to be made within a single OEO category and between OEO categories. "The major contribution of analysis to this political choice is to indicate to the political decisionmaker just what it is he will be losing by cutting back a program or be gaining by increasing one."

In discussing the economic analysis of noneconomic variables, Mr. Levine argues that the "economic style of cost-benefit thinking has led to improvements in policymaking and decisions." In the final part of his paper, he discusses the research on economic variables sponsored by OEO. This research involves data gathering, experimentation in the area of income maintenance, and the application of interdisciplinary social science research to the problem of poverty. He concludes his paper with a number of suggestions for next steps in applying policy analysis to poverty and economic opportunity programs.

Introduction

In recent years, the Federal Government has been making use of economic analysis and economic analysts in two rather different ways. Economists have been carrying out *basic* research on economic variables such as production, incomes, economic growth, and the like as well as on related demographic variables. In addition, and with increased recent stress, economists have been designing and carrying out direct program and policy related analysis.

The former sort of Government economic analysis is both older and more dignified. It is the Federal analog of university economic research. Some of it is interchangeable with such university research; some is carried out within the Government because of the need for data of a scope or type most easily obtainable within the Government. Large-scale surveys of the sort carried out by the Census Bureau or the Bureau of Labor Statistics are difficult to do with private resources and facilities. In addition, the Government can obtain information which would simply not be made available to private sources. Census data provide one example here; Internal Revenue data perhaps a better one. This sort of Federal economic research may be policy relevant, but it is not designed with policy relevance as the prime goal.

Economic analysis for policy is quite different from, and is for the most part newer than, the broad-based economic research discussed above. To be sure, it has been carried out for the last 20 years by the Council of Economic Advisers, the Treasury Department, the Federal Reserve System and, to some extent, by the Budget Bureau. But until recently these efforts have been pretty well limited to "classical" economic subjects of production, growth, money, and income and the like.

What is really new is the Federal use of economists and economics for policy analysis of problems in which major variables are not economic in nature (e.g., deterrence, educational techniques, health organization); as well as for economic problems previously slighted, such as those associated with special segments of the income distribution (e.g., the poor). These problems handled by economists certainly are almost all economic in one sense—the policy decisions for which the analyses are made concern allocation of scarce resources among competing programs and such allocation is an economic problem. In this sense, very many Government programs which would not seem economic in nature—notably Defense—have come to be considered economic programs at least in part. Economic modes of thinking such as choice being made at the margin have proved useful in a wide variety of Government programs and problems. If a distinction is to be made within the set of programs now analyzed by economists, it is that although practically all of them are economic on the cost (i.e., resource allocation) side, not all of them are economic on the benefit side—i.e., their major effects are not on changing incomes, production, etc. In this sense, the benefits of a Defense program are not economic, the benefits of a transportation program are largely so. The benefits of economic opportunity programs are ultimately economic, but as discussed below, the conceptual links between health programs, housing programs, etc., and poverty are still poorly formulated, and thus the economist spends most of his time dealing directly with non-economic benefits such as health and housing.*

ECONOMIC ANALYSIS IN THE OFFICE OF ECONOMIC OPPORTUNITY

Outside of the early examples of the Council of Economic Advisers, Treasury, and so forth, the first extensive use of economic thinking in existing Federal decisionmaking was in the Department of Defense, coming to a climax under Secretary McNamara.** Program Budgeting—the phrase used to define the economic style of analysis as applied earliest to Defense problems—was extended by the Bureau of the Budget to the rest of the Government in 1965. Before the famed PPBS directive, however, OEO was in the program budgeting business. Starting in the summer of 1965, the first of a annual series of 5-year anti-poverty plans was turned out by the Office of

* Further discussion of this issue is found in the papers by Rivlin, Brandl, Grosse, and Ross in this volume.

** Further discussion of this issue is found in the papers by Enthoven, and Enthoven & Smith in this volume.

Research, Plans, Programs, and Evaluation, under the direction of Joseph Kershaw. These were total national anti-poverty plans, encompassing not only the budget of OEO—then \$1.5 billion and now nearly \$2 billion—but the entire Federal anti-poverty effort budgeted in fiscal year 1966 at about \$20 billion and now at more than \$30 billion. The program budgeting activity in OEO has included three levels of economic analysis:

1. General systems analysis. By this is meant the setting of a framework for examination of alternative programs, the making of choices at the margins, the cost-benefit sort of thinking as applied to all policy decisions. It should be emphasized that the cost-benefit framework is more important as a style of thinking than as a rigid mode of analysis. The data now available are seldom of a quality sufficient to support cost-benefit analysis which is both rigorous and relevant to decision problems, but the idea of looking at alternatives to see which expenditure of dollars is more effective or which path to a goal is less expensive is crucial.

2. The economic style of analysis applied to “non-economic” variables such as health, education, community action activities, etc. Terminology here is tricky, for some of these “non-economic” programs clearly have economic effects—e.g., the earnings increments which may stem from additional education. The crucial point, however, is not the classification of an area as “economic” or “non-economic,” but rather the asking of the economist’s standard question as to whether or not the activity has a payoff which justifies the outlays. For example, evaluation of the impact of a program on the problems to which it is addressed is not what has been ordinarily meant by program evaluation. More often, program evaluation has meant operational and administrative evaluation of the way the program is working rather than its effectiveness in reaching its objectives. The concept of impact evaluation is one which seems natural to economists, whose background stresses the profit and loss aspects of a production program, for example, rather the engineer’s preoccupation with the meshing of the production lines. The latter is more analogous to typical operational evaluation in Federal programs.

3. Finally, OEO has carried out or sponsored substantial economic analysis of economic variables such as income, labor markets, etc. But, even in these cases, the analysis has been pushed to include non-economic phenomena. For example, in the study of the Negative Income Tax discussed below, the main focus is on labor market response but part of the study involves social and psychological phenomena.

What must be strongly stressed in regard to all three of these types of analysis, however, is that *analysis does not make decisions*. Analysis on no level does this. Decisions are and must be made by a process which brings together political considerations, interest reconciliation, feasibility, and program desirability, with the last two being the chief realms of the kind of analysis discussed here. For this reason, it is always difficult to draw a straight line from a study to an action program; the world simply doesn’t work that way. Rather, good analysis becomes *one* factor in the decision process, and a pro-

gram "whose time has come" can be substantially structured and guided by such analysis.*

To take some examples from OEO experience, OEO began in 1966 to study the desirability, the feasibility, and the numerical parameters of getting private business into the business of training the poor for jobs on a large-scale basis. It was decided that such a program was desirable, it was feasible, and costs and needed program size were estimated. In early 1967, the concentrated employment program was set up *without* subsidized business participation of the type suggested; OEO settled for a footnote expressing the viewpoint that it would work better *with* the business program. By 1968, the time had arrived; the administration was highly desirous of such a business participation program. The National Alliance of Businessmen-JOBS program resulted from the political desire. Its direction, size, and shape, however, depended substantially on the previous OEO analysis, and without this analysis the key business participation program might have been quite different indeed.

On the other hand, OEO analysts have long advocated basic changes in the income maintenance system of the United States. The reasoning behind these changes has been polished up to a high shine; politically we may be just on the verge of viability.

GENERAL SYSTEMS ANALYSIS

The basis of general systems analysis for policy choice lies in the setting up of program categories. The importance of such categories is great because the categories themselves guide the way choices are made by delineating the scope of the alternatives to be compared.

The first four annual antipoverty plans prepared by OEO divided the major antipoverty programs into four categories:

1. Manpower programs: Those programs under the Economic Opportunity Act or elsewhere designed to attack poverty by improving the ability of the poor to enter the labor market and work in decent jobs. These programs include both job training projects and job creation efforts such as public employment.

2. Individual improvement programs: Those programs other than directly job-oriented programs in the first category, whose impact is primarily on *individuals* in poverty. Most of such programs are educational in nature; in more recent years, health programs have also been put into this category.

3. Community betterment programs: These are programs designed to change the physical and social environments which cause and perpetuate poverty. They include Federal antipoverty housing programs such as public housing and rent supplements, but perhaps of most interest are the OEO Community Action Programs. The community betterment category in general, and Community Action Programs in particular, need more extensive description because each program within the group has three inseparable objectives:

*Further discussion of this issue is found in the papers by Wholey in vol. 1 of this collection, and Hoffman and Rivlin in this volume.

First, each Community Action Program is designed to deliver new services to the poor or to deliver old services in an innovative way. For example, the legal services program of OEO delivers services to poor persons in need of legal counsel whereas such services had for the most part simply not been available before. The neighborhood health center program delivers medical services in a comprehensive family care mode which is not only unique to poverty areas but has broken ground for delivery of modern medicine to families at all income levels.

Second, the programs in this category were supposed to be drawn into coordinated patterns where they would reinforce one another. Frankly, this coordination has not worked well.

Finally, and perhaps most important, each program in this category ordinarily has a self-help and organizational aspect. That is, following the mandate of the much maligned phrase "participation of the poor," the poor, particularly in the urban areas of most concentrated poverty, have brought themselves together for two purposes. They have organized around the services of Community Action and other programs self-help institutions previously lacking in the worst ghetto and slum areas. And in addition to self-help, these institutions have had a political effect in providing the poor with a political power base previously largely lacking.

4. Income maintenance programs: The poor now receive on the order of \$14 billion of Federal payments under income maintenance programs—primarily that portion of social security going to the poor, but also public assistance. These income maintenance programs together with suggested new ones such as the so-called negative income tax form a fourth category of antipoverty programs.

The important thing to be noticed about the foregoing categories is that they are not mutually exclusive; rather they are mutually supportive. It is sometimes argued that the war on poverty should have a "job strategy" or an "income maintenance strategy," as compared to the strategy of the first 4 years, which is characterized as a "community action strategy." This delineation of strategies is close to nonsense. If there is one clear point to be made about a strategy it is that it must be a mix of all of these categories and that none will work very well without the others.¹

For example, category 1, manpower, is in some sense central. That is, if we are talking about economic opportunity, in our society and economy such opportunity means the right to obtain and work at a gainful and useful job. But one reason the poor have been unable to get such jobs is the poor education given by slum schools and thus category 2, individual improvement, supports manpower programs. Yet on the other hand, one reason for the failure of slum education has been that even educated members of minority groups have been unable to get reasonable jobs and thus educational motivation has

¹The notion that community action is the war on poverty is simply not valid. For example in the \$1.948 billion budget of OEO for fiscal 1969, \$924.6 million is in the work and training programs of title I and \$940.1 million in the Community Action Programs of title II. Of the latter \$348 million is in Headstart, which is in the individual improvement category. Even more indicative is the fact that of the overall Federal antipoverty budget of \$50.5 billion, \$375 million, less than 1 percent, is devoted to locally determined community action types of programs. This compares to \$14 billion in income maintenance and \$2.4 billion in manpower programs.

been low. In this sense, manpower programs support individual improvement programs as well as the reverse.

The community betterment programs support the others because a major reason for the failure of individuals in education and job programs has been the stultifying environment of poverty—both the physical environment and the lack of hope consequent upon being surrounded by failure. And finally, the income maintenance programs support the rest not only because over one-half of the poverty population as of the end of 1967 were in families headed by individuals who for reasons of age, disability, or other are outside the labor market and whose only hope for a decent life lies in income support—not only for this reason, but also because income *means* opportunity. A child in Headstart, for example, is going to benefit more from this program if the family income is sufficient to feed and clothe him at least at minimal levels.

These four categories, then, provide a reasonable structure for overall program analysis. Again, final choices can never and should never be made *completely* on the basis of such analysis, but at least rational analytical inputs can assist in making decisions. The category structure allows three kinds of choices. First, it allows analytically based choice between programs *within* a single category. Not all programs in the category are directly comparable—manpower programs, for example, may be aimed at different age groups—but for the most part there is some basis for comparison. And in principle at least, analyses based on the cost-benefit kind of thinking (sometimes even on direct cost-benefit analysis) can lead to recommendations of the type which state that one program seems capable of achieving an objective more effectively (or cheaply) than another. In the individual improvement/education category, for example, early evaluative analysis indicated that, although summer Headstart programs did lead to “cognitive gains” among preschool children, other first-grade pupils in the same classrooms who had not been in Headstart tended to catch up by the end of the year. And in part as a result of this, the Headstart follow-through program was created to try to conserve in school the gains obtained from the preschool program.

The second type of choice is that *between* categories. As suggested above, this is not an either-or choice, since the categories are mutually supportive. The real question is the particular mix of programs in different categories. For example, in the early days of OEO, little money or effort was being put into job programs for adults. This apparent lack of emphasis on such programs was not *necessarily* bad because no standard existed to set the amount of money which should have been going into such programs but analysis of the numbers and needs of the poor led to the conclusion that in fact the need for an adult job program was very high. And this analytic conclusion in turn led to the major dollar increases in fiscal years 1968 and 1969 going into the manpower category for programs such as these rather than elsewhere. (Marginal choice in Government programs is far more easily translated into allocation of program increments than into increases of one program at the expense of cutting another back. Thus programs in other categories were not substantially cut back to increase manpower, but the relative emphasis on adult jobs was substantially increased by putting most of the new money there.)

The final sort of choice is the one for which analysis is least relevant. This is the choice between putting money into the antipoverty program in any category as against putting it into other programs entirely—the supersonic transport for example. Since no paradigm exists by which one single overall national objective can be translated back to a series of widely disparate programs for achieving this objective, analysis cannot answer the question of the relative contribution of the disparate programs to such an objective. Rather the choice among multiple objectives is a case of value judgment and in our democratic system this value judgment is properly expressed by political processes. The President and the Congress make the decisions as to the relative stress to be put on objective as widely different as the end of poverty, the SST, and national defense. This system of choice may or may not work well, but as Winston Churchill said, it works better than anything else. It certainly works better than trying to make such choices analytically. The major contribution of analysis to this political choice process is to indicate to the political decisionmaker just what it is he will be losing by cutting back a program or be gaining by increasing one.

In any case, the categorization of programs and the implicit or explicit analyses indicated by the categorization have provided the basis for our annual series of 5-year antipoverty plans. The 5-year plan consists of recommendations for each program in each category, together with similar if softer recommendations for the subsequent 4 years. These recommendations—for all antipoverty programs, not just OEO administered ones, it should be repeated—are initially based on calculations of “universes of need” for different programs and a capability for reaching and servicing these universes over the indicated time period. Such recommendations, constrained by the limitations of program management and of real resources—e.g., limitations on the number of doctors for health programs—are not constrained fiscally and are therefore not terribly realistic fiscally.

The next step is to prepare a list of cuts to the outermost recommendations with the cuts being listed in a reverse priority order. That is, the least essential programs are cut back first and so forth, down the list. And by this process the 5-year plan can be brought back to any level of fiscal reality, as the Budget Bureau defines reality. Again the priority order of the cuts is based on a cost benefit style of thinking using as the standard of benefit the contribution of each program to ending poverty. Thus, programs like housing which treat with spectacular symptoms of poverty but cannot easily be established as attacking the causes of poverty have generally been considered of relatively low priority in our 5-year plans.

It is important to realize in any case that the 5-year plan is not a blueprint to be followed in even gross detail nor is it an economic model. It is not a blueprint because choices of the nature laid out are never made entirely analytically nor should they be. The analysis is just not that good, nor can the nonanalytical factors—political and other—be considered irrelevant. Choice at all levels is a political process within which analytical factors as well as others are considered. The objective of economic analysis as structured by the 5-year plan is to increase the analytic input into this process but the analytic input will never dominate, nor should it. As for an economic model which will

solve the problems of poverty, we are so far from this that it is not worth thinking about at all. After some 30 years of work, a few economic models exist which can predict aggregate magnitudes fairly well, but these do not come to grips with income distribution, which is essential to the poverty problem of course. Whether such models are conceptually possible is an epistemological question which may be of interest to some, but that they are impossible in any meaningful future is likely.

Rather than a blueprint or economic model, the 5-year plan, then, is a framework for making the choices discussed above. It is a guide to the kinds of analysis needed to clarify the choices and make recommendations and it is a statement of goals and the feasibility of reaching goals. For example, Sargent Shriver's frequently quoted statement made in 1966 that poverty in the United States could be ended in 10 years was not a casual one, it was based on a program plan. And as stated and intended it was not a forecast that poverty would be ended in this period of time but rather a statement of objectives and feasibility. The planning process showed then and has continued to show that ending poverty as defined was well within our resources and capabilities and could be done at a cost of less than the incremental tax revenues stemming from a growing economy. In fact the dramatic increase in the numbers of people in poverty since Shriver's 1966 statement (at the time the statement was made, the latest estimate of the number of poor, for the full year 1965, was 31.9 million people. In the 2 subsequent years, the number dropped to 25.9 million, a 5 million decrease allowing for a statistical series shift²) indicates that even without full implementation of the 5-year plan proposals, we have been heading rapidly in the projected direction.

ECONOMIC ANALYSIS OF NONECONOMIC VARIABLES

Setting up the framework of categories of course does not in itself create an analysis of choice. Rather the structure indicates the analyses which must be done. Such analyses—economic in type because they concern resource allocation and choice at the margin—cover both noneconomic and economic variables.

Looking first at the analysis of variables which are not essentially economic in nature, two examples may be most illustrative. The table below, for example—a summary of some much more complex data—treats with noneconomic variables. That is, given that the numbers of people indicated are poor by an economic definition, the chief variables within the table are age and marital status. These are matters most ordinarily dealt with by social demographers and other sociologists but the social analysis indicated by the table is nonetheless one of an economic type. The clear lesson of the table is that the character of the poverty problem has shifted dramatically since 1959 and that the easiest part of the problem—that having to do with families headed by an "able-bodied male" presumed to be capable of gainful labor—is that against which most progress has been made. The analysis indicates a shift of weight within the overall poverty population toward those categories less capable of entering the labor market and thus indicates

² These statistics are based on the so-called Orshansky poverty line, in which four-person families with annual incomes under \$3,300 (1967 prices) are counted as poor, with roughly \$500 added (or subtracted) for additional (or fewer) family members. The line is adjusted annually for price change.

increasing reliance in future years (if the trend continues as it is projected to do) on programs with the income maintenance category as against those within the manpower category.³

TABLE 1

	1959 (millions)	1967 (millions)	Change (millions)	Percent
Total poor persons.....	38.7	25.9	-12.8	-33
Poor persons in non-aged-male-headed families.....	24.5	12.3	-12.2	-50
Poor persons in non-aged-female-headed families.....	8.3	7.7	-.6	-7
Poor persons in families with an aged head.....	5.9	5.9	0	0

Similarly, far more detailed calculations are made periodically of the shifting universes of need for all antipoverty programs. The job is not always easy—the need for a legal services program, for example depends not only on the number of poor people in target areas, but also on the rate at which they generate legal actions. As experience with existing programs comes in, these factors are becoming possible to estimate, and are reflected in budget requests and projections. In other fields—manpower as always being the best example of relatively easy quantification—the universe of need estimates have already been good enough to affect policy. It was determined early, for example, that the number of out-of-school poor youths, particularly boys, in need of training programs like Job Corps and Neighborhood Youth Corps was being sharply cut into by existing programs. And as a result, spending on these programs was held down and moneys put instead into adult job programs, where the need was much greater relative to existing activity.

Perhaps the main impact of the economic style of thinking on the analysis of noneconomic variables, however, has been in the field of program evaluation. Typically, program evaluation, when carried out at all in government agencies, has been evaluation of operations. That is, programs have been looked at to see whether rules have been followed, administration was working smoothly, unnecessary duplication was not present, people were feeding into and out of the programs at a reasonable rate, and suchlike. Little was done to see whether the impact of the program as measured against its objectives was high, low, or nonexistent. The evaluation of programs as measured against their objectives is sometimes called impact evaluation and sometimes program outcome evaluation. As put by my colleague Walter Williams:

In the past, social action agencies have measured operating "performance" in terms of honesty (no embezzlement), prudence (no profligacy), cost control (not using too many paper clips), and occasionally relatively crude output standards (the number of job placements in a training program). However, under cost-benefit standards, for example, the program manager or operator can be honest, prudent, and thrifty (all no doubt great virtues) and still look like a clod with a shockingly low benefit-cost ratio. Beyond embarrassment, evaluation data have a potential for

³ The analysis referred to in the previous section which indicated that adult job programs were understressed at the beginning led to corrective action. There can be little doubt now that greatest immediate needs are for more and better income maintenance.

either restricting program funds or forcing major changes in program direction. One can hardly assume passive acceptance of such an outcome by program managers and operators.⁴

This perhaps explains one reason why the concept of impact or outcome evaluation is one which has not been widely implemented.

Given that impact evaluation should and will be carried out, the question may still be asked "what kind of impact should be evaluated for?" Each program evaluated has both *proximate* and *ultimate* effects. The proximate effects are those which a program has upon its immediate objective—an antipoverty health program upon the objective of improving the health of the poor, for example. The evaluation of the ultimate effects is that which measures the program effects against the overall program objectives—effectiveness of the health program as a means of decreasing poverty through improvement of the productive capacity of the poor.

The economic style of cost-benefit thinking indicates that the ultimate effects are the important ones. That is, if a program was created and funded in order to attack poverty, its benefits should be evaluated by how well it does attack poverty. This is obvious, but it is an ideal very difficult to reach in practice. The theory which connects an activity like a health program with ending poverty is not a clear one. To what extent does family health care provide an efficient way of making wage earners out of disabled adults, and to what extent does preventive medicine applied to children under the same program prevent them from disability later? These questions are not really answerable nor is the state of the art in evaluating such programs even for their proximate effectiveness very far advanced.

For these reasons, OEO program evaluations have for the most part concentrated on improving evaluation for proximate effects and only for programs like manpower where the connection between proximate program effects and ultimate antipoverty effects is pretty clear (i.e., the proximate effect of a manpower program is increased earnings and increased earnings cut directly into poverty) have we been able to pay much attention to ultimate effects. Even the proximate evaluations, however have affected ongoing policy decisions. The dropping of the Work-Experience Program, for example, was due in large measure to an evaluation of its ineffectiveness in getting people jobs, and the Small Business Development Corporation program was taken out of OEO when it was shown that, whatever its virtues it simply could not reach poor people. More generally, budget decisions at the margin—what programs to increase how much—have consistently been affected by evaluative results.

In any case, the point is that impact or outcome evaluation—evaluation for effects rather than smooth operations—is in itself a major advance due in large part to the economic style of thinking. As in the analog mentioned above, the economist looking at a private production process is interested in maximizing profit. For this reason he may recommend against that engineering process which is most efficient in terms of producing at the lowest unit cost, if in order to carry out this process so many units have to be produced that oversupply depresses price and profit is less than it would have been with a process

⁴ Williams, Walter, "Developing an Agency Evaluation Strategy for Social Action Programs," Office of Economic Opportunity, Dec. 16, 1963.

less "efficient" in engineering terms. Conversely, economics recommends hiring less "efficient" workers if they will add to total profits because they can be paid less.

Similarly, the smoothest bureaucracy does not necessarily produce the greatest social effect, and it is the economist's duty to point this out. The Community Action Program of OEO, for example, has frequently been accused of sins ranging from political activity to mismanagement and certainly not all of these accusations are unjustified. Yet looked at for impact on its objective, the community action program has had undoubted major effects in improving the meager institutional base and the sense of powerlessness endemic in the ghettos before 1964 and as such has had a major effect in its objective of combating poverty.

ECONOMIC ANALYSIS OF ECONOMIC VARIABLES

Much of the analysis done by economists in OEO is, of course, analysis of economic variables. And in a sense the discussion of such analysis completes the circle which has begun with the discussion at the beginning of this paper concerning the broad economic research done by agencies such as the BLS and the Census Bureau. Much of the economic analysis necessary for the war on poverty is of such a basic type not immediately oriented to policy. And because there is a Gresham's law of policy analysis—not that bad analysis drives out good but that immediate policy crises drive out long-range analysis in a policy oriented government office—most of this longrun sort of analysis has been sponsored by OEO rather than being done by OEO's own economists. For example, OEO has sponsored substantial special data gathering efforts by the Census Bureau to gain information not previously available on the poverty population. Such information on matters like the assets and liabilities of the poor, the training of the poor, the migration of the poor, had not previously been carried out systematically, and it was gathered by special additions to the current population survey taken in March 1966 and 1967. These data are being tabulated by the Census Bureau and The Brookings Institution. In addition, Brookings is taking on the basic research duty of planning a program of analysis for these new data once they are tabulated.

A significant part of the basic economic (and noneconomic) research done for OEO is done by a newly created institution, the Institute for Research on Poverty set up at the University of Wisconsin, Madison. Realizing early that the Gresham's law of crises was in effect and that no matter how much it expanded, the economic research staff of OEO was going to find immediate policy oriented research which had to be done (a Parkinson's law which underlies the Gresham's law) the decision was made to set up a research institute. The Institute for the Research on Poverty has been carrying out research in fields as diverse as income maintenance, the earnings effect of education, methods of program evaluation, and the relationship of selective service to the war on poverty. Few of these studies would have been carried out by OEO. Most of them have at least a longrun policy relevance although no demand is made that they be closely connected to policy so long as they concern aspects of poverty.

The role of economic analysis within OEO, then, is largely to bring together and integrate the policy implications of the various economic studies being carried on within and outside the OEO organization. For example, in the field of income maintenance, economists early became interested in the device known as the negative income tax. Starting several years ago economists have been playing around with variants of the negative income tax, designing schemes with different rates of payments, different groups of recipients, different costs, etc. The common basis for all of these schemes has been an untested assumption: That an income maintenance program of the negative income tax type, by taking away only part of a dollar for each dollar earned by a recipient would provide far more incentive to work than the kind of welfare plans which take away a dollar for a dollar and not much less incentive than no payment at all. Logically—particularly by the logic of the economic man—this seems reasonable. Actually perhaps it is not so reasonable.

To get information on this, the OEO is sponsoring an experiment in the State of New Jersey with a negative income tax type of scheme called the graduated work incentives plan.* This carefully designed social experiment, one of the first of its type, provides a sample of 1,000 families with income-related payments on a negative income pattern, and it compares the effects of these payments on labor market and other variables with the experience of a similar control group of 200 families receiving no payments. This is an experiment designed by economists and other social scientists to gain information on economic variables such as labor market participation as well as a large number of sociological variables. The administrative structure of the experiment leaves OEO economists with the final responsibility for management decisions and with the particular responsibility of relating results to policy. Economists as well as sociologists of the Wisconsin Institute for Research on Poverty carry out the overall policy management of the experiment, and a group from the Mathematica Corp. of Princeton, composed primarily of economists, manages the experiment in detail. As such, the New Jersey experiment will provide not only a test of certain aspects of the negative income tax but perhaps a test of the relevance of economists at all levels of policymaking.

One other example of the utilization of economic policy research on economic variables which deserves mention is the study of rural to urban migration—both the incentives to move and the effects of such movement. Here again the economic policy analysts at OEO are managing and drawing the policy implications from a set of external studies being done both by economists, sociologists (particularly those specializing in demography), and psychologists.

The preparation of tabulations and basic data analysis of migration data from the special survey gathered by the Census Bureau for the OEO is being made by the Population Section of the Economic Development Division of the Economic Research Service of the Department of Agriculture. This will yield, for the first time, basic data on the migration patterns of the poor.

At Boston College, Marc Fried is investigating retrospectively the problems faced by rural Negro migrants in adjusting to the new atmos-

*Further discussion of this issue is found in the papers by Weisbrod in vol. 1 of this collection, and Rivlin in this volume.

phere of the urban environment. In particular, it focuses upon the question of the severity of the problem of adjustments to the urban work routine.

A study under the direction of Daniel O. Price, a sociologist at the University of Texas, examines the variances of a number of variables both within and between migrant Negro, Latin American, and Anglo groups as compared to control groups of nonmigrants from matched points of origin. Insofar as possible, information will be obtained on the characteristics of migrants before and after migration and for a nonmigrant control group. Variables like type of employment, education, income, family and marital status, job satisfaction, etc., will be assessed in order to find out whether or not socioeconomic status has improved as a consequence of migration.

A new study being conducted by Dr. Barbara Reagan at Southern Methodist University is a longitudinal analyses of low-income families (Mexican-Americans, primarily) who make occupation and location changes in an attempt to break the cycle of disadvantage. Basic to the study is a group of migrants trained and moved to the Dallas area as part of a Ling-Temco-Vought JOBS program. As control groups there are a rural nonmigrant group, a migrant control group which is not participating in the training, and a control group which is not disadvantaged and not migrants but which works at similar jobs in the same plants. The SMU study focuses heavily on the dynamics of the spending decisions made by these groups.

The most recent study being financed by OEO in this area is a study by Stanley Masters at Rutgers University of the impact of rural migrants on urban areas. This study uses an established set of data—the 1-in-a-1,000 sample from the 1960 decennial census—to examine the effects of rural migrants upon the urban scene. It addresses questions like: Are urban problems concentrated among migrants from rural areas? Are differences in education an important factor? Do the experiences of Negro immigrants differ even after standardizing for differences in education?

These are widely varying studies designed to fit a pattern of policy relevance. The overall question to be answered is: What are the positive and negative economic and social effects of rural-to-urban migration; how do they balance out? Policy now operates on the basis of hunches and as noted in the next section, hunches differ. Objective analytical answers will greatly strengthen one side or the other in the debate over the values and costs of such migration.

THE APPLICATION OF POLICY ANALYSIS

The example of the rural to urban migration studies provides an introduction to the final question, that of the effect of all this analysis on policy as it is actually made. The economist's standard hypothesis for migration studies is that such migration is both natural and useful. Migration from country to cities in response to economic incentives has been going on in the United States and the world for centuries. In terms of the economy we are all better off with people moving to locations where they can be more productive; in terms of the individuals who move, their economic welfare is likely to be increased by the move.

Politically, however, these hypotheses are not popular. For one thing, civil disturbance is blamed on the inpouring of rural migrants

to the city. This idea continues although data on the rate of such migration and on the riot participants (usually young, lifelong residents of the riot city) in disturbances indicate that recent migration is not a major riot factor. There is also concern about the sending areas with distress at what seems to be the emptying out of some rural areas. In any case without going into the rights and wrongs, the 1967 amendments to the Economic Opportunity Act as passed by the Congress contain the following language in section 201 (b) :

It is further declared to be the purpose of this title and the policy of the Office of Economic Opportunity to provide for basic education, health care, vocational training, and employment opportunities in rural America to enable the poor living in rural areas to remain in such areas and become self-sufficient therein. It shall not be the purpose of this title or the policy of the Office of Economic Opportunity to encourage the rural poor to migrate to urban areas, inasmuch as it is the finding of Congress that continuation of such migration is frequently not in the best interests of the poor and tends to further congest the already overcrowded slums and ghettos of our Nation's cities.

The matter is thus settled for public policy purposes and although our ability to do studies discussed above is not limited by the language, certainly confirmation by the studies of the economist's hypothesis could not be translated into policy under the Economic Opportunity Act as it now stands.

All this comes to the reiterated point that decisionmaking is not an analytic process, it is a political one.* And in spite of the fact that in the migration example I do not think the action of the Congress was a wise one, I do feel strongly that the political process is the necessary one and that analysis should and will be only a partial input to this process. It is impossible to really separate value judgment from analysis and delineate clearly a field in which analysis should be supreme. In the migration case, for example, I believe it will be demonstrated that migration is an economic process likely to help in ending poverty far more quickly than various programs for keeping people in rural areas. What cannot be demonstrated is that the value of ending poverty this way is superior to the other values held by those who oppose the migration. Whether these opposing values are based on a belief in the superior physical and psychological health of the countryside or anything else, I would hope that the political process would lead to a decision favoring my value judgment but I would not argue that my analysis proves that I am "right" and others are "wrong." In a sense what I am reaffirming is Lionel Robbins old dictum that goals *per se* are not the special domain of the economist as "scientist." Rather economic analysis is aimed primarily at shedding light on the alternative means of reaching goals. I believe that by carrying out meaningful and plausible analysis we have illuminated the nature of alternative choices. OEO more than many agencies looks at the problems of choice as being one made at the margin. OEO more than many agencies thinks in cost-benefit terms. OEO more than any other agency stresses objective impact evaluation as an input to decisionmaking.

Three factors came together at OEO to make the use of economic analysis real, rather than window dressing :

*Further discussion of this issue is found in the paper by Polsby in this volume.

1. Because we were able to hire good economists we were able to do good analysis.

2. Because these economists were willing to focus on hard, dirty, but relevant policy problems which usually carried them beyond their economic tools, our analyses were taken seriously.

3. Because both the Directors of OEO were interested in utilizing economic types of analysis and thinking it was taken seriously by the rest of the agency.

These three, I think, are necessary conditions for successful utilization of economic analysis. Conversely, poor analysis, irrelevant analysis, or uninterested bureaucrats will inevitably lead to analysis with little impact.* If success is defined modestly—again, in terms of improving the rational input to decisionmaking rather than making final decisions by analysis alone—I believe that through combination of the three favorable factors, OEO and the war on poverty have made successful use of the analysis.

WHERE NOW?

The question of the future for economic analysis in poverty programs breaks down into two parts. The first is where such a capability should be located as the new administration restructures the anti-poverty effort; the second, what the capability should be used for.

It is a fact that the 1965 program budgeting directive of the Bureau of the Budget did not create effective analytical capabilities in most agencies overnight; or for the most part, at all. In the social welfare field, such capabilities have really only existed in OEO and in the Department of Health, Education, and Welfare. The OEO capability, together with the legislative mandate to coordinate all antipoverty programs, made it possible for analysis to have an effect on a very wide variety of programs both within and outside of OEO. As discussed above it affected budgetary and other decisions for OEO-managed programs; it also affected decisions for programs less closely connected with OEO such as JOBS. OEO analysis has had much to say even about programs as remote from OEO management as income maintenance.

In general, however, it is almost impossible for a single agency both to coordinate a wide set of programs such as the antipoverty effort has been and to operate directly a subset of these programs, a fact which is behind the efforts of the new administration to separate independent antipoverty activities in OEO or a successor agency from overall coordination of social welfare programs. Such a separation, however, can make the analytical activity carried out in large part within OEO in the past even more crucial than before. Analysis can be a vital instrument helping decisionmakers exert true control over policy. Knowledge of the implications of alternative courses of action—which is what this whole thing is about, after all—must lead to decisionmaking which is improved in that it is more likely for the courses of action decided upon to have the expected effects.

Where such an analytical capability belongs, then, is very much a function of where the administration wants to have its major control

*Further discussion of this issue is found in the papers by Hoffman, Rivlin, and Marvin & Rouse in this volume.

node. If the desire is to put the control in the Bureau of the Budget, for example, adding capability for economic analysis to the Bureau staff would increase greatly the degree of control already possessed by the Bureau.* The otherwise excellent program examining staff of the Budget Bureau substantially lacks technical expertise in economic analysis, and the addition of such analytic capability could make the Bureau an even more powerful controller. Or such a capability could be added to the staff of the new Urban Affairs Council, if the desire is to put the power there. It could also be put into the upper echelon of the Department of Health, Education, and Welfare (which already has substantial capability along these lines) or the Department of Housing and Urban Development. The point is that there is no "correct" place; if, as contended here, there is power in this kind of knowledge and analysis then it must be a political decision where this power should be located.

So far as the future subject matter for such antipoverty/social welfare economic analysis is concerned, there are three categories into which such substance might fall. First, it is possible to recommence an augmented effort in 5-year planning.** Frankly, I think such an effort would be misplaced. The general outlines of a 5-year antipoverty plan exist; they can be changed as new analyses or political imperatives indicate. But because the 5-year plan is a general outline and statement of direction rather than a blueprint and because it has been done not once but four times in OEO it would seem something of a waste of time to start all over again. Review and change, yes; recommence, no.

Second, it would be possible and it seems to me desirable to continue with the kinds of noneconomic and economic studies described above. Certainly the type of program evaluation commenced all too recently should be continued. So should studies like the rural-to-urban migration work and related studies on rural and urban economic development. Additional experimentation of the New Jersey graduated work incentives type should be expanded substantially; in the areas of income maintenance, job training, and, particularly, education, much information is needed which can be gained only by such experimentation. And the more general and academic type of work being carried out at the Wisconsin Institute for Research on Poverty must be continued.

I do not think, however, that any of the above separable studies and experimental efforts completes the picture. The keystone to the utility of economic analysis is not readily describable in any listing of such studies. Rather it is the application of the analytical, alternative-examining, cost-benefit type of thinking to the ongoing policy decisions which must be made at the top echelon of government. To the extent this is done, economic analysis in government will be effective. If it is not done, policy choice will be made in ignorance of the likely effects of such choice and results achieved will be only a random function of results intended.

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

**Further discussion of this issue is found in the papers by Carlson in vol. 2 of this collection, and Rivlin in this volume.

PROBLEMS OF RESOURCE ALLOCATION IN HEALTH

BY ROBERT N. GROSSE

Robert N. Grosse is Professor of Health Planning at the School of Public Health of the University of Michigan.

In the field of health, allocation decisions must be made in many dimensions: in terms of the organizations and professionals involved, the beneficiary or target populations, geographical locations, time horizons, and the health problems to be attacked. Complexities notwithstanding, planning and allocation decisions must be made throughout the entire spectrum of health activities, taking into account both the political environment and the objective problems of evaluation and measurement. In this paper, Dr. Grosse discusses some of the informational requirements for making rational decisions, and examines the progress made by the Department of Health, Education, and Welfare in developing appropriate information, analysis and planning systems in the health area.

He discusses the contributions made by program analysis, through program-budgeting and cost-benefit analysis, to the identification and evaluation of alternative approaches. He also cites some of the limitations of this approach: in particular, the considerable gaps in necessary information and the inability to quantify many health variables. Dr. Grosse argues that the PPB approach eliminates many of the drawbacks of the previous budgetary system. Because earlier systems tended to be formed from "the bottom up," budgetary allocation reflected the interests of each subagency group rather than an overall agency perspective.

Dr. Grosse provides examples of the types of analysis which have been done under the PPB system, and points out both the usefulness and the limitations of the PPB approach for decisionmaking. "Issues are sharpened, and quantitative estimates are developed to reduce the decisionmakers' uncertainty about costs and effects. Nevertheless the multiplicity of dimensions of output, and their basic incommensurabilities both with costs and the outputs of other claimants for public expenditure still requires the use of value judgments and political consensus."

Dr. Grosse discusses the effort by HEW to improve policy analysis by instituting a long-range budget process. He also points to some of the remaining problems, one of the greatest being the inadequacy of program evaluation efforts and commitment to it. He judges that progress is being made in this area, however.

Introduction

At this stage of our knowledge about how to improve resource allocations within the field of health, it is much easier to discuss problems than to point out solutions.

Approaches to better resource allocation decisions require insight into who participates in the allocating process, what the resources are, the groups to which we allocate and the accomplishments of differing allocations.

Allocations take place in the "market"—either the conventional one of money bidding for goods and services, or that of political forces and coalitions, or most usually some combination of the two. Rather than address the awesome challenge of explaining how resources are allocated throughout the entire health area—I would like to narrow

my comments to problems of resource allocation on the part of a governmental or quasi-public agency which has control over allocating some of the resources at stake. Such agencies function within economic and political marketplaces, and must take the existing operations of these markets into account, but they may also be concerned with improving the allocation process itself.

In particular, this paper discusses some of the informational requirements involved in improving the allocation process, and describes approaches taken by analysts in the Department of Health, Education, and Welfare to develop information, analyses, and a planning system.*

Resource allocation decisions usually appear as allocations among "programs," but that is probably too neutral a term to convey much meaning.

Clearly, we allocate among organizations. Governmental budget and legislative decisions do this, and voluntary groups such as community chests and welfare federations do. We allocate among institutions—hospitals, medical schools, research institutions, nursing homes, neighborhood centers, third party insurers.

We also allocate among beneficiary or target populations such as Indians, Negroes, whites, the poor, the middle class, the retarded, veterans, etc.

We allocate among professions—or between professionals and target populations, so much to psychiatrists, to other physicians, to nurses, to social workers, to professors, and to the individuals requiring the services of these different professional groups.

We allocate among locations—central city versus suburban versus rural, North as against South, Texas as against Massachusetts.

We also allocate over time, investing in building hospitals and nursing homes, training nurses and doctors, and biomedical research and development, as opposed to purchasing current services.

In another sense of time, we allocate among generations—such as children, working age, aged.

We also allocate among health problems such as diseases: tuberculosis, syphilis, mental illness, cancer, etc.; among approaches to disease control: research, prevention, and treatment; between approaches: categorical versus comprehensive programs.

Although I have touched on only a few examples, even thinking about targets for allocation introduces complexities. Learning how we have been allocating is hard, and deciding what to do may seem impossible. It is easier to select one facet—multiple sclerosis, underfed children in Texas, or cancer care—and develop tactics to secure more money for it than—the economic and political strategies for solving allocation problems—who or what gets less when some get more—is a rough business.

What is it that's allocated? We usually think of money—and that's a meaningful and convenient measure, but we recognize that scarcity of resources in physical terms sometimes may be more constraining and thus more significant—number of physicians and their time, availability of facilities for the mentally retarded, transplantable organs, kidney dialysis units, and personnel. Among the things we are allocating may be life and death.

*Further discussion of this issue is found in the papers by Rivlin and Brandl in this volume.

Nevertheless resources are allocated every day in any complex organization. But the problem which faces us is to make "better" allocation decisions. It seems reasonable that a better understanding and measurement of the costs (what is given up) and the effects (what is accomplished) of various possible courses of action will improve allocation decisions. The search for a clearer identification of what we are really trying to accomplish and how we are going about it has led to more systematic thinking about objectives and classification, information and analysis systems.

These ideas—few of them novel—are surfacing as aids in resource allocation decisions at a time when the health planning field itself is under great stimulation. This stimulation is being caused by increasing interest in health services on the part of the American people, evidenced by the growth of personal and governmental health expenditures, increasing concern over mounting costs and prices, and moves on the part of the Federal Government to require and support planning and decisionmaking at State, regional, area, and community levels. [1] Some hope that planning can work may also come from the assumption that the systematic techniques of systems engineering, operations research, cost-effectiveness analysis, and program budgeting make the task more feasible.

How do we proceed to better understanding? The first step would be to structure the significant elements of our health system. There are many approaches to this—let me discuss one that has been meaningful to me.

We start with identifying problems—health situations which need improvement and where intervention or change may be useful. This involves an assessment of our population and its subsets in terms of health status and access to health services. It also involves examining environmental hazards and social forces which threaten to affect adversely the normal development or health of our people.

Second, what are the current and potential activities that are or might be addressed to these problems—and we need to know not just the what of them, but also the who and the how—who manages and carries them out, with what instrumentalities, how are they organized, and who benefits? These include the delivery of personal health services, environmental control, consumer education, and programs to affect social factors related to mental health.

Third, we need to know what stock of assets are needed for health activities—knowledge, technology, manpower, and facilities. What do we have, how are these stocks added to, and how are they organized into desired activities?

In addition, we need to understand how resources are or could be financed, the effects of various financing mechanisms, and the barriers between consumers and services—financial and social.

We need evaluations to understand what programs accomplish—how the delivery of services affects health; how environmental control programs affect the ambient world; and at what costs?

All of these form a system of interdependencies or interrelationships. For example, through the political and budgetary process resources are allocated which, in turn, provide services or goods that benefit certain sectors or groups in our country. The process, furthermore, appears to be somewhat circular in that we have what students

of cybernetics would call a "feedback" effect. One of our problems in resource allocation is that we are never sure whether the feedback will be positive or negative. Will the beneficiaries of a policy alternative feel that they are actually getting benefits? I suppose that in Los Angeles if we were able to implement a program which would suddenly get rid of smog, most citizens would be quite pleased and we would expect to get a positive feedback to continue or increase our program. On the other hand, many health programs are not so visible, or it may take several generations for us to be able to discern their impact (for example, biomedical research activities).

Finally, we also need to understand the political environment in which we work. [2] We must be able not only to identify where it makes sense to intervene, i.e., where political and economic costs can be minimized, but also to appreciate the broader policy concerns which set the context for our activities.

Analysis of major policy areas like health is not just a mechanical exercise of mathematics; we have to consider qualitative factors as well which may affect the outcome of our studies. Indeed, sometimes this is all we have to work with. Knowing the number of beds in a hospital or the beds' utilization rate is only a rough index of capacity and not a measure of the quality of care. Similarly, there are trade-offs between health services and education for improving the communication flow between patient and doctor; such tradeoffs may actually involve value conflicts where we cannot measure the benefits of education and health in the same terms and in these situations the judgment of political decisionmakers is required to resolve the conflict.

Now I would like to be more explicit and give some content to terms like better resource allocation.

Improving the allocation of health resources does not necessarily mean saving money or cutting budgets. In fact, in order to make improvements sometimes we have to spend more. What is meant is getting more out of the resources such as money, time, doctors, drugs, nurses, etc., which are involved in health activities. As we are all aware, the country needs more physician and nursing services. But our problem is not just how much more; we are also concerned with the distribution of these scarce resources. We want to find ways to increase the productivity of the doctor, but we also want to make sure that all segments of our Nation are able to receive medical care when they require it. Thus, when we talk about improving or maximising the use of our scarce health resources, we can mean many things and what we mean exactly is a function of the analysis or problem under consideration.

Actually the annual cycle of the budget process establishes the context of resources allocation decisions in the public sector. We often talk about the budget as a plan. When we make budget decisions we are setting priorities for the attainment of various goals either by adding, cutting back, or modifying programs. There is some truth to the saying that budget decisions are program decisions. But the saying is also misleading because the relation of the budget to current and future programs is frequently obscure and uncertain. The budgetary categories are frequently either administrative organizations such as government bureaus or resource inputs such as construction or personnel. To determine the "program" for alleviating mental retardation, for example, one must analyze in some depth the programs of a score of

bureaus in several different agencies. Further, most often neither the impact on future years beyond those for which we are budgeting, nor the program objectives within whose context the budget has been formed are made explicit. Somehow we must attempt to reduce the uncertainty of dealing with the future; we have to plan even if most plans have to be continually revised.

To talk about uncertainty reduction is relatively easy but to do something about it is quite different. Health is a rapidly changing policy area. The technology of delivering medical services, the list of our health priorities and goals, and even the definition of what we mean by health—all are in flux. But the fact of a dynamic social and physical environment argues for more knowledge, for more analysis and understanding. For example, we have to know how well we have been doing with our current programs. In a complex organization, it is not unusual not even to be aware of just what these programs are, let alone to have some technique for evaluating them. Most studies of actual decisionmaking find that the problem solver starts looking for alternatives somewhere around the neighborhood of the present alternative. It is not just that planners or problem solvers are myopic; we also have our own resource problems. There are costs of getting additional information and there is also a scarcity of trained analysts. But in spite of these limitations, one of the signal contributions of planning and analysis is to extend the range of search, to seek out and develop new, imaginative, and hopefully better, alternatives.

This we attempt to do by developing a "model" or a framework to analyze the particular health system or subsystem. Such a model explicitly takes into account the same informational needs which I have mentioned. It abstracts the relevant features of the various institutions that are germane to the particular health problem which we are examining. This process of system definition sets the stage for our work by limiting the problem to which we pay attention. We have to delimit or arbitrarily set boundaries around the problem. Thus, one aspect of system definition or model construction is to define boundaries. Another aspect of system definition is to develop some understanding of the relationships which exists among the elements of the system. To do this we have to find out not only that a particular health agency exists, but also what aspects of it are related to the other variables in our analysis. The notion of interdependencies and the ability to specify functional relations is what we mean when we talk about a system.

Although in making resource allocations we may find it an analytical convenience to talk as if there is such an animal as a health system, in fact, the system that we know about is fragmented. We tend to be analyzers rather than synthesizers. This is the result of the fact that we, as health planners, are usually concerned with specific agencies of more or less specified jurisdictions. No single agency, public or private, makes authoritative resource allocations for the totality of the Nation's health. Thus, there are many actors who provide inputs into the Nation's health decision process.

It would be a misperception to view analytical tools such as cost-effectiveness studies or planning activities such as program-budgeting as centralizing decision making or compelling the creation of the health system *qua* system. What these tools are and what they are used for is very much a function of the particular organizational context in

which they are set. The point is that making resource allocations for the total field of health is not an appropriate description of what analysts do. We are always involved with some chunk or aspect of health as pollution, or the problem of the aging, and with the particular organizations which deal with each problem.

In addition to the fact that many agencies are involved in the field of health, we also cannot expect any single agency head to make all its decisions, on everything, for every budgetary cycle. Even if you assume that a particular agency functions like a pyramid with a single policy or decision maker at the top (which most agencies do not, in practice), this "omniscient" individual cannot possibly have the time nor capacity to look at everything. The planning staff has to be selective. And one interesting problem is just what criteria they will use in making their selections. Obviously the planner could ask the decision maker but sometimes the latter may not know what he wants. Or, if he does, then this just shifts the problem to finding out the selection criteria of the decision maker. One could anticipate that the decisionmaker might want to be able to make choices on the highly "visible" programs of the agency. An index of visibility may be a high dollar commitment or high resource type program.

Or the program may be low in the resources assigned to it but still be very politically visible because of the existence of a small and active constituency or clientele. Another likely criteria would be to present choices concerned with omissions or gaps in the health system. Where are the areas of policy in which the particular health agency ought to be involved? This is not particularly simple to answer in any comprehensive fashion. But when a significant area appears to a planner to have been overlooked, there is a high probability that he will select the area for further attention. In addition to feasibility and omissions, the policy preference of the planner, himself, undoubtedly influences his criteria of selection.

In other words, the line between analyst and decisionmaker is somewhat blurred. The planner or analyst is involved in what Herbert Simon once called uncertainty absorption. He structures the decisions or choice situation by selecting certain problems and alternatives, and planners and users of planning output ought to be aware of this. Because we do have so many people involved in the health field, because we do have to cut our information and time costs, and because no single decisionmaker even attempts to consider everything, and because planners themselves tend to also be selective, I sometimes think "*comprehensive* health planning" may be a contradiction in terms.

The use of planning and analytical tools, however, does make a great deal of sense when we view it within its organizational context. Therefore, it would be useful to discuss how program budgeting interacts with the use of tools like cost-benefit or cost-effectiveness analysis to improve the quality of organizational decisionmaking or resource allocation.

Program budgeting is one management tool which assists in health planning. It has two central features: (1) A framework designed to show the resource allocations which are made to problems, beneficiary groups, organizations; what activities and resource developments are being funded; and what results are anticipated from each; and (2)

a multiyear program designed to reflect in these same terms what we are moving toward in the future.

A major aspect of the program budget is the structuring of the health system—of that part which the organization engaged in planning affects. And it should be set in such a fashion that it relates to the broader health system.

The program budget I have described has a structure too complex (in that it has numerous dimensions) to be laid out as a simple laundry list. It is a management information system which can be called upon to tell us what we are doing in each area of interest.

If an area of interest is one of disease control, we must be able to pull together all of the principal actions directed against the particular problem—from State and regional programs, from projects, from research and training.

If we are interested in a target population, we must be able to pull programs affecting that population out from programs which may be aimed at disease control, financing of services, comprehensive care, and environmental protection.

When we have this capability, we have a framework for better planning. What is then *best* to do will not spring forth as did Athena from the head of Zeus. If all the programs could be related to a single dimensional output on whose virtues and validity we could all agree, the problem of allocating resources would be much simplified. But we know this is not so. Outputs of health programs have numerous dimensions—changes in morbidity, mortality, disability, services, contamination, etc., and these outputs apply to different components of our people.

For this reason, cost-effectiveness analyses cannot tell us the preferred mix of programs to be included in our program budget. Rather the analyses are aimed at one or another set of problems—air pollution, kidney disease, child health, delivery of services to the poor. They explore the costs and accomplishment of alternative approaches to these narrower problems.

When we have information on costs and benefits we can not only indicate the preferred alternative for each problem, but also have some additional information by which to improve the total allocation. This addition is simply more insight into what we get for what we pay out.

The program budget is a means of noting what we are doing in an organized fashion—with emphasis on objectives and accomplishments—rather than on the organization or line item inputs of conventional budgets. It may serve to give better insight into what we are doing, give us inspiration for useful change, and form a record for program decisions. It is neither a planning process nor a means of discovering better plans. Rather, its categories should serve as useful organization and communication devices for program decision.

The actual process of planning—the ingredients to be reflected in the program budget—are the analyses and the priority decisions. By analyses I refer to work addressed to designing and evaluating strategies for the solution of problems. Generally, analyses would be conducted in the framework of cost-effectiveness comparisons of alternatives, but in many cases the formal approaches are unsuitable, although use of analytical perspectives is helpful.

Formal measurement of costs and outputs may be of little help in cases where we are trying to select preferred methods of affecting the behavior of institutions; for example, hospital behavior, formation of group practices, or consumer education. We may have a measurable idea that certain behavior would make things better; i.e., resources better utilized, more people taken care of. Our preferred program is to figure what mix of incentives and regulations may induce the desired behavior. While in concept we may believe or hope that the desired effects are thus produced at minimum cost, our program's effects and measures are obscure. For example, if we want hospitals to become community oriented, or to work with other facilities, we may argue and even prove how this would happen, but what we pay them to do is to become concerned—to move in certain directions. How much it costs society and what will happen depends on what institutions respond, how they perceive themselves and their community, etc.*

For example, with regard to air pollution, we can study program alternatives which reduce contaminants at minimum cost within an airshed, paying attention to location of emitters, meteorological conditions, and end-stage costs, etc. But the Government program is designed to move in this direction by providing incentives or penalties. We are rarely certain of the response.

When we have made our analyses, we have more confidence that we have examined alternatives and investigated the relationship to objectives, that we have weighed the responses of society and the costs to society and to our own resources.

We can now begin the process of developing programs in each area of interest and forming mixes of these. But resources are scarce and we cannot buy all that we want. We must negotiate with other actors in the organization and in the broader social and political environment. Even though we may think we have identified a preferred economic solution, it may have to be modified in order to be implemented by the legislative and administrative process.

Thus, the program structure provides a cognitive map, a frame of reference to consider alternatives using cost-benefit analysis. Similarly when we have identified by analysis an alternative that we may have high expectations of being implemented, we will adjust the program structure to reflect this. Thus program budgeting and cost-benefit analysis interact. They are both different parts of the resource allocation process, but they are also intimately related. Problems of resource allocation are an intrinsic part of both of these activities.

The most recent and most comprehensive attempt to apply quantitative methods to the allocation of resources to health problems was introduced in the Federal Government under the title of the "Planning-Programming-Budgeting System." This approach was first generally used in Government by the Department of Defense beginning in the spring of 1961. [3] In August of 1965 the President ordered all principal agencies of Government to adopt similar systems. The new Secretary of Health, Education, and Welfare, John Gardner, took this charge seriously, creating a new office to develop and implement

*Further discussion of this issue is found in the paper by Schultze in vol. 1 of this collection.

the system, that of the Assistant Secretary for Program Coordination (later called Planning and Evaluation).

While the broad goal of the PPB system was to improve decision-making, especially budgetary decisions, those concerned had different ideas as to what its specific objectives and procedures ought to be. Some were most concerned to develop better insights into program objectives so that the Secretary and his agency heads and program managers would better understand program interrelationships and complementarities. Others believed the most important step was the delineation of long-range needs and goals. The Bureau of the Budget was asking for detailed 5-year plans and analytical bases for all budget decisions.

Program administrators feared not only that the volume of paperwork would increase, but also that program decisions might be made on improper bases, i.e., on narrow economic or quantitative grounds, and by individuals lacking in an understanding of the programs and the issues at stake. Budget and executive officers in the various agencies saw a potential dilution of their responsibilities and authority.

After a period of experimentation in 1966, a system was developed at HEW which was used in calendar 1967, as the basis for the fiscal year 1969 budget. It is this system and its problems which I will discuss in the remainder of this paper.

But first, it may be useful to outline some of the difficulties with the earlier budgetary system.

Historically, budgets tend to be formed "from the bottom up." The cycle commenced with a call for a preliminary budget from the Office of the Secretary with no guidelines as to scale or priorities. Agency heads, in turn, passed the call along to their bureaus, and the bureaus to their divisions, etc. It was usually assumed that existing budget levels were an inviolate base, not needing reexamination. The budget process focused on upward changes. The import of proposed legislation was not considered, but was channeled into a separate legislative proposal process, with little interaction between it and the formulation of the budget. The planning horizon was the budget year, with little longer range planning. Appropriation categories are, for the most part, coincident with administrative organizations, and little attention was paid to competing or complementary programs.

The general philosophy of program managers has been that the social problems their programs are addressing are so vast, and the resources allocated to these worthy objectives so miniscule, that their objective in the formation of budgets is the tactical one of increasing these resources. The effects of programs have not been evaluated in systematic fashion, and alternatives to present approaches remain largely unexplored, especially in the context of budgeting for "existing legislation."

It was not surprising, then, to find that budget proposals usually took the form of asking for increases in almost every program. Bases for these increases were either the ability to grow and satisfy more social needs, or the growing demand for Federal grants on the part of potential recipients. Workload increases, annualization of past midyear increases, and price rises also were considered.

Higher echelon reviews usually consisted of concern with whether the rates of growth were feasible and salable, whether the administrators of the programs were capable, and with giving visibility to com-

mitments of the administration as evidenced by recent legislative programs. Questions about interactions or effectiveness of programs were infrequently asked, and more rarely answered. Attention was paid, of course, to congressional desires and the power of constituencies.

Despite its lack of quantitative analysis and long-range strategy, on a tactical level the system had worked quite well. Budgets had increased, doubling about every 5 years, and scores of new programs had been created by the Congress. But problems loomed on the horizon: the multiplicity of new programs threatened management understanding of what was going on, and it seemed unlikely that the rapid pace of budget increase would be sustained. Problems of imbalance in programs could no longer be resolved by expansion. Choices would have to be made.

Much hope was held by some in developing a system similar to that which appeared to work so well in the Department of Defense. Of course, skeptics were quick to point out that social programs dealt with people, not military equipment, and that quantitative analysis was irrelevant to problems so irrational as protecting and improving the health of the American people.*

There were and are difficulties in transferring the Defense approaches, but the nature of the product was only one, and possibly not the most significant. In national security, the Federal Government has almost total responsibility, and controls most of the resources. In health, Federal expenditures accounted for only 16 percent of the total outlays in 1966. Even of these, HEW doesn't operate many of its own programs. Most of the funds go out in the form of grants-in-aid to State and local governments, universities, school districts, hospitals, and nonprofit agencies. Of its fiscal year 1969 budget, 94 percent were in the form of such grants-in-aid. HEW itself operates the Indian Health Program, the Food and Drug Administration, and relatively small intramural research programs. So the problems to be analyzed are largely affected by funds other than Federal and administered by others as well. There are a multiplicity of factors: 7,000 hospitals, 3,000 counties, hundreds of universities, several hundred thousand medical doctors, 50 States, etc.**

Compounding the confusion is the all too obvious fact that we know little about the cause and effect relationship in social areas. We don't know how Federal programs influence the operating institutions, we don't know the effects of most health services on health status, or what forms of health delivery systems produce better results than others. We lack models, coefficients, and data.

The first step toward improving budgetary decisionmaking in a huge, complicated organization like HEW was to provide comprehensible information about the current allocation of resources and a mechanism for showing how future changes in programs would affect this allocation. As a start one should be able to answer such questions as: What share of the Department's resources is going into health programs? What share is directed toward improving the lives of the poor? What share is directed at assisting old people, and how many

*Further discussion of this issue is found in the paper by Hoffman in this volume.

**Further discussion of this issue is found in the paper by Mushkin & Cotton in vol. 1 of this collection.

people are affected? What share of the Department's budget is devoted to research and is the share growing or declining?*

None of these questions can be answered easily by looking at the conventional budget of the Department. Health programs appear in several different agencies. The Public Health Service, the Social and Rehabilitation Service, the Social Security Administration, the Food and Drug Administration, the Administration on Aging and the Office of Education all have significant health programs. Activities such as research and training are often buried in other programs. The groups affected by programs are not identified in a conventional budget; nor are measures of output or accomplishment (classrooms built, patients treated, students supported) readily available.

For these reasons the Department developed a new information system which serves both as a classification system and as a planning tool. [4] Under this system an individual Department program is classified in a number of different ways—by objective, by the target group in the population at which it is directed, by type of financing (project grants, loans, etc.), by activities used in carrying out the program (construction, training, etc.). The result is a flexible information system which can be used to answer a great many questions quickly and easily and to give a clearer picture of how the Department's dollars are being used.

Along with the dollar information, measures of output of programs in nondollar terms are being developed. At present these outputs are largely limited to measures of initial impact of programs (square feet constructed, children enrolled, persons rehabilitated). Eventually we may be able to provide measures of more ultimate benefits of programs (cases cured, students graduated, individuals rescued from poverty) which will aid in evaluating the effectiveness of programs in meeting their goals.

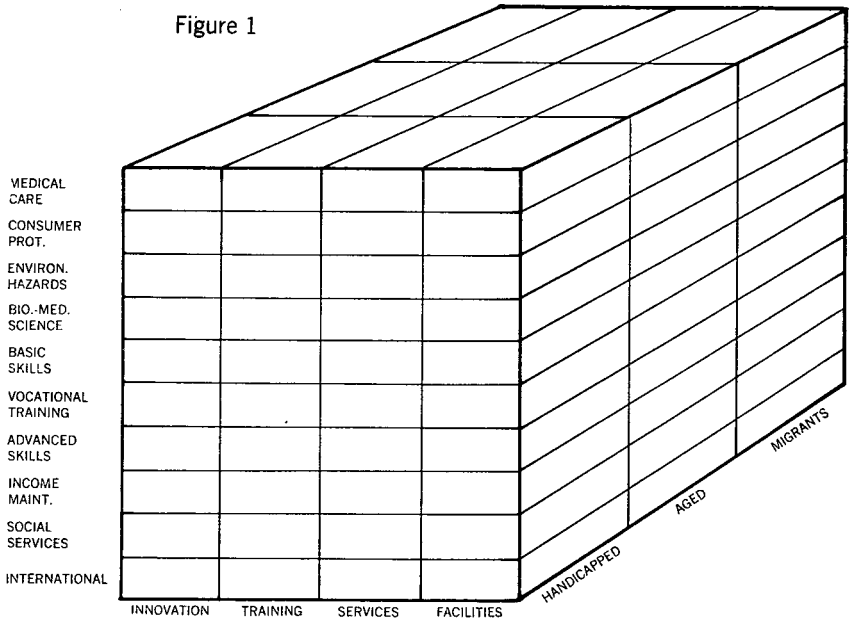
About each of the programs then, there are a number of questions. What is it for? What does it accomplish? Who is being helped? How is it being carried out? How much does it cost? Who carries it out in the Federal Government and elsewhere? How is it funded? These questions lead to the development of a program information structure.

Figure 1 gives some insight into the way in which the structure arrays the programs. On the left-hand stub, are the names of possible program objectives or purposes such as the provision of medical care, consumer protection, development of basic skills, income maintenance, social services, and the like. To answer the question of how, programs are subdivided into activities, a sampling of these is listed, innovation, the training of personnel, the delivery of beneficial services, the construction of facilities. For each we are interested also in whom are you doing it for—the target population. So in this three-dimensional diagram we also look at what is being done, for example, for the handicapped, the aged, and migrants.

A particular program, say facilities for medical care of the handicapped, may appear simply as a cell in the structure. And its program manager asked, "I filled out these forms and all I see is I'm in a box and it doesn't help me to decide anything at all." He's probably right.

*Further discussion of this issue is found in the paper by Wholey in vol. 1 of this collection.

Figure 1



What we're interested in, of course, is building insight into what takes place. We can add other activities to this cell which was concerned with building of facilities for the medical care of the handicapped and we can pick up the rest of the medical care activities for the handicapped and get some more understanding as to whether they are reasonably in balance or not for what we are trying to do. We can go further and pick up what we are doing in the area of medical care for the various target groups. Another way of looking at it is to ask the question of what are we doing for a particular target group in all programs.

Illustrated in the following table, for example, is a way program information can be classified. For a target group—children and youth of low-income families—we can identify these programs—the educational programs, the specific health programs which are aimed at children with respect to child development, crippled children, early case finding and treatment, various social services and money payments, as well. We can begin to look at programs from the point of view of the recipients of the benefits of these programs.

TARGET GROUP: CHILDREN AND YOUTH-INCOME UNDER \$5,000 (AGE 0-21)

EDUCATION PROGRAMS

Improving the education of the disadvantaged

- Educationally Deprived Children (ESEA Title I)**
- National Teacher Corps**
- Educational Opportunity Grants (HEA Title IV-A)**
- Educational Talent Contracts (HEA Title IV-A)**
- College Work Study Grants (HEA IV-C)**
- Vocational Work Study Grants (HEA IV-C)**

HEALTH PROGRAMS

Health problem classification

Child Development
 Crippled Children
 School and Pre-School Children
 Maternity and Infant Care
 Maternal and Child Health
 Comprehensive Maternal and Child Care
 Early Case Finding and Treatment

General health care programs

Hospital Care
 Physicians
 Dental Services
 Nurse Services
 Home Health Services
 Out Patient

SOCIAL SERVICES PROGRAMS

Individual and family services

Day Care
 Foster Care
 Other Child Welfare Services

Strengthening resources and organization of social services institutions

Juvenile Delinquency

INCOME MAINTENANCE PROGRAMS

Other individual and family support

Aid to Families with Dependent Children

If the first step toward rational decisionmaking is a good information system, the second is a strong capability for analyzing the consequences of alternative courses of action. In the past 2 years HEW has undertaken a series of analytical studies of existing health programs and possible alternatives.

One of the first analytical studies of the PPB era at DHEW was a study of disease control programs. [5] Considerable work had been done during the last ten years in estimating the economic costs of particular diseases. Among the best known of these are Rashi Fein's *Economics of Mental Illness*, [6] Burton Weisbrod's *Economics of Public Health* [7] in which he estimated the costs of cancer, tuberculosis, and poliomyelitis, Herbert Klarman's paper on syphilis control programs, [8] and Dorothy Rice's studies covering the international classification of diseases. [9] A generation earlier Dublin and Lotka's classic explored the impact of disease and disability and their relation to changes in earning power. [10] The economic implications of disability were, of course, a matter of central interest in the area of workmen's compensation insurance. [11] It was not surprising, then, that when systematic quantitative analysis of government programs and policies began to spread from defense to civilian applications, one of the first analytical studies was a study of disease control programs.

The basic concept of the study was a simple one. HEW supports (or could support) a number of categorical disease control programs, whose objectives are to save lives or to prevent disability by controlling specific diseases. The study was an attempt to answer the question: If additional money were to be allocated to disease control programs, which programs would show the highest payoff in terms of lives saved and disability prevented per dollar spent? The study defines "disease"

liberally. Motor vehicle accidents were included along with tuberculosis, syphilis, cancer, and arthritis.

I'm talking here not about research, but where a technology exists and the problem is whether to put the same, more, or less Federal funds behind these control programs to support activities in hospitals, States, and communities. The question we address is where should we allocate the resources available for this purpose.

Chart 3 illustrates the approach to one set of diseases, cancer. We looked at cancer of the uterine cervix, breast, head and neck and colon-rectum. We estimated cost per examination, and the probable number of examinations that would be required for each case found. From this was derived the number of cases that would be found for an expenditure level, and estimates of the cost per case found. An estimate was made of the number of deaths that could be averted by the treatment following the detection of the cancers and then we calculated the cost per death averted which ranged from about \$2,200 in the case of cervical cancer up to \$40,000 to \$45,000 in the case of head and neck and colon-rectum cancer.

CHART 3.—CANCER CONTROL PROGRAM: 1968-72

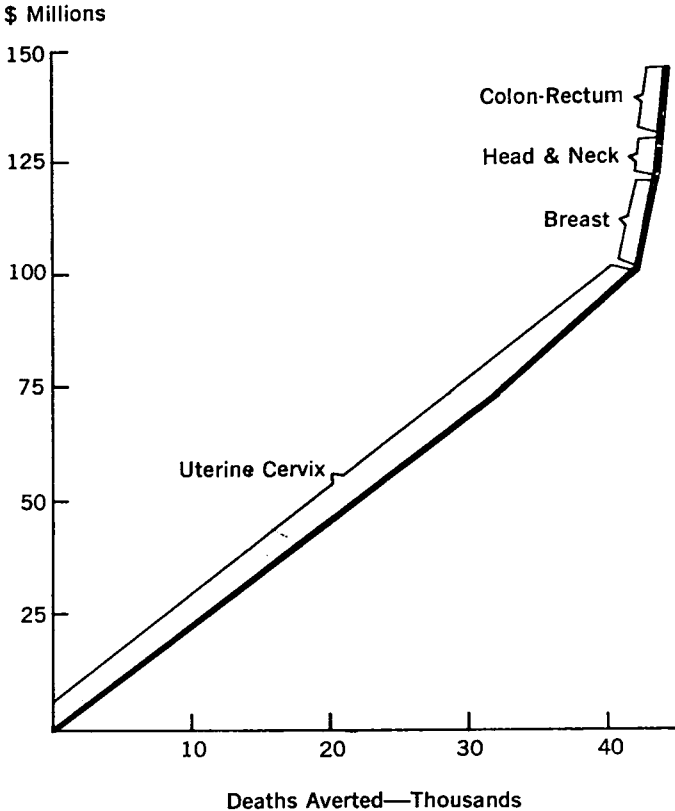
	Uterine-cervix	Breast	Head and neck	Colon-rectum
Grant costs (in thousands).....	\$97,750	\$17,750	\$13,250	\$13,300
Number of examinations (in thousands).....	9,363	2,280	609	662
Cost per examination.....	\$10.44	\$7.79	\$21.76	\$20.10
Examinations per case found.....	87.5	167.3	620.2	496.0
Cancer cases found.....	107,045	13,628	982	1,334
Cost per case found.....	\$913	\$1,302	\$13,493	\$9,970
Cancer deaths averted.....	44,084	2,936	303	288
Cost per death averted.....	\$2,217	\$6,046	\$43,729	\$46,181

On the vertical axis of chart 4 we have plotted the program costs; this includes the cost of the treatment in addition to the Federal detection program. On the horizontal axis estimates of deaths averted are ordered by increase in cost per death averted in each program. Segments of the curve identified to each disease cover the extent of the program which it was estimated could be mounted in the years 1968-72 before running into sharply increasing costs. In concept, the cervical cancer curve is cut off where costs become higher than the breast cancer program, etc. From this analysis one might say that if there is only available \$50 million, cervical cancer should get all the funds. If we have \$115 million, then breast cancer control programs look quite competitive. Head and neck and colon-rectum cancer detection program as major control programs did not look attractive when viewed in this context. The analysts recommended that they concentrate on research and development.

The same kind of analysis was performed for each of the five programs studied (chart 5). There seemed to be a very high potential payoff for certain educational programs in motor vehicle injury prevention trying to persuade people to use seatbelts, not to walk in front of a car, and so on. And then as we move up this curve, again ordered by cost of averting death we begin adding the others. This particular criterion, deaths averted, was not completely satisfactory. The number of fatalities attributed to arthritis were negligible. Secondly, there is the question, did it matter who died? Did it matter whether it was

a 30-year-old mother or a 40-year-old father of a family or a 75-year-old grandfather? On chart 6, dollar savings summing avoided medical treatments and a crude estimate of the average (discounted) lifetime earnings saved are plotted as a variable in place of deaths averted. There are two changes in results: Cervical cancer and syphilis control programs change places in priority order, and we are able to introduce the arthritis program.

Chart 4.



Allocations of resources to programs are developed from such analyses by using information such as this and the preceding charts as an additional insight to give an additional feel for what were relatively high-priority and what were relatively low-priority programs, and then to feed these insights into the decisionmaking process which also considers existing commitments, the political situation, feasible changes in the rates of spending, the ability to get people moving on programs, and so on.

Chart 5.

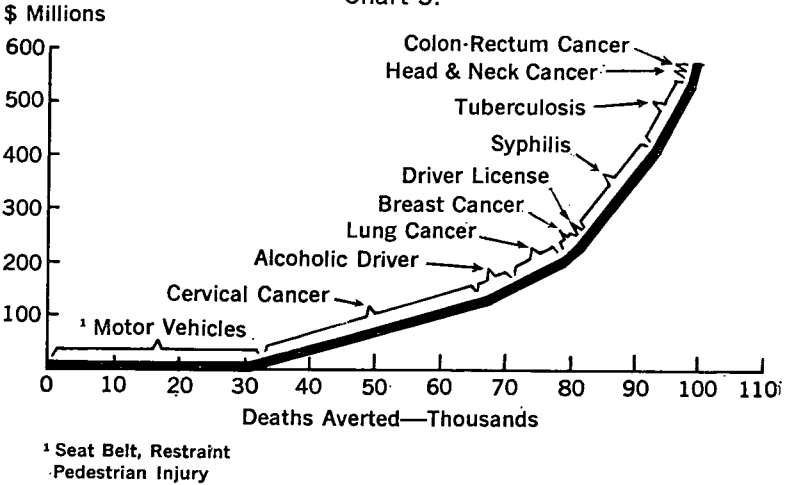
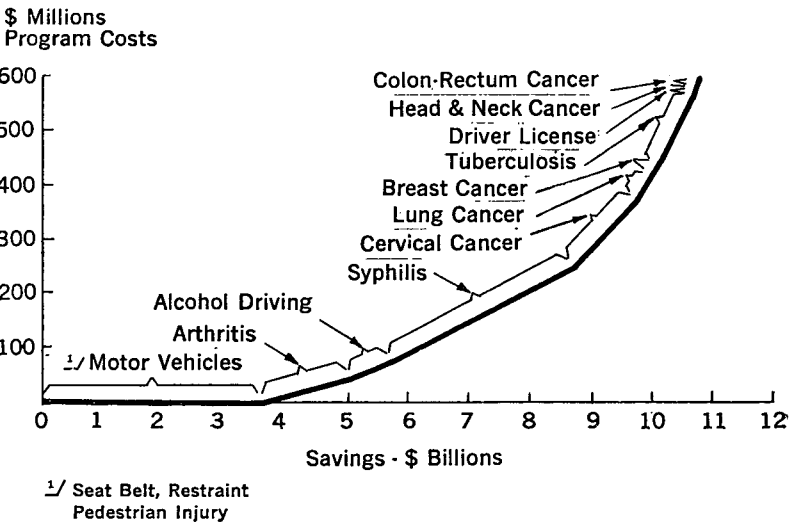


Chart 6.



These studies were not greeted with universal acclaim. Criticisms focused on a number of problems. First, with almost no exception the conclusions were based on average relationships. That is, the total benefits were divided by the total costs. There was little evidence of what the actual impact of increasing or decreasing programs by small amounts might be. If we actually believed the average ratios to be valid at the margin, ought we not to put all our funds into the program with the highest benefit-cost or deaths averted per dollar ratios?

Let me illustrate with a hypothetical example how such marginal information might be used to determine the preferred mix of disease control programs. Assume that we can determine as in the following tables the number of lives saved by different expenditures on disease A and disease B:

		<i>Disease A</i>	
<i>Expenditures</i>			<i>Lives saved</i>
\$500,000	-----		360
\$1,000,000	-----		465
		<i>Disease B</i>	
\$500,000	-----		200
\$1,000,000	-----		270

If we only knew the effect of spending \$1 million, we might opt for a program where all our money was spent on controlling disease A, as we could save 465 lives instead of 270 if we spent it all on disease B. Similarly, if we only knew the effects of programs of a half million dollars, we would probably prefer A, as we'd save 360 rather than only 200 lives.

But if we knew the results for expenditures of both half a million and 1 million dollars in each program, we would quickly see that spending half our money in each program was better than putting it all in one assuming we have \$1 million available:

Our calculations would be:

<i>Expenditures</i>			<i>Lives saved</i>
\$1,000,000	on A	-----	465
\$1,000,000	on B	-----	270
\$1,000,000	\$500,000 on A	----- 360	
	\$500,000 on B	----- 200	560

But suppose we had still more discrete data, as in the following tables which give us the effect of each hundred thousand dollars spent on each control program:

		<i>Disease A</i>	
<i>Expenditures</i>			<i>Lives saved</i>
\$100,000	-----		100
\$200,000	-----		180
\$300,000	-----		250
\$400,000	-----		310
\$500,000	-----		360
\$600,000	-----		400
\$700,000	-----		430
\$800,000	-----		450
\$900,000	-----		460
\$1,000,000	-----		465

		<i>Disease B</i>	
\$100,000	-----		50
\$200,000	-----		95
\$300,000	-----		135
\$400,000	-----		170
\$500,000	-----		200
\$600,000	-----		225
\$700,000	-----		240
\$800,000	-----		255
\$900,000	-----		265
\$1,000,000	-----		270

We could then spend the million dollars even more effectively :

	<i>Lives saved</i>
\$600,000 on A -----	400
\$400,000 on B -----	170
<hr/>	
\$1,000,000 -----	570

The lack of marginal data resulted from both a lack of such data for most programs, together with a lack of economic sophistication on the part of the Public Health Service analysts who performed the studies. Despite the theoretical shortcomings, the results were useful when applied with some common sense.

Practical obstacles of existing commitments made it almost impossible to recommend *reductions* in any program. So the decisions dealt with the allocation of modest increments.

In the case of oral and colon-rectum cancers, the average cost per death averted seemed so high that the Department recommended emphasis on research and development, rather than a control program to demonstrate and extend current technology.

In cervical cancer, investigation indicated a sizable number of hospitals in low socioeconomic areas without detection programs which would be willing to establish these if supported by Federal funds. The unit costs of increasing the number of hospitals seemed to be the same as that of those already in the program. Shifting the approach to reach out for additional women in the community would increase costs per examination, but not so high as to change the relative position of this program. At most, it raised costs to about those of the breast cancer control program.

Despite the seeming high potential payoff of some of the motor vehicle programs, there was considerable uncertainty about the success. As a consequence recommendations were for small programs with a large emphasis on evaluation for use in future decisions. The same philosophy was applied to the arthritis program.

What resulted then, was a setting of priorities for additional funding, based on the analytical results, judgment about their reliability, and practical considerations.

A second type of criticism of the analysis described above was concerned with the criteria, especially the calculation of benefits. [11a] They were considered inadequate in that they paid attention to economic productivity alone, and omitted other considerations. In particular, they were thought to discriminate against the old who might be past employment years, and women whose earning were relatively low. It was also feared that the logic, if vigorously pursued, would penalize not only health programs for the aged such as the newly launched medicare, but also programs aimed at assisting the poor whose relative earning power is low by definition.

In actual practice in the programs studied, these concerns were only hypothetical. The programs for cervical and breast cancer looked to be good despite their being for women. As for the poor, most of the programs considered, especially cervical cancer, syphilis, and tuberculosis were aimed primarily at them, and projects were usually located to serve low income residents.

Another type of objection was raised not against the technique of analysis, but against its being done at all. Choices among diseases

to be controlled and concern with costs of saving lives can be viewed as contrary to physicians' attitudes in the care of an individual patient. Yet, such decisions are made, analysis or no. Prior decisions on allocations to various health problems rested upon a combination of perception of the magnitude of the problem and the political strength organized to secure funding, e.g., the National Tuberculosis Association.

The disease control cost-benefit analyses suggest that additional considerations are very relevant. Given scarce resources (and if they are not, there is no allocation problem), one ought to estimate the costs of achieving improvements in health. If we can save more lives by applying resources to a small (in numbers affected) problem than a large one, we ought to consider doing so.

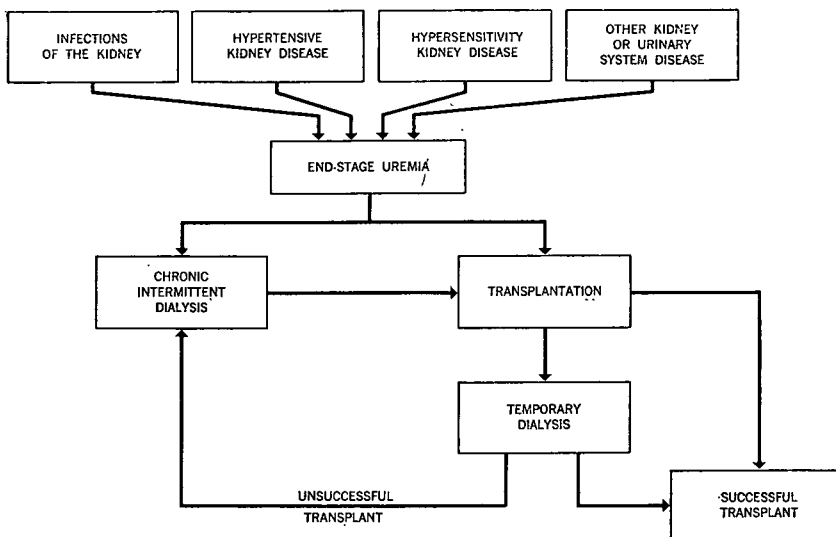
A somewhat separate issue is that of the disease control approach to personal health. This is too large an issue to deal with in this paper, but it may make more sense to develop programs of delivering comprehensive health care, including preventive services, than to maintain categorical disease programs.

The following year a number of additional control studies were performed. One of the most interesting and important was on kidney diseases. [12] This analysis was launched at a time when the public was becoming conscious of a new technique, the artificial kidney (chronic dialysis), which could preserve the life and productivity of individuals who would otherwise die of end-stage kidney disease. About 50,000 persons a year do so die. It is estimated that about 7,500 of these were "suited" by criteria of age, temperament, and the absence of other damaging illnesses for dialysis treatment. The national capacity could handle only about 900, who would remain on intermittent dialysis the rest of their lives. About 90 percent would survive from one year to the next. The operating cost of dialysis treatment in hospitals was estimated at about \$15,000 per patient per year. A home treatment approach might reduce this to about \$5,000 per year.

The Federal Government was under great pressure to expand the national capacity, which was limited not only by the large money costs, but also by shortages of trained personnel and supplies of blood. Indeed, at the same time as this analysis was being performed, an advisory group to the U.S. Bureau of the Budget was studying the problem of end-stage kidney disease. This group came in with the recommendation for a massive national dialysis program. [13]

The HEW program analysis was somewhat more broadly charged, and took a more systems oriented approach. It concerned itself not only about the 7,500 annual candidates for dialysis, but also about the other 40,000 or so who would suffer the end-stage disease, but were unsuited to dialysis. If some way could be found to reduce the numbers falling into the pool of end-stage patients, perhaps a larger number of people could be helped. Chart 7 illustrates the classes of kidney diseases leading to end-stage disease. If these could be better prevented or treated we might keep down the number of patients requiring dialysis or transplantation.

Chart 7 SCHEMATIC OF TRANSPLANT AND DIALYSIS PATIENTS



The analysis group, therefore, examined a number of mechanisms or program components. Among these were:

1. Expanded use of existing preventive techniques.
2. Expanded use of existing diagnostic techniques.
3. Expanded use of existing treatments, including chronic dialysis, kidney transplantation and conservative management (drugs, diets, etc.).
4. Laboratory and clinical research to produce new preventive, diagnostic, therapeutic and rehabilitative methods.
5. Increased specialized scientific medical and paramedical training to provide the manpower needed for the research and treatment attack on the kidney disease problem. This also includes continued postgraduate education to train practicing physicians in the use of the latest diagnostic and treatment modalities.
6. Increased public education to alert potential victims of kidney disease to seek medical help at the earliest possible emergence of warning signs.
7. Provision of specialized facilities not currently in existence which are essential for the execution of any of the above programs.

It must be understood from the outset that these program components are interdependent in most cases. For example, preventive techniques exist that need further research to make them maximally effective for broad application. New treatment methods are useless if existing diagnostic techniques are not being applied in medical practice. Because of the present inadequacies of existing treatments, be they dialysis, transplantation, or conservative management, a considerable research effort is called for to increase their efficacy and economy to make them more broadly useful.

Time does not permit a detailed description of the analysis. Costs were estimated for relevant public and private expenditures for the nationwide treatment of kidney disease. The latter includes cost of physician care, hospital care, nursing home care, and other professional services for diagnosis and therapy of kidney diseases, as well as the cost of drugs and net insurance costs. In addition, the cost was estimated for ongoing research efforts, for demonstration, screening and detection programs, for education and training efforts, and for that portion of the cost of construction of hospital and medical facilities which can be prorated to the use of patients with kidney disease.

Based on the substantive information obtained and statistical and economic data collected, estimates were made of the benefits to be gained by different approaches to the solution or amelioration of the overall national kidney disease problem at different expenditure levels of HEW funds.

Several different funding levels were assumed, and estimates were made assuming both the current state-of-art and an expected advanced state-of-art in 1975.

Each program consisted of a hypothetical situation where a specific level of HEW program funding was divided among a rational mix of program components (screening, diagnosis and treatment, research, training, etc.) based on the particular characteristics of the specific disease group involved, and was applied to specifically involved or particularly vulnerable groups or, as the case may be, to the entire population. The benefits accruable from these programs were their estimated and stated in terms of overall reduction of mortality, prevalence, and morbidity due to kidney disease.

Benefit indices were quantified in terms of the reduction in annual mortality, the reduction in annual morbidity (number of sick days per year) and in terms of the disease prevalence in the total population due to the specific type of kidney disorder analyzed, which would accrue thanks to the impact of the various program components—such as research advances, disease prevention and improved treatment.

The analysis group avoided estimates of the impact on economic productivity in their results, although such calculations have been made independently. [14]

The HEW study concluded that concentration in future programs merely on the treatment of end-stage kidney disease is not likely to solve the problem of annual deaths due to irreversible uremia unless unlimited funds are available for an indefinite continuation of such a program. Thus, steps must be taken to decrease the number of people who enter the irreversible fatal stage each year by a systematic prevention or treatment of the primary kidney diseases which initiate their progressive downhill course. It is obvious from the analyses in the three major kidney disease groups—infectious, hypersensitive, and hypertensive—that the otherwise inevitable annual reservoir of patients with irreversible kidney failure can be diminished considerably through vigorous programs activated to deal with each of these groups. The application of relatively minor funds in the group of infectious kidney diseases to stimulate systematic screening of high-risk groups followed by diagnosis and treatment, even within the current state-of-the-art and without awaiting additional advances due to ongoing or future research, can bring about a significant future reduction in the number

of end-stage patients. Continued and expanded research activities will be necessary to increase the percentage of patients ultimately benefited by this approach.

In the area of hypersensitivity diseases involving the kidney there appears to be no promising mode of attack in sight except for the launching of a systematic research effort intended to increase our knowledge of the disease mechanisms involved. Here, the sooner this effort is started the greater the likelihood of a reduction of the number of end-stage victims in the near future. The promise for benefits to be derived from this type of research effort is such that it should not be postponed—particularly since any new effective treatment or prevention modality would produce major benefits in the entire field of hypersensitivity diseases, such as rheumatic heart disease, rheumatoid arthritis and others.

In the group of hypertensive diseases of the kidney an immediate start, within the current state-of-the-art, of screening, diagnosis and treatment can begin to diminish the number of patients who will eventually require end-stage treatment because of their progressive renal involvement. Simultaneous research efforts are likely to make this particular portion of the overall program more effective as time goes by, in the same fashion in which the new antihypertensive drugs developed during the last 10 years have succeeded in decreasing by about 50 percent the mortality due to malignant hypertension.

Thus, a meaningful Federal program to reduce the annual mortality due to kidney disease and aimed at a general reduction of the prevalence of the various kidney diseases must perforce be a multifactorial one which brings into play all of the program components—research, prevention, treatment and education—available in our arsenal. An optimally proportioned mix of these program components must be present to yield maximum benefits in overall number of lives saved. This last concept includes not only deaths avoided today but deaths to be prevented in the years to come. Needless to say, such a total program, to be meaningful and productive, must be aimed at all three major primary kidney diseases, as well as at end-stage kidney failure.

Chart 8 shows a hypothetical program mix that might come from such conclusions. Note the early emphasis on research to affect the state-of-the-art, and the growth in allocations to the prevention and treatment of primary kidney diseases as relative allocations to dialysis are diminished.

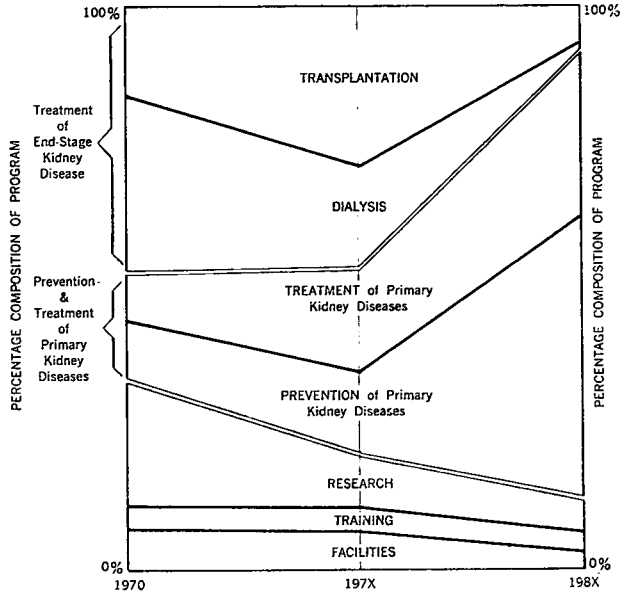
In 1966, HEW also did a rather different type of analysis in the field of health: a study of alternative ways of improving the health of children. [15] The President had focused public attention on the problem of child health and expressed a desire to introduce new legislation in this field. The HEW study was an attempt to assess the state of health of the Nation's children (to what extent the children have correctable health problems and in what groups in the population were the problems concentrated) and to estimate the cost and effectiveness of various kinds of programs to improve the health of children.

This study proved more difficult than anticipated. Hard information on the state of health of children is hard to come by. Surprisingly, estimates of improvement in health attributable to medical care are almost nonexistent. It is not easy to demonstrate statistically that children who see doctors regularly are healthier than children who do not.

Chart 8

EFFECT OF
ADVANCING
STATE OF THE
ART ON FUTURE
PROGRAM
COMPOSITION

(Percentages are wholly arbitrary and merely serve to illustrate shifting trends.)



In regard to maternal and child care programs the stated goal was to make needed maternal and child health services available and accessible to all, in particular to all expectant mothers and children in health depressed areas. Health depressed areas could be characterized as areas with excessive infant mortality rates. There is no universal index of good or bad health among children. Two measurable areas were selected—mortality and the prevalence of chronic handicapping conditions. Over a dozen possible programs aimed at reducing these were examined. On chart 9, three selected programs addressed to the problem of coverage of maternal and child health are illustrated, two of them comprehensive programs of care to expectant mothers and children. This table shows the annual effects of spending the same amount of money, \$10 million a year, in different ways. The analysts examined comprehensive care programs covering up to age 18 and up to age 5 with estimates based on the best assumptions derived from the literature and advisers on the probabilities of prevention of maternal deaths, premature deaths, infant deaths, and mental retardation, and handicapping conditions prevented or corrected by age 18. They also looked at a program of early case finding and assured treatment which focused on children at ages 4 days and again every other year until they were 9. Expending the same amounts, where you put the money yields different results. With respect to reduction of infant mortality, several other programs had higher payoffs than these. For example, a program of intensive care units for high-risk newborns was estimated to reduce annually 367 deaths if we put all our money in that basket—it would cost about \$27 thousand per infant death prevented. The programs shown cost about four times that, but they do other good things too.

CHART 9.—YEARLY EFFECTS PER \$10,000,000 EXPENDED IN HEALTH DEPRESSED AREAS

	Comprehensive programs to age—		Case finding of treatment 0, 1, 3, 5, 7, 9
	18	5	
Maternal deaths prevented.....	1.6	3	-----
Premature births prevented.....	100-250	200-485	-----
Infant deaths prevented.....	40-60	85-120	-----
Mental retardation prevented.....	5-7	7-14	-----
Handicaps prevented or corrected by age 18:			
Vision problems:			
All.....	350	195	3,470
Amblyopia.....	60	119	1,140
Hearing loss:			
All.....	90	70	7,290
Binaural.....	6	5	60
Other physical handicaps.....	200	63	1,470

The HEW analysts also looked at programs with a given amount of money (chart 10) aimed at reducing the number of children who will have decayed and unfilled teeth by age 18. Fluoridation programs in communities which do not possess this, will, for the same amount of money, give us close to 300,000 fewer children in this condition, compared to 18,000 or 44,000 fewer in other programs noted. Fluoridation looks like a very attractive program. It was so attractive that it could be inferred that a program as cheap as this is not being inhibited by lack of financial support by the Federal Government; there are other factors at work.

CHART 10.—*Reduction in number of 18-year-olds with decayed and unfilled teeth per \$10,000,000 expended in health-depressed areas*

Fluoridation	294,000
Comprehensive dental care without fluoridation.....	18,000
Comprehensive dental care with fluoridation.....	44,000

One other program, additional funds on family planning, looked like a very good way not only to reduce the number of infant deaths, but also the rate of infant mortality in high-risk communities.

Despite the information difficulties, several conclusions emerged clearly from the study. Two of these conclusions resulted in new legislation being requested from Congress. First, it seemed clear that a program of early casefindings and treatment of handicapping conditions would have considerable payoff. It was also clear that if the large number of children who do not now have access to good medical care were to be provided with pediatric services, an acute shortage of doctors would be precipitated. Ways have to be found to use medical manpower more efficiently. The Social Security Amendments of 1967 include provision for programs of early casefinding and treatment of defects and chronic conditions in children, and for research and demonstration programs in the training and use of physician assistants.

These condensed discussions of some of HEW's applications of cost-benefit analysis to disease-control programs illustrate both the usefulness and limitations of such analyses for decisionmaking. [16] Issues are sharpened, and quantitative estimates are developed to reduce the decisionmakers' uncertainty about costs and effects. Nevertheless, the multiplicity of dimensions of output, and their basic incommensurabilities both with costs and the outputs of other claimants for public expenditure, still requires the use of value judgments and political consensus.

Prior to the introduction of the planning-programing and budgeting system, long-range planning in HEW was sporadic and generally

not departmentwide. No mechanisms existed for focusing attention on longer range objectives, deciding which types of programs should be given highest priority over the next several years, and then drawing up a budget consistent with those objectives and priorities.

In 1967 and 1968, the Department experimented with a new procedure for making budget decisions in the context of a long-range plan.

The procedure involves several steps. First, very early in the calendar year the planning and evaluation staff drew up a list of significant issues which would have to be addressed in formulating the budget and legislative program. This list of issues was discussed within the Office of the Secretary, with the operating agencies, and with the Bureau of the Budget. Decisions were made as to which of these issues seemed likely to be illuminated by analytical work, and studies of many of them were initiated.

The second step in 1967 was the development of a set of tentative departmental objectives for 1973. The operating agencies were asked to formulate their objectives for 1973 in program terms. Each agency was given two ceilings by the Secretary for 1973—a “low” which implied continued budget stringency, and a “high” which implied somewhat greater availability of funds. Each of them was asked to answer the question: How would you allocate these sums in 1973 among existing programs or new programs which could be developed between now and then?

The agencies took this assignment seriously, despite the difficulties of forcing busy administrators to take the time away from daily crises to think 5 years into the future. The 1973 objectives which the agencies sent back to the Secretary reflected considerable thought and effort on the part of agency heads and their bureau chiefs.

The agency 1973 objectives were reviewed by the Secretary and his staff and a set of departmental objectives for 1973 was formulated.

In both the agency plans and those of the Department, the tentative results of analyses were considered. For example, the study of the delivery of health services to the poor made recommendations which involved policy decisions with respect to the coverage of the medicaid program, the training of physician assistants and family health advisers, reorganization of delivery systems (especially those dealing with ambulatory care), hospital-community links, and comprehensive care versus categorical control programs. The departmental objectives, reflecting the Secretary's judgment about priorities for 1973, were then transmitted back to the operating agencies as guidance for formulating their fiscal year 1969 budget submissions and fiscal year 1969-73 suggested program and financial plan, and legislative program. These were reviewed for conformance to Department objectives, and a Department program and financial plan (1969-73), fiscal year 1969 budget and framework for legislative proposals were then developed and transmitted to the Bureau of the Budget.

The HEW system has proven of some use. A better understanding of the health programs of the Department and their interrelationships have been achieved. This was true not only at the Office of the Secretary, but also at the Bureau of the Budget. The primitive analyses have assisted the dialog on budget and legislative programs. The 5-year planning system has enabled the Secretary and his staff to control the processes somewhat more by testing budget and legislative proposals against the Secretary's program and financial plan.

Problems, of course, remain. One of the greatest is inadequate program evaluation.[17] Very little is really known of the impact of programs. Partly this is because of the complications in sorting out Federal funding impacts from all the others. Partly it is because health effects take considerable time to become evident. But a large measure of the reason is because it has not been a matter of high interest to program managers. This is beginning to change. New health legislation increasingly contains authorization of a portion of the funds for evaluation. For example, Public Law 90-174, the Partnership for Health amendments of 1967, contains wording affecting formula grants to the States, project grants, and training and demonstration grants in the following manner:

“. . . such portion of the appropriations for grants under this subsection as the Secretary may determine, but not exceeding 1 percentum thereof, shall be available to the Secretary for evaluation (directly or by grants or contracts) of the program authorized by this subsection.”

Under the direction of the Office of the Secretary, agencies are developing evaluation plans which may lead to significant gains in information for policy decisions.

REFERENCES

[1] Public Law 89-749 establishes planning requirements for State Health Planning Councils, State Health Planning Agencies, Area-wide Health Planning Councils, and State Health Authorities and/or State Mental Health Authorities. Public Law 89-239 supports planning by Regional Medical Programs and Regional Advisory Groups. Public Law 88-443 required planning activities by State Hill-Burton Agencies and State Advisory Councils. Public Law 88-164, title II, provided grants to designated State agencies and State Advisory Councils for planning community mental health centers. Public Law 88-164, title I, part C did the same for planning mental retardation facilities. Public Law 89-272, title II provides money to designated State or interstate agencies for planning solid waste disposal activities. Public Law 90-148 provides planning funds to Air Pollution Control Agencies and to Interstate Air Quality Agencies or Commissions. The Social Security Act, as amended, title V, parts 1 and 2 provides for the State plans by State Health Agencies, State Crippled Children's Agencies or State Health Departments of programs for maternal and child health. Title XIX of the act requires State plans for Medicaid. The Vocational Rehabilitation Act, as amended, section 4(a)(2)(B) provides for Comprehensive Statewide Planning for Vocational Rehabilitation Services.

[2] For some interesting discussion on the interactions between systems analysis and politics see:

Charles Schultze, *The Politics of PPB*, Gaither Memorial Lectures, University of California (to be published); William N. Capron, "The Impact of Analysis on Bargaining in Government," delivered to a meeting of the American Political Science Association, Sept. 1966; James R. Schlesinger, *Systems Analysis and the Political Process*, the RAND Corp., June 1967; Ralph Sanders, "Systems Analysis and the Political Process," *Perspectives in Defense Management*, Dec. 1968; and Aaron Wildavsky, "The Political Economy of Efficiency: Cost-Benefit Analysis, Systems Analysis and Program Budgeting," *Public Administration Review*, Dec. 1966. See also the papers by Schick and Wildavsky in this volume.

Specifically in the health area, see, for example:

Henrik L. Blum and Alvin R. Leonard, *Public Administration—A Public Health Viewpoint*, The Macmillan Company, 1963; Ralph W. Conant, *The Politics of Community Health*, report of the Community Action Studies project, National Commission on Community Health Services, Public Affairs Press, 1968; J. Enoch Powell, *A New Look at Medicine and Politics*, Pitman Medical Publishing Co., 1966; and Eugene Feingold, *Medicare's Policy and Politics*, Chandlers Publishing Co., 1966.

[3] For descriptions of the Department of Defense experience see:

Charles J. Hitch, *Decisionmaking for Defense*, University of California Press, 1965, and Stephen Enke, editor, *Defense Management*, Prentice-Hall, 1967.

[4] U.S. Department of Health, Education, and Welfare, Office of the Assistant Secretary (planning and evaluation), *Planning-Programing-Budgeting: Guidance for Program and Financial Plan*, revised February 1968.

[5] U.S. Department of Health, Education, and Welfare, Office of the Assistant Secretary for Program Coordination: *Motor Vehicle Injury Prevention Program*, August 1966; *Arthritis*, Sept. 1966; *Selected Disease Control Programs*, September 1966; and *Cancer*, October 1966.

[6] Rashi Fein, *Economics of Mental Illness*. Basic Books, Inc. New York, 1958. For later study using a new conceptual framework resulting in cost estimates almost 10 times higher see Ronald Conley, Margaret Cromwell, and Mildred Arrill. "An Approach to Measuring the Cost of Mental Illness," *American Journal of Psychiatry*, December 1967, pp. 63-70.

[7] Burton A. Weisbrod, *Economics of Public Health: Measuring the Economic Impact of Diseases*. University of Pennsylvania Press, Philadelphia, 1961.

[8] Herbert E. Klarman, "Syphilis Control Programs," *Measuring Benefits of Government Investments*, edited by Robert Dorfman. The Brookings Institution, Washington, D.C., 1965, pp. 367-410.

[9] Dorothy P. Rice, *Estimating the Cost of Illness*, Public Health Service Publication 947-6, Washington, D.C., May 1966; Jacob Cohen, "Routine Morbidity Statistics as a Tool for Defining Public Health Priorities," *Israel Journal of Medical Sciences*, May 1965, pp. 457-460, estimated the weighted impact of 25 mass diseases on deaths, loss of life years under 65, hospitalization, days of hospitalization and cases in Workers' Sick Fund.

[10] Louis I. Dublin and Alfred J. Latka, *The Money Value of a Man*, Ronald Press, New York, 1930.

[11] See, for example, Earl F. Cheit, *Injury and Recovery in the Course of Employment*. John Wiley, New York and London, 1961.

[11a] For discussion of some of these issues see Dorothy P. Rice, "Measurement and Application of Illness Costs," *Public Health Reports*, February 1969, pp. 95-101; T. C. Schelling, "The Life You Save May Be Your Own," *Problems in Public Expenditure Analysis*, edited by Samuel B. Chase, Jr., The Brookings Institution, 1968, pp. 127-176; and Pan American Health Organization, *Health Planning: Problems of Concept and Method*, Scientific Publication No. 111, April 1965, esp. pp. 4-6.

[12] U.S. Department of Health, Education, and Welfare, Office of the Assistant Secretary (Planning and Evaluation), *Kidney Disease*, December 1967.

[13] The Bureau of the Budget convened an expert Committee on Chronic Kidney Disease. See *Report* by this committee, Carl W. Gottschalk, chairman, Washington, September 1967. Herbert E. Klarman, John O'S. Francis, and Gerald D. Rosenthal, "Cost Effectiveness Analysis Applied to the Treatment of Chronic Renal Disease," *Medical Care*, vol. VI, No. 1, January-February 1968, pp. 48-54, analyzed the committee's data to explore what is the best mix of center dialysis, home dialysis, and kidney transplants. The authors restricted their beneficiaries to those in end-stage kidney disease, and concluded that transplantation is economically the most effective way to increase life expectancy of persons with chronic kidney disease, although they recognize the factors that constrain the expansion of transplantation capability.

[14] Jerome B. Hallan and Benjamin S. H. Harris, III, "The Economic Cost of End-Stage Uremia," *Inquiry*, volume V, No. 4, December 1968, pp. 20-25, and J. B. Hallan, B.S.H. Harris, III, and A. V. Alhadeff, *The Economic Costs of Kidney Disease*, Research Triangle Institute, North Carolina, 1967.

[15] U.S. Dept. of Health, Education, and Welfare, Office of the Assistant Secretary for Program Coordination, *Maternal and Child Health Care Programs*, October 1966.

[16] Other studies included U.S. Dept. of Health, Education, and Welfare, Office of the Assistant Secretary (Planning and Evaluation): *Delivery of Health Services for the Poor*, December 1967; *An Economic Analysis of the Control of Sulphur Oxides Air Pollution*, December 1967; *Nursing Manpower Programs*, March 1968; Public Health Service, Bureau of Health Services, *Recommendations and Summary: Program Analysis of Health Care Facilities*.

[17] A useful reference to the existing literature on evaluation is Willy De Dyndt and Karen B. Ross, *Evaluation of Health Programs—An Annotated Bibliography*, Systems Research Project, University of Minnesota, comment series No. 8-9(9).

EDUCATION PROGRAM ANALYSIS AT HEW

BY JOHN E. BRANDL*

John Brandl is Deputy Assistant Secretary for Education Planning at the Department of Health, Education, and Welfare.

"HEW is concerned both to reallocate resources within this society, and to improve the quality of certain activities—i.e., with both equity and efficiency matters." Dr. Brandl here evaluates the contributions of program analysis in promoting rational decisions on both facets of HEW's education programs. He finds that program budgeting is helpful in isolating factors relevant to some operational decisions, but that it does not "contribute directly to answering * * * the more difficult question of how effective the various programs are. This is partly because of disagreement as to what the programs are supposed to be effective at, and partly because of technical measurement problems." He asserts that conflicts also mar the effectiveness of the program memorandum as a component of the PPB System. "Plans and (especially) budgets are made up over a long period of time by large numbers of people often not in communication with each other and having different sets of preferences." He suggests, in fact, that "in agencies where this sort of fencing (over preferences and the weighting of objectives) takes place, it may not be possible to produce a program memorandum that makes an integral whole out of all the programs of a department." He offers three possible methods of enhancing the effectiveness and relevance of program budgeting: increasing the secretarial initiative and direction in the planning cycle; more closely coordinating the planning and budgeting cycles; and obtaining greater cooperation from the Bureau of the Budget in the planning-budgeting process.

Dr. Brandl criticizes the fact that the efforts at analysis and evaluation at HEW have concentrated on redistribution questions at the expense of effectiveness questions. "The question(s) asked and answered implicitly assumed that education should be left to the educators—with Government providing *financial* assistance, but not assessment or evaluation." He also notes the political implications of analysis, and the impact which analytic studies can have on education policy.

Dr. Brandl concludes by stressing that his discussion of the shortcomings of program analysis should not obscure the considerable progress which has already been made. He also emphasizes the need for major future efforts at experimentation and evaluation in the field of education.

Introduction

There are three characteristics of an ideal program budgeting system:

1. A management information framework for keeping track of information and fostering intelligent, timely decisions;
2. Good analysis; and
3. Bureaucratic and institutional structures incorporating incentives for socially productive activity.

This paper reports on program budgeting for education decision-making at the Department of Health, Education, and Welfare, where

* The author thanks his colleagues, Worth Bateman, Robert Hartman and Alice Rivlin who may recognize some of their ideas in this paper. But the paper contains the author's views, for which neither his friends nor the Department of Health, Education, and Welfare should be blamed.

some progress has been made on desiderata (1) and (2) but little on (3). Only the first two will be discussed here.

I. MANAGEMENT SYSTEM

THE USES OF INFORMATION

There is a tendency for those engaged in program analysis in the domestic agencies of the Federal Government to compare themselves with (or be compared by others with) the systems analysis and programming operations of the Department of Defense, if only because the Defense Department pioneered in these endeavors.* But comparing soon becomes contrasting; the differences start to appear as soon as one asks what kinds of decisions are made and what kinds of information should be collected and funneled to decisionmakers in the Department. HEW is basically a conduit of funds from the Federal Government to the States and localities which run programs and spend money. Whereas the Secretary of Defense operates what has been called the third largest Socialist organization in the world, and can actually (if he wants to) direct men and materiel around from one day to the next, the Secretary of HEW can almost only make big decisions. He can reorient programs by affecting legislation or by requesting a reallocation of his next year's budget,¹ but he does not have control over the ultimate carrying out of most of the programs for which his Department provides planning, advice, funds, and guidelines. Even more than the Secretary of Defense, the Secretary of HEW ought to be sheltered from minutiae. This paper is concerned with program budgeting and secretarial decisionmaking, so that the great bulk of the enormous amounts of information collected, classified, printed, and distributed in the Department is not of relevance here.**

THE PROGRAM BUDGET: EQUITY AND EFFICIENCY

To facilitate understanding and control of a Department's activities a program budget system involves first the development of a set of program categories, a supposed advantage of which is that they present information on "outputs" of programs—which information is often more meaningful than the "input" categories of traditional line item budgeting.³

HEW is concerned both to reallocate resources within this society, and to improve the quality of certain activities; that is, with both equity and efficiency matters. But the questions, "who gets what?" and "what effect does it have?" can both be answered in a variety of ways. The first question is the easier of the two and can be answered descriptively. But no single type of breakdown, whether by income class, race, geography, or age, can wholly summarize the population to whom the programs are directed. The Department has, however, fashioned a

¹ He even has very little authority to reprogram funds from one category to another within the budget appropriated by the Congress.

² Here and elsewhere in this paper I borrow words and ideas from the *Program Memorandum on Education Programs of the Department of Health, Education, and Welfare* (Washington, D.C.; Office of the Secretary of Health, Education, and Welfare, Dec. 1, 1968). This document was prepared by myself and my staff.

³ Further discussion of this issue is found in the papers by Enthoven & Smith, and Enthoven in this volume.

^{**} Further discussion of this issue is found in the paper by Rivlin in this volume.

set of program distinctions which are useful in showing how money is spent. At the most general level, we use program categories organized roughly along chronological lines as follows:

- Development of basic skills;
- Development of vocational and occupational skills;
- Development of advanced academic and professional skills;
- Library and community development;
- General research;
- General support.

These categories are then subdivided into programs for the general population and those for the handicapped or disadvantaged. They are also displayed according to the type of operation involved—operational aid, student aid, personnel training, innovation or research. These levels of distinction enable us to make some decisions based upon what we know about the general nature of the categories, populations, and activities, but they do not contribute directly to answering the second, the more difficult question of how effective the various programs are. This is partly because of disagreement as to what the programs are supposed to be effective *at*, and partly because of technical measurement problems. These difficulties are taken up in the paragraphs immediately following and again in part II of this paper.

THE PROGRAM MEMORANDUM: INHERENT CONFLICTS

Key elements of the Federal Government's program budgeting system are the annual program memorandums from the departments to the Bureau of the Budget for each major area of activity of the Government.* Ideally each of these documents assesses progress toward attaining the objectives represented by the program categories, lays out a multiyear plan for meeting the objectives, and indicates how the department's budget fits into and contributes to the plan. But no single set of program categories adequately expresses all that a department is interested in. It should be clear that writing a program memorandum is not a straightforward process, but the difficulties are not simply in finding categories for filing information. The main difficulty is that no single set of preferences determines the makeup of the plan and the budget. Programs whose purposes conflict with each other appear side by side. Plans and (especially) budgets are made up over a long period of time by large numbers of people often not in communication with each other and having different sets of preferences.

Some people involved in the process might argue that the Federal Government's role in education is to foster equality of opportunity (an equity argument), others that it should develop new and innovative educational approaches (on public good grounds), and some would say that it should strive for excellence in education throughout the Nation's school system (supposedly a Federal function because of the beneficial externalities arising from education). There is no assurance that the plan and budget which result from the bureaucratic haggling are compatible with *any* self-consistent set of preferences or weighting of the several objectives,³ and certainly no

³ For examples of grant-in-aid formulas the implications of which are apparently incompatible even with the intentions of their designers, see my paper, "On Budget Allocation in Government Agencies," *Review of Social Economy*, March 1967.

*Further discussion of this issue is found in the paper by Carlson in vol. 2 of this collection.

guarantee that they will represent the desires of any single individual or group involved in the process. Indeed, a decisionmaker may not wish to issue a program memorandum which is *too* explicit as to objectives and preferences—hoping to assuage several competing constituencies.⁴ So, in agencies where this sort of fencing takes place, it may not be possible to produce a program memorandum that makes an integral whole out of all the programs of a department. There is something basically naive about the idea of a program memorandum which is supposed to make sense out of the conglomerate of a department's programs; but perhaps the attempt to be open and explicit in such a document has the merit of sharpening preferences—and maybe of narrowing differences of opinion.

THE SYSTEM: SUGGESTIONS FOR CHANGE⁵

Despite the difficulties outlined above, HEW operates a fairly orderly planning-budgeting cycle at least partly in program budget terms. The 5-year plan projects programs and their costs and outputs. The program budget is sophisticatedly computerized to permit translation back and forth to the line-item budget. Presentable program memorandums are published annually for each of the areas of health, education and income maintenance-social services.

Nevertheless, we have not yet been able to avoid perennial chaotic rushes in September and October to produce numbers to comply with the inexorable schedule of the Bureau of the Budget.* Planning and budgeting get jumbled in the fall every year. The main planning documents, the program memorandums, tend to appear each year only *after* the budget has been completed. Alternatives are not always given sufficient attention; conflicting items appear in the same budget. Some suggestions which might improve on this situation are the following:

1. More secretarial initiative in the planning cycle.

Tentative decisions could be made by the Secretary early in the planning cycle (i.e., in the spring of each year) on the guiding objectives and major thrusts of the Department's programs.

His office could communicate these decisions to the Department's constituent agencies in the form of a written rationale and a statement of its implications for specific major programs over the next several years.

2. Fit the budget to the plan.

After the agencies have received tentative guidance from the Secretary, an orderly mechanism would be needed whereby they could appeal for changes. (E.g., a request for change could be submitted to the Office of the Secretary, where the appeal would be considered by two or more parts of his office, and the conflicting views together with recommendations presented to him for decision.)

⁴ It may also be that the lack of any single set of preferences governing the determination of the optimal set of programs for a department contributes to a "more is better" approach which will seize on any opportunity to expand any of a department's programs since someone who counts is probably in favor of it.

⁵ What follows requires some basic knowledge of the Federal budgeting calendar. In HEW, preliminary plans for the fiscal 1970 (starting July 1, 1969) budget began in the spring of 1968; by October several alternative budgets had been drawn up; in October the Department's budget went to the Bureau of the Budget which made several more changes to it; in December it was practically completed. The President (Johnson) submitted it to Congress in January 1969. If past practice holds, the Congress will not make its appropriations until the fall of 1969, i.e., until after the fiscal year has begun.

* Further discussion of this issue is found in the papers by Schick and Wildavsky in this volume.

In this way the tentative plan would evolve throughout the summer and fall. Eventually the first year of the plan would become the following year's budget. Legislative proposals could be handled in a similar way.

The advantages of this approach are clear. It would avoid the fall pileup. Alternatives would be given systematic attention. Programs would be more apt to reflect a set of rational objectives. The budget would have something to do with the plan.

But the drawbacks of such an approach are also clear. By definition, imposing consistency, order, and rationale involves centralizing decisionmaking, which has its problems. Also candor, explicitness, openness, may be politically counter-productive if they mean alienating constituents. Another desirable change is:

3. Get the Bureau of the Budget to cooperate in the planning-budgeting process.*

At present the Bureau encourages the development of a program budget and the analysis of alternative courses of action. It is also eager to receive written rationale for decisions made within the Department. But it gives little indication of making its own decisions in the program budget framework; it is highly secretive and uncommunicative about whatever work it produces that influences its decisions; and at budget time it tends to accept arguments for cutting programs but not for expanding them. In sum, the Bureau seems not to practice what it preaches about program budgeting.

II. ANALYSIS

The foregoing plea for orderliness in planning can hardly depend on the argument that neatness for its own sake is a good thing. Its justification is, rather, that in a respectable planning-budgeting system, *analysis of alternatives* gets done—on time—and gets a hearing. Analysis, not order is the *sine qua non*. By analysis I mean asking "what is it that we are trying to do?" (determining objectives), "how can we do it?" (planning and budgeting), and finally "how are we doing? can we do better?" (evaluation).

THE FOCUS OF CURRENT ANALYSIS

Without agreement on objectives (as discussed in sec. I) it is hard to get past the first of the above questions. I conclude⁶ that the Federal Government's and HEW's (in particular) responsibilities in education are twofold: (1) to foster equality of opportunity by assisting in the education of the disadvantaged of this society; (2) to improve education for all by sponsoring research and evaluation activities which benefit the entire country but would not be undertaken by other individuals, groups or governments. These two objectives indicate a rough rationale for allocation of education resources by HEW which goes as follows. We do not know very much about how to affect the educational lives of disadvantaged youngsters, but at least we can orient Federal funds into programs which reach them. Meanwhile we should foster innovations and new approaches. Despite its embar-

⁶ The first person singular is used here because what follows is my personal opinion to what the Federal Government's responsibilities in education should be.

*Further discussion of this issue is found in the papers by Carlson in vol. 2, of this collection and Hoffman, and Marvin & Rouse in this volume.

raising simplicity this approach can keep a lot of planners busy planning changes precisely because many programs contributing little to these objectives are among those now funded by the Federal Government. For example, the executive branch has tended to want more money for title I ESEA and to request less than Congress appropriated for aid to federally impacted areas. Assistance under the former is more closely correlated with income than is that under the latter.

To the extent that redistribution of resources from better off individuals and governmental jurisdictions to worse off is itself an objective, evaluation consists of determining who gets what.* And indeed our programing system now contains much information of this sort. The further, tougher, question of whether poor children not only receive attention but benefit by it has to do with the very nature of the Federal-State system and in particular with the way in which the Federal Government expends funds through HEW. Categorical grant programs (which constitute the bulk of HEW's appropriations) are targeted to particular problem areas, with Federal guidelines defining precisely for what purposes the money can be spent. A pure revenue-sharing or bloc-grant program would be simply redistributive (from better off governmental jurisdictions and people to worse off). Many existing categorical HEW programs have redistributive effects while having as their main purpose the application of funds to a particular problem area. Evaluation of such programs is bound to be frustrating, since an evaluator or program analyst at the Federal level will want to try to relate program inputs to outputs (are children doing better at school?). But the nature of the programs makes this difficult and America's decentralized school system resists (actively and passively) national evaluation. Whether Federal money is distributed with or without strings, that is, whether by categorical or by bloc grants, the funds are not contingent on performance, do not come with a built-in incentive to produce. And local school districts do different things with the money precisely because they *want* to. Different Americans, different American school boards do have different values, different preferences as to what they would like to have schoolchildren accomplish—frustrating though that may be to Federal bureaucrats who would like to be able to say more about how well we are doing educationally as a nation. So Federal education planners and evaluators are stuck with "evaluating" mostly where money goes, rather than how well it does when it gets there. (As will be seen below, another reason for this is that the analytical difficulties of relating inputs to outputs in education are horrendous.)

Still another factor contributing to this state of affairs in the past, at least as far as higher education is concerned,⁷ is the implicit conviction that educators knew what they were doing. Recently, two major studies of higher education have been completed—one inside the Government and one outside.⁸ In neither was there an

⁷ For elementary and secondary education, America's long and solid history of dissatisfaction with the quality of its schools has been matched by its insistence that the Federal Government not stipulate how they might be improved. See Richard Hofstadter, *Anti-Intellectualism in American Life* (New York: Random House, 1963), part V.

⁸ See *Toward a Long-Range Plan for Federal Financial Support for Higher Education*—A report to the President, U.S. Department of Health, Education, and Welfare, Assistant Secretary for Planning and Evaluation, January 1969; and *Quality and Equality: New Levels of Federal Responsibility for Higher Education*—A special report and recommendations by the Carnegie Commission on Higher Education, December 1968.

* Further discussion of this issue is found in the papers by Weisbrod, Freeman, and Boanen in vol. 1 of this collection and Feldman in this volume.

attempt to determine how to improve higher education or even to describe what happens at that educational level. Both were concerned not with the quality of higher education, but with the manner and extent to which the Federal Government should contribute assistance. The conclusions of the two studies were similar—the Federal Government should contribute more to higher education, and since resources are scarce, the funds should be oriented to students rather than to institutions, since in that way the students who need the money can be given it (whereas assistance to institutions is presumably spread over rich and poor students alike). The question asked and answered implicitly assumed that education should be left to the educators—with Government providing *financial* assistance, but not assessment or evaluation.*

So analytical and evaluative efforts at HEW (which it should be noted, are still in their infancy) have been devoted largely to redistributive rather than effectiveness questions.⁹

SHORTCOMINGS OF EXISTING ANALYTICAL TECHNIQUES

The typical evaluative effort attempting to get at the question of effectiveness in education proceeds as follows: Collect information on an "output" measure—any output measure—such as achievement scores of schoolchildren, and relate that output or dependent variable (through a linear regression) to a host of "input" variables—school characteristics as well as attributes of the child and his parents. This approach grows out of economists' attempts to estimate "production functions" for firms. But the economist has two advantages over the education evaluator:

(1) Economics provides a theoretical rationale (growing out of the profit maximizing hypothesis) for arguing that the relationship which he finds will be efficient—that it will indicate the lowest cost way of achieving any particular level of output. Since we do not know what school administrators are maximizing, the economists' approach can only yield a description of current practice in education, not necessarily any insight into how to do things better.

(2) The profit maximization hypothesis simplifies statistical or econometric analysis. Whatever school administrators are maximizing—or the American public(s) wants schools to do—it is more complicated than a single measure can show.¹⁰ To carry over the economists' approach might then involve attempting to maximize an amalgam of achievement measures, attitudinal measures, and whatever else is relevant, subject to the *set* of production functions—one for each of the output measures of interest.¹¹ To the author's knowledge, no one has yet attempted to do this for education.

* Exceptions to this statement include some unpublished work of George Mayeske and Harry Piccarriello of the Office of Education. Both are attempting to relate inputs and outputs in elementary and secondary education. See also Joseph Froomkin, *et al.*, *Students and Buildings: An Analysis of Selected Federal Programs for Higher Education* (Washington, D.C.: U.S. Government Printing Office, 1968).

⁹ Cf. my "Comment on Estimating Education Production Functions" in *Studies of Income and Wealth*, vol. 35 (New York: National Bureau of Economic Research, 1969 (forthcoming)).

¹¹ This approach has been attempted empirically in economics, most often for macroeconomic problems. See Henri Theil, *Optimal Decision Rules for Government and Industry* (Chicago: Rand McNally & Co., 1964).

* Further discussion of this issue is found in the paper by Rivlin in this volume.

It is not as though traditional methods are hopeless, however.¹² There are some cases where one approach to an education problem appears to be so much more effective—even on the basis of rudimentary uniequational analysis—as to provide grounds for implementing the approach. For example, Henry Levin (following the typical pattern) has run linear regressions of student verbal score on a host of variables representing student, environment, and teacher characteristics, in an effort to determine what advice could be given to persons hiring teachers.¹³ In particular, he compared the relationships between teacher verbal score and teacher experience with student verbal score. He found that to accomplish a given increase in measured student achievement “for Negroes, it appears that obtaining teachers with higher verbal scores is about one-fifth as costly as obtaining more teacher experience, and for white students the verbal score route is 10 times as efficient.”¹⁴ The strong and interesting implication of this is that hiring bright teachers might be more effective and/or less costly than hiring experienced ones.

POLITICAL IMPLICATIONS OF ANALYSIS

The academically hallowed distinction between analysts and decisionmakers is often not useful in Government. One reason for this is that analysis itself has political implications. What a Department requests from the Congress, how it requests it and how it is spent—these are all related. To admit publicly that a particular program of a department is doing poorly, may be to invite cuts in that program—and others. The economist or program analyst likes to talk of trade-offs—of the (to him) obvious fact that if one wants more of one thing he must have less of something else. In fact though, an agency head may get a larger budget to work with by declaring that he needs an increment of X and an increment of Y. It is not lost on agency or department heads that evaluation of programs can have adverse effects on budgets. And a decision not to evaluate a program, or to downplay negative results of evaluations can be rationalized not only for this reason, but also by arguing that other programs of other departments, which are “obviously” a waste have not been evaluated. Why then should this program—which has great potential—be jeopardized?

There are other ways in which analysis can have political implications. The two recent studies of higher education referred to above are political phenomena by their very existence and the prestige of the individuals associated with producing them. By ignoring in the main the question of the effectiveness of higher education they may contribute to the difficulties of convincing people that it is important to ask effectiveness questions. Politically, the studies—both of which recommend a surge in Federal assistance to higher education—could lead to a new Federal emphasis on higher education. But this would be at a time when, if there is an educational crisis it is in urban ele-

¹² Samuel Bowles has written an elegant description and critique of current practice in estimating the relationship between inputs and outputs in education. See “Toward an Educational Production Function” in *Proceedings of a Conference on Research in Income and Wealth* (New York: National Bureau of Economic Research, 1969 (forthcoming)).

¹³ “Cost-Effectiveness Analysis and Educational Policy—Profusion, Confusion, Promise,” (mimeo), School of Education, Stanford University, Stanford, Calif., December 1966.

¹⁴ *Ibid.*, p. 12.

mentary and secondary schools. It may be that a countervailing force to higher education is needed in the form of a large visible study of precollege education in the United States.

WHAT MIGHT BE DONE NEXT

This has been a gloomy recital. I have intentionally concentrated on difficulties and shortcomings because they need airing, but in the confidence that observers of the practice of program analysis in Washington already recognize that progress at HEW has been encouraging and the process helpful to the Department.

This paper closes with a commercial for an approach to improving this country's knowledge of how education happens.

We are now seeing that the evaluation of existing programs is intimately related to the development of new approaches. Several educational models have emerged, some of which have shown promising results in particular cases. Development and evaluation merge in implementing and observing these models as applied in actual schools.

Particular Federal programs are difficult to evaluate since they are just a small part of the enormous American education system. We have now neither sufficient educational theory nor powerful enough statistical techniques to separate out—and determine the relative importance of—the various factors influencing a child's educational progress. The typical evaluative effort drastically oversimplifies the real world, but even if it did not it would only describe current educational practice; it would not reveal the most effective way to use our educational resources. The meager evaluative results we do have, for example on federally assisted compensatory education programs for disadvantaged children, suggest that although some youngsters are being helped, the average student's performance may not be improving—and even where results are hopeful it is difficult to specify what it is that makes the difference.

This is not a simple matter of a social scientist wishing the real world were less complicated. What I am saying is that the present state of the relevant social and statistical sciences is such that in the absence of large scale, scrupulously controlled educational experiments we are not going to be able to identify productive approaches to compensatory education. In the moon race America had the luxury of being allowed to succeed through brute expensive force. We are not allowing ourselves that luxury in education, where \$100 million increments to operational programs are a drop in a \$50 billion bucket. But \$100 million could support a lot of demonstration and model schools, a lot of experimentation and development in education.

POLICY ANALYSIS AND HOUSING AND URBAN DEVELOPMENT PROGRAMS

BY WILLIAM B. ROSS

William B. Ross is Deputy Under Secretary for Policy and Program Evaluation at the Department of Housing and Urban Development.

In his discussion, Mr. Ross separates housing programs from programs for other aspects of urban development. In dealing with the evaluation of program strategy in the case of housing, Mr. Ross accepts congressional statements of goals as expressed in the major legislation, and explains the relationship of available output measures to these goals. He appraises the ability of the Nation to meet the housing goals expressed in the Housing and Urban Development Act of 1968, consisting of the construction of 26 million additional dwellings within the next decade. He argues that: "the financial capital, labor, and material resources can be made available in reasonable proportion to meet the national housing goals, but it may require use of other tools of economic management to reach this goal."

In discussing broader goals related to the quality of public facilities and neighborhood life, Mr. Ross sees major problems in undertaking appropriate and quantitative program evaluation. He appraises the kind of data necessary for evaluating the results of public programs with these broad urban development objectives. He concludes, however, that "the ultimate measure of the effectiveness of Federal involvement in such things as urban renewal, comprehensive planning, and model cities will be found not within program data itself, but in the social and economic conditions of the city as a whole."

Introduction

The long-run strategy of Federal agencies—if they presume to pursue a strategic course—must evolve from policy analysis. Its foundation is expected to be a set of clearly enunciated goals whose attainment can be widely agreed upon and which, hopefully, are distinct enough that progress toward attainment can be witnessed. However, the formulation of pertinent goals is deceptively difficult because they must be phrased in utter simplicity and have the clarity of the obvious.

GOAL IDENTIFICATION

After the identification and expression of major goals, they in turn become the means of grouping the agency's programs in the program categories which collectively constitute the program structure. It is not imperative that the agency's table of organization follow the program structure rigidly, but a general parallelism is helpful, both in placing executive responsibility for goal attainment and in program evaluation. The program structure is thus an integrative tool and goes significantly beyond mass cost-benefit analysis.

Most of the program structures currently in use are understood to be experimental and subject to change as they are tested by the criterion of how satisfactorily they contribute to orderly and perceptive identification and resolution of major issues. Change may also become

mandatory following legislative enactments which introduce new missions and programs, or when the perceptible national climate of opinion alters national priorities or objectives.

For the Department of Housing and Urban Development, the current program category structure is derived—with liberal doses of selection, interpretation, and interpolation—from major congressional declarations of purpose in the Housing Act of 1949, the Department of Housing and Urban Development Act of 1965, and other major enactments where the Congress has provided a clear statutory statement of its intent.

Several explicit statements of purpose have clearly enunciated national housing goals and stress the role of private activities in serving national housing goals. For example:

From the declaration of purpose in the Department of Housing and Urban Development Act “. . . to encourage the maximum contributions that may be made by vigorous private homebuilding and mortgage lending industries to housing, urban development, and the national economy . . .”;

From the declaration of national housing policy in the Housing Act of 1949 “. . . the realization as soon as feasible of the goal of a decent home and a suitable living environment for every American family, . . .” “The policy to be followed in attaining the national housing objective hereby established shall be: (1) private enterprise shall be encouraged to serve as large a part of the total need as it can; (2) governmental assistance shall be utilized where feasible to enable private enterprise to serve more of the total need; . . . departments or agencies . . . shall exercise their powers, functions, and duties . . . in such a manner as will encourage and assist . . . (3) the reduction of the costs of housing without sacrifice of . . . sound standards; . . . (5) the stabilization of the housing industry at a high annual volume of residential construction”; and

From the Federal National Mortgage Association Charter Act, which authorized specific activities as . . . “thereby improving the distribution of investment capital available for home mortgage financing. . .” and “. . . as a means of retarding or stopping a decline in mortgage lending and homebuilding activities which threatens materially the stability of a high level national economy. . . .”

The most explicit statement of a national housing goal is to be found in title XVI of the Housing and Urban Development Act of 1968: “The Congress reaffirms this national housing goal and determines that it can be substantially achieved within the next decade by the construction or rehabilitation of 26 million housing units, 6 million of these for low- and moderate-income families.”

Based on these congressional statements, we have selected as one of our program categories “Decent housing for all Americans” with subcategories for missions which would contribute to this objective “through efficiently functioning private markets” and “through assisting low- and moderate-income housing.”

OUTPUT MEASUREMENT—THE CASE OF HOUSING

Both the evaluation of long-term program strategy and guidance of year-to-year program tactics require that some output measures be reasonably available, and that these be viewed in the light of both of broad national goals or needs and the costs expended in recent program efforts to reach those goals. Of all of the programs administered by HUD, its housing programs are the most susceptible to measurement, both in terms of needs and output. The reasons for this are quite clear.

First, standard housing has qualities of homogeneity even though there are marked differences between various units produced. A standard housing unit—by definition—provides decent, safe, and sanitary shelter for a household (usually a family). Also, it is fixed to a location, and few housing units are moved until demolished—even many “mobile homes.” Thus, the construction of a new housing unit is a satisfactory measure of output in evaluating progress toward the goal of “a decent home for every American family.”

Second, housing units are enumerable: through the decennial census of housing, and intercensal sample surveys; through local land-parcel data banks that are coming into being; and through local building permit records issued for new construction, rehabilitation, and, sometimes, demolitions.

Third, in broad measures, substandard housing can be counted separately from standard units. While the criteria of quality regarding structural condition may vary somewhat from place to place and even experts are apt to disagree where individual structures are near the margin between sound and dilapidated, yet for the most part there is a common understanding as to which of the total housing stock is good and which is bad.

Ideally, in order to measure the progress made toward meeting the housing needs and removing the units which became substandard, there should be annual data to measure accurately the losses from the housing supply and the units which have become substandard. Until such data are developed, the measures of progress must rely on loss estimates which leave an unsatisfactory margin for error, particularly in measuring progress toward meeting the goals year by year.

ESTABLISHING LONG-TERM HOUSING GOALS

The strategy of our national housing goals rests on estimates of long-term needs, expressed by annual increments, in sufficient detail to show for whom the housing will be needed and the degree of Federal assistance which will be needed to convert “need” to “effective demand.” Such estimates were prepared by HUD and submitted to the Senate Banking and Currency Committee in connection with the hearings on “Housing and Urban Development Legislation of 1968.” The following statement on housing needs is reproduced from those hearings.

“The program goal of 26 million units to be added to the standard housing supply over the next decade is based on estimates of the Nation’s overall housing requirements * * *.” About half of that number of units is required to accommodate the expected net increase in the number of households.

“Over 5 million units will be required for increases in vacant units to permit desired mobility of households plus an expected increase of seasonal or ‘second’ homes, and to compensate for units abandoned as a result of population shifts from areas of declining population to growth areas. Another 6 million units will be required for replacement in about equal proportions of (a) demolition, casualty, and other losses of nondilapidated units; (b) the removal of presently occupied dilapidated units; and (c) the removal of units that will become dilapidated over the decade.

Only about one-half of close to 4 million occupied nondilapidated but substandard units are included in the 26 million total. These are units which are substandard by virtue of lack of plumbing, although they are in a sound or deteriorating condition. Only 2 million of these that would be rehabilitated through public assistance programs are included in the 26 million unit total for which financing and cost calculations are included. The balance, it is estimated on the basis of past trends, would become standard through repair and modernization without public assistance. The installation of lacking plumbing amenities will often bring such units up to standard conditions. Since such improvements will not involve either major rehabilitation expenditures, mortgage financing or public assistance, units to be included in this manner are not included in the annual housing production program schedules shown in Table 1.

As mentioned previously, the Congress—in title XVI of the Housing and Urban Development Act of 1968—determined that the national housing goals could be met by the production of 26 million new and rehabilitated units over the next decade. In that title, the Congress also required that the President should submit to it annually a report on the progress and problems in meeting the national housing goals, and that the first report should set forth a plan which would indicate the number of new or rehabilitated housing units that would have to be provided in each fiscal year of the decade. Such a plan was provided in the First Annual Report on National Housing Goals, January 1969. As set forth in the following production schedule taken from the report, a growth of annual housing production which would permit stability in residential construction and in the economy is proposed.

TABLE 1.—ESTIMATED ANNUAL STARTS OF NEW DWELLINGS AND ASSISTED REHABILITATIONS

[Numbers are in thousands and are rounded to nearest 25,000]

Fiscal year	Total starts and assisted rehabilitations	Total private unassisted starts	Publicly assisted starts and rehabilitations
Total	26,200	20,200	6,000
1969	1,675	1,450	225
1970	2,000	1,500	500
1971	2,225	1,600	625
1972	2,375	1,750	625
1973	2,575	1,950	625
1974	2,650	2,000	650
1975	2,950	2,300	650
1976	3,200	2,500	700
1977	3,250	2,550	700
1978	3,300	2,600	700

As that table indicates, the target projections would call for some 20.2 million unassisted new housing unit starts and 6 million publicly assisted housing units. The latter would include about 2 million rehabilitated units under the (Federal) publicly assisted programs. The production schedule for the 10-year program has been projected with reasonable annual increases. As compared with starts and rehabilitations of about 1.5 million in fiscal 1968, about a 1.7 million unit volume is projected for fiscal year 1969, increasing to a level of 3.3 million units over the decade.

HOUSING GOALS IN RELATION TO GNP

The production of housing to meet the 10-year housing goals will require a significant increase in economic resources devoted to housing construction. From a long-range point of view, the economic resources to meet the specified housing requirements over the next decade should be available. New residential construction generally would account for measurably less than the 5—5.5 percent of GNP experienced in prior high housing activity years of the past two decades. Only toward the end of the coming decade would this ratio be about 5 percent.

Following the quantification of projected housing needs over the decade ahead, careful attention was given to an examination of the capacity of industry to produce at such levels, and of its institutional structure. Of equal—if not greater—importance will be the ability of the capital markets to furnish adequate quantities of housing finance. Because the flow of funds into home mortgages is such a large part of total new investment, this part of the study had to embrace projections for each of the major users of capital funds under stated assumptions of growth in GNP and certain rates of savings.

RELATIONSHIP TO OTHER NATIONAL PRIORITIES

This estimate of housing needs has been compiled in the absence of any overall national goals structure—or even a similar estimate of long-term resource and investment needs for other sectors of national life. It has not been our national custom to engage in long-range national economic planning; we have, for the most part, limited our Federal fiscal planning to the ensuing budget year. Hence, this national commitment to satisfying housing needs per se for a decade ahead is a novelty in Federal affairs.

Will these goals be met? The pattern of housing production to occur during the next 10 years will be a function of all of the private and governmental decisions which bear upon the distribution of resources and capital throughout the economy. Competing demands for limited resources always govern the markets in a nation with freedom of economic decisions. Under a number of explicit assumptions which have had to be made, HUD's testimony has shown that the goals might be met. But we do not presume to have the omniscience to know that these assumptions will prevail.

If the alternative possible courses of future economic forces are to be assessed so that realistic policies to attain the national housing goals can be devised, account must be taken of the likely departures from the assumed balanced economic growth that would encompass the required housing production.

Based on experience of the past two decades, it must be noted that housing activity is most responsive to tight money conditions and restrictive monetary policies. This is to be expected, since more than 90 percent of new home purchases and more than 80 percent of existing home transfers, as well as practically all multifamily housing transfers, require long-term mortgage financing. Although structural innovations in obtaining mortgage financing, such as through Government guaranty of bonds backed by pools of insured mortgages, may help over the long run, the problem of recurring cyclical credit shortages probably would require some fundamental fiscal and monetary policy reforms.

As far as the assisted housing programs, are concerned, to produce 6 million units, there is a need for continuing timely appropriation action, to assure funds that can be committed for contractual subsidy payments, and assure continuity of the construction organizations willing to undertake these commitments to produce units.

Building materials, on the whole, do not seem to pose a significant impediment to the attainment of the national housing goals. In many building materials producing industries, there is excess capacity and adequate additional capacity would probably be provided in response to growth of demand. There are special problems that arise in connection with limited natural resources, however, such as the current lumber shortage. In such instances, special planning may be necessary to guide the allocation of available resources and to foster substitution of other materials, insofar as possible, in order to counter price rises, as well as to remove supply shortages that retard construction.

The problem of land for building sites needed for the national housing goal construction was summarized in the "First Annual Report on National Housing Goals," as follows: "To the extent that the availability of building sites constitutes a potential problem, it focuses on sites for assisted housing. Recent experience suggests that there is not an insurmountable problem, however, as long as Government assistance in providing sites is continued. Such assistance is being provided through availability of urban renewal land, Federal surplus land, and the encouragement of new communities. Another possibility is the provision of air-right platforms over federally assisted highways for multifamily housing. Rehabilitation units under the various assisted programs, primarily in central cities, will not require vacant sites. Assisted homes for owner-occupancy, under both the HUD section 235 and the Farmers Home Administration programs will, to a large extent, be built in outlying areas where land availability should not be a problem. Experience with some of the older assisted rental housing programs also indicates that an increasing proportion of projects are being located outside of central cities. The evidence suggests that sufficient sites can be made available to accommodate the required housing if the Federal programs that help provide the sites are pursued vigorously."

In the "First Annual Report on National Housing Goals" it was also concluded that "Significant increase in manpower for on-site residential construction will be needed each year if the housing goals production schedule is maintained." Similar to the general problem of allocation of resources, the labor resources will be present in our economy from growth of the labor force and those presently unemployed. Continuing recruiting and training efforts will be needed through

deliberate actions to lend assurance that the labor requirements will be met. The on-site labor requirements will also be reduced to some extent as more use is made of prefabricated building components.

In brief, within the context of the overall economy, the financial capital, labor, and material resources can be made available in reasonable proportion to meet the national housing goals, but it may require use of other tools of economic management to reach this goal.

If the national housing goals are to be met in the context of a full employment economy, there is an implied priority to hold down the level of other activities so that necessary resources can be allocated to housing production. The tools of economic stabilization embrace the range of both fiscal and monetary policy, some of which are less direct than others. On the fiscal side, one tool which could be used would be the delegation of limited discretion in tax powers to the executive branch. This is cited only as an illustration, as there are other ways by which the legislative and executive branches, as well as the monetary authorities, can—and do—influence aggregate economic activity and its components.

GOALS FOR OTHER HUD RESPONSIBILITIES

Urban housing goals require that more than housing structures be provided. Population growth—for which most of the new housing will have to be built—calls for a rather long list of facilities and services. These include streets, water, sewers, parks, schools, hospitals, churches, fire, police, and health protection, public utilities, and commercial structures for mercantile and service functions. The relationship of these adjuncts of urban growth to the production of housing is not quantitatively precise but the complexities of variations in income levels, distance to alternative facilities, local customs, and personal tastes, all influence the quantity—and the quality—of public facilities and services needed for a given increment of growth.

The amount of land which will be required to accommodate units of population growth is also subject to the same kind of imprecision as are the requirements for public facilities and services. Housing and all of its urban adjuncts have rather wide variations in land use requirements. Unfortunately, we do not have a tidy definition of agreed-upon minimum needs for anything but housing; one standard dwelling unit for one household is about as neat as you can get. This same variability and imprecision carries over, with magnification, into the amount of financial capital that will be required for equity funding and loan financing of facilities.

HUD's current program structure employs the following language for functions other than housing and general management:

Assuring adequate and efficient local public and private facilities and services.

Improving the physical environment of urban communities.

Improving the social environment of urban communities.

Improving management of community development activities.

Under each of these goal categories are the various programs which contribute to those ends. However, this sorting of programs is far from sharp because some of them make contributions, perhaps secondarily, to the accomplishment of other goals as well. For example, grants for

neighborhood facilities are categorized under the local public and private facilities goal, since HUD's role is to fund part of the cost of construction. However, the goal of the facilities themselves is to improve the social environment of the community. Again, urban renewal and neighborhood development programs are carried under improving the physical environment—which they do. But they also contribute to adequate local public and private facilities and to improving the social environment.

MEASUREMENT PROBLEMS IN NONHOUSING PROGRAMS

In the case of these complex programs where a single project may involve the lives of thousands of people and hundred, or thousands, of acres of land, the basic statistic is one city, one grant, or one loan. Yet we know that evaluation of the effectiveness of these programs rests on the success of individual projects in city after city. The ultimate measure of the effectiveness of Federal involvement in such things as urban renewal, comprehensive planning, and model cities, will be found not within program data itself, but in the social and economic conditions of the city as a whole, or in some cases, the change in the physical environment, the level of living, and the attitudes prevailing in the neighborhood where the action took place.

In programs where output is an inert object, it may be relatively easy to measure cost effectiveness in terms of alternate means of production, at least where quality is homogeneous. But as soon as the inert product enters into the level of living of people, effectiveness may have to include both plus and minus factors that would reflect changes in the quality of life—if a consensus could be reached on pertinent criteria. For example, housing as an inert output product can be counted and priced with respect to both standard and sub-standard units. Further, when information can be gathered, it can be shown whether the new housing had relieved prior overcrowding; had reduced the time and cost of transportation for those employed; had improved access to public schools, stores and other facilities; and/or had lowered family monthly housing expenses. We have tested the possibility of gathering such information by asking tenants of some HUD-assisted housing projects to answer questionnaires, and find that it is possible, although expensive, to make such evaluations.

But when we turn to programs which finance public facilities and services, the real output must be directly identified in terms of the effectiveness of local government or the general welfare of large groups of citizens rather than the lives of specific individual families. Moreover, some of the requirements for services and facilities may depend in whole or part upon decisions of numerous private consumers and investors, for example, mass transit, hospital facilities, open space tracts, et cetera. Private decisions made or yet to be made are not grist for any statistical mill. While goal formulation for programs which involve social and governmental processes can be expressed in terms of abstract ends, real outputs are often measurable only after a long period in terms of improvement of lives of identifiable groups of people. Progress, however, can sometimes be reflected by proxies, provided they are at least symptomatic of the goals.

During the past 2 years, there has been considerable discussion and planning of social indicators to complement economic indicators as

measures of the general welfare.* Just as the concept of the economic poverty line is fluid with respect to both changes in the cost of living and the standard of living, the social happiness line is fluid regarding the incidence of discontent and the standard of contentedness. How much illness, ignorance, crime, and divorce can be accepted in a contented population?

In the case of HUD's nonhousing programs, there are two other unresolved problems besides the matter of identifying outputs relative to abstract goals. One of these is estimating the dimensions of national need, which as shown above, was relatively straightforward for housing. The other problem, assuming the feasibility of making heroic assumptions of need, is in deciding on the proper Federal role in meeting those needs. As cases in point, consider programs such as comprehensive planning, urban renewal, historic preservation and urban beautification. What are the real outputs of planning? They are probably the quality of local public decisions, and certainly not the number of maps or volumes of reports prepared. But how much planning is needed and what is the effective or proper Federal role in such a local responsibility and activity.

In the case of urban renewal, a number of observers have placed figures on the total dollar needs of American cities, but the range has been very wide. And in none of these estimates has blight been defined, let alone the calculation of how fast new blight arises under varying circumstances. Historic preservation and urban beautification involve similar vagueness of definition, and hence of quantification. What is historic and what is ugly? And even under abstract definitions, what is the current universe in terms of size and cost?

All of these ambiguous problems related to the measurement of urban needs and progress in meeting them, reach a culmination in model city evaluation, where the real program output is the quality of life of neighborhood residents and the narrowing of differences within the total community. It is becoming more apparent that wherever Federal programs have social and economic benefits for citizens or improve the effectiveness of local government, the measurement of need can most closely be approximated from various benchmark data that are compiled on the scale of the decennial census enumerations or by very large special and local intercensal public and private studies. And the real outputs, that is, improvement in the problem areas described, will be measured by changes in such socioeconomic series.

But equipped with such data, we still have not answered the more fundamental questions of how far and how fast we should strive for improvement; at what cost; what are the respective roles of the private sector and of those elements of the public sector consisting of Federal, State, and local governments. Nor are we yet in a position to demonstrate the relative effectiveness of categorical grants, block grants, and revenue sharing. In the broader concept of policy analysis, this last issue is a most important one for Federal cost-effectiveness decisions.**

*Further discussion of this issue is found in the paper by Sawhill in vol. 1 of this collection.

** Further discussion of this issue is found in the papers by Mushkin & Cotton, and Olson in vol. 1 of this collection.