Dr. Herbert Friedman
December 18, 1972

MEMORANDUM TO: Dave Beckler

SUBJECT: Organization and Work of PSAC

I agree with the consensus that the panel activities are the most purposeful aspects of the PSAC operation. While it is probably impractical to increase the burden of panel activities on individual members of PSAC, Tape's suggested groups of four to six members with special expertise in various sub-sets of PSAC problem areas could undertake overviews of those areas that need to be followed closely. Time for such sub-group activities could be part of the agenda of each monthly meeting. Any group could expand its activity beyond the monthly meeting if the members deemed it worthwhile.

If the Chairman finds that the general monthly meetings do help him sharpen his own analyses, the meetings are worth continuing. It is easier for members to plan a regular calendar than to be simply on call at the behest of the Chairman.

Programs in which the science and technology aspects can be most directly assessed include:

basic research in the physical and biological sciences
national security
health care
energy
transportation
environment
natural hazards
space science and applications
international scientific cooperation

Bell Laboratories

S. J. Buchsbaum
Executive Director
Research, Communications Sciences Division

Crawford Corner Road
Holmdel, N.J. 07733
Phone (201) 949-5564

December 13, 1972

Dr. E. E. David, Jr.
Director
Office of Science and Technology
Executive Office Building
Washington, D. C. 20506

Dear Ed:

This is in response to your memorandum of October 27 requesting comments on the operation of PSAC. I apologize for being tardy in replying.

I have a simple view of advisory committees. They are useful and effective only if and when their sponsor needs and uses their advice. If this simple criterion were to be applied to PSAC, the conclusion would probably be that PSAC has outlived its usefulness. In my view it has outlived its usefulness so far as one of its intended functions is concerned, namely, to advise the President.

However, PSAC has played other important roles. Its position in the White House has allowed it to exert influence (good or bad) on government agencies and departments (as well as to serve as a sounding board for the Science Adviser when he so desired). In my view this second role has been and can continue to be an important one. Any reorganization of the science advisory apparatus should recognize the fact that the Executive, and especially the OMB, must have access to independent, well-informed and (hopefully) unbiased views on science and technology and, in particular, on how use is made of S&T by the mission agencies. PSAC, or a suitably renamed body like PSAC, can help fill

that need. Under the chairmanship of the Science Adviser (or even another, but highly placed individual) it would operate somewhat in the manner it had in the past, that is, through PSAC panels and through membership on and/or chairmanship of OST panels.

The change that I would advocate would be that PSAC work as a committee-of-the-whole on a few (two or three) of the large, long-term problems which will obviously continue to face the Nation for many years to come. The first and obvious one is the energy problem. I would get this one going immediately.

An alternative to the committee-of-the-whole would be for PSAC to divide itself into, say, four or five groups with overlapping membership. Each group consisting of at least five members would cover and take responsibility for a major area, say, national security, energy, health and quality of environment.

PSAC should not tackle too many problems. In the past we looked at many things but only superficially. Well thought through recommendations in a few key areas might go a long way towards restoring our vitality.

Sincerely yours,

Sol

NOTES ON PSAC AGENDA
December 18-19, 1972

Item 1 PSAC Organization and Work Program

In his memorandum of October 27, Dr. David asked each PSAC member to submit suggestions on:

a. questions and issues that should be considered by PSAC during 1973, and

b. improvements in PSAC organization and procedures that will enhance its effectiveness.

To date, only five responses have been received. An abstract of the principal points suggesting possibilities for "action" is attached, together with copies of the full correspondence.

Although the comments generally supported the need for and importance of PSAC, all respondents felt that changes should be made in its organizational approach and work program.

All stressed the importance of panel activities and generally endorsed more emphasis on problems in the civilian sector. This, it was pointed out, has implications for the selection of new members and the PSAC-OST relationship.

Dr. Coleman's letter raised the most serious questions of whether and how PSAC, meeting as a committee-of-the-whole, can perform a valuable function. He stressed the importance of panel activities and questioned the desirability of the Committee continuing its general two-day meetings once a month. This suggests the need for re-examination of PSAC's mode of operation and whether it should be used primarily as a resource for initiating panel activities and for reviewing selected panel reports (PSAC, OST and other) that have policy conclusions and recommendations deserving White House consideration. Operating in this way, the Committee would meet at the call of its Chairman and its membership would primarily be engaged in panel activities. Following Dr. Tape's suggestion, a few PSAC members could take responsibility in each of several areas of major concern to PSAC to follow developments and identify issues requiring special attention or study by ad hoc panels.

It is hoped that those PSAC members who have not submitted their views will do so before or at the December meeting, at which the first half day has been set aside for this matter.

Item 2 — Report of the PSAC Panel on Youth

This report was discussed in detail at the November PSAC meeting. The comments of Committee members have been discussed by the Panel and revisions have been made in the report. These will be sent directly to PSAC members by Dr. Coleman on Tuesday, December 12.

PSAC is asked to endorse the report and its transmittal to the President (and interested Federal agencies), with a recommendation that it be made public.

Item 3 — National Science Board Report on Health of Science

The enclosed draft report is the Fifth NSB Report prepared for transmittal by the President to the Congress in accordance with its statutory mandate. The NSB Ad Hoc Committee responsible for preparing this draft was chaired by Dr. Norman Hackerman, President of Rice University. Accompanying Dr. Hackerman to the PSAC meeting will be Dr. Roger W. Heyns, President of the American Council on Education and Vice-Chairman of the NSB, and Dr. H. G. Stever, NSF Director.

Although the report has its general approval, the NSB has requested PSAC comments before taking final action.

Item 4 — Report of the PSAC Panel on Chemicals and Health

This report was presented and discussed in detail at the November PSAC meeting. PSAC member comments have been taken into consideration in preparing the revised, final draft of the report. Dr. John Tukey, the Panel Chairman, will summarize the changes that have been made. The report is before the Committee for approval and transmittal to the President.

David Beckler

David Z. Beckler
Executive Officer

PSAC USE ONLY

Abstract of Member Comments on the Organization and Work of PSAC

A. Organization

1. There should be a nucleus of four/six members with expertise in each sub-set of PSAC problems; e.g., national security, medical research, health care, civil R&D; plus a few experts who can contribute to all sub-sets; e.g., economist, educator, etc. (Tape)

2. The Vice-Chairman should be a non-government employee to serve as a civilian focus and to lead PSAC discussions when the Chairman is constrained because of his relationship. (Tape and Garwin)

3. Several PSAC members should perform an overview function in given areas, to consider what areas should be examined, recommend the establishment of specific task forces, report to PSAC as-a-whole, and advise the Chairman and OST staff. (Tape)

4. PSAC is of value through its panel activities, not through general two-day meetings once a month. Change composition to reflect more fully some important areas of social policy (add two economists familiar with the economics of transportation, housing, health, education, and welfare). Use PSAC's unique capabilities of bringing together men from diverse disciplines. (Coleman)

5. The clearances of new members and panel members should be expedited. (Garwin)

6. The role of PSAC as "spokesman for science" needs to be clarified. (Garwin)

7. The relative merits of OST staff and PSAC member chairmanship of panels. (Garwin)

8. Greater use of OST panels if PSAC endorsement is required; use of PSAC panels for reports aimed at public use. (Tape)

9. Examine the activities of PSAC military panels to determine whether they are performing the dual function of contributing to military R&D and reviewing for the President current capabilities and DOD performance. (Garwin)

B. Program Emphasis

1. Programs that have a large science and technology component (issues requiring sociological and political solutions or where the S&T pay-offs are small should be handled under OST auspices or elsewhere):

   -- national security (defense and economic viability)
   -- health research
   -- energy, mass transportation, other civilian needs
   -- quality of life (environment, economic development)
   -- space

   (Tape)

2. Direct work toward specific problem areas in which the Committee could be effective:

   -- quality of life (transportation, drug control, etc.)
   -- balance-of-payments and unemployment problems associated with technology

   (Truxal)

3. Concentrate on:

   -- energy needs, natural resources, and air and water quality standards
   -- urban mass transit
   -- technological exchange with Russia
   -- spokesman for science

   (Cairns)

4. PSAC can most fruitfully function through panel activities; (1) acting as a watchdog on an agency's proposals and providing alternative proposals for the President and OMB, and (2) reporting on an area in which work should be stimulated or policies developed, independent of any particular agency:

   -- aerospace and defense
   -- housing, transportation, and welfare
   -- new opportunities, such as the Panel on Youth

   (Coleman)

5. Examine the need for a panel to consider ways to quantify the quality of life contributions to our over-all national product.

(Garwin)

DZB/EED/mas-12/19/72

f:1r

MEMORANDUM FOR

THE PRESIDENT

FROM: The President's Science Advisory Committee

In accordance with your request, each member of your Science Advisory Committee will shortly submit his pro forma resignation, to take effect at your pleasure.

Your request has occasioned the Committee to ~~re-examine~~ review the role of the science advisory function in the White House. We bring these views to you with the thought that they may be of use in your reorganization of the White House/Executive Office structure. ~~However, we wish to make it clear that we are not advocating a continuation of the~~ status quo.

You will recall that the President's Science Advisory Committee and the post of Science Adviser were created by President Eisenhower fifteen years ago. Over those years, their activities have revealed the values they can contribute. First, there is excellence and quality. Much of federal R&D tends to be routine and even pedestrian. Most, but not all, of the best scientists and engineers work outside the federal establishment in industry and the universities. Yet it has been these people through their direct advisory efforts who have animated federal R&D to the achievements of the past 25 years including the Polaris submarines, the space program, the biological warfare ban, the polio and other vaccines, and so on. This coalition of outside-government people of the highest quality and the federal establishment is unique. The Science Adviser and PSAC have played this creative role at the Presidential level. Second, science and technology are

the headlights of our society. The future can be seen only through an evolution of embryo technologies. This foresight is an essential ingredient in planning for the future and in managing today's enterprises.

Despite these values, there remains the question of why an advisory mechanism at the White House level. ~~The answer lies in the matter~~ of ~~quality mentioned above~~. The federal establishment has not traditionally been creative in solving the nation's problems. The agency and departmental viewpoints ~~are necessarily~~ tend to be fixed on ~~defense of~~ the protection of their on-going activities. So perhaps the principal raison d'etre for a science advisory mechanism is to aid in identifying solutions of problems vexing to you. This creative activity has benefitted uniquely from the ingenuity of engineers and scientists outside the federal establishment. A second reason for scientific advice at the highest level is the need for detecting incipient failures early in technical programs. Again, this is a function difficult to perform in the agency responsible for the work.

Finally, the technical problems facing the White House today have broadened beyond space and military technology to many domestic problems -- energy, transportation, health care, environment, and natural resources -- that cut across the interests of many federal departments and agencies. It is necessary to bring together scientific and technological resources dispersed throughout government in support of common objectives in these areas of concern and to assess trade-offs among agency R&D efforts that can only be weighed at the highest level of government. This, in turn, requires a

high quality technical staff mechanism at the Presidential level, assisted by the best scientific and technical expertise available from outside of government.

We believe that if the advisory apparatus is to be effective it must have your confidence. Thus the advisory mechanism must recognize only two allegiances -- the President, his programs and policies -- and the national interest rather than the interests of the scientific community, some sector of industry, academia, ~~or~~ labor, ~~and rather than the interests of~~ or any agency bureaucracy. Furthermore, the value of an outside scientific and technological advisory committee rests on mutual trust between you and the Committee and your willingness to call upon its services. The members of the Committee must maintain the confidentiality of this relationship, avoiding all public discussion and testimony regarding its work.

We are anxious to insure that you have the most authoritative technical advisory apparatus that the nation can provide, and one which will devote itself to serving your interests and those of the nation. We are prepared to assist you and the Science Adviser in changing the advisory mechanism in any way which will make it more useful to you. In short, we support the advisory function in the White House, but not necessarily in its present form.

International Business Machines Corporation

Thomas J. Watson Research Center
P. O. Box 218
Yorktown Heights, New York 10598
914/945-2555

December 12, 1972

Dr. Edward E. David, Jr.
Science Adviser to the President
The White House
Washington, D. C. 20500

Dear Ed:

In response to your request of October 27, 1972, for a letter with suggestions on

a) questions and issues that should be considered by PSAC during 1973, and

b) improvements in PSAC organization and procedures that enhance its effectiveness

I want to discuss PSAC's organization, procedures, and mission first. I think that questions and issues for 1973 will then follow.

1. PSAC OPERATIONS

New members come to the Committee with little idea of the nature of PSAC. I think that this could be remedied as follows:

-- Maintain a list of all former PSAC members, with their terms and associations. Provide potential PSAC members with such a list, together with any current public write-up of the function of the Committee.

-- Give to new PSAC members a bibliography (Secret or Top Secret) of all old PSAC reports (naked if necessary, preferably with a paragraph of description as of date of transmission). In this way, we can educate, if not indoctrinate, new PSAC members. We cannot deny what PSAC has been in deciding what it should be.

-- Much more preparation should be made for the meeting of other groups with PSAC and with its panels. If PSAC is important, its time should be well-spent and its effectiveness maximized. The presentation or discussion should be directed by personal contact between the relevant OST staff or PSAC panel chairman on the one hand and the leader of the visitors on the other. Where contact has been made through higher echelons (as from the Science Adviser to the DDR&E

and then passed in the form of a directive down to one of the Services) the staff must contact the group which is in charge of the presentation in order to inform the individual of the identity of the individuals to whom he will be talking and of their specific interests. My analogy of the way these things are too often handled is that of the Western Union telegraph system, where the message is sent from the Committee through many hands and eventually arrives at the recipient. I prefer the mechanism of the Telephone Company, where the company sets up the communication link, and then communication then takes place instantaneously over that link between the staff on the one hand and the preparer of the briefing or the leader of the visitors on the other.

2. THE MISSION OF PSAC

PSAC can only exceptionally be involved in management. Its primary purpose should not be public education. It should not do those things which can be done as well or almost as well by others.

PSAC's strength in the past has been in defense and national security. By having a hunting license deriving from the President to learn from the agencies and departments, it fulfilled a primary mission unfettered by departmental loyalty or partisan politics, to report on US capabilities, on US needs, on US opportunities, and on foreign threats.

The secondary contribution of PSAC in this field has been the frequent and important proposal of better ways to build our military capability and our intelligence systems. Those who criticize PSAC do not know its contributions; and those who constitute it now, for the most part, cannot defend it because they are ignorant of its history.

The necessity for PSAC is to have free access to government agencies on behalf of the President. Given the choice of one only among

-- open publication of PSAC reports,

-- requests from the President for PSAC activities,

-- free access to government agencies on behalf of the President,

the last would allow the President and the nation to benefit most from PSAC activities and initiatives.

Most importantly, PSAC could in this way provide the President with interesting and important insights and alternatives -- it would then be PSAC's fault if the President did not occasionally call on the Committee when he was uneasy.

It would of course be necessary for the White House staff to allow the occasional PSAC report to reach the President. It would also be desirable for the President to believe that knowing the facts and the alternatives, whatever the final choice, is desirable for him and for the government.

Thus, I don't believe that PSAC can or should do the work of the rest of the government. It can show its expertise to the President by making concrete contributions when it involves itself, but that would not be its reason to exist. Contacts with an agency should leave agency personnel with admiration for the quality of PSAC (or its agents), as befits the Office of the President.

-- I think the Committee can fill these important functions only if it has a strong Vice Chairman, outside of government, a good part of whose concern for his term is to maximize the effectiveness of PSAC in serving the President. I think, too, that PSAC activities would have to command some OST staff resources. PSAC now has little idea of what transpires in OST, even to the knowledge of what panels exist. PSAC members and prospective members should have a clear understanding of the relationship between PSAC and OST.

-- I think that PSAC should avoid tasks which can be done without privileged access to government departments and agencies, suggesting their performance, instead, by NAS, foundation support, or other groups.

Sincerely yours,

Dick Garwin

Richard L. Garwin

cc: PSAC Members and Consultants

PSAC

DEC 8 1972

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SCHOOL OF MEDICINE
DEPARTMENT OF MEDICINE

4 December 1972

Dr. Kenneth Olsen
Digital Equipment Corporation
Maynard, Massachusetts

Dear Ken:

Many thanks for your note of November the 28th and for sending me the information about the glucose monitor. I shall certainly look it over and give it some thought. Looking at my schedule, it seems unlikely that I shall be able to make much of an analysis about this within the next week or ten days. Perhaps we can talk together about it at the December meeting of PSAC. Thank you for sending me these data.

Sincerely,

Holly

Lloyd H. Smith, Jr., M.D.
Professor and Chairman
Department of Medicine

LHS:cb

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DEC 11 1972

AGENDA
PRESIDENT'S SCIENCE ADVISORY COMMITTEE
Room 208, Old Executive Office Building
Washington, D. C. 20506
December 18-19, 1972

Monday, December 18

Item 1
9:30 - 12:00

Chairman's Report

a. PSAC Organization and Work Program
b. Other Business:
PSAC Membership

Lunch
12:00 - 1:00

Executive Dining Room
Room 22 OEOB

Item 1 (Continued)
1:00 - 2:00

Item 2
2:00 - 3:00

Report of PSAC Panel on Youth (further discussion) - J. Coleman

Item 3
3:00 - 5:30

National Science Board Report on Health of Science - R. Heyns, N. Hackerman, et al

Tuesday, December 19

Item 4
9:00 - 11:00

Report of the PSAC Panel on Chemicals and Health (further discussion) - J. Tukey

Item 5
11:00 - 12:00

Other Business

Lunch
12:00 - 1:00

Executive Dining Room

Item 5 (Continued)
1:00 - 3:00

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DEPARTMENT OF
SOCIAL RELATIONS

8 November 1972

Dr. Edward E. David, Jr., Chairman
The President's Science Advisory Committee
Executive Office Building
Washington, D.C. 20506

Dear Ed:

I am responding to the October 27 memorandum, asking for views about the general task and organization of PSAC. Ever since I have been a member of PSAC, I have not had a good sense of what the functions of PSAC are, or even what they should be. I think the Committee is useful in a small way as a sounding board for you, and through you, for the President, concerning policies that affect the scientific community. But if that were sufficient cause to assemble 18 persons two days a month in Washington, there should also be a President's Industrial Advisory Committee, a President's Educational Advisory Committee, and so on, for a variety of interest groups.

On the other hand, as a Committee to provide advice on scientific matters affecting public policy, which is in general the role I believe is appropriate, the convening of 18 persons for two days a month from assorted disciplines seems of small value. The briefings from agencies are sometimes good, sometimes bad, but have the common characteristic that they are briefings that lead to nothing. Such briefings are useful when a body has the task of making a decision. PSAC does not. Thus the major portions of the meetings, designed to inform PSAC about something, are, I think, a waste of the government's time and PSAC members' as well.

Accordingly, I have felt that the one fruitful way a PSAC member can function is through panel activities. There, however, two kinds of functions seem to exist: first, acting as a watchdog on an agency's proposals and providing alternative proposals for the

President and OMB; and second, reporting on an area in which work should be stimulated, or policies should be developed, independent of any particular agency.

I think PSAC's greatest value has been in the former of these capacities, and that value can be great indeed, particularly in aerospace and defense. I think it perhaps could equally in housing, transportation, and welfare, if PSAC had appropriate members. In the second kind of panel activity, I think PSAC has a more limited potential, although this is the kind of panel I've created.

Thus overall, I think, PSAC is of value through its panel activities, but not through its general two-day meetings once a month. I don't know that any reorganization would aid its functioning, however. Probably the only thing that would aid is for PSAC's role and function to be differently defined. Short of that, the panel activities would be greatly improved by changing the composition to reflect more fully some of the important areas of social policy: probably two economists who have some familiarity with economics of transportation, housing, health, education, welfare, would be the best additions. PSAC has, I think, a unique capability of bringing men from diverse disciplines to work on a problem involving those disciplines. But that capability has been little used, at least in domestic policy areas.

Sincerely,

James S. Coleman

JSC/vrb

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WILMINGTON, DELAWARE 19898

CENTRAL RESEARCH DEPARTMENT

Copy to:
David Z. Beckler

November 7, 1972

Dr. Edward E. David, Jr.
Science Advisor to the President
The White House
Washington, D. C. 20500

Dear Ed:

You recently asked for comments on the operation of PSAC. I hope the following will be helpful:

1. Interation of Energy Needs, Natural Resources, and Air and Water Quality Standards

I doubt that I can say anything new here that is not already well known to you and others in OST. I think, however, the magnitude of the problem is such that it might be desirable for PSAC to give extensive and continuing attention to these problems over the next few years.

2. Urban Mass Transit

I feel there will be a major technological input into radically new methods of urban mass transit, and while PSAC has been exposed to at least one briefing recently it is possible that a panel study in this area might influence an appropriation of funds for major demonstration projects.

3. Technological Exchange with Russia

I believe our new exchange agreements with Russia represent a major turning point in our relationships with the Soviet. I think that all the exchange programs should be supported with the very best scientific and technical inputs which can be brought to bear. Perhaps PSAC should have a panel or two specifically supporting some of the science and technology exchanges or some of the more technical aspects in the other exchange agreements.

4. Spokesman for Science

I will look forward to more discussion regarding Dick Garwin's point regarding PSAC as a spokesman for science. I have not really felt this reluctance that he refers to.

5. Organization

(a) I do not object to Garwin's point about the role of the Vice Chairman but do not feel very strongly about it.

(b) There have been times when I felt slightly embarrassed that PSAC business resulted in outside visitors being kept waiting for really quite long periods of time. Many of the visitors attending PSAC have gone to a great deal of pains to prepare first-class presentations and I feel a little uncomfortable that we cannot arrange our schedule to receive them promptly.

(c) On at least one occasion I have heard the comment from an outside visitor that he didn't have any idea to whom he was talking. Perhaps a list of PSAC members should be available to any visitor who wants it and perhaps we should have name tags around the table.

Sincerely yours,

Tecl

T. L. Cairns

TLC:ecl
11/7/72

Distributed 10-16-72
Dr Garwin

MEMORANDUM FOR DISCUSSION
AT THE OCTOBER PSAC MEETING

The approaching end of my PSAC term has caused me to reflect upon my experiences as a member from 1962 to 1965 and 1969 to 1972, together with my contact with the Committee since 1956 as a consultant when I was not a member. I propose that we devote several hours during the November meeting to a discussion of the role of PSAC. In discussing what PSAC should be, we will of course become involved in what it is, has been, and could be.

The discussion might be organized somewhat as follows, although the list of topics is by no means complete:

ORGANIZATION

The Vice Chairmanship. In the past, it was found desirable for PSAC to have a Vice Chairman who was not a full-time government employee and so was not under the same constraints as the Chairman. The ultimate utility of the Vice Chairman would be to carry the conclusions and recommendations of the Committee to the President even when these conflicted with expressed Administration policy. But the Vice Chairman has a more important role to fill in leading the discussions of the Committee in those cases in which the Chairman feels constrained by the privileged nature of his relationship with the President. In recent times, John Baldeschwieler has been Acting Chairman in Ed David's absence, not a Vice Chairman of the Committee.

MECHANICS

The process of clearance of new members and panel members takes so long that it interferes seriously with the work of the Committee. Furthermore, no explanation is given for the delay. What steps have been taken or could be taken to ensure that the clearance process occurs on a schedule adequate for the purposes of the Committee?

PSAC AS SPOKESMAN FOR SCIENCE

The Committee at times has expressed reluctance to speak out for science in part because it would be accused of "special pleading." On the other hand, no other organization can speak to the President or to OMB for all of science. I think that we should get straight in discussion a consensus as to our role in this regard.

QUALITY OF LIFE

The GNP and the distribution of wealth provide an indication of progress and an orientation of goal which can be fairly readily understood. For at least ten years, we have recognized the desirability of modifying the GNP indicator so that it would include quality. For instance, one could include pollution as a negative component of the GNP, so that its removal would be reflected as an increase in GNP. So long as these other aspects of product and life are unquantified, they will be ignored in comparison with those aspects which are not only "nice to have" but also reflected in numbers as in the current GNP. However, much of the "product" in the current GNP is also only "nice to have," and we ought to determine by discussion to what extent a panel activity in this area might have important impact. This can be connected, of course, to the whole question of fines and incentives for pollution, etc.

MILITARY PANELS

PSAC panels in the military area have traditionally had a dual function -- (1) to be aware of and to contribute to military R&D, and (2) to review for the President our current capabilities and the performance of the Defense Department in matters not necessarily restricted to R&D. Discussion and self-criticism on this subject should reassure us that our panels are performing this function as well as ever, or alternatively should lead to changes in our operation.

OST-PSAC RELATIONS

A panel chaired by an OST staff man and a panel chaired by a PSAC member might be alternatives for investigations in the same field. The arguments for and against the two approaches ought to be reviewed, as well as our experience in this regard.

This is only a suggestion of some items for a discussion of the future of PSAC. I hope that a fuller agenda may be agreed at the October meeting.

Richard L. Garwin
October 9, 1972

PSAC

Dr Tape

General -- PSAC serves a vital function. The government needs friendly, constructive criticism at the highest levels and PSAC serves that function in selected areas for the White House. Further, it permits the OST to function effectively without a large "career" staff.

The days when all of the OST problems could be addressed by a cadre of physical scientists seem to be gone. This change is reflected by the variety of backgrounds of the present PSAC membership. The change likewise makes more difficult the in-depth consideration of a given specific subject by the full PSAC membership; thus the greater emphasis on panels with a PSAC overview.

The size of PSAC is about right, if all members take an active role. To put it another way, there should be a nucleus of 4 to 6 members with expertise in each sub-set of PSAC problems, e.g., national security, medical research and health care, civil R&D, etc. There should be a few experts who can contribute to all sub-sets, e.g., economist, educator, etc. The size of PSAC need not be fixed but could change from time to time with need.

With regard to Vice Chairman, a non-government employee Vice Chairman can serve a most useful function. The position identifies a back-up man, provides a "civilian" focus, gives the Chairman a contact point, etc.

Program -- PSAC output is used by the system in three related but different ways.

1. Advice and counsel to the Science Adviser.

2. Advice to the White House (President, NSC, OMB, etc.) through the Science Adviser and OST staff.

3. Advice to the Departments and Agencies (DOD, NASA, AEC, HEW, DOT, etc.) through the Science Adviser and OST staff.

This is a useful pattern and should continue. The big question is to what extent should PSAC output be available to the public (Congress) and to what extent is it privileged advice to the Executive Branch. Both objectives can be served if one is alert to the differences and the objectives.

At present there is some formalistic differentiation between PSAC and OST panels. If the output of a PSAC panel always requires PSAC endorsement, it may be better to have more OST panels because of diluted PSAC expertise in certain areas. Contrarywise, it may be better to have more output for use by the public and therefore have the PSAC imprint.

Areas which have a large science and technology component fall naturally within the purview of PSAC. The classical examples are national security, space exploration and utilization, health, energy, etc. In the areas which have a small component of science and technology, it is more difficult for PSAC to make a meaningful contribution. Although the S&T component is often helpful it is rarely sufficient to the solution, since the major issues require sociological and political solutions. PSAC should "stick to its last" and beware of giving advice solely on the basis of being a group of educated individuals! If the Science Adviser needs help on such peripheral topics, let him assemble the right group under OST auspices.

Program areas for PSAC emphasis would include:

1. National security (defense and economic viability).

2. Health research including health care.

3. Energy, mass transportation, other civil needs.

4. Quality of life (environment, economic development).

5. Space.

Lesser attention would be given to program areas for which the S&T payoff opportunities are small, e.g., housing, education. This is not to say that assisting in the use of the scientific process to understand the problems, to ask the right questions, and to seek meaningful solutions shouldn't be pushed.

Suggestion

In DSB we are now trying a panel structure in which several DSB members perform an overview function in given areas, e.g., strategic, tactical, C^3I. These panels consider what areas should be examined, recommend specific task forces to be established, critique the work of task forces, report to DSB as a whole, advise the Chairman and the DDRE and his staff, etc. This may be useful for selected subjects in PSAC, e.g., national security, health, etc.

G. F. Tape
November 17, 1972

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November 8, 1972

Dr. E. E. David, Jr.
Chairman
President's Science Advisory Committee
Executive Office Building
Washington, D. C. 20506

Dear Ed:

As my second year on PSAC draws to an end, I have a variety of thoughts and reactions. I suppose my principal concern is that the meetings and the energy of the members should be more strongly directed toward specific problem areas in which the Committee might be effective. I agree generally with Dick Garwin's comments, but I believe the major change should be in the nature of the meetings.

If technology is to be directed successfully toward the improvement of the "quality of life", PSAC should be considering in depth the federal programs in transportation, drug control, EMS, and so forth. From such deliberations could evolve the outline of the scope of a possible federal program -- hopefully in advance of the time when the program is politically attractive and socially acceptable. Thus, PSAC's primary responsibility should be to apply the Science Advisor with an armamentarium of important programs, so that he can respond to non-scientific opportunities and constraints.

I believe such problems (along with the balance-of-payments and unemployment problems associated with technology) are vastly more important than such housekeeping trivia as the national medals for technological innovation and such important (but somewhat secondary for PSAC) items as the Soviet-American interchange. | Agreed!

Sincerely,

John

John G. Truxal
Dean

JGT:jc

PROPOSED PSAC MEMBERSHIP

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New Business

Northrop Corp. has a contract totaling $29 million for improved Hawk loaders and launchers from the U. S. Army Missile Command.

McDonnell Douglas Corp. will receive a $4-million contract modification for the advanced control experiment flight test program in support of the Advanced Ballistic Re-entry System (ABRES). USAF Space and Missile Systems Organization is the contracting agency.

RCA Corp. has a $14.97-million contract from the U. S. Army Safeguard System Command for advanced Ballistic Missile Defense Agency field-operated phased-array radar development and installation at Kwajalein Missile Range, Marshall Islands.

Boeing Co. has a $4.5-million contract modification from USAF for short-range attack missile (SRAM) rocket motors.

ITT Corp. will receive a $4-million contract from the U. S. Army for a super-high-frequency communications terminal and ancillary items.

USAF Research and Development Procurement Div, Wright-Patterson AFB, Ohio, is seeking proposals for a study to evaluate air defense weaknesses in combat theaters.

U. S. Army Air Mobility Research and Development Laboratory, Fort Eustis, Va., is seeking contractors to perform a 2,000-man-hour study for aircraft camouflage that will be effective during low-altitude flight.

Automation Industries, Inc., Silver Spring, Md., is being issued a $4.1-million contract modification from the Navy for engineering services associated with the Trident fleet ballistic missile.

McDonnell Douglas Corp. will receive a contract modification from the Naval Air Systems Command totaling $10 million for engineering development of the Harpoon weapons system.

General Electric has a $174.5-million Navy contract for design and installation of nuclear components in submarines.

Thiokol Chemical Corp., Bristol, Pa., has a $31.95-million contract from the Space and Missile Systems Organization for first-stage Minuteman 3 motors.

Bechtel, Inc., San Francisco, Calif., has a contract from the Government of American Samoa for design of expanded airport facilities at Pago Pago. Total costs are estimated at $5 million.

Major Weapons Cost Trends Detailed

Defense Dept.'s latest status report shows a cost growth of $17 billion in 43 major weapons systems. At the start of development, the total cost was estimated at $87 billion. This was reduced by a net $3.6 billion because of changes in programed quantities. The estimated current cost of the 43 systems was set at more than $100 billion.

General Accounting Office last week released the report, designated Selected Acquisition Report (SAR), June 30, 1972, at the request of Rep. Les Aspin (D.-Wis.). GAO now is analyzing the figures for its own report to Congress on major weapons system procurement.

Below are totals for each of the services, plus breakdowns on individual aerospace systems. GAO said that the fiscal status of the Trident submarine missile, BQQ submarine sonar and Harpoon missile programs have been classified by the Navy. Figures are in billions of dollars and have been rounded.

	Development Estimate	Cost Change Quantity	Cost Change Other	Estimate June 30, 1972
Air Force				
Total, 12 systems	**$41.5**	**($3.77)†**	**$9.82**	**$47.5**
B-1	11.22	(0.033)	(0.072)	11.1
F-15	7.35	.....	0.447	7.80
C-5A	3.41	(0.710)	1.82	4.53
F-111	5.50	(2.63)	4.12	6.99
A-7D	1.38	(0.283)	0.228	1.32
F-5E	0.31	(0.005)	(0.013)	0.297
AX	0.084	.....	.....	0.084
AWACS	2.66	.....	.....	2.66
Maverick	0.384	(0.082)	0.084	0.385
SRAM	0.237	0.126	0.964	1.33
Minuteman 2	4.25	0.004	0.647	4.90
Minuteman 3	4.67	(0.155)	1.59	6.11
Navy				
Total, 20 systems	**34.8**	**0.678**	**6.01**	**41.5**
F-14	6.17	(1.12)	0.222	5.27
A-7E	1.46	0.241	1.07	2.78
Harrier	0.503	.....	0.021	0.525
E-2C	0.586	.....	0.288	0.874
P-3C	1.29	1.03	0.165	2.49
S-3A	2.89	.....	0.261	3.15
EA-6B	0.818	0.102	0.656	1.57
Phoenix	0.536	0.015	0.562	1.11
Condor	0.441	(0.147)	0.230	0.525
Poseidon	4.57	(0.244)	0.425	4.75
Sparrow E	0.741	(0.528)	0.126	0.339
Sparrow F	0.708	0.028	0.541	1.28
Aegis	0.428	.....	0.056	0.484
VAST (target)	0.312	(0.187)	0.310	0.435
LHA (assault ship)	1.38	(0.481)	0.070	0.970
Army				
Total, 10 systems	**10.5**	**(0.554)**	**1.35**	**11.3**
Cheyenne	0.126	0.015	0.190	0.332
Dragon	0.404	(0.132)	0.212	0.485
SAM-D	5.24	.....	.....	5.24
TOW	0.727	(0.307)	0.232	0.652
Lance	0.653	0.006	0.118	0.777
Improved Hawk	0.588	(0.114)	0.284	0.758
HLH	0.119	.....	0.004	0.123
UTTAS	2.31	.....	0.038	2.34
Tacfire	0.161	0.024	0.033	0.218
Defense Communications Agency				
DSCS-2*	0.261	.....	0.015	0.276
Grand Total, 46 systems	$87.0	($3.6)	$17.2	$100.

† **Parenthesis indicates a decrease**
* **Defense Satellite Communications System**

Appendix IV-B

Other Perspectives on Health

In Chapter 5 we have looked at what linked deaths can tell us about threats to health when the linked deaths are looked at as a percentage of all deaths. We recognized that this measure did not give adequate attention to the greater seriousness of early deaths, and gave, in addition, some figures for "adjusted percentages" of deaths. We now look at linked deaths from a different aspect -- how many people "might be alive" -- which also helps to explain how these adjustments were made.

We then go on to consider long-term changes in survival, and what can be learned by comparing long-term changes in women with those in men.

As we will see in Section 2, the details of exactly what "might be alive in 1967" are mildly complex, just as are the details of "1967 expectation of life at birth." For all this, the reader who takes the "number who might be alive" as a reasonable pointer to the number who really might be alive is, we feel, being as well guided as is presently possible. (The uncertainties in "linking" almost certainly outweigh any that are added.)

1. Number who might be alive

Let us compare two quite hypothetical situations: one in which people continue to die, year after year, in the same numbers at

each age that died in 1967; another in which every death linked to a particular threat is postponed, so postponed that the distribution of continued life is the same for all linked deaths postponed from a given age as it is for all those who reached that age without dying. We next calculate the number that "might be alive", that is how many more people would be alive in the second hypothetical situation than in the first. (Details of the calculation are given in Appendix C.)

Exhibit B1 sets out the numbers that result. Let us look at the first line, that for cigarette smoking, in some detail. If we ask for those who "might be alive" at all ages, we find 4,280 thousand -- about 4 1/4 millions of people. This does NOT mean that had no one born in the last eight or nine decades smoked cigarettes, that 4 1/4 million more would be alive. It DOES mean, though, that thinking about 4 1/4 million more now alive is a reasonable way to grasp the importance of deaths linked to cigarette smoking.

Moving to the right, if we only consider those under 85, about 3,360 thousands "might be alive." Similarly about 2350 thousand under 75 and 1080 thousand (about 1 million) under 65. On the right-hand part of the exhibit we take these numbers apart, and reach, as something to give us a feeling for the impact of cigarette smoking.

1.1 million who "might be alive" under 65
1.3 million who "might be alive" between 65 and 75
1.0 million who "might be alive" between 75 and 85
0.9 million who "might be alive" above 85

"Numbers who might be alive" = difference between
(1) result of continuing all 1969 deaths in succeeding years and
(2) same except that each linked death is replaced
by the average continuation of life from that age.

Cumulative (thousands)				Linked to	Separated* (thousands)			
all ages	up to 85	up to 75	up to 65		up to 65	65 to 75	75 to 85	85 up
4280	3360	2350	1080	smoking	1100	1300	1000	900
1830	1760	1430	1120	alcohol abuse	1100	300	400	100
?	?	?	?	illicit drug abuse	?	?	?	?
125	107	68	31	air pollution	30	40	40	20
?	?	?	?	adverse reactions to medication	?	?	?	?
131	121	106	79	suicides	79	27	15	10
?	?	?	?	coffee	?	?	?	?
?	?	?	?	dust-like particles	?	?	?	?
?	?	?	?	accidents with chem'ls	?	?	?	?
15	14	12	10	toxic exposures	10	2	2	1
5.5	6.5	7.5	7.2	oral contraceptives	5.5	1	0.5	0.2

*Rounded further.

This is to be compared, for example, with the corresponding figures for alcohcol abuse.

1. 1 million who "might be alive" under 65
0. 3 million who "might be alive" between 65 and 75
0. 4 million who "might be alive" between 75 and 85
0. 1 million who "might be alive" above 85

Clearly the impact of these two threats is about the same if we only look at ages under 65. The greater impact of cigarette smoking occurs at the ages beyond 65.

Exhibit B1 allows one to gain similar impressions for the other threats considered above that are neither too uncertain nor too small to be worth such treatment.

Clearly we are talking of large numbers "who might be alive" -- something like six million for cigarette smoking and alcohol abuse, perhaps twice this number if we include both choice of diet composition and unknown chemical initators or promoters of cancer and if these two turn out to be very important. How does the impact of these big threats, which clearly far outweigh all the others we have considered, compare with the favorable impact of chemicals?

2. Adjusted percent of deaths

We can now calculate on adjusted percent of deaths -- either for all ages or for ages up to a given limit -- as:

$$\text{adjusted \% of deaths} = \frac{\text{number who might be alive (linked to given threat)}}{\text{number who might be alive (all causes)}}$$

The adjustments used in Chapter 5 were:

Adjustment A: all ages included.

Adjustment B: only ages up to 65.

3. Long term changes

We said earlier that a significant fraction of the improvement in health in this century could be credited to chemicals. What does this mean in terms of those who might, or might not, be alive?

If the 1901 death rates had continued throughout the lifetimes of those now alive, nearly 50 million people now alive would have died. If the 1968 death rates had applied instead, nearly 20 million people now dead would still be alive. The probable impact of today's large chemical threats, say 6 to 12 million who might be alive, may well not be as large as the benefit we have already had from chemicals, but it is at least a large fraction.

For those who want a little more detailed feel, we give age breakdowns for what 1901 and 1968 death rates would mean. If 1901 death rates had been in existence (with no allowance for children born of parents who would have died before having the children).

-- about 40 million people under 65 would not now be alive.

-- about 4 million people between 65 and 75 would not now be alive.

-- about 2.5 million people between 75 and 85 would not now be alive.

-- about 1 million people over 85 would not now be alive.

If the opposite had happened, if 1968 death rates had applied in the past,

-- about 10 million more people under 65 would be alive.

-- about 4 million more people between 65 and 75 would be alive.

-- about 3 million more people between 75 and 85 would be alive.

-- about 1.5 million more people over 80 would be alive.

These figures offer a more detailed feeling for what the differences in death rates -- (1) as they were in 1901, (2) as they changed through this century, (3) as they were in 1968 -- mean in terms of our present population.

4. Effects on lengths of life

Rather than think of how many might still be with us, some wish to think about what impact these threats are likely to make on ones own life. Careful calculation here is a little more complicated, so we will content ourselves with a very rough approximation, namely:

1 year of extra life for every 2 million who "might be alive"

1 month of extra life for every 160 thousand who "might be alive"

1 week of extra life for every 40 thousand who "might be alive"

1 day of extra life for every 5 thousand who "might be alive"

1 hour of extra life for every 230 who "might be alive"

1 minute of extra life for every 4 who "might be alive"

With these rules of thumb the figures of the left-most column of Exhibit B1 have a different, useful (and still more approximate) interpretation.

The conversion applies to "the average person." So far as risks due to the choice of others go, the result if roughly correct for anyone. But where it is a matter of own choice, we need to allow for how many choose. We have taken the fraction of cigarette smokers to be about 3/8 (1/2 for men, 1/4 for women). Accordingly, while removing deaths linked to cigarette smoking would give an average of two years of extra life, non-smokers would gain nothing and the average smoker would gain about 2/(3/8) = about 5 years.

Similar, but often much more extreme, adjustments would be appropriate for other self-chosen threats.

5. Females vs. males

We have noticed how much more the expectation of continued life has been improved for females as compared to that for males. Two major reasons for this are clear: More men than women smoke

cigarettes. (Indeed our estimates link about 140,000 more male deaths than female to cigarette smoking.) More men than women die from accidents, homicides and suicides. This raises such questions as: How much faster do men die? How much of this is due to these two major effects? How rapidly has the pattern changed?

Exhibit B2 sets out the ratios comparing death rates for men to those for women for various ages, as it used to be in 1901, and as it was in 1968. The ratios are the relative number of deaths among equal numbers of men and women at a given age. In 1901, males died about 1.1 times as fast as females. In 1968, males died more nearly 1.8 times as fast as females. In large measure, this came about from the removal of causes of death that affected both sexes more or less equally. In almost equally large measure this came about from the increasing importance of threats that were more important for males than for females.

As a first step in understanding the implications of Exhibit B2, we can look at the corresponding ratios when we set aside all deaths due to accidents, homicides, and suicides. The result is shown in Exhibit B3. We see that in 1901, removing all deaths due to external causes leaves men dying slower than women between 10 and 40 years of age, and, except for the first year of life, never dying more than 1.1 times as fast. In 1967, the omission of deaths from external

Exhibit B2

Relative death rate -- fraction of men dying as a multiple of fraction of women dying -- for various ages, both in 1901 and 1968.

age	ratio in 1901	ratio in 1968	age
0	1.20	1.3	0-1
5	1.03		
		1.4	5-9
10	1.10		
		1.8	10-14
15	0.98		
		2.6	15-19
20	1.06		
		2.9	20-24
25	1.04		
		2.3	25-29
30	1.04		
		1.9	30-34
35	1.12		
		1.7	35-39
40	1.13		
		1.7	40-44
45	1.17		
		1.8	45-49
50	1.15		
		2.0	50-54
55	1.13		
		2.1	55-59
60	1.14		
		2.1	60-64
65	1.14		
		2.0	65-69
70	1.10		
		1.8	70-74
75	1.10		
		1.6	75-79
80	1.10		
		1.3	80 and up
(median)	(1.10)	(1.8)	

Exhibit B3

Relative death rate -- fraction of men dying as a fraction of women dying -- when deaths from external causes (accidents, homicides, suicides, etc.) are excluded.

age	ratio in 1901	ratio in 1967	1967 further adjusted*
(0)	(1.20)	(1.30)	
1-4	1.07	1.11	
5-9	1.01	1.10	
10-14	.86	1.18	
15-19	.85	1.32	
20-24	.91	1.23	
25-29	.90	1.14	
30-34	.97	1.24	(1.13)
35-39	.98	1.31	(1.08)
40-44	1.01	1.49	(1.19)
45-49	1.04	1.65	(1.34)
50-54	1.05	1.89	(1.57)
55-59	1.08	2.02	(1.69)
60-64	1.09	2.08	(1.72)
65-69	1.09	1.94	(1.53)
70-74	1.08	1.84	(1.48)
75-79	1.10	1.56	(1.21)
80-84	1.10	1.32	(1.19)

* (These are too rough for final use.) With both deaths linked to cigarette smoking and other deaths assigned to external causes removed.

causes has reduced the first peak in the ratio -- the one falling in the late teens -- from 2.9 to 1.3. (The small peak that remains would be accounted for if about 10 percent of those dying because of external causes do so from complications, one of which is then entered on the death certificate as the cause of death.) The second peak -- the one falling in the early 60's, is not appreciably reduced.

It is natural to try to go somewhat further by excluding both deaths from external causes and deaths linked to cigarette smoking. The right-hand column (all in parenthesis) of Exhibit B3 shows that the peak in the early 60's is reduced from 2.08 to 1.72, which is by about one-third. (The fraction of reduction increases away from this peak, reading one-half in the late 40's and early 70's. This leaves us with the impression that differences in frequency of cigarette smoking accounts for a sizeable fraction of the excess death rate for men, as compared to women, but probably for less than half this excess.

What about the remainder? Some will believe that a large part of the remaining excess is due to environmental exposures of some sort or other. Others will believe that the stresses of working life are the major cause. As yet there is no clear answer.

We can say, however:

- that men die at a rate almost twice that of women between 50 and 70 years of age.

- this direction is consistent with more men smoking cigarettes,
- the estimates we have made of deaths linked to cigarette smoking are not large enough to account for the full difference.

Looking at the comparison of men and women does nothing to contradict our earlier analyses; indeed it offers a small amount of indirect support.

6. Measures of life and death

Human life terminating in death is a lengthy process, yet the world we live in changes rather rapidly. As a result, measures of health based on how and when we die tend to be somewhat less than straightforward in their interpretation, not for malicious reasons but rather because making the best use of current information is not a trivial task.

counting deaths

One thing we can do is just to count deaths according to a standard set of causes. This throws some light on the situation: a cause of 500,000 deaths a year is almost certainly more serious than one that causes only 5,000. Besides the absence of a natural reference, two considerations weaken a mere death count: First, all we can do is to postpone death -- the total number of deaths is essentially fixed by the

total number of births. Second, death of a younger person is almost universally agreed to be more serious than that of an older one.

To get around some of the difficulties, we can compare official causes in terms of the percent of all deaths. In 1967, for example, we have such results as those shown in Exhibit B4, where numbers are in one column and percents in the other.

Most readers will agree that they can get a clearer picture from the percent column than from the count column.

* expected years of life *

Expected years of life is a measure that sounds easier to understand than it is. What would probably be most meaningful would be some measure of how long an average individual born at a given date lives. If average is meant in the technical sense -- as an arithmetic mean -- we do not yet know the answer for any group born in this century, since it is not till almost all have died that we will know enough to find an average. (If we really meant "median" we know the answer for those born in the early and middle 1890's, where we cannot yet be sure of the average.) Such "cohort" figures -- quite relevant for individuals -- are of little help in watching changes in current public health. After all they combine what has happened to each cohort (at various ages) over some eight or nine decades.

Exhibit B4

Comparison of number deaths with % of deaths in 1967

(1,852,000)	(Total deaths)	(100.0%)
575,540	arteriosclerotic heart disease	31.0%
315,996	cancer (all forms)	17.0%
202,940	vascular lesions affecting central nervous system	17.0%
108,960	all accidents	5.9%
53,140	motor vehicle accidents	2.9%
27,410	cirrhosis of liver	1.5%
14,120	rheumatic fever,	0.76%
6,560	tuberculosis	0.35%
3,138	hyperplasia of prostate	0.17%
1,450	influenza	0.078%
710	infectious hepatitis	0.038%
371	accidental poisonings	0.020%
110	syphilis and sequelae	9.006%
40	whooping cough	0.002%
20	diphtheria	0.001%

As a result, most expectation of life figures refer to some brief period of time -- often one year, sometimes three years. What they tell us, for instance, is the average age of death of a composite person who spent all his or her life in the short period. If the period were January to December 1967, for example, this imaginary person would be born on 1 January and, if he or she lived to 31 December, would reappear again, 12 months earlier, at 1 January of the same year, aged exactly one year old. And so on, each year of life being lived -- or terminating in death -- in exactly the same calendar year. Clearly this measure makes it easier to watch public health from year to year, since it is calculated from observed deaths in that year and that year alone. (Events that were the underlying causes of some of these deaths happened a decade or more earlier.) Equally clearly, it is at least correspondingly harder to explain just what we are talking about. (This seems to be characteristic of measuring life and death: the more useful the measure, the harder it is to explain.)

* professional measures *

Demographers and epidemiologists need to know about deaths in greater detail than we will really need here. They are likely to use death-rates for, say, given age and sex. This means, of a hypothetical 100,000 people, all of the given sex and all having their nth -- say

their -- 58th -- birthday on 1 January of the year in question, how many will die -- or die of a given cause during that year. (In practice results are quoted for ages spread out to some reasonable degree.) There is no substitute for the use of death rates by age if we need a detailed look at what is happening. Fortunately we will need to make only limited use of death rates here.

Fortunately, also, if death rates at all ages go up, the corresponding expected years of life goes down, while if death at all ages go down, the corresponding expected years of life goes up. Thus, it is usually safe to use "live longer" as a shorthand for "all death rates coming down" and "live shorter" as a shorthand for "all death rates going up."

* impact of deaths *

We said above that the difficulty with merely counting deaths was that it took no account of at what age they occurred. There are various ways to try to take account of this. Some try to do it by assigning an "economic value" to death at a given age, often considering both what society has spent (education, etc.) and what the future return may be in the absence of death (useful work, etc.). We find none of these satisfactory for our purposes here. Our considerations are health considerations, and we resist mixing in economic ones.

Our concern with causes of death is to ask what would be the

impact if they were weakened or removed, thus postponing some or all of the related deaths. What would this mean in health terms?

The simplest -- and most optimistic way of valuing not dying at a specific age is to calculate as if, were death to be postponed at a given age, those for whom it would be postponed would live as long as the average person of that age and sex. If the cause of death that might be postponed has little connection with general healthiness -- as we would expect for accidents, homicides, and being struck by lightning, for example -- this calculation should come close to corresponding to the truth. For other causes of death it may be optimistic. But it is a well defined calculation in any event, and probably does quite well in making a relatively satisfactory allowance for the importance of death at different ages.

To value each death from a given cause in terms of the expected years of life at that age and sex and to add these values up to find a total value associated with all the deaths is numerically the same as to find an average value, here an average years of expected life for all the deaths, and multiply the number of deaths by this factor. We will often find it useful to speak and think in this latter way.

Average years of expected life for a cause of death, then, grade down from largest values for causes of early deaths to smallish values for causes of late deaths. Some examples are:

Cause of death	Average years
deaths from oral contraceptives	50
motor vehicle accidents	33
cancer of the lung	17
cardiovascular disease	11
adverse reaction to medication in hospital	5

In the last example, we have made a rough (judgement-based) correction for the fact that many deaths in hospital linked to adverse reaction to medication involve patients who were, in any case, near death. So far, this is the only case where such a judgement-based assumption seems justified.

* number who might be alive *

If our optimism were correct, and if nothing changed -- death rates and population size remaining constant -- for many years, then the number of people who would be alive if deaths associated with a cause of death were eliminated, but who would not be alive if these deaths were not eliminated would just be this product of annual number of deaths by average years of expected life at death. Accordingly, we will refer to this product as the "number of people who might be alive." We would be more concerned about the difficulties of giving a precise and relevant interpretation to this measure, and about the approximations it involves, were it not true that other measures have, to greater or lesser degree, the same difficulties. Expected years of life, for

example, as we have explained, refers to hypothetical people living all their life in one single calendar year. Indeed, careful analysis shows that the calculation of "number who might be alive" also makes assumptions about how large a fraction of less generally healthy people have died in comparison with more healthy ones.

Once we are prepared to assign an "expected years of life" to a death specified by age (and often, also, by sex) we have only the arithmetic to change when we want to use "expected years of life before age 75" -- or before any other specified age -- in its place.

* chosen measures *

The result of these considerations is thus two-fold. When, as we usually should, we want to give early deaths a higher value, we use -- and recommend the use of --

number who might be alive (before age --)

When we feel that we must use as measure tied as close to observation as we can, we use -- and recommend the use of

percent of all deaths.

science news

Fiftieth Anniversary 1922-1972

A SPECIAL REPORT

Oct. 7, 1972
vol. 102, no. 15, 225-240

Attending an open meeting of a formerly closed science committee: A reporter's view

Executive Order 11671, signed by President Nixon in June, opened to the press meetings of advisory committees to Federal agencies. Two weeks ago, Everly Driscoll, SCIENCE NEWS' *space sciences editor, traveled to Houston to attend the first meeting of the* NASA *Physical Sciences Committee held since the executive order was issued. Following is her personal report on the meeting. (Reports on the scientific matters discussed will appear in future issues.)*

by Everly Driscoll

"We'll feel our way along this morass somehow," sighed William A. Fowler, chairman of the physical sciences committee, an advisory group to NASA, at the start of its first open meeting. Seven of the 12 committee members were present, plus the usual NASA personnel, a scientist who wished to present the details of an experiment he wants to fly on Apollo 17, and one member of the press. The meeting had been announced only three days before in the Federal Register. NASA plans in the future to give a one-week to two-week notice, says one official.

On this rather humid Texas day, the committee was meeting for the first time at the Lunar Science Institute in Houston. The institute used to be the mansion of James Marion West ("Silver Dollar Jim"). It overlooks Clear Lake, adjacent to the Manned Spacecraft Center (MSC), and is the site for many of the lunar and planetary "think" sessions where scientists meet to discuss, most often in private, their latest experimental results and theories. The Italian Renaissance-styled mansion is almost clubby in decor.

Fowler began by rearranging the approved agenda to accommodate an executive session he now felt he needed because a member of the press was present. (Executive sessions are not open to the public.) It was already obvious that even without the unscheduled executive session, the committee would have a hard time completing all items on the agenda in two days. If a closed session were worked in, something would have to go. "We won't get around to the 'thrust for space research in the 1980's' [item 7 on the agenda]. We have too many problems left in the 1970's," the chairman noted. "We have to have an executive session," Fowler said to the senior NASA official present. "Does Homer Newell [associate administrator for NASA who has to approve advisory committee agendas] have to approve an executive session?" The answer was yes.

While the NASA official looked through the text of the Presidential order for the rules governing closed meetings, the chairman listed four items he wanted to discuss: the atomic clock experiment proposal, problems of the physics and astronomy program, the composition and function of the committee, and the current NASA budget problems.

"I don't understand why some of these items should be discussed in executive session," one committeeman said. Answer: "I rule these sound to me like executive session and that's that. They can fire me." (Laughter because he had already announced his plan to retire from the committee.) According to interpretation of the Presidential order by the NASA official present, the consideration of the proposed clock experiment for Apollo 17 and the role and membership of the committee could be discussed in a closed session. He left the meeting to make one of several calls to Newell in Washington.

"It appears to me we must become guard-house lawyers very quickly," quipped one scientist.

"It sounds to me that if there is no public interest in the items, we will discuss them in public; if the public is interested, we will do it in private," observed another.

"Things *are* confusing."

And that was the end of the official response of the physical sciences committee to the new ruling about open

ed science committee: A reporter's view

advisory meetings. The NASA official returned saying Washingon would call back with the approval or disapproval of a closed session. Based on this experience, I surmised that NASA would take steps to see that such "quicky" closed sessions weren't requested in the future. I had been told before my trip to Houston that no executive session was on the agenda.

During this awkward beginning, it was difficult not to sympathize with the apparent struggle of the committee. Although the committee members were aware of the Presidential order, they were not really prepared for the consequences. "This sort of changes the role of the adviser to the advisee," one scientist told me during a brief break. "We are used to arguing, debating and evaluating the NASA physical science policies without fear of misinterpretation by the public. Our opinions are given to NASA and they are either accepted or rejected. Now we will have to measure our words more carefully."

Another scientist walked up and tried rather apologetically to explain to me their dilemma. "It's not you," he explained. "You are just the first. What if you were the vice president of an industry that NASA contracts with regularly? What if you were an alumnus of one of our universities and you contributed heavily to our school, and you wanted to be sure our advice to NASA is what you wanted? . . . And some members of the press are notorious for their penchant for controversial topics. . . . "

The executive session was approved for later that day and I was asked not to attend. (It would last only two hours.) The meeting then resumed. Only one other related question came up—one that appears to bother some well-meaning scientists. A NASA official was presenting the results of an experiment done with Mariner 9. He was interrupted and asked whether the scientist who had done the work knew the results were being presented to the public before presentation in a scientific journal. The answer was yes.

The two-day meeting then proceeded smoothly. There was no apparent mincing of words. The committee was briefed on the current status of Mariner 9, NASA's hopes for a Jupiter-Saturn mission, the Viking Mars landing site committee's work, Viking instruments and how they were progressing, proposed Venus probes and possible cooperative efforts with ESRO (European Space Research Organization). The Large Space Telescope (LST), the High-Energy Astronomical Observatory (HEAO), and future solar observatories, small astronomy satellites and interplanetary monitoring platforms (IMPS) were discussed. John Naugle, associate administrator for space sciences at NASA headquarters, outlined the current cuts in NASA funding (imposed by the Office of Management and Budget in August) and the deletions and rearrangements of various programs as a result.

Committee members toured the new facilities for housing moon rocks at MSC, which they evaluated afterward as "barely adequate." (One response: "So *that* is where the moon rocks are kept!") A lengthy discussion followed about budget and personnel cuts that had affected the curatorial facilities. Recommendations were written and approved. The committee was briefed on a summer study of the combined assets of the Apollo program and proposals for the use of the Apollo returns after the last Apollo mission in December. Scientific uses of the shuttle were briefly discussed.

The meeting was informal, open, rewarding and informative to me as a space sciences writer. Except for those awkward early moments the first day, the chairman and the vice-chairman both assumed a low-key approach that encouraged uninhibited debate. "I was really surprised at how uninhibited the remarks were," remarked one NASA observer after the meetings. "Your presence didn't seem to curb them at all."

Evidently the scientists had decided business as usual. They had not staged the meetings, softened their criticisms or camouflaged their doubts. That they would go underground had been my principal fear.

National Science Board Report on Health of Science

To be present for briefing on Monday, December 18, 1:00 p.m.

Dr. Norman Hackerman
President
William Marsh Rice University

Dr. Roger W. Heyns
President
American Council on Education

Dr. H. Guyford Stever
Director
National Science Foundation

Dr. Robert W. Brainard
Staff Associate
Division of Science Resources Studies
National Science Foundation
(Executive Officer, Fifth NSB Report)

Miss Vernice Anderson
Executive Secretary
National Science Board

December 22, 1972

PREPARE BEFORE NEXT PSAC MEETING: (information for new PSAC members)

History:

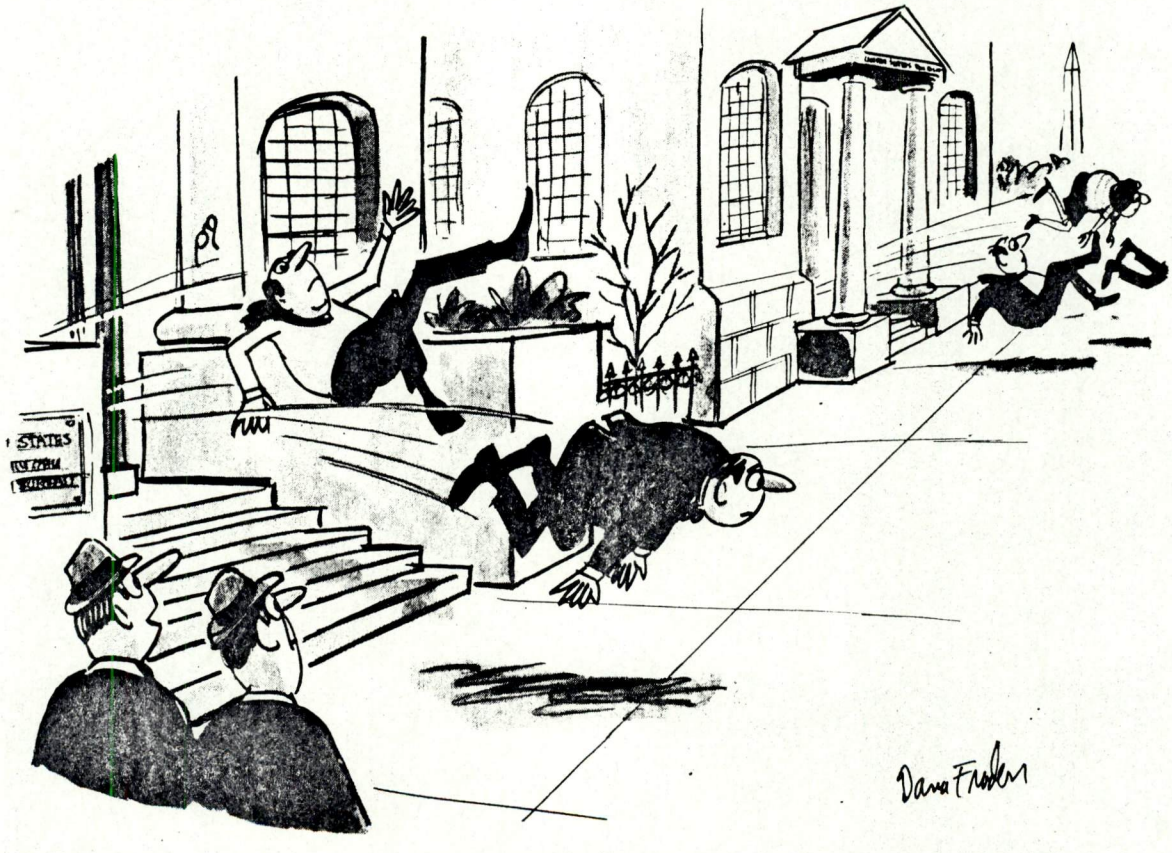
Under previous administrations; under previous chairman

Relation to:

President's Adviser, OST, Federal Council, OMB, NSF, and relationship to Defense Department.

Notes on Members:

Might just be from Who's Who with few editorial comments



"Well, it looks as if Nixon's reorganization is moving along as promised."

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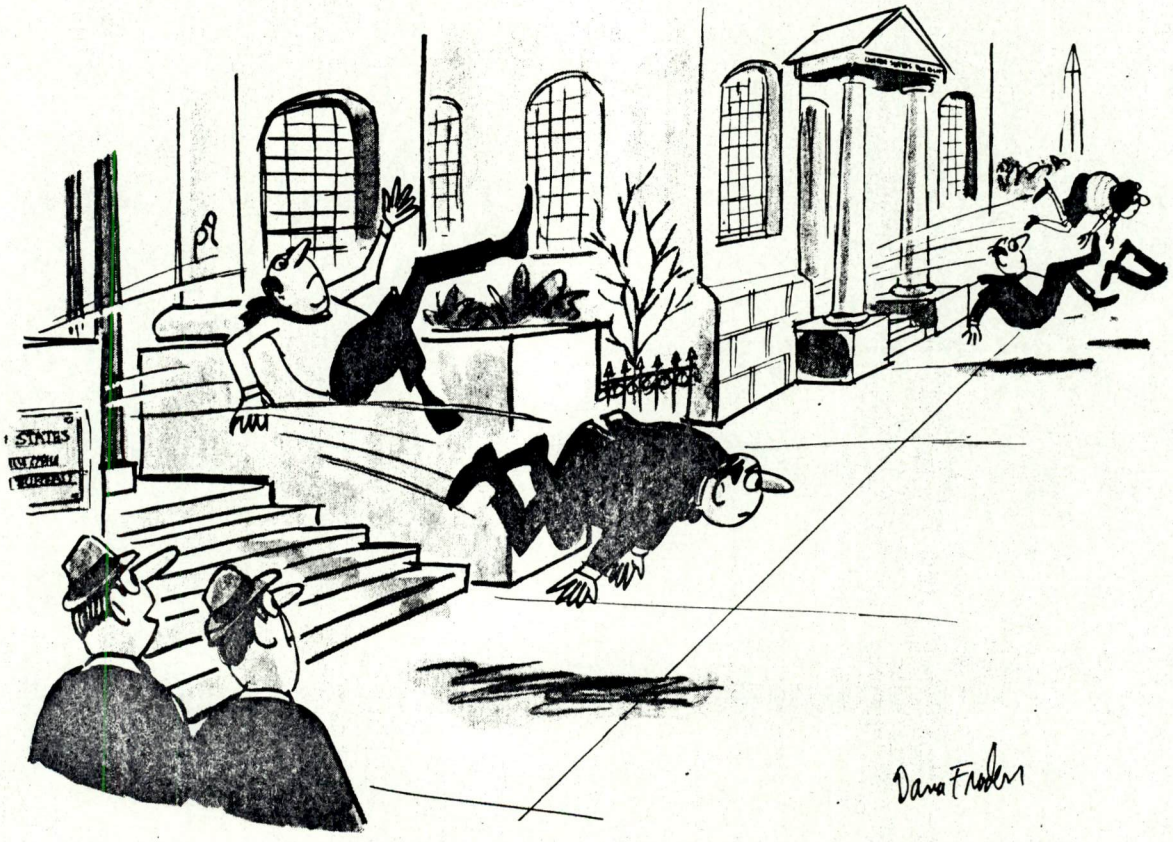

"Well, it looks as if Nixon's reorganization is moving along as promised."

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NATIONAL RESEARCH COUNCIL

NATIONAL ACADEMY OF SCIENCES NATIONAL ACADEMY OF ENGINEERING

2101 CONSTITUTION AVENUE WASHINGTON, D.C. 20418

DIVISION OF BEHAVIORAL SCIENCES

OFFICE OF THE EXECUTIVE SECRETARY

TELEPHONE (202) 961-1230 OR 1239

April 9, 1971

MEMORANDUM

TO: The Members and Consultants of the
President's Science Advisory Committee

FROM: Henry David, Executive Secretary
Division of Behavioral Sciences

You will find enclosed the draft Statement to be presented and discussed at 1:30 p.m. under Item III of the Agenda for the PSAC meeting on April 19, 1971.

This draft, it should be emphasized, has not been reviewed by the Executive Committee as a whole. The substance and organization of the Statement reflect the views developed by the Executive Committee to date, but the Recommendations presented at the close should be taken as tentative. The preparation of a final version of the Statement, including the Recommendations, will take place when the Executive Committee next meets on April 24.

D R A F T

NOT FOR QUOTATION
OR ATTRIBUTION

PROGRAM EVALUATION POLICY: A STATEMENT

Prepared for the Office of Science and Technology

by

The Executive Committee of the Division of Behavioral Sciences

National Research Council

April, 1971

EXECUTIVE COMMITTEE
DIVISION OF BEHAVIORAL SCIENCES

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Harold W. Stevenson
Robin M. Williams, Jr.
Henry David (ex officio)

I. INTRODUCTION

Based on the experience of the last few years, it would be hard to overstate the present need in social policymaking for well conceived and executed investigations that measure the effectiveness of existing programs or assess the merits of new policy ideas on a small scale before decisions are made to launch new large-scale programs. If policymakers are to have a reasonable basis for deciding whether present programs should be continued, changed, or replaced by alternative policies, evaluative results must be available showing whether the new ideas that often appear persuasive on paper will in fact work in the field.

A basic issue, then, is what steps the federal government should take--both internally and relative to the social science research communities--to increase materially the amount of policy-oriented research in social policy areas. What, for example, should federal departments and agencies as consumers of policy research do to provide incentives to the social science research communities, to engage in such research and advance the state of its art? How can the government best encourage social scientists not only to undertake soundly conceived evaluative studies directly relevant to social policy questions but also to carry out other research that will develop the new concepts and techniques that are essential to increase the capability for evaluative studies over time?

In the 1960's, social programs were expanded rapidly and reoriented toward two overlapping groups--persons who were poor or likely to fall into poverty, and minorities (primarily blacks and Indian Mexican, and Puerto Rican Americans) facing severe barriers to reasonable oppor-

tunities for income and/or various amenities, such as housing, education, and health care. The federal departments and agencies principally concerned with these disadvantaged groups--the Department of Health, Education, and Welfare, the Department of Housing and Urban Development, the Department of Labor, and the Office of Economic Opportunity--started or expanded programs in the areas of welfare and income maintenance, education, manpower, urban and rural community programs, health and housing. This extensive effort represented much needed attempts to deal with pressing social problems of the times.

The grave problems to which these meliorative efforts were directed still exist. The experiences of the 1960's, moreover, have led to widespread doubts concerning the effectiveness of most of the major social programs tried. There has been also a lessening of the confidence that programs can be expected to yield significant improvements in the lives of the disadvantaged, if they are launched full scale without prior testing. The atmosphere of confidence and enthusiasm in the early years of the war on poverty led many people to disregard the fact that they had lacked both the benefit of experience with social programs of this nature and the results of significant research pertaining directly to them. Nor had the difficulties involved in developing effective techniques for evaluating their effects been fully recognized. The experiences of the 1960's thus served to highlight the difficulty of bridging the gap between brilliantly conceived but untested ideas and the actual effects of programs based on these ideas as they work in practice.

Actually, only a few sound evaluations and even fewer rigorous small-scale projects have been undertaken as yet in the areas of social policy, and a corresponding disquiet has set in concerning the existing capacity for evaluative research, perhaps in reaction to the high hopes that accompanied the government-wide introduction of the Planning, Programming, Budgeting System in 1965. Since the initiation of PPBS, the major federal social policy agencies have made a start toward using evaluative analysis as a significant guide to policy. All these agencies have tried, with varying degrees of success, to establish a separate evaluation staff, independent of the major operating programs, whose function is to aid their heads in assessing the performance of programs. However, progress in this direction has been slow. Present limitations of staff size and skills within the government--and this is true for the Executive Office, the Congress, and the agencies alike--still restrict both the level and the quality of evaluative activities in the social program areas.

Moreover, partly because of past lack of demand for and support of such activities, the social scientists have not been strongly oriented toward social policy research. Nor have enough research organizations been developed with the capacity to undertake the kinds of large-scale activities with multidisciplinary staffs within a realistic time period that are now required for major evaluative studies. As a result there has been a shortage of interested policy-oriented researchers and a slow development of evaluative techniques and concepts.

Nevertheless, enough high-quality evaluative activity has developed to show that in some important areas (e.g., education and manpower) even

existing evaluative methods offer a powerful means for increasing materially the useful information available for social policy decisions. Recent experience also shows that future evaluative studies will be expensive and time consuming, and demand a higher level of technical competence from both government sponsors and outside researchers than was exhibited in the past.

The urgent calls for the evaluation of social policies and programs, combined with the present methodological and organizational limitations in the evaluation area, require that extensive research be done to improve evaluative capability, if the potential risks and dangers of hasty, poorly-conceived evaluations are to be avoided. What has already occurred warns that relatively good evaluative studies may be misused by social scientists, bureaucrats, or politicians for political purposes, and that a close and continuing relationship between the government agencies and the social scientists in developing policy studies could imperil the latter's independence and objectivity. Such problems and risks cannot be totally avoided, but they can be reduced if they are fully recognized and directly faced.

The potential benefits flowing from soundly developed evaluative studies are high indeed. Their results could be a key factor in diminishing the chance of failure of large-scale social policies. Consequently, a commitment by the federal government to the assessment of existing programs and to the systematic search for new ideas holds great promise for improving social programs in the future. In addition, it would contribute to a basic fund of social science knowledge.

II. SOME KEY DEFINITIONS AND DISTINCTIONS

The term "evaluation" has been used to describe many types of governmental activities ranging from brief visits by a staff person to observe how well a project is being administered to elaborate attempts to quantify benefits and costs (e.g., estimating a benefit/cost ratio for a new water project). In this broad sense, a great deal of governmental evaluation is undertaken to ascertain whether policies and programs have or are likely to reach their objectives. Thus, agencies keep program records showing, for example, categories and levels of expenditures, and numbers of participants. On the basis of such and other data, programs may be evaluated to show whether monies are properly spent, management practices are sound, program guidelines are adhered to. and the like. In preparing yearly budgets, staffs from the agencies and the Office of Management and Budget analyze information to determine if a program should be continued as is, changed, or discontinued in favor of a new one.

Past evaluation efforts have focused generally upon program inputs or efficiency, and, therefore, seek to find out primarily if a project or program is well and properly administered. Much more difficult are evaluative activities that seek to assess outcomes or effectiveness. These fall into two categories.

1. Outcome evaluations--studies designed to measure the effects of an agency's existing programs or projects on their direct participants, other

designated groups, or specific institutions, and, consequently, to determine the relationship between benefits and costs of particular programs or projects.

2. Field experiments--a type of evaluative activity in which the merits of new policy ideas with programmatic implications are assessed in terms of outcomes in a setting corresponding at least in part to actual field operating conditions.

Outcome evaluations and field experimentations--which together will be referred to as evaluative studies, research, or activities--are given special attention in this statement for two reasons. First, the test of an activity's worth in the final analysis is whether or not it is reaching its goals: Does the project benefit the people or institutions it is designed to reach? This is a question quite apart from: Is it well administered? Other kinds of evaluation are important and useful, but the fact remains that good inputs do not necessarily imply positive benefits for program or project participants. Only outcome studies can yield this type of information. Second, evaluative research is the type of social science research that is (a) most likely to have direct relevance for social policy and (b) most in need of conceptual and methodological development before sound, usable results can be obtained. For obvious reasons, the emphasis here is upon the evaluation of new programs that are intended to produce change of some sort.

III. EVALUATIVE METHODS AND PROCEDURES: THE STATE OF THE ART

Outcome evaluations and field experiments can provide information that will improve greatly the basis for decision-making. The former can be used to pose two questions: Do major programs or components of programs show positive enough results to warrant continuation at present or expanded levels? Do particular smaller segments of programs (e.g., an exemplary education program, a new curriculum being tried in a few settings) provide evidence to justify their expansion? The latter question poses the standard experimental issue of whether a new untested idea will give indication under carefully measured test conditions that it merits implementation in large numbers of projects.

1. Outcome Evaluations--Major Methodological and Procedural Issues

The major methodological and procedural issues in evaluation concern the design of an explanatory model in which the dependent--that is, the outcome--variable and independent variables are properly specified and their relationships clearly articulated; the implementation of that design in the field setting in which the project or program operates; and the determination of whether the results obtained provide a valid basis for generalization.

A. Defining Output Variables

The development of measures of outcomes is a major technical problem in part because social programs often have multiple objectives about which there may be legitimate disagreement. Of course, even where there is agreement on basic objectives, the problem remains of translating

these goals into terms amenable to measurement.

The technical difficulties in specifying and measuring outcome (dependent) variables are greatest for some of the newer programs, such as Community Action and Model Cities, that are aimed at broad social and institutional change. For example, in the Community Action Program, it is not at all clear whether an objective such as resident participation or community organization is to be viewed as a goal, a means, or as both. If the former, what is to be scored as participation: Walking through the door of a local Community Action Agency, attending a meeting, serving on a committee, being in a policy position? If participation is a means, what is the goal: A happier person, or a more productive one? It is not necessary to pose more complex questions to see why greater progress has been made to date in developing and measuring output factors. Some students of evaluation have even argued that these newer social programs with a number of broad objectives should not be evaluated both because of the difficulties of determining objectives and because of the absence of reasonable social science theories upon which to develop evaluative criteria.

Even with some of the more established programs, such as those in education or manpower, difficult problems of measurement remain. For example, both the relevance and validity of educational achievement tests, especially for the disadvantaged, are much in question. In the manpower area, the standard practice of using differences in wages just prior to and just after training as a proxy for the lifetime earnings streams of adult workers is certainly open to question. With youths who frequently

change jobs, the validity of differences in near-term wages is even more suspect.

B. Input Variables and the Use of Single Project Studies for Decisions

In the past, much evaluation effort has focused on the measurement of unique local projects. Yet such studies seldom yield information useful either for program or even project decisions, and not because a local project's performance is technically more difficult to measure than a total program outcome. What generally cannot be done is to assess the "why" of that individual performance, and this deficiency blocks generalization to other projects. A claim that the techniques used in a local project should be replicated widely must be based on evaluation data showing explicitly that success did not derive from atypical quality factors (e.g., a charismatic teacher) or from exogenous variables (e.g., the level of local economic activity).

Problems in the evaluation of a manpower training project can be used to demonstrate why it is risky to generalize from the results of a single project. First, the specification of treatment variables (e.g., translating a factor such as counseling into measurable terms) and the actual measurement of the individual contributions of these variables to project success (e.g., determining how much counseling adds to effectiveness) present methodological problems that call for innovative research. Similar statements could be made about measuring the quality of project administration and operating components (how good is counseling?), and the assessment of how well program inputs are employed or coordinated over time (the dynamic process of training).

The development of relevant social and economic data, such as unemployment rates for smaller areas and types of jobs available that might be used to measure exogenous factors affecting outcomes, while presenting difficult methodological problems, would improve evaluations. In short, present methods and concepts are seldom sufficiently developed for the specification and measurement of treatment and control variables in an individual project in a form that would permit either an assessment of the individual effect or generalization to other projects.

C. Large-Scale Surveys

The type of studies most likely to yield a basis for making valid decisions about large numbers of projects are large-scale sample surveys that provide measures of effectiveness for a total program or its major parts. For example, a study might assess the effectiveness of the Head Start program, major divisions in it such as the summer and full year components, or major elements (e.g., teacher aides) within the program or its main divisions. The difference in relevance for policymaking of an evaluation of a single project as compared to an evaluation of a total program derives from the law of large numbers. In a well-designed large sample of a total program, one can expect a wide range for the values of quality-of-input variables (e.g., good and bad administrators) and of exogenous factors (high and low unemployment rates). Under such circumstances a program statistic (e.g., a mean benefit/cost ratio) may have statistical validity even though these quality and exogenous factors are not included explicitly in the study design. In short, one may

derive the highly useful information that on the average a total program is performing at some level, or that one component helps participants more than another. Of course, the variability of results from one local program to another is also relevant and useful information.

The critical question is whether evaluation methodology can be improved in the future so as to provide far more detailed information about programs or projects for use in policymaking. It is reasonable to assume that over the longer-run attempts to develop individual project data so as to determine exemplary projects will be useful. This is particularly true for efforts aimed at detecting new program approaches that appear promising and may warrant testing in field experiments. The most promising near-term evaluation approaches are those that will rely on surveys of a large number of projects, but which will attempt more than in the past either to rate individual projects in comparative terms or to assess smaller elements of a program. For example, one could compare On-the-Job Training (OJT) projects having prevocational training and coaching follow-up with sets of other OJT variations. The characteristics of the components in the sets might include a quantity dimension (e.g., a full-time coach for no more than twenty participants or at least two weeks of prevocational training), but not the quality of each input or the detailed nature of the process being used. All projects in the same set would have the same general and easily determined descriptive characteristics, but would surely vary widely as to details (e.g., the specifics of the coaching process). Data showing that one OJT variant had a higher benefit-cost ratio than others would certainly

prove beneficial in deciding on major changes. They would also add to a developing body of theory about why things happen.

D. Field Procedure Issues

In the development of evaluations, problems of field procedure range from those typically found in field sample surveys (e.g., not finding interviewees or losing them, particularly in longitudinal studies) to those arising from disagreements or even conflicts between evaluators and project operators. The potentiality for conflict may be appreciated by considering the statistical design requirement of making a random assignment of project participants among different treatment groups and a control group. Given a larger number of applicants than of positions, random assignment is feasible in theory. It is not clear, however, that professionally trained project personnel are anxious to allow a table of random number to supersede their functions. Nor should researchers be surprised by this attitude. After all, it is part of the professional's values that a trained interviewer should be able to pick the persons who need the program most (or will benefit most) from the range of eligible program participants interviewed.

Moreover, project operators may not wish to implement specified procedures, or may not be willing to leave a project unchanged, if they believe that new modifications will benefit participants. Thus, for existing programs, these administrative problems would seem to indicate (a) that program and project operators may not want to allow evaluators to modify projects in terms of participant selection, treatment variation, etc.; and (b) it remains questionable that participant selection

procedures and design modifications may not be implemented properly or carried through for a sufficiently long time to permit a meaningful evaluation, even if such modifications are allowed. These problems intensify the task of evaluating individual projects, but do not complicate greatly large scale surveys in which the primary activity is the interviewing of participants, not the detailed investigation of project or program procedures.

Cooperation between project directors and evaluators is crucial. The success of the program and of the evaluation are both at stake, and so also are the reputations of the individuals involved. Able administrators and researchers expect to have their work scrutinized and to risk their reputations, but will insist that they be given a fair chance to show what they can do. Ideally, coordination and cooperation between them should begin before the program or project or pilot study or experiment starts. And there should be clear agreement on working arrangements, on the extent to which the evaluator will provide a continuing flow of information to the project director, on the basis of which he may alter the program to overcoming bottlenecks or improving effectiveness.

An optimal arrangement would be one with a free flow of information to a project director who is free to make whatever changes are needed. At the same time and from the same data, the evaluator would be assembling measurements that could be used to evaluate the total outcome, compare it with other programs, and develop policies for future or expanded programs. Such arrangements would increase the variability

among individual, local programs, since they would be affected by the quality both of the direction and of the flow of monitoring information. Tightly controlled experiments would not be possible under such arrangements, but they are, in any case, often impossible in field situations.

The policy questions are clear: To what extent should the desire to keep the evaluator and the evaluation independent, unbiased, and precise, lead to prohibiting any feedback or advice to the project director that might help improve the project and increase its chances for success? To what extent should the project director be restricted in the way he runs a project in the interest of experimental designs or measurements unaffected by the information available to the evaluator? *

E. The Usefulness of Present Evaluation Techniques

Evaluations employing present techniques of large-scale sample surveys may in the near-term increase materially the quantity of useful information available to the policy-maker. However, there must be a far greater concern for the requirements of statistical and conceptual design than has generally been exhibited in the past. For example, in a study comparing earnings before and after training, these requirements will generally include a well-designed sample, early field interviewing to maximize the retrieval of information, repeated follow-up to reduce sample attrition, and a reduction in the importance of heroic assumptions in the model. In short, good individual outcome evaluation studies

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* See Walter Williams, "Developing an Evaluation Strategy for a Local Action Agency," Journal of Human Resources 4 (Fall, 1969) 451-465, especially p. 459 on the conflict between valid statistical design and changes to improve program effectiveness.

will require well-qualified researchers with sufficient financial support so that they are not compelled to adopt excessive shortcuts and are assured realistic planning time for the development of a sound evaluation model.

Even under such circumstances, it will be necessary to make arbitrary decisions about evaluation design and measurement and to recognize that many crucial questions cannot be answered with present capabilities. For example, in a national sample of manpower training programs, whatever the conceptual problems, one may simple accept--agree on the rule--that earnings for the six months or one year just prior to and just after training will be the proxy for lifetime earnings. Thus, some caution is needed in interpreting outcome evaluation data which generally will mean comparing the evaluation evidence with other reasonable evidence to "validate" a decision. At the present stage of development, outcome evaluations generally should be viewed as one of several pieces of information. They should not be regarded as constituting definitive evidence that would cause one to rule out any reasonable indications of a different policy decision.

While such caveats are needed, they should not be allowed to obscure the fact that good program outcome evaluations using present techniques are likely to produce a signal improvement in data available for policymaking. It is now possible for decision-makers in social areas to have empirical evidence indicating that certain programs or major components perform relatively well or poorly as a basis for analyzing available policy alternatives.

2. Field Experiments

Social field experiments offer social scientists both great opportunities and great challenges. Through experimentation social scientists will be able to deal with major social problems by addressing the theoretical issues they raise and thus expand the frontiers of knowledge. Yet, it well may be that field experimentation, on three counts, is the most difficult form of social science research. First, conceptual and theoretical problems are particularly formidable. For example, in order to increase classroom educational achievement, it might be necessary to develop a complex model hypothesizing about the factors affecting such achievement. Second, the researcher must encounter the many methodolofical problems of evaluating individual projects discussed earlier that defy solution through existing methodological capabilities. Third, field procedure problems are many and difficult. For example, the researcher probably must deal with people with all kinds of problems, attempt to hold constant over time a complex treatment process, and operate under various community hinderances over which he will at best have limited control.

What are the necessary (if not sufficient) conditions for a field experiment to have a reasonably high probability of producing outcome evidence indicating potential programmatic success? There are at a minimum four: (a) a clearly defined model and set of specified treatment variables, implemented in the field to meet these specifications; (b) a design sufficiently general so that the final results are likely to have broad application; (c) a data retrieval system likely to produce the statistically valid data required to measure significant interrelation-

ships among critical variables and/or the project's effectiveness; and (d) either a design in a single project with sufficient diversity in treatments, so as to allow meaningful comparisons with feasible alternatives, or a broader experimental design presenting a set of treatment alternatives (about which data may or may not be available), into which the proposed field experiment can be fitted.

The first condition in effect states that a treatment idea such as a new teaching method should be specified clearly, translated into terms enabling the teacher to use it, and implemented in such a way that the teacher does in fact carry out what is specified in the design.

The second requirement states that the results from an experimental project should have general applicability. This cannot result if a project has a unique teacher, a unique group of students, and a unique setting. The need in social policy is for approaches that can be adopted widely so as to yield benefits to large numbers of people. This means that what is being sought are new ways of doing things that can be used by ordinary people in ordinary circumstances. Only very infrequently, if at all, can the generality of a project be demonstrated by a single case. Successful single projects will undoubtedly need to be repeated in sufficient number to allow statistical generalization.

The essence of a field experiment is the generating of empirical evidence from statistically valid procedures that will permit a decision to be made on whether the project does or does not work in terms of producing positive outcomes for participants and for others. This is a crucial point. It is seldom sufficient to <u>demonstrate</u> merely that

an institutional or procedural change can be made in the sense that it is politically or administratively feasible. Change is a neutral concept, and the effects of change must be measured in terms of outcomes. Hence, there is seldom an escape from the requirements for statistical evidence.

The fourth requirement sets out a need for a planned and systematic design, so that a set of projects yields a network of useful and comparable evidence. Planned field testing by definition requires the planning of a diversity of projects in order to have comparability of results across a set of related projects.

The improvement of social field experiments will require much basic research over an extended period of time to provide the necessary conceptual models and methodologies. Inadequacies in research staff in federal agencies may, as will be seen shortly, inhibit development of either field experiments or basic research. At the same time, it is important to recognize that in some areas of social policy field experimentation is now possible--witness the extensive work in income maintenance starting with the New Jersey Negative Income Tax experiment. Moreover, both the basic research that is needed and the conduct of field experiments will not only increase policy information but also expand the frontiers of knowledge in the social sciences. The social problems with which public policies attempt to deal--for example, how diverse people are to live together in harmony, or how all children can be assured a decent education--also present major theoretical problems to researchers in the social sciences.

IV. DEVELOPING EVALUATIVE STUDIES: PROBLEMS AND DANGERS

1. Deficient Government Staff Capability and Organization

Shortages of qualified staffs within the federal government have been a major factor limiting the development of sound evaluations and social field experiments. The agencies directly responsible for evaluative activities need well-trained staffs with sufficient technical and administrative capability to (a) determine evaluative requirements, (b) define clearly the types of needed studies and the unanswered questions that block policymaking, (c) design or work with contractors and grantees to design studies and methodologies, and (d) supervise closely the ongoing evaluative effort. Higher level government offices not responsible directly for carrying out evaluations need staffs able to ascertain evaluation requirements and monitor evaluative activity in sufficient detail to assess the validity of the results and interpret their implications for policymaking.

The skills and knowledge required for an evaluation staff member to fulfill these functions are high: (a) substantive knowledge about specialized areas (e.g., education), including the ability to specify evaluative needs; (b) a sound background in designing evaluative studies and using statistical techniques, including the ability to translate variables into measurable concepts usable in the field; and (c) the administrative ability towork with program personnel and researchers over time. Such a set of skills in the social program areas is in short supply both within and outside the federal government. Yet, in every agency the number of people needed for a viable evaluation staff will generally be substantial.

In the last few years, the social policy agencies, the General Accounting Office, and the Executive Office have begun to develop evaluative capability. Such studies as the New Jersey Negative Income Tax experiment, the performance contract experiment developed at the Office of Economic Opportunity, and the OEO-Department of Labor longitudinal evaluation of five manpower programs clearly indicate competence. Furthermore, given the difficulties of assessing social programs and projects, the progress to date in developing evaluative capability is encouraging. A concern with the future of evaluation activities makes it clear that a significant increase is required in staff size and skills within the government--in the Executive Office, the Congress, and the agencies. This is essential to develop the high level of evaluative activities needed to improve the basis for decision-making in social program areas.

What is also needed is a more appropriate mode of organization within the government for obtaining evaluative information. Thus, it must be ascertained in organizational terms whether major program evaluations should be performed by the programs' own staff, a separate evaluation office responsible to the agency head, an Executive Office staff, a Congressional staff such as the General Accounting Office, or a combination of these staffs. Further, in cases where higher levels of government do not carry out evaluations, the organizational issue to be resolved is the way they are to be related to the staff directly responsible for the evaluation so as to insure that the kinds of questions they wish considered are in fact addressed.

Three criteria--relative objectivity, evaluative capability, and the capacity to implement the findings--are important in determining where responsibility for performing evaluations is to be located. For obvious reasons it is clear that those who run programs or projects should not be solely responsible for measuring their own effectiveness. Not only is direct involvement in operations likely to bias assessments, but is is also true that those who use the evaluative information supplied by the program are likely to suspect it to be biased, even when it is not. Since present evaluative methods do not preclude the possibility of biased judgments, the question of how a relatively objective office institutionally responsible for program measurement, and not program defense, can be assured is an important one.

Determining who is objective, however, is not a simple task. For example, in an agency, the agency head or a separate evaluation office reporting to him would appear to be relatively objective with regard to major program decisions than those who operate programs. Yet, viewed from the perspective of the Congress and Executive level, the agency head in many respects is the chief program operator in the agency. The evaluative data that he might use to alter his programs might just as well be used by the Congress or the Office of Management and Budget to cut back agency funds or to question his competence. Both within and outside government there are always many interested parties. Presidents and Congressmen have favored programs and policies which well may bear their name, and beyond the President and Congress are groups which may exert pressure to avoid a potentially dangerous evaluation. The reasonable

traditional areas such as monetary and fiscal policy, not social policy) and psychologists tend to be viewed as second-class citizens by their peers in most of the other disciplines. The recent survey of the social and behavioral sciences, sponsored by the National Academy of Sciences and the Social Science Research Council observes:

> Although there is a close relationship, in principle, between basic research and applied and developmental work, basic research tends to receive more attention from behavioral scientists in universities. Many academic scientists value the prestige that their contributions to basic research and theory give them in the eyes of their peers more than whatever rewards might be obtained from clients who would find their work useful . . . . Thus, much of the applied work in disciplinary departments is done by those who for one reason or another do not compete for the highest prizes of their disciplines. *

Social problems cut across the established disciplines. An effort to investigate the means for enhancing the capacity of the public schools to educate minority children may well require research not only by sociologists, psychologists, economists, and linguists but also biological scientists in such areas as nutrition and brain functioning. But collaboration by members of different social science disciplines--much less joint research with biological and physical scientists--is exceptional rather than common, and this is particularly true for university researchers.

Finally, the social sciences in general have only a few organizations with the capacity to perform large-scale field research. Yet it seems clear that the need for major program evaluations and field experi-

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* The Behavioral and Social Sciences: Outlook and Needs, National Academy of Sciences, Washington, D. C., 1969, p. 193.

ments that generally will require replication will usher in an era of "big social science." The first signs are already apparent. The OEO-Department of Labor longitudinal evaluation of five manpower programs has a sample of over 10,000 people and its estimated cost if $4.5 million; OEO's performance contracting experiment is expected to cost $6.5 million; and the cost for the several negative income tax experiments now in progress will run into the tens of millions of dollars.

Many more studies of this scale will be required if social policy makers are to have sound evaluative data indicating whether or not present programs are effective and whether alternative policies are likely to prove successful. Such extensive evaluative research will require major social science organizations with large multidisciplinary staffs, a high level of administrative capability, elaborate divisions of labor, and hierarchies of authority and status.

3. The Time Dimensions of Decision-making and Evaluative Studies

The time exigencies of decision-making on policies and programs and the time requirements of sound evaluative studies are at odds and represent a source of potential conflict. Haste is not compatible with the present state of the art in evaluative studies and their administration. If present techniques are used, it will take significant amounts of effort, time, and money to produce evaluative studies that are substantially better than those of the past. Take, for example, this conservative time estimate for an evaluation performed extramurally under contract to measure prospectively a manpower training program of six months duration. Two or three months are required to get bids and

award the contract; for the contractor to develop the evaluation methodology and sample will take two to six months; the manpower training program itself will run for six months; six to twelve months of on-the-job time by participants after the training (depending on whether or not one accepts six months or one year wage experience) will be needed; and then two to six months will be required to process the data and prepare a report. This estimate gives a time range of from one and a half to nearly three years from the start of an evaluation until results come in. Consequently, the minimum data for decision-making would be available not for the upcoming fiscal year but for the one after that.

Moreover, for some areas present evaluative methods and concepts are so limited that one cannot envision studies that will feed directly into the decision process but only activities that will lead in succeeding stages to a decision-making input. Such exploratory activity is expected over time to increase the capacity to produce significantly better future outcome data, but the payoff in terms of results directly relevant for decision-making may be many years away.

It is difficult for key officials to accept the notion of a long-time horizon for evaluative activities emphasizing exploratory work that does not lead directly to inputs into the decision process. The pressures on them to act quickly are tremendous. Yet, if decision-makers do not cultivate and adopt realistic and sympathetic attitudes toward the time required for competent evaluative research, it is difficult to see how real progress can be made toward providing them with the kind of information they sorely need.

4. Risks Associated with Evaluative Research

The process of evaluation itself may be disruptive for program personnel and participants, and bring conflict in an agency between evaluation staffs and operating bureaus. Moreover, evaluative data--just as any other type of information--can be wrong, misleading, misused, or unused. For example, an outcome evaluation indicating incorrectly that a program is not effective can bring reductions in program funding, unwarranted changes in staff and policies, and a shattering of the morale of staff and participants. Or, in the most general terms, the argument that every new operating program ought to be tested and shown to be effective before operating on a large scale can be used to disparage new ideas and retrench on commitments.

Legislators and administrators may overvalue and hence overreact to quantitative data because of their appearance of scientific accuracy. Scientists may carry the implications of data beyond legitimate limits when pressed to answer more questions than the research allows. Personal values or political considerations can then easily intrude.

Conversely, the undertaking of evaluative research may have harmful consequences for individual scientists and the institutions of science. The context in which information is used is very important, the same data cited in a scholarly journal and on the floor of the legislature have very different implications. Thus, any information, including evaluative data, which can have a material effect on policy decisions (e.g., bring significant cuts or increases in program funding) is best viewed in this context as "political" information. That information

carrying this kind of "charge" can bring the researcher into the center of a raging controversy is well illustrated by the Westinghouse Learning Corporation evaluation of Head Start. Here the debate over the validity of the results was carried on not only in the scholarly journals but also in major newspapers, the Executive Office, and the Congress. One examination of the Westinghouse study concludes that

> [A]s a general rule the absolute methodological and logistical deficiencies in any evaluation make political infighting a near certainty when evaluation results threaten a popular program. In short, "questionable evaluation practices" can always be attacked on methodological grounds for political and bureaucratic purposes. *

Furthermore, the institutional relationship between the government sponsor and researcher may threaten academic independence and objectivity. Sponsors of evaluative studies may attempt to suppress unfavorable findings, to tell an investigator what to find or to change results, or to force the release of preliminary results in support of a particular policy position. Even without overt influence, a very close and continuing relationship between an agency and a research organization may either put in doubt the latter's objectivity (even when there is no basis for it) or blunt its sensitivity to bad policies of the client. It is difficult to maintain over long periods of time mutual trust and the capacity to ask embarrassing questions that might put in jeopardy basic programs of the agency.

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* W. Williams, Social Policy Research and Evaluation: The Experience in the Federal Agencies, American Elsevier, forthcoming.

Finally, special social policy research institutes whose main sources of income are derived from policy studies (particularly if they are funded by a single or a small number of agencies) run particularly heavy risks of being susceptible to government influence. They may come to a point at which servicing the client means telling him what he wants to hear, or helping the sponsor search for evidence to support his upcoming budget request.

There are also dangers that whole programs will be cancelled or not started on the basis of evaluation studies which show that a majority of current projects or test projects are not working, when in fact the evaluation studies also show the potential for very successful programs, if the procedures of the few successful ones were used everywhere. Here again, the range of purposes of evaluation--from strict assessment through finding what succeeds and why, to continuous monitoring and advice used to adjust a program in motion--must be kept in mind. A major government policy decision that must be made is the mix of purposes of evaluation.

5. Protection of the Rights of Individuals

Most government programs affect individuals, and most evaluations require assessing the impact of programs on individuals. Whether the outcome measurement is of changed skills, knowledge, condition, or behavior of individuals, some of the information is sensitive and considered generally to be private. The government has a right to know, of course, but the individuals have their rights to privacy as well. The problem becomes particularly acute when other government agencies want

to use the information for law enforcement purposes, or to locate dramatic cases for political purposes.

As a matter of professional ethics, the evaluator will want to promise those from whom he elicits information that their individual responses will not be revealed, or used for purposes other than the evaluation of the program. If he cannot make this promise, he is unlikely to be willing to collect the information, and respondents are unlikely to be willing to give it.

V. FUTURE DIRECTIONS

The present situation concerning evaluative studies may be summarized as follows: The federal government greatly needs sound evaluative results, indicating whether social programs and policies are reaching their goals and whether alternative means might work better, as an important input to the policy process. At the same time, the need to improve present evaluative methods and procedures, the shortages of competent evaluators, and the organizational deficiencies both within and without the government present a number of problems in developing and using evaluative results.

In light of this situation, the overriding issue concerns the steps that the federal government should take both internally and externally with respect to the social science research community in order to encourage materially the development of soundly conceived and executed evaluative studies and to reduce the potential dangers attendant upon such development. These steps include the improvement of the manning of the government internally so as to establish stronger evalua-

tive capabilities; and the development of better government policy and funding procedures aimed at increasing the organizational capacity to undertake, monitor, and use evaluative studies.

1. Strengthening Government's Evaluation Capability

Social policy agencies--the primary developers and users of social program evaluative results within the government--must establish large, well-trained staffs with sufficient technical and administrative skills to (a) determine evaluative needs; (b) articulate these needs to outside researchers; (c) design or work with contractors and grantees to design studies and methodologies; (d) supervise the ongoing evaluative effort; and (e) use evaluative results so as to improve policy-making in the agencies.

Only if these agencies take seriously the evaluation of existing programs and the systematic search for new ideas through field experiments is it likely that evaluation results will affect both agency level decisions and their implementation at the operating levels of the agency. Such a formulation implies a significant evaluative capability in the operating programs--evaluation all along the line, even though in many cases at lower levels it may focus primarily on program inputs. Furthermore, major bureaus may establish central evaluation staffs that are independent of the bureau's operating elements to give a more independent assessment of the operations.

However, given both the program operator's normal propensity to be an advocate for his program and the possibilities that present evaluative techniques may allow a biasing of results, the agency head needs

to be able to turn to a separate evaluation staff or agency to develop evaluative studies in support of major decisions. Only an autonomous group or a high level agency office independent of the operating programs can institutionalize a relative degree of objectivity, in that it can be responsible to the agency head for program-measurement, not program-advocacy.

Moreover, the great complexity of evaluative research means that the major social policy agencies are going to require large and talented staffs. For example, it has been estimated that at least a GS-13 to GS-15 level (and perhaps higher) staff member is required for every two to four (or $500,000 worth of) outside studies, with additional staff needed for special functions such as developing overall evaluative needs. * Moreover, the government policy research staff must be made up of people with sufficient technical training and/or experience to interact with academic or other non-government social scientists in a peer relationship. One party may have a comparative advantage in terms of techniques and disciplinary knowledge, and the other in terms of knowledge about policy and policy needs, but between them they must have all the necessary competencies, and they must communicate well.

The Congress and the Executive Office also need evaluative data, and face problems of lack of objectivity relative to an agency head similar to those that the latter encounters with program operators.

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* J. S. Wholey and others, Federal Evaluation Policy, Urban Institute, Washington, D. C., 1970, pp. 82-85.

Under such circumstances the Congress and the Executive Office at a minimum must have the staff capability to articulate their evaluative concerns to the agencies and to be intelligent interpreters and users of evaluative information. Beyond this, both may wish to acquire sufficient staff to carry out a limited number of evaluations on their own.

A strong case can be made for some duplication of effort in the evaluation areas with operating programs, independent agency staffs, and the Congress or Executive Office involved in the same or closely related evaluations. In fact, in the next stage of improved but still imperfect evaluation efforts, one can envision a milieu of advocacy in which the main parties at interest develop evaluative capabilities both to carry out their decision-making and operating responsibilities and to protect their interests. To the extent that these activities produce checks on evaluative efforts and an intensification of the search for better techniques, rather than more forceful political arguments, it is much to be desired.

The limiting factor here--and it well may be an overriding one--is the shortage of competent evaluators. The present situation is analogous to the one existing at the start of the Planning, Programming, Budgeting System government-wide in 1965, where the severe shortage of policy analysts thwarted the implementation of a basically sound concept for improving the governmental decision process. In the present situation there is the danger that the demand upon the limited supply of competent people will be so heavy that agencies will not be able to

secure staffs of sufficient size and skill to carry on a high level of evaluative activity.

If the federal government is to increase significantly the flow of sound evaluative results, it must fund relatively more research, including evaluative studies, directed specifically toward major problems in the social areas. At the project level these studies should be amply funded so as to avoid excessive shortcuts, and permit, as has been noted, a realistically long time schedule for careful planning, design, and execution. Finally, the development of this research should involve far more interaction between government policy research staffs and outside researchers.

It is important to stress that what will be required is not only "applied" work but also "basic" research, and the structuring of the research both in terms of the areas of concern and of the interaction with government staffs. For example, the adequate education of lower socio-economic class, minority children is clearly a major social problem that urgently needs investigation. In the search for causes of poor education and means of improving it, research might range from studies of the possible relationship between malnutrition and brain damage in a fetus to field experiments testing a new teaching process. Not only may policy-oriented research include "basic" research, but it also seems likely that in many social areas, major new applications must await the development of new knowledge from fundamental research.

It should be recognized that all policy research will be structured in the sense that government policy research staffs will specify

gaps in knowledge blocking more intelligent policy making, and that the researcher will be committed to thinking about these needs. This commitment by the researcher to policy concerns, including interaction with a government policy research staff, is extremely important. The need for interaction rests on the premise that the lack of useful social policy research in the past stemmed in part from ignorance about programs and policies, policy needs, and the form in which research results would prove useful in the policy process. What the government policy research staff should have relative to the outside researcher is better knowledge about policy needs. Research is likely to be more pertinent to policy when performed by a researcher with a sound appreciation of policy issues and needs. This holds even for the most basic research by social scientists in which the researcher has great freedom to determine the scope and character (including timing) of the research; here also the value of the study for policy is likely to be greater when the researcher has an appreciation of policy needs gained through interaction with government staff members.

These observations may raise in the mind of social scientists a specter of government staff members with a new shibboleth, "policy relevance," dictating to scholars the "what" and "how" of their investigations. This is not a wholly imaginary danger. But if government policy research staffs are strengthened and properly understand their function, the interaction may for the first time provide researchers with knowledge about policy needs in sufficient detail to encourage fruitful policy research.

In this regard, one other point needs to be made. There has been significant interaction between researchers and government research staffs which are not informed about or oriented toward program and policy concerns. The proposed changes then indicate a shift in the government's general effort to stimulate research toward the cultivation of a greater level of social policy concern among social science researchers.

In the development of government staffs and the social science community in terms of carrying out evaluative studies, the shift toward more policy-oriented research should be relatively small and gradual. The great bulk of social research should continue to be guided by the same concerns as in the past, with scientists performing research that in the long run may facilitate policy but is not framed with policy concerns in mind. At the present time, it would not be efficient or productive to attempt a rapid large shift in allocation of research funds toward explicit policy questions. The federal government still lacks the technical capacity to use vast sums in search of policy information. Money is a critical factor in stimulating more policy research; but it is not a surrogate for technical capacity. The time that might be concentrated on developing a few sound projects with a high potential for producing policy results may instead be widely distributed over many projects of dubious quality and relevance in order to expend all funds.

For this reason, it is not appropriate now to attempt to specify desirable absolute or relative levels of expenditure on policy-oriented research, or the time path of such increases. A rapid build-up of

policy research staffs is first needed, together with a firm commitment to much more policy-oriented research, particularly to evaluative studies, "validated" by immediate (but still relatively small) funding increases for policy studies, perhaps even at the expense of other research. In short, evidence of staff capability should precede major increases in funding of research. The relatively higher level of investment in policy research--although still small in absolute terms--and concomitantly the more extensive interaction between government policy staffs and outside researchers could have tremendous implications in beginning a movement toward a significant social science research contribution to social policy.

2. Increasing the Organizational Capacity for Policy Research

The types and levels of research on social problems required to facilitate social policy-making points toward the need for more special organizations--e.g., non-profit and for profit research organizations, institutes, academic departments or schools--with an explicit mission of large-scale, multidisciplinary research and/or teaching in the social policy areas. The establishment of large-scale special social policy organizations has recently been recommended by a number of groups and individuals, including a special commission of the National Science Foundation and the National Academy of Sciences-Social Science Research Council survey of the behavioral and social sciences. *

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* Knowledge Into Action: Improving the Nation's Use of the Social Sciences, National Science Foundation, Washington, D. C., 1969; and The Behavioral and Social Sciences: Outlook and Needs, National Academy of Sciences, Washington, D. C., 1969.

The first proposed the creation of "social policy research institutes" and the second recommended the establishment of new "graduate schools of applied behavioral science." The NAS-SSRC proposal suggests that the new organizations be a part of the university and have a regular teaching function. The NSF proposal leaves the location issue open, neither requiring that the institutes be at universities or rejecting that location, but stressing a close relationship with the agencies. It is the question of location--in or outside of universities and where in the university--that seems to be the most controversial one. Nevertheless, both the NSF and the NAS-SSRC recommendations explicitly and strongly urge that the new organizations should be financially and administratively independent of the established social science departments.

The difficulties of performing policy research in an academic setting and the potential conflict of large-scale, policy research with other university functions have led some to recommend that policy research organizations be separate from the universities. This theme has been particularly strong in physics where some have gone so far as to recommend that, with or without collaboration with established universities, applied laboratories should be given the task of educating the next generation of applied scientists. That large-scale, multidisciplinary policy research and the education of "applied" scientists should not be university functions is hardly an agreed upon point even in the physical sciences. In the social sciences with far more limited experience and debate, it would be premature to suggest a divorcing of policy research and teaching from the universities.

However, a distinction can be made as to types of research that may lead to a significant difference in functions between universities and the non-university research organizations, including in this latter group institutes located on campuses but having mainly non-academic professional staff. For purposes of analysis, a distinction may be made between research studies in which the results are expected to have a direct bearing upon major agency policy decisions (e.g., an outcome evaluation of Head Start) and those in which the results are expected to have an impact on decisions only after additional research or testing (e.g., a tightly controlled laboratory experiment in early childhood learning).

As compared to universities, non-university research organizations generally seem better able in an institutional sense to perform large-scale research, the results of which are expected to have a direct effect on decisions. Over the near term--say, five years--non-university policy research organizations, far more than universities, should be capable of rewarding, both in money and status, direct policy work. Moreover, they should find it easier to take the political "heat" from direct decision studies as a part of doing business, and to mass the key substantive area, administrative, and field procedure experts needed to mount a concerned effort. These organizations do frequently face the problem of finding top flight scientists, but here the selective use of university people will often supplement the operation.

At least in the near term, these institutions outside of or on the periphery of the university, drawing on individual faculty members

on occasion, seem the likely candidates for expanding the supply of direct decision studies both significantly and relatively quickly. Nor should this comparative advantage for direct decision studies rule out more fundamental research. Not only may direct decision studies give the institutions great insight into more basic problems but they may also be able to attract competent researchers only if some basic work is performed.

The universities, however, probably should have the major role in the more fundamental policy-oriented research. Here the articulation of needs supported by a compelling intellectual rationale and incentive by the federal government will be critical. Basic research on social problems fits well with the reward structure of social and behavioral scientists. The point is that these more basic studies offer not only traditional incentives but an opportunity for social scientists to work on national problems to which advances in science itself was often a contributor.

Over the longer run, the question of significant university involvement in direct decision studies invites fuller exploration. Past experience with universities suggests that funding agencies will want to take a much firmer stance in requiring that university organizations demonstrate policy research commitment and competence. This requirement should, of course, extend to non-university organizations. And funding agencies should be far less willing than in the past to take for granted either university capability or desire to undertake policy relevant research.

3. Minimizing the Risks of Evaluative Research

There is no way of guaranteeing that invalid evaluative results will not be used in policy, or that sound results will not be misused by an interpretation of them beyond their legitimate limits. But, a wide and careful scrutiny of evaluative results in terms of validity and interpretation by various parties with political interests and by relatively disinterested researchers before decisions are made seems the most likely means of minimizing these dangers.

Analysis must occur before decisions are made for the obvious reason that after the decision is often too late. What is not obvious is how to get results on the table, given both the real time pressures of fiscal year decision-making and the desires of decision-makers for flexibility. Furthermore, wide discussion and debate will often leave the proper policy choice still debatable. In many ways the situation will resemble a courtroom setting in which each side has experts who score points with the final verdict resting on contradictory evidence. But it is far better that the validity of the evaluative results and the interpretation of them be subjected to wide political and technical scrutiny than to have them looked at only in the comparative isolation of an agency or Congressional committee. In short, public knowledge of evaluative results is in the public interest.

Agencies supporting research and the researchers themselves are unlikely to want outsiders looking over their shoulders. Consequently, steps must be taken to facilitate and institutionalize access to evaluative information at a reasonably early stage. Possible measures might

include (a) a legal requirement for public disclosure at the letting of evaluative contracts and grants not scattered amid a deluge of other announcements but in a single, readily available source; (b) a requirement that contractors prepare for public distribution interim progress reports, including methodological and procedural discussions; and (c) the use of existing or the establishment of independent bodies, perhaps funded by private foundations, to perform thorough methodological critiques.

To date insufficient attention has been given to the detailed procedures required for establishing an institutional structure that will bring evaluative activity under careful scrutiny. Hence, the most reasonable course of action is an immediate search for such institutional means rather than a specification of particular approaches. Moreover, the task may not be simple--research in a fishbowl atmosphere can turn out to be counterproductive for a variety of reasons ranging from researcher annoyance to strong political pressure to block or abort evaluations to popular programs or field experiments portending changes that may threaten major interest groups. However, it is difficult to see how evaluative evidence itself can be evaluated unless it is made as widely available as other research. Given their potential effect on major decisions, it is imperative that evaluative results are subjected to a critique before decisions are made.

There is also an urgent need to investigate institutional means for protecting researchers from government or other interference that lessens the independence and objectivity of the researcher, or unduly

restricts the scope of his investigations. For these studies the federal government could draw on well-qualified, academically-oriented researchers including scholars recognized as outstanding by their disciplinary peers. With ample funds the government can always procure much second-rate research. What is difficult is to involve top flight people who adhere to the academic standards of independence and objectivity. In order to attract these types of people to perform the kinds of policy research it needs, the government must establish a milieu in which research requirements including those for direct policy work do not restrict unduly the researcher's scope, objectivity, and independence; in which scholars can obtain detailed information about policy needs; and in which ample funds are available to scholars knowledgeable about policy needs for basic policy research.

Studies expected to have a direct policy impact will often legitimately constrain the researcher's effort in terms of firm time deadlines, the relatively detailed specification of the objectives of the study and methods and procedures to be used, and detailed monitoring of the ongoing work by the agency policy research staff. However, unwarranted restrictions involving attempts to influence the findings, to suppress them, or to force early release of information must at the very least be minimized, if not prevented. The steps taken to make evaluative results widely available should also serve to reduce the dangers of undue influence by keeping the study before the public.

Even more difficult to treat may be the subtle threat to research organizations dependent for the bulk of their funding on a single or a

small number of government sources. Here the potential influence may be a fear of losing future contracts that leads the research organization to "please" the big client unduly. The problem, however, is not only that of assuring the objectivity of research organizations but also of protecting the public against the unwarranted use of evaluative results. A single evaluative activity could, at the extreme, influence decisions involving billions of dollars and millions of people--as in the case of the New Jersey Negative Income Tax experiment and the Family Assistance Plan. Whether steps beyond wide public disclosure and discussion are required to protect researchers and the public is an issue that needs an immediate public debate.

Policy research--even of the most basic type--will be enhanced if the researcher can interact sufficiently with policy research staff members to gain detailed knowledge of policy problems and concomitant research needs. This gives a major role to mission agencies in basic policy research, even if they are not the funding sources. Under ideal circumstances, however, the social policy agencies would be the primary source of basic research funds in their areas of concern. This is so not only because of the interaction but also because the agencies will be more likely to use research results supported by their own funds.

The funding of basic policy research by mission agencies is not without its problems. First, they do have reasons to want to exert influence on or put restrictions on research organizations. Second, basic research funds in mission agencies tend to be highly vulnerable in times of budget cutbacks. In light of these problems it is worth

considering whether such research-supporting agencies as the NSF or the National Institute of Mental Health (which is part of a mission agency but historically has been considered an independent funding source) should not be given additional funds to be allocated to more policy research in the social areas. The problems to be considered in making such organizations major funding sources of basic policy research concern the means for (a) developing greater knowledge about policy problems among potential researchers and (b) for getting the research results considered by the mission agencies in their policy process. These are complex institutional problems for which solutions are not immediately apparent. But it does seem clear that the threat to basic research funds in the mission agencies does make some increase in NSF and NIMH funding of more basic policy research in the social areas necessary. The support of such research, coupled with satisfactory institutional means of imparting policy needs to researchers, will increase the value to society of evaluative activities.

RECOMMENDATIONS

1. Substantial national resources have been, and increasingly will be, devoted to vast programs of social action intended to achieve emerging national goals. It is important that these resources be economically used, and that they have the intended effects. To assure this we must regularly evaluate our action programs to assess how far they are actually achieving the objectives declared for them by the Congress and the government. Even before great new programs are launched, we should test how far available program alternatives seem likely to attain the objectives sought.

These objectives require systematic, scientifically rigorous program testing and evaluation, and commitment of substantial resources to support that effort. We can think of no more prudent investment to insure that the nation gets what it wants at a reasonable price.

2. Social agencies--the primary developers and users of the results of social program evaluations within the government--must establish relatively large, well-trained staffs with sufficient technical and administrative skills to determine evaluative needs, to articulate these needs to outside researchers, to design or work with contractors and grantees to design studies and methodologies, and to supervise the ongoing evaluative effort.

3. Evaluations of major programs and policies should be performed by a high level agency staff independent of the operating program, since a separate office can at least institutionalize a relative degree of objectivity. It can be charged specifically, within the agency, with the task of program measurement, not program defense.

4. The Congress and the Executive Office should also establish staffs of sufficient technical and administrative capability to determine and articulate evaluative requirements necessary for effective decision making, and to monitor the evaluative activities of the social agencies in enough detail to assess the validity of the results.

5. Major outcome evaluations aimed at providing results directly relevant to social policy should be undertaken now as a high priority activity; and, as a general rule, should be amply funded so as to avoid excessive shortcuts, and to allow a realistically long time schedule for careful planning, design, and execution.

6. An equally high priority activity should be a systematic, concerted experimental effort to develop new program and policy ideas and to test the merits of these ideas on a small scale, yielding evaluative data relevant for decisions about changing or starting new large scale national programs.

7. Compared with the past, relatively more research directed specifically toward major problems in the social areas, including evaluative studies, should be undertaken. The development of this research should involve far more interaction between government policy research staffs and outside researchers.

8. The actual conduct of evaluative studies can and should be done under a variety of auspices--by the evaluation divisions of social agencies themselves, by university based research organizations, and by more special organizations, non-profit or profit. Competition and diversity should be encouraged particularly during a period of rapid development.

9. Developing the necessary capacity to conduct evaluative research calls for encouraging the creation or expansion of relatively large organizations with an enduring commitment to research and teaching in social policy areas. This implies a multidisciplinary approach, and provisions for training and teaching. It also means a willingness to interact extensively over time with government officials.

10. Some specialization should be encouraged, though not required, with very large scale evaluations expected to have a direct effect on immediate decisions being conducted predominantly by non-university organizations, and somewhat more of the training and the methodological developments being focused at universities.

11. In order to assure the intellectual independence of these research and training organizations, they should have administrative arrangements that avoid domination by any one academic discipline or point of view. And in order to assure freedom from political pressures, they should have some longer run financing and a diversity of funding sources. An expansion of the role of the National Science Foundation and the National Institute of Mental Health in supporting policy research, will contribute greatly to this goal.

12. In individual evaluation projects, means must be developed to ensure both the cooperation and the independence of the researchers and the project directors. Ideally they should work together from the beginning, with understanding of the extent to which the information generated for evaluation could also be used to monitor and improve the project in an ongoing feedback process.

13. Indeed, a major policy issue involves the relative emphasis on evaluating as testing of programs as against its use as a continuing source of information and advice how to improve projects. Whether the programs are new and experimental, or ongoing and accepted, the conflict of objectives remains. Hopefully, ways can be devised to achieve both objectives.

14. Since most of the projects or policies being evaluated involve individual persons, and many evaluations will involve securing information from or about individuals, protection of their rights to privacy must be assured. The problem is particularly acute since the data are often both personally and politically sensitive, and because in the absence of protection, even our ability to secure such data from individuals will be impaired.

15. Institutional means must be sought through which evaluative results can be widely and promptly distributed, so that large numbers of people can scrutinize them thoroughly in terms of validity and interpretation, thus improving their contribution to policy decisions. Where important policy decisions will be based on major evaluation studies, public scrutiny is essential before final decisions are made.

PROBLEM AREAS ADDRESSED

Education of the Public

- As to means of keeping well
- Means of seeking and obtaining help

Relieving the real and apparent shortage of health personnel

- Improved training methods
- Continuing education
- Assist in more equitable distribution of personnel
- Improved access to specialists

Increased efficiency

- Better health records readily accessible
- Sharing of resources; human and facilities
- Coordination of access to consultation and treatment

DIAL ACCESS COMMUNICATION SYSTEM

To make available to any person, at his discretion and at a time of his choosing, information services relative to health facilities in the area and solutions to common daily problems.

HEALTH PROFESSIONAL COMMUNICATION SYSTEM

To provide communication among the many persons whose knowledge and skills represent an ever-increasing number of health disciplines, any or all of which may need to be brought to bear on the health problems of an individual or family.

BIOMEDICAL DATA NETWORK

To provide a network for transmission of patient information to attending health professionals so essential in a mobile society.

To provide management with the means for rapid transmission of data required for sound local and regional planning for health care, development of educational and training programs, construction and renovation of facilities, and purchase and use of equipment.

HEALTH TELEVISION

To educate the public in their own health care

and to understand and take part in the health care

system.

NEIGHBORHOOD HEALTH CARE STATION

For the many people who are separated by geographic distance or by the lack of transportation or whose infirmity limits their mobility, development of systems of communication can provide better coordinated access to consultation and treatment than is presently the case.