The NAS Surveys of Fundamental Research 1962–1974, in Retrospect

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In this article I review the origination, design, conduct, policy impact, and general usefulness of the ten surveys of the health and direction of major fields of science that were conducted from 1962 through 1974, by committees working under the aegis of the National Academy of Sciences' Committee on Science and Public Policy (COSPUP). The reports (*1*) were:

1964 *Ground-Based Astronomy: A Ten-Year Program* (A. E. Whitford, chairman)

1965 *Chemistry: Opportunities and Needs* (Frank H. Westheimer)

1966 *Physics: Survey and Outlook* (George E. Pake)

1966 *The Plant Sciences: Now and in the Coming Decade* (Kenneth V. Thimann)

1968 *The Mathematical Sciences: A Report* (Lipman Bers)

1969 *The Behavioral and Social Sciences: Outlook and Needs* (Ernest R. Hilgard)

1970 *Report on the Life Sciences* (Philip Handler)

1972 *Astronomy and Astrophysics for the 1970's* (Jesse L. Greenstein)

1972 *Physics in Perspective* (D. Allan Bromley)

1974 *Materials and Man's Needs* (Morris Cohen).

Sponsored for the most part by the federal agencies that support basic research, and costing about $2 million, the surveys were prepared with the help of hundreds of scientists from industry, government, and the universities. The reports profiled funding, described manpower trends, portrayed the contributions of fundamental research to the advance of technology, and estimated the requirements for continued vigorous development of the fields. In this article I will discuss these surveys as instruments of the scientific community's self-governance and of national policy-making, giving particular attention to the surveys of physics.

It is important from the beginning to appreciate the almost unresolvable difficulties of the surveys. The surveyors were asked to present the best "cases" for the fields to which they had devoted their lives—objectively, and with restraint. They were asked to be critical and to recommend measures of economy and efficiency—measures that would affect themselves, their colleagues, and their institutions. They were asked to avoid being influenced by the government, industry, and others—but at the same time to learn from, and eventually influence the work of, those same institutions. The surveyors were asked to express opinions representative of the broad diversity of scientists' opinions—while working in what might (nonpejoratively) be called "elitist" modes of operation-by-committee. And the surveyors were asked to make projections and predictions—of the basic research endeavor, which almost by definition defies projection and prediction.

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Origins and Procedures

In the early 1960's, the Academy and its working arm, the National Research Council, taking advantage of their position in the scientific community and their charter as quasi-governmental organizations, became increasingly involved in matters of national policy. The newly formed Committee on Science and Public Policy, under the chairmanship of G. B. Kistiakowsky, began preparing the influential report *Basic Research and National Goals* (which would be published in 1965) for the U.S. House of Representatives' Committee on Science and Astronautics (*2*). Kistiakowsky had often felt when he was science adviser to President Eisenhower that he would have been helped in weighing and recommending programs and policies if broad-based appraisals of fields had been available to him, especially studies that might have buffered some of the strong arguments made by the highly organized practitioners of "big science." Kistiakowsky gained support from Academy president Detlev Bronk and others, and the COSPUP undertook sponsorship of the series of studies that form the subject of this review. COSPUP's role was to explore the need for particular surveys, assist the disciplinary representatives in organizing their surveys, coordinate liaison with the appropriate government agencies, and offer prepublication critique. More will be said about COSPUP's role later.

All of the surveyors served without pay, being compensated only for travel and direct project expenses. Many of the participants were drawn from outside Academy membership. Committees were formed, and they worked in various ways, for the most part in small specialized panels. Not only were research-discipline panels set up, but also panels on personnel, instrumentation, communication, and other functional problems.

The committees handled their data-collection work in different ways; all found analysis difficult. The Bromley committee convened a special panel to oversee the collection and analysis of statistical data, in order to coordinate the many different working groups' approaches to the government statistics offices and in order to ensure that the data were expressed in a form that would allow intercomparison. The desirability of coordinating all this work through a single data panel was a lesson learned from the Westheimer and other surveys. Those who have had to deal with the numbers marvel at the heroic efforts of the Bromley data panel—even though, as several people have only half jokingly suggested, nobody has ever fully comprehended that intricate and forbidding work besides panel chairman Conyers Herring. And Herring himself disclaims even that.

Throughout their deliberations the committees maintained liaison with the National Science Foundation, the mission agencies, the Office of Science and Technology, the Bureau of the Budget (later Office of Management and Budget), congressional committees, and the professional societies.

The Surveys, in Vignette

The surveys differed in quality, tone, and style, each reflecting its policy context and the complexion of the field.

The Whitford report (1964) described the instruments upon which the small, cohesive astronomy community is so dependent, and it outlined the arguments over ground-based versus space-based observation that still split the field. With-

out evidencing special concern over where everyone would work, the report projected a doubling of the doctoral population within a decade.

The Westheimer chemistry report (1965) portrayed an extremely large and vigorous field emphasizing application and predominant industrial involvement. It gave much attention to personnel issues. The feature most often cited is the Westheimer report's emphasis on modernization of chemistry to take advantage of advances in physics and mathematics, and the associated need for substantial investment in such instruments as spectrometers and computers.

"Big science" got full treatment in the Pake physics survey (1966). Arrayed page after page were the steep funding charts that now seem like astonishing historical inscriptions: "During the period 1959–1963, there was fairly rapid growth in federal support of basic physics research, corresponding to an average annual growth rate of at least 20 percent per year [not including NASA expenditures]. . . ." Special support was given to the meson-factory proposal.

Academic botany was covered in the Thiman report (1966), but such huge fields as forestry and agricultural research were not. The impression conveyed was of a diffuse "small science." The report urged that such essential fields as taxonomy, without which much ecological investigation would be helpless, not be neglected in the rush toward the more glamorous molecular biological approaches.

The Bers survey of mathematics (1968) described how "little" the field was: the total federal investment in mathematics to date had been only $300 million. A primary function of the profession is educating people in other fields. The report argued for greater support for applied math and the computer sciences.

The Hilgard survey of the behavioral and social sciences (1969) portrayed a set of diffuse fields working at quite some remove from the "hard" sciences, making forays into applied territory with only moderate success.

Biochemistry as reviewed by the Handler survey (1970) was a vigorous enterprise broadening beyond its traditional contexts (but the report still carried a biomedical tone), drawing upon the sophisticated resources of biology, chemistry, and physics, and making a reputation for being able, at long last, to tackle the problems of nutrition, disease, food and fiber production, and preservation of the biospheric environment.

The astronomy and astrophysics of the Greenstein report (1972) were keyed closely to the development of new facilities, for which the committee presented a list of priorities. The Greenstein report, coming 8 years after the Whitford report, noted that the astronomers' employment market had become saturated.

Bromley's physics committee prepared the largest and in many ways the most ambitious report (1972) of the series. As though a latter-day explorer-emperor had commissioned it to encyclopedize the territory and peoples of physics, the committee catalogued a myriad of research activities, drew profiles of the physicist population, described in detail the community's dynamics and education and communication, diagnosed the endeavor's overall health, analyzed the intellectual commerce between physics and its neighboring fields, and developed a method for setting priorities—all in the effort to place "physics in perspective."

In the Cohen report (1974) the representatives of a variety of endeavors struggled with their identity problem and sought definition and recognition as a new field, "materials science."

Summary

The Academy's surveys of the health and direction of various fields of science have served as instruments for the sciences' self-appraisal, aided fields in asserting their identity and arguing for support, fostered mutual education and heightening of self-understanding among those involved in science policy-making, served to express the general tenor of the scientific community's opinion, functioned as "seconding" documents, and had the effect of lending endorsement to particular major projects. There were, indeed, shortcomings and limitations, but the surveys appear to have been worth the expense and effort. As with other such endeavors, process may well have been as important as product.

Policy Axes

Through all the surveys run the perennial themes of science policy-making. Whether to respect "critical size" arguments and concentrate efforts in a few centers, or to disperse efforts broadly. How to balance research per se with research-in-the-educational-context. "Big science" versus "little science." University labs, industrial labs, government labs. How much to preserve the established fields, and how much to encourage their evolution. "In-house" research as compared to "outside-user" participation, and the problems of "suitcase science." Capital expansion versus fuller use of existing facilities. The relative advantage of investigator grants, project grants, contracts, and institutional block awards. In many of the surveys these themes, which delineate the decisional axes, were discussed explicitly. Others were discernible only from reading between the lines.

Usefulness

First, the surveys served as instruments for the sciences' self-appraisal. Most observers agree with American Institute of Physics director William Koch that "The principal value of the Pake and Bromley surveys was that they caused physicists to confront *each other* on these issues." This opinion is widely shared, and it is reflected in the many recommendations most of the surveys have directed to the scientific community at large. Robert Green, who served as COSPUP's executive secretary through all of the surveys, points to a kind of effect that he, as a "broker" in the policy arena, must always be concerned for: "One of the most important aspects of the surveys was the interaction between the committee scientists and government officials. This was very much a matter of mutual education and resulted in revelations for both sides." Allan Bromley has said, "Few aspects of our work were more important than our trying to learn how to wrestle with the issue of priorities." And Wayne Gruner, of the National Science Foundation, expressed the opinion of many in government when he observed that "What has been important has not so much been the specific recommendations as the reflection of the overall mood of the scientific community in the *tenor* of the reports. The Bromley report, for instance, gave us in the government an indication of the willingness of the physics community to make certain kinds of hard decisions."

As Frank Westheimer has commented, "*Writing about chemistry* is entirely different from *writing chemistry*; in our survey we found ourselves outlining things about the art that we knew but had not previously become fully aware of." Among his committee's conclusions, for example, was the judgment that what

chemistry needed was not so much increased output of Ph.D.'s as improved quality of their training nationwide—an appraisal which was arrived at carefully, could have been deduced reliably by few other means, and which contributed to the "tenor" of opinion about the field. The Bromley study examined the question of professional mobility in depth for the first time, and found itself concluding that "physics PhD manpower is much more mobile than has been commonly believed. During the period 1968–1970 about one third of the PhD's changed their subfields of major interest."

Despite connotations in the overused term to the contrary, scientists usually interact as "community" only indirectly, except in projects like the surveys—and obviously even then there are extreme difficulties of representativeness and consensus. If the surveys are to be reviewed at all sympathetically, this aspect of the community—an intellectual diaspora striving for cohesiveness—has to be appreciated.

Second, the surveys have aided fields in asserting their identity and arguing for continued support. This has been especially important for the classical but fading disciplines, for highly diffuse fields, and for emerging disciplines. The Bromley report observed that "For many years, an outstanding problem facing acoustics as a field has been the preservation of its identity. Some subject matter has been taken into other disciplines and subfields because acoustics provides a successful tool (for example, ultrasonic measurements in condensed-matter physics), some has always spread across other classifications (noise *vis-à-vis* turbulence), and some has been so largely outside physics that it has virtually been forgotten (speech, hearing, and bioacoustics). It matters little what label attaches to the subfield, but it is important that it be maintained." (But I must register the complaint that since there must surely have been good reasons for that historical development the Bromley report missed a chance to present the pro's and con's of this argument openly instead of just passively acquiescing in the acousticians' pleas; nowhere did the report explain *why* acoustics had to be maintained *as a field*. Is it not true that one of the surest signs that a discipline has fulfilled its purpose is that it pervades the other fields so thoroughly that it loses its own identity and thereby passes on, as many venerated disciplines have, into the history of science?) As was mentioned earlier, *The Plant Sciences* championed the importance of such classical fields as taxonomy as underpinnings for modern ecological research. *Materials and Man's Needs* opened with the question, "What is materials science and engineering?"

On one hand, there certainly are times when such self-preserving arguments need to be presented, and the few suitable forums available, such as the surveys, should indeed be used in this way. But on the other hand, such claims may prove spurious. Broad, critical surveys should be able to entertain minority views, weigh the claims fairly, and describe the shape of the debate; but the Academy surveys have often failed to do this with full candidness.

Third, by both process and product the surveys have fostered much-needed mutual education and heightening of self-understanding among those involved in science policy-making. This has not been inconsequential. Many people have acknowledged the passing of their scientifico-political innocence in working on the surveys. Alvin Weinberg expressed this intention in a letter to Allan Bromley in 1969: "The problem that we face is to elevate the level of discourse underlying the politics of physics, so that there is little venality or self-serving in that politics. In some ways I conceive of this as one of the central jobs of the whole exercise of the [Bromley] committee: to extend and deepen the intellectual basis of the political discourse which ultimately determines allocations in physics, as it does in any non-market situation."

Learning as a community, scientists have improved their self-analytical techniques through the surveys, and they have learned much about how to conduct these complicated projects. The Bromley survey reflected many lessons from the Pake and Westheimer exercises.

Officials of the government, as well, have in many cases found the survey experiences subtly enlightening; they have come to know the leaders of the scientific fields they oversee and serve and have learned from them not only matters of specific fact but also some of the intuitions that guide decisions about basic research.

Fourth, the surveys have served to express the general tenor of the scientific community's opinion. The Greenstein astronomy report expressed strong apprehension over the shift of research activity away from the universities and into the national laboratories. The Bers mathematics report recommended "support of research and education in the applied mathematical sciences as such, and not merely in connection with either mathematics or the particular sciences that use mathematics." Other than surveys, there are not many mechanisms for gathering and expressing opinions on such general issues. A good survey reestablishes bench marks, legitimates boundary shifts, exposes claims that otherwise would be disputed only in private, and commits to easily examined record a combination history, inventory, census, and projection.

In this regard, what government officials generally hoped for from the surveys was a broad overview, and most have thought the surveys' level of detail adequate. As one program officer observed, "We have to study every proposal in the field that comes to this agency, we receive all sorts of advice, and we continually have to argue for our budget. About some things, we know as much as the survey committee does. Don't get me wrong, but as for more detail—no, we don't need it. What we want is a feeling for the general current of opinion."

Fifth, the surveys have frequently been put to use as "seconding" documents. This is not unique to science, nor is it necessarily bad. Those conducting surveys simply have to expect it. This use of the surveys—selective, rather passive citation to buttress arguments arrived at from other directions—is perfectly exemplified by Atomic Energy Commission (AEC) physical research director John Teem's testimony before the Joint Committee on Atomic Energy in the authorization hearings on the 1974 AEC budget. Teem called attention to the fact that "In its 1972 report, *Physics in Perspective*, the National Academy of Sciences Physics Survey Committee identified heavy-ion physics as an important new frontier of nuclear physics." He quoted the relevant passage from the survey, then went on to affirm that "We in AEC also identify heavy-ion physics as an important new thrust. . . ." Later, in presenting the case for the meson factories, he drew support from the Bromley report again. In its official report of the hearings, the Joint Committee reprinted the "executive version" of the Bromley survey in its entirety.

Sixth, the surveys have often had the effect of lending endorsement to particular major projects. In many cases the Academy reports have repeated recommendations promoted more unrestrainedly by other groups, thereby exerting a "tilting" infuence. Some believe that the Pake report's concurrence with the Bethe panel's report, *Meson Factories* (*3*), had some influence on the decision to go ahead with building the Los Alamos meson physics facility; certainly this additional "vote" did not

hurt, and a Pake disapproval would have been a challenge. The Westheimer survey is unfailingly acknowledged as having helped to catalyze the National Science Foundation's establishment of line-item provision for chemical instrumentation in its budget. The Bromley report added its impetus to the push for the National Accelerator Laboratory and the Oak Ridge heavy-ion accelerator. On this point Harvey Brooks has commented, "I think the most useful contribution of the Bromley report was in crystallizing a consensus on the many competing projects in 'big physics,' especially in astrophysics (jointly with Greenstein), particle physics, and nuclear physics."

Inherent Limitations

Recalling some of the wearisome internal Academy battles over survey questions, Harvey Brooks stresses the importance of recognizing that whereas scientists can be drafted to work with others in conducting surveys, "they cannot be required to be parties to what they see as their own execution." Candor is commendable—so long as it does not offend too many colleagues or debilitate the politicking that scientists engage in no less than others having special interests. In this, of course, lies one of the major inherent limitations of the surveys. The surveyors were charged to be intellectually honest and not to try to "second-guess" the political system; nevertheless, because political aloofness can come close to being political naiveté, the surveys presented their cases with restraint but, as should be expected, never lost their character as political documents. In no way should this be interpreted to mean that government staff or other groups could have done better; in fundamental research there is no substitute for the experience and guidance of active researchers. But limitations should be recognized.

A second limitation of surveys is that they must unavoidably sacrifice some precision of analysis and impact as the price of breadth of coverage. Studies tend to be more competent and effective when they are closely focused on neatly defined issues. As the field of survey is broadened, comparison of its disparate elements becomes more difficult and the criteria for judgment become less a matter of science. (In the extreme, the task would lose all validity: how can space research be compared with Antarctic exploration, or botany with mathematics?) The very breadth of the Academy surveys is perhaps their most valuable aspect; on narrow issues, there is never a shortage of government and other advisory committees willing to render advice. Broad surveys can provide much-needed perspective.

How important it is that widely different fields be compared directly, and in what way, is not clear; anxiety over this point may be largely unfounded. The federal budgetary process is largely incremental and segmented, with modifications made rather independently around the periphery of a free-form national budgetary "pie," and the allocations to basic research are based not so much on comparisons among the research sectors as on judgments of the contribution each kind of research promises to make to the societal sector to which it belongs. Only after the long budget fight is over does the pie get "trued up" for neat depiction in documents. There is hardly a definable science and technology budget per se; to continue with the above metaphor, we might say the science budget is an annulus of varied width cutting through all the various sectors of defense, health, education, agriculture, and so on. It is probably only vaguely true that allocation of funds to cancer research, or high-energy physics, deprives plant physiology, or optics, of support, except in certain special cases. Indeed the result may even be the opposite, in which stimulation of one area may carry coupled enthusiasm for science over into other areas. The budget is finite, so of course the final budget is a statement of comparison; but although checks are made to ensure that no principal area of science is neglected, the budgetary process does not strongly intercompare the different basic research programs. Thus the perennial suggestion that the Academy "do the global survey" and appraise the value of all the disciplines relative to each other appears to be not only nearly impossible but perhaps even not very useful.

Also with regard to focus, a limitation is that widely dispersed "little science" fields, with their patchwork patronage, are in many ways more difficult to survey than coherent, highly organized, facilities-dependent ones supported largely by a single powerful agency. On this point, those who resent the self-serving nature of the surveys always mutter something like, "To those who have, more will be given. . . ."

A third set of limitations has to do with the intrinsically elitist or meritocratic nature of the survey process. The scientists selected for the committees are chosen because of their scientific accomplishment and public stature. No plan of representation by age, sex, origin, institutional affiliation, or research interest will avoid all objections. Nor can conflicts of interest be eliminated completely. Nor can the possibility of "special pleading." Some ways of dealing with these problems are discussed below.

A fourth set of limitations arises in that, except in the most general sense, the surveys are not oriented toward either social problems or agency missions. But in large part, these are the lines along which the government and industry run their business. The surveys have principally examined what was needed to advance the sciences themselves, looking for "opportunities for exploitation of the field," rather than looking at the requirements for bettering society as a whole. This makes it difficult to match agency plans to survey findings.

The problem of priorities-setting is the fifth limitation. Never, in these surveys or elsewhere, has a perfect program been developed for deciding on the relative importance of different research programs. All of the surveys made recommendations. The Greenstein report systematically examined the needs for facilities in astronomy. The Bromley committee engaged the problem most thoroughly, conducting an elaborate jury-rating experiment to develop criteria and then using the system to rank major facilities proposals and evaluate subfield program elements. The Bromley report said that "The purpose of the ratings was to test the feasibility of arriving at a consensus regarding the desirable relative emphasis among subfields of physics and among program elements within each subfield. Such judgments might then guide decisions as to increased or decreased support for each program element and subfield within whatever total might become available for physics as a whole. It is the Committee's view that the outcome of this exercise is properly described in terms of program emphases rather than priorities." The aim was to develop something more refined than a monotonically valued "shopping list." The committee cautioned that its ratings were primarily illustrations of the method, not incontrovertible evaluations. Much more work on approaches to priorities needs to be done.

Sixth, these studies take time. Thorough surveys will always proceed slowly, which will invariably frustrate government officials, especially research program officers. As one complained, "We began to wonder whether, with all

the panel meetings and writing and reviewing and rewriting, the Bromley report would *ever* be published!" (It took over 2½ years.)

And last, idealism aside, the surveys are human documents. Traces of personalities will always pervade the reports. As has been said, "Committees may deliberate, but individuals do the writing."

Moderating the Claims

In an effort to enhance the credibility of the surveys, filtering and buffering were applied at every stage. First the specialist panels prepared their reports, without pretending to consider the overall state of the science; they were primarily to be concerned for the health of their chosen enterprises and, within reason, to defend them cogently and vigorously. Then the survey committee, receiving the panels' reports, tempered those findings in preparing the survey volume. This and the panel reports went through several iterations, with modification by liaison commentators and outside readers. Further buffering occurred when the COSPUP, which is composed of scientists from many different disciplines, reviewed the drafts. Only then were the reports published.

How effective this process has been, has differed with the various cases. In some instances there is evidence that, in an effort to restrain themselves and forestall disapproval by their peers on the broader survey committee, the panels themselves went through considerable "head-knocking" in developing their statements. In the Bromley study, the nuclear physics panel apparently considered the idea of an intense-neutron generator of the sort that Canadian physicists had long been pursuing, but decided not to ask for one. The plasma physics panel decided that until plasma dynamics were better understood, building a large plasma reactor could not be justified. The Bromley report also declined to endorse proposals for national laboratories for high-pressure research, intense-laser research, low-temperature research, and a computing center for solid-state calculations. The Bromley survey committee "concurred with and supported" many of its panels' findings, but remarked that "on occasion, however, the Committee, from its broader viewpoint covering not only all of physics but also its broader external interaction, not unexpectedly reached somewhat different conclusions." Despite its shortcomings and occasional failures, this kind of procedure has strong advantages. Such a broad-based, iterative, critical mechanism is not usually available to, say, agency staff or advisory committees.

But still, it is obvious that each survey, all said and done, has not only "put its best foot forward" but has tried to gain a step or two on its rivals as well. Practitioners of "small science" can rarely resist complaining, "It just seems unfair that physics, one of the wealthiest fields, got a half-million bucks to sell its own case." What responses to that have to be considered? First of all, we remind ourselves that the professionals in the government presumably interpret the surveys with the same toughness and eye for bias they apply to other reports that flood their desks. Then we ask whether the economies the surveys have led to have offset their expenses. The point will always be debatable, but most observers remark that with an annual federal physics budget in excess of $300 million, an investment of $500,000 every 5 years or so in a major stocktaking survey is not unreasonable. Officials of the funding agencies have in general said that their agencies have obtained their money's worth from the surveys.

The Academy, COSUP, and the Government

Academy sponsorship ensured broadness of representation and access. Many people have remarked that no staff, advisory committee, or contract-research institute effort could have marshaled the services of so many perceptive and influential scientists from such diverse backgrounds, or so effectively coordinated their work. Academy sponsorship has "headed off" some charges of special pleading and self-interest, partly because of the internal review process involving the COSPUP. The Academy committees were better able to cut across agency missions lines and discern the shape of emerging areas than specialized agency advisory committees usually are.

As to the government's role, the agencies helped initiate and fund the projects, and in most cases they appointed active liaison representatives to the committees and panels. The survey committees solicited information and advice from the various appropriate offices of government, worked with them in developing much of the factual material, and sought to present the findings in a manner that would assist and influence the government's decision-making.

What should be appreciated is the way in which the survey groups and the government strove to keep themselves insulated from each other during the survey period. Communication took a rather curious form different from that of more conventional government advisory work. During the survey process the surveyors tried to avoid being influenced and the government avoided becoming obligated; then, after the reports were published, the survey groups actively campaigned to convince the agencies of the usefulness of the documents, and the agencies, usually quietly and privately, tried to extract from the reports as much guidance as possible.

COSPUP helped to initiate each of the projects, serving as a broker for the interests of the various groups involved. Later, as the survey reports were prepared, COSPUP gave them confidential prepublication review, recommended specific improvements, then placed its imprimatur on the final versions and transmitted them to the president of the Academy for publication.

As Harvey Brooks explained when he was COSPUP's chairman, COSPUP's review was intended to buffer the tone of the reports—"to insure some common denominator in degree of optimism between the different fields, to make sure that some disciplines or subdisciplines do not make highly inflated claims of needs in comparison with others."

In transmitting the documents, which it had reviewed but had not itself prepared, COSPUP in essence attested to the public that the surveys presented reasonable conclusions and recommendations warranting serious consideration. To the survey committees, COSPUP offered the assurance that, although the documents would have to stand on their own merits, they would be conveyed and recommended to the highest appropriate officials of the government. In both of these brokerage roles, in relation with the public and with the survey committees, COSPUP carefully maintained its third-party status, insisting that the reports expressed the findings not of COSPUP but of the surveying committees.

On occasion, the COSPUP letter of transmittal, bound into the final document, implied that the report had shortcomings. The Pake report was passively chastized: "No attempt has been made to identify relative priorities of the various fiscal needs in case that the total budget does not grow at the recommended rate [of 21 percent per year]." As was the Greenstein astronomy report: "For reasons that are readily understandable, in view of the present enormous promise of the field, the report has given, per-

haps, inadequate attention to the probable scientific consequences of more constricted financial support than is implied by even its first four priorities or to how the national program could be reoriented to minimize the damage from such austerity. . . ." None of the prefacing letters, however, was ever openly disparaging of the reports. The need for restraint is obvious. But if these letters had been drawn more critically, perhaps outlining problem areas or simply recounting the essence of the debate COSPUP had had with the surveyors (perhaps printed as an appendix so as not to interfere with the opening statements of the report), they would probably have been more useful.

Shortcomings

One of the most painful problems for the surveys was to confront the prospect of cutbacks in funding. I will illustrate with the Pake survey, but will insist that the problem afflicted all of the surveys to a greater or lesser degree and that much of the Pake's shortcoming was due to historical circumstances.

In the Pake panel reports and in the project's correspondence there are enough acknowledgments of the downturn impending in the mid-1960's to make it evident that the committee was aware of the funding problem, or at the very least aware of ominous signs. Relevant discussion had already appeared in the scientific press and in budget hearings. The Bureau of the Budget was deeply concerned. Several government officials privately urged the Pake group to appraise the consequences of possible budgetary retardation. But the report hardly touched the issue. Why? Those involved give several reasons: it was not really clear that a severe cut would come; scientists knew how to handle funding increases but not decreases; nobody was willing to offend his colleagues by indicating that some kinds of work were more worthwhile than others; and everyone was loath to generate what might turn out to be "self-fulfilling prophecy." The last is mentioned universally. Many people felt that if it were even implied that physics might survive a squeeze, support would indeed be reduced. Thus the strategy of not admitting to options and of avoiding discussion of alternative budgets. While this feigning may have had some marginal advantages, there is no evidence that it influenced the construction of the federal budget. Indeed, it simply denied potentially useful guidance to the science-sympathetic officials who, in any event, had to wage the budgetary infighting, with or without advice.

After publication of the Pake report in 1966 a series of changes occurred that could hardly have been anticipated singly, let alone as a cluster: the national economic climate began to cool; the criterion of "social relevance" began to gain importance; environmental and energy-supply problems became prominent; the nation's commitments to space exploration began to wane; the once-insatiable market for scientific talent approached saturation; the "Mansfield amendment" and shifts in public sentiment curtailed military research in the universities; students began to express their disaffection; and professional migration dramatically changed the complexion of a number of fields. Hence, to be fair historically we should acknowledge that at the time the Pake report was prepared the need for what would later be referred to as "tough decisions" was not compellingly evident. But this episode exemplifies the problem perfectly.

Conversely, in their earnest and only lightly bridled pursuit of support the survey committees—like most of the scientific and technical community—have generally neglected to examine the dangers of excess: What might be the consequence of *too much* support? Science sometimes becomes an instrument of politics—mounting an extravagant offensive on cancer in an election year, or racing to the moon—and in this plays a risky game. Science is also susceptible to a kind of self-defeating overenthusiasm for its own enterprise. Specialist panel studies seem not to be able to face these issues except when confronted with the most extreme emergencies. In their breadth and sophistication, the surveys have had the opportunity to address these issues squarely, but they have mostly declined.

There is one aspect of tactics that cannot be stressed too strongly. It has to do with summaries and recommendations, and is not limited to these surveys. Surprisingly, scientists, who demand terse, informative abstracts and summaries in scientific reporting, often turn around and deliver to busy public officials plump documents having only bland summaries and weakly focused recommendations. Yet, especially at the higher levels of management and government, these very well may be the only parts of the reports that get read closely. Asked about this shortcoming, some of the Academy surveyors have recounted with regret how, after many months of meeting and writing and rewriting, the committee finally sat down to pull together some recommendations at the last minute before going to press. The "fatique factor" is hard to compensate for; perhaps a few people could be designated from the start as preparers of the summary and recommendations, be kept fully informed throughout the project but spared the writing chores, and then relied upon to prepare those special sections and refine them in collaboration with the rest of the surveyors. Abridged "executive versions" of the reports usually prove helpful.

Some of the reports' overall recommendations were only poorly substantiated and their justification by the findings was left unclear. In some cases, although the recommendations probably had merit, *as printed* they were not well supported by either evidence or argument. We are obliged to ask, then, to what extent committees should be expected to buttress their recommendations with data, or with reasoned speculation, in print so as to establish a firm basis for government and public consideration of the claims. Speaking of his committee's report, George Pake has commented in retrospect that "the recommendations having to do with the sociological and economic fabric of physics research and educational enterprises were inherently subjective. Contrived documentation would have been meaningless. My view remains that the collective—indeed essentially unanimous—judgment of the Physics Survey Committee had substantial intrinsic value. The committee consisted of recognized outstanding physics researchers and scholars drawn broadly from diverse branches of physics. They were a thoughtful, experienced, and generally wise group. Any exhortations that such a group puts forward with near-unanimity strike me as worthy of consideration by the physics community and the agencies supporting physics." Still, though, it should be useful to recipients of the report, to other practitioners of the discipline, and to the surveying committee's own case, to publish the bases upon which the recommendations are founded in as much detail as possible.

Early in a survey project it may be useful to discuss the complexion of the recommendations and set up some hypothetical findings, or even pairs of conflicting tentative recommendations, which could serve as ideas to be tested and enriched during the course of the survey. Several of the surveys explicitly addressed their recommendations item-by-item to particular audiences—the federal government, the universities, pri-

vate industry, philanthropic foundations—and this may have helped focus their effect.

Usually the survey documents have been released with some fanfare of press conferences, news reports, and public appearances by the survey chairmen. Briefing sessions have been held with certain key officials, and there has occasionally been some review by the agencies concerned. Otherwise, as is a common weakness in the Academy's work, follow-up has often been light (this currently seems to be improving).

As an example of the kind of review that can be conducted, in 1966 the planning staff of the National Science Foundation (NSF) reviewed the underlying assumptions and limitations of the Westheimer report and prepared a rather elaborate 37-page internal document, *An analytical review of Chemistry: Opportunities and Needs*. Called by NSF associate director Bowen Dees "an effort to look at some of the issues dealt with in the Westheimer Report from a rather general and completely 'field-of-science neutral' point of view," that paper critiqued the Westheimer report's manpower analyses and its recommendations on instrumentation and computer work. Also in 1966, the Academy surveys were discussed over a weekend by COSPUP and the chairmen of the surveys to that date, and by a Woods Hole summer study session of the President's Science Advisory Committee.

One can imagine that the value of the surveys might have been enhanced if they had been subjected to active examination in public hearings before the supporting agencies, their congressional oversight committees, and other interested parties, after a suitable but brief period of private review. Assumptions could be exposed, questions of timing raised, data-base problems aired, and needs for further study detailed. This could aid the surveyors as well as the government, and could preserve the momentum built up over the preceding months and elicit commitments to give the issues further attention. Perhaps the closest model for such examination was the review devoted to the 1965 COSPUP report, *Basic Research and National Goals* (*2*), in which the House Committee on Science and Astronautics heard presentations by the Academy panelists and asked questions, in hearings fashion, about the report.

In another kind of follow-up, the National Research Council's Committee on Nuclear Science in 1975 prepared a report, *Nuclear Science: A Survey of Funding, Facilities, and Manpower* (*4*), which is explicitly an updating of the Bromley nuclear physics panel's report.

By final note of criticism I will repeat my overall complaint over how little the reports reveal the push-and-pull of opinion, the arguments over ideas, and the rejection of alternatives. Competent surveys hewing to their charters should easily be able to withstand the openness, and by revealing the essence of the debates and the foundations of the prevailing arguments they would render fuller service.

Conclusion

Altogether, their various flaws notwithstanding, the Academy surveys appear to have been worth the trouble and expense. They have influenced policy, they have provided occasions for scientists to interact with other public leaders in a mutually profitable way, and they have taught us some valuable lessons. Such surveys cannot be done with such sophistication by any group other than working scientists themselves.

For the present, the Committee on Science and Public Policy does not anticipate sponsoring any more major surveys. It has, however, indicated a willingness to lend assistance to groups calling upon it, and in 1975, for instance, it sponsored an Astronomy Manpower Committee in producing a report on *Employment Problems in Astronomy* (*5*).

The question of whether the nation needs more work of this sort cannot be answered in summary fashion. It will depend on how the fields develop and will differ from field to field. Periodic updating of previous reports may suffice, as may ad hoc studies of special problems. Or maybe we need another round of major surveys, benefitting from accumulated experience. Or perhaps we need sophisticated studies of applied science that draw implications for basic research where appropriate. Or maybe we should survey the health of basic research explicitly and as a matter of public record in the federal budget process, building upon a broader informational and political base in that effort than has been the practice.

It bears repeating in closing that with such endeavors as these surveys, process may well be as important as product.

References and Notes

1. The reports were published by the National Academy of Sciences, Washington, D.C.
2. Committee on Science and Public Policy, *Basic Research and National Goals* (National Academy of Sciences, Washington, D.C. 1965).
3. President's Scientific Advisory Committee, *Meson Factories*, H. Berthe, chairman (Government Printing Office, Washington, D.C., 1965).
4. Committee on Nuclear Science of the National Research Council, *Nuclear Science: A Survey of Funding, Facilities, and Manpower*, P. D. Parker, chairman (National Academy of Sciences, Washington, D.C., 1975).
5. Astronomy Manpower Committee of the Committee of Science and Public Policy, *Employment Problems in Astronomy*, L. Goldberg, chairman (National Academy of Sciences, Washington, D.C., 1975).

6. The research for this article was done in the context of the U.S.–U.S.S.R. Joint Science Academies Study of Policy for Fundamental Research, to which I make grateful acknowledgment. This article is not an official publication of that project. I completed the review while a research fellow of Harvard University's Program for Science and International Affairs.