

What Criteria Should Be Used to Establish Funding Priorities?

D. Allan Bromley

When I served as director of the Office of Science and Technology Policy and the assistant for science and technology to President George H. W. Bush (1989–93), the administration often needed to deal with funding priorities for science and technology. It's essential that a committee or panel recommending funding base its decisions on some fundamental criteria for making scientific choices.

Forty years ago, Alvin Weinberg, then director of Oak Ridge National Laboratory, pioneered such an analysis (see PHYSICS TODAY, March 1964, page 42). He formulated a scale of values that might help establish priorities among those scientific fields that were supported by the government. In particular, he distinguished between two kinds of criteria: internal and external. The internal criteria, Weinberg wrote, are generated within the scientific field itself. Two such criteria are: Is the field ready for exploitation? And are the scientists in the field really competent? But if scientists expect society to support their work that meets these internal criteria, they must seek external criteria, that is, those external to science.

Weinberg identified three external criteria: technological merit, scientific merit, and social merit. If a certain technology is deemed valuable, society needs to support the scientific research required to achieve that technology. He asserted, "... That field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighboring scientific disciplines." Social merit is a more difficult thing to identify, but Weinberg singled out one example—science can help foster international understanding and cooperation.

In 1969, I was invited to chair the Physics Survey Committee of the National Academy of Sciences (NAS) and the National Research Council (NRC). We were asked to identify the opportunities that would be available to physicists during the 1970s. Weinberg

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was also on the 17-member committee. The heart of our report, published in 1972, attempted to address the tough questions of priority, program emphases, and levels of support. After looking at other attempts to develop criteria, we came up with our own set, which we divided into three classes: intrinsic, relating to the internal logic of a science and its fundamental bases; extrinsic, relating to the science's potential for application to other sciences; and structural, concerning available scientists, instrumentation, and institutions, as well as questions of opportunity and continuity. We then used the three classes as a basis for priority decisions.

Even earlier, Viki Weisskopf (PHYSICS TODAY, May 1967, page 23) had recognized a crucial aspect of the entire federal investment process—that scientific fields were becoming ever more interdependent. He found that the interdependence coupled the intrinsic and extrinsic Weinberg criteria. Plotting scores for one against the other makes it clear that optimal progress results when the scientific enterprise moves along a front, filling the intrinsic–extrinsic plane and making rough angles of 45° with both axes (see the figure on page 55). This optimization ensures that there will be no gaps or tears in the fabric of the overall scientific enterprise.

In recent years, Harold Varmus, former director of the National Institutes of Health, has emphasized this interdependence and its growing importance. He has noted that progress in the health and life sciences over the past five years has been very much driven by breakthroughs in chemistry, physics, and engineering.

Decadal surveys

As science adviser, I received a number of subsequent reports on physics and could read them with the unique perspective of having led such a so-called decadal survey myself. Other physics survey committees were formed: They were chaired by George Pake (1966), William Brinkman (1991), and Thomas Appelquist (2001). In astronomy and astrophysics, the National Academy

decadal survey committees were chaired by Albert Whitford (1964), Jesse Greenstein (1972), George Field (1982), John Bahcall (1991), and jointly by Joseph Taylor and Christopher McKee (2001). More recently (2002), Michael Turner chaired a committee that addressed the growing overlapping challenges between elementary particle physics and cosmology.

The reports on astronomy and astrophysics made clear-cut statements of priorities, in part because the smaller number of active scientists in the field allowed a consensus to be reached.

State your priorities

Such statements of priorities in a major field present arguments for continued or increased investment in those priority areas, so they are of enormous importance and value to the decision makers in Congress and the administration. The federal government has not always followed the priority recommendations in the precise order given in the reports because the reports are only one of several inputs to budgetary decisions. Over a 40-year period, however, the government has indeed followed rather closely the priority ordering in astronomy and astrophysics, funding projects from the front of the queue while each new report added new projects to the back of the queue.

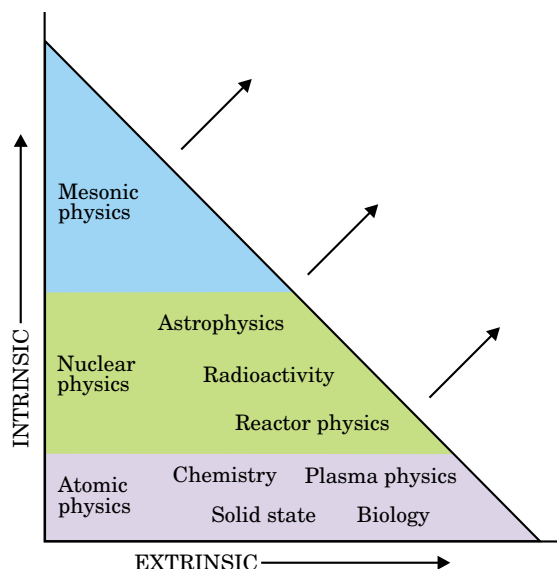
However, there is a danger in such a priority ordering: After decades of success in moving the queue smoothly forward toward success, the astronomy and astrophysics community has developed an entirely understandable reluctance to change items partway through the queue. Freezing an intermediate priority leaves no room for changes and surprises, such as the discovery of accelerating expansion of our universe, the nonzero mass of the neutrino, and the existence of dark matter and dark energy—whatever they may be. The Bahcall committee was clearly aware of this risk of freezing intermediate priorities, and the Taylor and McKee committee took great care to avoid this danger. Only a year after that

committee produced its report, the Turner committee was established to consider whether, in the light of major new discoveries, the midlevel items in the queue deserved further examination. Such sensitivity to the needs of those in Congress and the administration who are charged with broader funding decisions has served the astronomy and astrophysics communities extremely well.

When the Whitford and Pake reports were prepared in the mid-1960s, federal investment in science and technology was generally increasing at roughly 20% per year as part of the somewhat hysterical response to the Soviet launch of Sputnik 1. In that atmosphere, it's not surprising that both reports predicted future federal investment that turned out to be wildly unrealistic by the end of the 1960s, when the federal investment surge ended abruptly. The NAS realized that more realistic projections were needed, so it established the Greenstein and Bromley committees.

The Bromley, Brinkman, and Appelquist committees (and the later astronomy and astrophysics survey committees) all convened panels of experts to advise the committees. The Bromley report, published in six volumes, was the most extensive of the entire series of surveys formed by the NAS. It was also unique among the physics reports because it explicitly addressed the priorities issue and provided priority listings of some 69 subelements of physics. Clearly, the larger the field, the more difficult the question of priorities becomes. Even though 200 physicists participated in the Bromley survey, we never claimed that the published priorities represented a consensus of the entire community.

Although I can personally attest to the effectiveness of priority ranking as an approach to the making of enemies, our efforts were enthusiastically welcomed by people in Congress and the administration who were responsible for decision making in physics. I can also attest that those charged with budgetary responsibility greatly need help from the scientific community. When I was working as a physicist I read numerous field survey reports. For example, I thoroughly enjoyed reading the Brinkman report. But when I was in the White House, I didn't find such reports as helpful as I had hoped they would be



Viki Weisskopf's plot shows that science makes optimal progress when both intrinsic, and extrinsic criteria are used. (Adapted from V. F. Weisskopf, *PHYSICS TODAY*, May 1967, page 23.)

in the budgetary process. Everything in the Brinkman report and all such other reports sounded more than worthy of increased investment, not surprisingly, because it was written by enthusiastic proponents. The Appelquist report (see *PHYSICS TODAY*, November 2001, page 34) was engagingly written and should be read carefully by every working physicist and physics student.

I emphasize that, in the absence of explicit priority setting, all of the NAS-NRC decadal reports are significantly more interesting and valuable to the scientific community involved than to government officials. But watch for this growing danger: Because interdisciplinary and multidisciplinary activities are rapidly becoming more important in our society and are appropriately emphasized in the recent NAS-NRC reports, the fundamental core areas of science are increasingly being neglected.

Focus first on criteria

Because establishing priorities in as large and diverse a field as physics is extremely difficult, I have long urged that both survey reports and related governmental discussions should focus on the criteria that underlie the priorities rather than on the priorities themselves until the final budget decisions are made. Priorities can change dramatically and almost instantaneously, as they did on 11 September 2001, but the criteria estab-

lished before September 11th remained largely valid and in general have much more stability and longevity than do the priorities derived from them since that event. Were we to focus on criteria, we would be much less dependent on details and even on the scientific field under consideration.

I suggest a specific approach wherein the priorities in any field of science and between fields would be based on objective answers to the following questions:

- To what extent does the research have the potential of providing fundamental new understanding of our universe?
- To what extent does the research have the potential of affecting other areas of scientific research?
- To what extent does the research have the potential of leading to new generic technologies?
- To what extent does the research contribute to national security, economic competitive-

ness, or improvement in our quality of life?

- To what extent does the research hold promise of significant return on earlier scientific facility investments (for example, large telescopes, accelerators, or light sources)?

- To what extent is the research at or near the international frontiers of work in the field?

By answering these questions quantitatively with scores of 1 to 10 and summing the results, one attains a first approximation to a set of priorities. Further refinement can be obtained by agreeing in advance to assign different weights to the individual questions.

I and a great many others who have had to develop scientific components of presidential budgets and congressional appropriations have used similar approaches either consciously or unconsciously. However, if a group such as the President's Council of Advisors on Science & Technology were to provide their own set of such questions applicable to the entire spectrum of federal science and technology budgeting, those questions could provide a more coherent approach to the establishment of priorities than currently exists.

Since 1980, and perhaps even before, all US administrations have used the criteria-based approach either explicitly or implicitly, but unfortunately without much help from the scientific community. ■