Science needs both the big and the little

Herman Feshbach

Some time ago, a brilliant young colleague in the chemistry department met me by chance in one of the corridors at MIT. He had just seen the DOE particle-physics budget and somewhat aggressively said, "Wow, what I could do with all that money!" Contrast this with the complementary attitude of some particle physicists that one should not fund fields that are not at the forefront—an evaluation to be made, of course, by the speaker. Both attitudes, although understandable, are regrettable and in the long term destructive.

The arrogant pronouncement that some field is no longer interesting has been made often and has been wrong just as often. When I was a graduate student, atomic physics was considered to be dead. All important problems had been resolved. No one then had the soothsayer's ability to predict that most fundamental discovery, the Lamb shift, or the renaissance that the laser made possible. Take classical mechanics: Not interesting since the 19th century? Hardly. The problems of galactic formation, the structure of the rings around Saturn, the onset of chaos and turbulence are all recent exciting developments in classical mechanics. Fields don't die. They have cyclical renewals when new instruments or concepts open up new domains of experimental parameters for study.

"Big science" has been the subject of sharp attacks (for example, in recent letters to the editor of PHYSICS TODAY), with particle physics bearing the brunt, although other fields have or are beginning to develop big-science facilities. To some, big science means large expenditures for capital equipment. For others, its hallmark is the large size of the experimental teams.

Surely the first question we should ask about any field is, Is this field an essential component of our science? It's unlikely that anyone would deny that role to particle physics. The answer would be the same for most fields. Once we admit this, it follows that the support for each field should reflect the scientific opportunities in that field and the manpower available now and projected for the future. Each field has its own imperatives, which must be satisfied for it to remain healthy.

Some have voiced concern that support of one field of physics could result in support being decreased for others. This is a pernicious bit of nonsense: "pernicious" because that perception would lead to destructive competition among the fields for support; "nonsense" because in my experience, total science funding is no zero-sum game. Indeed one can easily argue that increasing support in one field may induce increased support in another. Happily the US has no official physics budget—our support comes from many sources. But zero-sum games do exist within each field. So it is important for each field to develop its own long-range plan, allotting support for big and small science as demanded by the field's scientific goals.

The large experimental teams of big science bring new problems to the university. Providing a proper educational experience for the graduate students on a team requires thoughtful planning. (See the article by Lawrence Jones and Malcolm Beasley on page 36.) The students relish the experience of being part of an important enterprise, but they may miss the on-campus interaction with their peers, especially those from other fields of physics. For their academic careers to prosper, the postdoctoral fellow and the assistant professor need to be more than cogs in a cooperative enterprise. Under the circumstances that prevail in big science, it is difficult to become known for important contributions or to document a case for tenure. Lack of attention to these problems can result in unacceptable damage to the young physicists on the experimental teams and to the field itself.

Some have argued that the fields we should support are those that can be justified by the benefits they bring to the support's source—the taxpayer. I wonder if those who disseminate this notion realize that exactly this argument has also been used by those who oppose the support of all of basic science. To be sure, some fields, such as materials science, have rather obvious connections with technology. Upon closer study, one finds that all fields of physics have a significant impact on technology, although I am not suggesting that the links are equally strong. Basic science, in its need to expand the ranges of the experimental variables under study, or because it must investigate altogether new variables, often cannot rely on state-of-the-art equipment. Physicists must often push beyond what can be purchased and in doing so may create new capabilities that may prove useful eventually to industry and medicine and sometimes may lead to a new technology. This push is not a new phenomenon, as advances in metallurgy in primitive societies were often made in response

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Reference Frame is a column of opinion written by a number of regular contributors, each an eminent physicist. This month's columnist is again Herman Feshbach, a nuclear theorist whose research in recent years has focused on nuclear reactions. He and the late Philip Morse wrote the classic text *Methods of Theoretical Physics*. Feshbach was chairman of the MIT physics department for ten years, President of The American Physical Society in 1980–81 and chairman of the Nuclear Science Advisory Committee in 1981–84.

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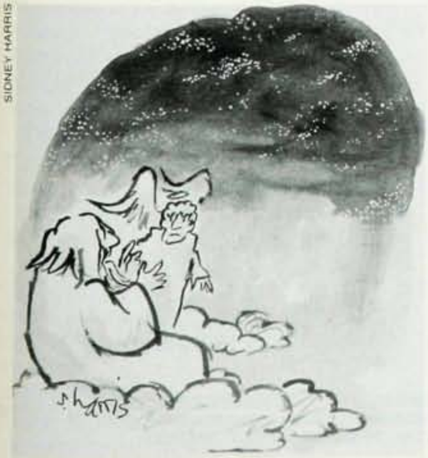
to religious or artistic needs. The craftsman went beyond the state of the art to create objects with properties dictated by dogma or taste and in doing so created new materials and discovered new techniques. Physicists' needs and discoveries may lead to the industrial production of new products and materials and to stricter production standards. I have witnessed such developments, which a CERN study made by H. Schmied a decade ago documents in great detail.

The demagogic complaint that money spent on science competes with aid to the underprivileged and hungry misses the real villain entirely. One should compare the funds spent on science and the funds allocated to the underprivileged with the cost of installing 50 MX missiles or a fleet of bombers. The cost of either could pay for many experiments or feed many children. Which of these is more important for national security? The whole budget for basic physical sciences, including even the Superconducting Super Collider, is a very small fraction of the defense budget.

The contest between big and small science is based on a false dichotomy. Such internecine quarrels will in the long run cause inestimable damage. The allocation of resources to big and small science should be determined by the scientific needs of each field. This is the most important step, and one with which we can deal with some confidence. Each field must face independently the question of whether or not the required funding can be obtained, in view of the vagaries of the political process and the fluctuating programs of each of the supporting agencies. Let us work together, not against one another. □

SIDNEY HARRIS



"It was a lot easier to keep an eye on things before the Big Bang. Everything was all in one place then."