GOALS FOR THE FEDERAL ROLE IN SCIENCE AND TECHNOLOGY

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The arrival of a new Administration in Washington raises once more the question of setting priorities for science and technology. However, this may not be quite the right question. Setting priorities can be difficult without goals. If we don't have goals, if we don't know where we are going, it's hard to have a sensible discussion about the best way of getting there.

Experience has convinced me that when goals are agreed on, then sensible priorities can usually be reached. In corporate research it is necessary to deal with priorities between technologies all the time. For instance, choices need to be made between effort on logic and effort on displays; and even within displays, between the amount of work to be done on liquid crystal displays versus the cathode-ray tube. On these questions there are always conflicting views, but usually, after discussion, a reasonable conclusion is reached. This happens because goals are reasonably well understood, although they are certainly not precisely understood.

I am inclined to believe that a lack of agreed-on goals has complicated the discussion of scientific and technological priorities in the Federal government. So I will attempt to suggest some possible goals for various aspects of Federal government support for science and technology.

First, let's discuss support of basic science, especially support for individual investigators. This Federal policy of support emerged from the nation's experience during and immediately after World War II, when the achievements of scientists and engineers gave both politicians and the public a better understanding of the immense practical results that could flow from the application of scientific knowledge. "Knowledge is power" is hardly a modern concept, but what *is* modern is the tendency of many governments to support on a considerable scale work in the most basic science. However, behind that support there usually lies the expectation of concrete benefits for the nation. "Without scientific progress," Vannevar Bush wrote in 1945, "the national health would deteriorate; without scientific progress we could not hope for improvement in our standard of living or for an increased number of jobs for our citizens; and without scientific progress we could not have maintained our liberties against tyranny."[1]

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With a new Administration in Washington and the collapse of America's arch military rival, there is now the possibility of establishing goals for science and technology. This will enable us to set reasonable priorities and to stabilize support for scientific research.

Governments support research in different ways. The German government partially funds research in companies and underwrites, along with industry, the Fraunhofer and Max Planck Institutes, which pursue both basic and applied research. In Japan some research is conducted in laboratories connected with the Ministry of Education and the Science and Technology Agency, while the Ministry of International Trade and Industry and other government agencies sponsor part of cooperative research projects involving firms—though only a relatively modest amount of university research is funded by the government. The US government puts up slightly more than $12 billion per year for civilian basic research, more than half at and for the National Institutes of Health and lesser amounts funneled through the National Science Foundation, the Department of Energy and the Department of Agriculture.

By any sensible standard the US government's support of basic science and of the work of individual researchers has been enormously successful. The achievements of science have transformed the world and promise to continue that process. Consider, for instance, the transistor, an invention that grew out of decades of work on quantum mechanics leading to a whole new understanding of solids. Physicists, working in their own way, driven by their own sense of direction, eventually made possible these remarkable devices, which through the many uses of the transistor and the computer, have fundamentally changed the world. Clearly, no one setting out on that long voyage of discovery could have foreseen its enormous practical consequences. Another example is molecular biology, with all its remarkable revelations and the practical consequences for health and for industry that flow from understanding the functioning of living beings at the molecular level. And that understanding was the result of years of basic research.

When we seek to justify Federal grants to individual investigators to do basic research, we have an easy task. We don't need to look forward and speculate on the outcomes of particular research efforts; we only need to look back at the great history of this type of work. It has been an astounding success, whether measured in terms of understanding natural phenomena or improving the material wealth and living standards of the world.

Nevertheless, despite the success, there are obvious problems today within the basic research community. Researchers are experiencing high rejection rates at NIH and NSF because of increasing competition for available grants; students who see a life of chasing grants as increasingly unattractive are losing interest in pursuing careers in academic research. The long pipeline to the PhD and the short availability of jobs in universities and corporate labs are discouraging some of the country's best and brightest from scientific training.

There are many who say the answer to the problem of high rejection rates is simple: Just provide more money to fund the thousands of excellent research proposals that are turned down each year. Some argue that NSF and NIH should fund any good idea because the work is likely to be worth it. But others contend that government

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funding of basic science is rising steadily and that the rejection rate is in fact a self-fulfilling prophecy: The success of science has attracted an ever-increasing population of researchers who are claimants for the limited numbers of desirable jobs and research grants. So science has become a victim of its success.

The fact is that we do not know what is going on. We don't have the most basic model of the process of generating researchers. We don't even know how many scientists are working at any given time. In real life, what is happening is much more an uncertain and rather political process than it is a thought-out process aimed even roughly towards goals.

It is also unclear what we would do if we had a more realistic picture of what is going on. Would we create some goals for science and its practitioners? Would those goals be good for the nation and for science? Is it possible to articulate goals for basic science anyway, even if we are able to develop a clear picture of what is going on? The examples given above show the importance of undirected work. We know that in the long run the unanticipated happens—not as a quirk but as a pattern of great importance.

Despite these difficulties, we cannot avoid answering in some way the question of how much science is enough, even if we answer it, as we do today, by being spasmodically effective and often ineffective participants in the political process. We must answer or we will have others answer for us.

I believe that *as a nation we should establish a goal of being world class in all major scientific fields*. Such a goal has many rationales. It has an economic justification, because it puts this country in a position to react quickly when knowledge in a field reaches the point where it can have practical consequences. And that is true whether the work is done here or abroad. It has a military justification, because our military strategy is based on superior technology, not superior numbers, and that strategy continues to be valid, as the recent Persian Gulf War shows. Also, by embracing all fields this goal acknowledges the unpredictability of the usefulness of scientific knowledge.

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This goal would mean determining what it takes to be world class and deciding that we will support in a steady and ongoing way the researchers and the equipment needed to be at that level. While this sounds hard, it is in fact doable. It does not involve comparisons between different fields but only a comparison within a field with other efforts in the same field around the world. This can be done and indeed has been done in specific fields in the past. This goal could stabilize support and thus help to provide a productive life for those engaged in basic research.

While this sounds like an expensive national goal, I don't think it is. My guess is that we are spending today in less productive ways more than the amount needed to achieve this goal.

In this context I want to convey some thoughts about megaprojects. I will discuss two types: those that are real science and those that are often referred to as science, sometimes even justified as science, but are not science.

The real type includes the Superconducting Super Collider, various orbiting telescopes, such as the Hubble, and other scientific satellites and space probes, such as the Cosmic Background Explorer. A megaproject of either type has certain elements of natural support that an individual investigator lacks. It is intelligible, at least by comparison with the apparently pointless activities of basic research, and it often generates excitement. In addition, a megaproject brings money into one or more districts and states represented by members of Congress, and as such it is supported by the normal political process.

Megaprojects are also supported by the ongoing actions of a government agency such as NASA or the Department of Energy. These organizations, once started, are driven by many motives to continue to propose and then forcefully advocate a succession of megaprojects.

It is only fair to observe that the support of basic research by agencies such as NSF and NIH is helped by some of the same institutional factors that drive NASA and DOE to produce a series of megaprojects. It is a characteristic of our Federal system that funds appropriated by Congress and spent by the different agencies are not compared.

Often the type of megaproject I am discussing provides good science. Notwithstanding, we need to ask some questions about that amount. After all, the roughly $2 billion per year spent on space probes compares with the total amount that NSF dedicates to individual investigators. The historic record indicates that support of individual investigators has been far more productive. The national goal given above would suggest that we could deal better with scientific megaprojects by incorporating their costs into the relevant scientific fields—astronomy or Earth sciences and physics—and making sure that this is the way we really want to spend money to obtain world-class standing in each field. I believe that with that goal in mind a sensible debate could ensue.

There are also the nonscience megaprojects. Here, too, space offers the best example. We should not forget that our space program originated in our military race with the Soviet Union. Out of an extremely agitated national reaction to Sputnik came a political decision to put men on the Moon and return them to Earth. Almost a decade later, when we put astronauts on the Moon's surface, we knew we had surpassed the Soviets, not just settled the question of what the surface of the Moon is made of. One wonders why the space program should be so important today when the military rivalry with the Soviet Union no longer exists. Still, we are spending more money on the space program than the combined science budgets of NSF and NIH.

If we were to ask about the importance of the space program, we would get more than a single answer. We would be told, for instance, that the space program (a) is important science, (b) recruits talented people into science, (c) contributes to civilian technology and (d) yields innovative "spinoffs" in the form of commercial products. All of these explanations involve science and technology. They all possess small elements of truth. But it is clear, at least to me, that they come nowhere near justifying a price tag of $14 billion per year on the basis of science and technology contributions.

We might also be told—and here I think we are closer to the truth—that the unmanned exploration of space—and possibly the eventual colonization of space—is a national goal in itself, quite independent of science. But if it is a national goal, with the intention to explore and settle space, then we ought to articulate that goal and to debate it, rather than to obscure it with fanciful justifications of science discoveries and technological wonders. And if we decide to shoot for the Moon, we should go for it at a measured pace that is no longer pushed by a race with the Soviets.

In contrast to basic science, space projects—whatever their rationale—do not live up to the expectations for providing either new knowledge or new technology in the absence of the US–USSR rivalry. For this reason, we need to clarify our goals for a space program. Clearly, no science could justify the huge bill the government pays NASA. If the goal is the exploration of space by humans, then we ought to engage in a public debate about the worth and pace of that effort.

Finally I want to discuss goals for the government's involvement in technology and its involvement with industry.

In recent years the US has finally come to realize that science alone does not guarantee industrial leadership. To be competitive in world markets takes a combination of science and technology plus rapid commercialization of new ideas, high-quality production and improvement of existing products. Competitiveness is far more than innovation, although innovation is a word often used. *Time* magazine some time ago ran a cover story on industrial competitiveness under the headline "Innovation in America"—as if technological innovation and industrial competitiveness were synonymous.

The confusion between technological and scientific progress and industrial leadership reached a high point in 1987 when high-temperature superconductivity was discovered by IBM scientists in Zurich, Switzerland. The US government's reaction was immediate and intense. President Reagan showed his enthusiasm for the subject by being host to a daylong conference on the subject in a Washington, DC, hotel ballroom. There was strong sentiment expressed by many at the conference that we shouldn't let the Japanese get ahead of us again with "our" (actually Swiss and German) ideas.

Behind all this, the belief that developing new technologies into marketable products is the issue: We have the innovative ideas but others commercialize them before we do. If commercializing new technologies were really the problem, it would be relatively easy to apply a science and technology policy as a substitute for an industrial policy, and this would be simpler because industrial policy is a much more complicated and controversial subject. However, the US has not had an innovation problem to date, nor has the problem been technological commercialization. The industries that make up the balance-of-payments deficit are textiles, automobiles, semiconductors and consumer electronics. I cannot comment on textiles because I know nothing about the industry, but the problems of the other three have little to do with innovation, and everything to do with quality and manufacturing.

These are not industries in which we have had the bold ideas and companies in other countries commercialized them. They are all industries where we commercialized the original ideas and achieved a strong position in the market, but lost out later to products with superior quality, lower production costs and rapid improvement cycles. It is higher quality, lower costs and development speed that have been the real strength of the competition, rather than the much more publicized advanced technology efforts of MITI. Until we face up to our real difficulties, we are unlikely to make real progress.

In this area, too, we need to set a clear goal: *to contribute to the nation's industrial competitiveness through science and technology.* It is essential to determine, in close cooperation with industry, in what industries and in what ways science and technology programs funded by government can actually contribute to making our commercial products world class. We need to work back from the national goal of industrial competitiveness, rather than forward either from the latest scientific event or from a largely government-originated idea of what matters. There will be conflicting views and much debate before a sensible outcome emerges. The result will likely be a mix of the old and the new, of high-tech developments and production-line methods.

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It is my belief that in both basic science and government support of industrial technology, we can set goals, and then and only then, we will be able to decide on priorities. If we do agree on our goals, we will be able to establish priorities for projects that will last and will provide outcomes that benefit all of society.

References

1. V. Bush, "Science—the Endless Frontier," report to the President (1945), reprinted by Natl. Sci. Foundation, Washington, D.C. (1990).