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The Government Role in Science and Technology

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So *pleasant* a setting and such a pleasant occasion make it hard for me to get my heart into being a Cassandra and to give a speech that is mostly about confusion, lack of purpose, and the many *unpleasant* problems that face science today.

It is particularly hard to talk about the more negative side of science when one is aware, as I am, of the tremendous positive role of science and technology in making this a better world. The contributions of our various disciplines are so great and so transforming that it is hard at times to believe that there is any way this can come out other than positive. Nevertheless, there are real problems, though in my opinion not unsolvable ones.

We do see that science is not sought after by American students, that younger people are having a hard time starting a scientific career, and that there is difficulty in supporting even the most distinguished researchers. Money for science, which once seemed limitless, seems today to be very finite, there are all sorts of rationales being brought forward for more money, and all sorts of thoughts for setting priorities for the money there is.

All this fuels debate within science itself, between big science and little science. Some people will assert that any good science should be funded, others will assert that you can't have priorities in science, so the picture that emerges is confused and priorities go on being set not by rational thought, but simply by the march of events. Consequently, we sometimes

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end up not supporting really good research while we are still firing billions of dollars into space.

My talk is, however not about priorities, but rather about goals. My message is that it is very difficult to set priorities if we don't have clear goals. If we don't know where we are going it is hard to have a sensible debate about the best way to get there.

On the other hand, my long experience as a director of research has convinced me that when there *are* goals, sensible priorities can in fact be reached. In industrial research we deal with priorities between science and technology, within technology between logic and memory and displays, and within displays with gas panel vs. the cathode ray tube. On these questions there was always conflict and different views, but usually, after discussion, a reasonable conclusion could be reached. This in the presence of fairly well understood, but not precisely understood, goals.

I am inclined to believe that in reality a *lack of agreed-on goals* has complicated the scientific priorities discussion, so I will attempt to suggest some possible goals for various aspects of Federal Government science support.

I will talk first about support of basic science, especially the individual investigator.

By any reasonable standard this has been enormously successful, and by far the most successful of the government roles. This policy of basic science support came from the post World War II period, when the great achievements of scientists during the war, for example the atomic bomb and radar, gave both politicians and the public a feeling, and in my opinion a correct feeling, for the immense power that could be unleashed by scientific knowledge.

And this thought — science is power — which led to this policy of support, was in fact rewarded by scientific successes that have transformed and continue to transform the world.

One example is the transistor, an invention that grew out of the basic understanding of solid state physics, in the same way that the atomic bomb grew out of the understanding of the atomic nucleus; another is molecular biology with all its remarkable revelations and all its consequences as a technology.

When we seek to justify federal money spent on the individual investigator we have an easy task. We don't have to look forward and speculate, we only need to look back at a great history of success. And it is success, whether it is measured in terms of scientific progress or in terms of advancing the material level of the world.

Nevertheless, and despite that success, there are clearly problems today within the basic science community itself. Some of these were mentioned before. Researchers face high rejection rates at the science supporting agencies, such as The National Institutes of Health (NIH) and The National Science Foundation (NSF), a diminution of interest in science and engineering on the part of students, a long pipeline to the Ph.D., and some difficulty getting jobs at the other end of that long pipeline. So despite a remarkable record of success, *we may not be producing a reasonable way of life for scientists.*

In trying to understand what is going on and what to do about it, we immediately encounter confusion.

Some say the answer to the high rejection rate is simple: Scientists clearly do good, we should simply give them more money. Others say that the money spent on science has been in fact increasing steadily, and to increase it more under the present ground rules will produce an ever-increasing population of research scientists who will be claimants for the same limited number of desirable jobs, and provide still more competition for grants.

The remarkable fact is that in fact *we don't know* what is going on. We don't have the most basic model of the process of generating researchers. We don't know how many there are out there. We simply don't know what is happening today. As a result, what does happen is much more a political process than a thought out process.

What we would do if we had a clear picture is also uncertain. What would our goals be? Is it possible to articulate goals for basic science anyway, even if you have a clear picture of what is going on?

Most of us automatically reject goals that set specific aims for scientific subjects. But, as a country we could set goals in a different way. *We could have a goal of being world-class in most major scientific fields, while at the same time providing a decent life for those who pursue basic research.* We could list the scientific fields, see what it takes to be world-class, and try to get the means to support the needed researchers and their equipment and good equipment. We could estimate, debate, and work toward goals formulated that way. Today we don't have such a process, we don't have such a debate, and in addition we don't have reasonable data.

Basic science and the federal government support of it has really worked. It has undoubtedly benefited the world. We should keep going. But we should stop flying blind toward an unknown destination, for the good of the researchers themselves, as well as for the rest of the world.

Next I would like to say a few words about megaprojects. I will talk about two types of megaprojects: those that I call real science, and those that are often referred to as science, sometimes justified as science, but aren't science.

The real science type includes, as examples, the Superconducting Supercollider, various orbiting telescopes, and other scientific satellites and space probes.

Any megaproject has certain curious elements of natural support, which the individual investigator lacks. It is intelligible (at least compared to more general basic research) and exciting, and it spends money in someone's home district or home state, and as such it is competitive with other forms of home-district government spending.

Then there is the support through the ongoing actions of a government agency such as NASA or DOE. These are large (i.e., multi-billion dollar) organizations that are driven by natural desires for continuing their work to propose and powerfully advocate a succession of megaprojects. For example, the various *scientific* satellites of NASA cost a few billion a year.

It would only be fair to observe that the support of individual investigator basic research is aided by the same institutional factors or institutional autonomy of the agencies, such as NSF and NIH, that support that kind of work. It is a characteristic of our present system that the moneys spent in these different ways are not compared.

Often this kind of megaproject is good science. But the question is, is this the right way to prioritize and spend our science money? After all, two billion a year on space probes compares with the total amount that NSF spends on individual investigators. And historically, the individual investigator has been far more productive.

Perhaps we could deal better with *scientific* megaprojects by not dealing with them as stand-alone projects, but by *incorporating their cost into the relevant scientific fields*, astronomy, or earth sciences, or physics, and making sure that each megaproject is part of the way we want to spend money to obtain world-class standing in that field. In this way we could get away from the abstract big science/little science debate, and discuss rather within each field what the proper balance is to be a world-class player. I believe that with that goal in mind a sensible debate could ensue.

Then there is the nonscience megaproject. Space is the best example. although there is also the National Aerospace Plane.

The space program originated in our race with the Soviets. Who can forget the extreme national reaction that greeted Sputnik? Edward Teller, in his usual picturesque way, asserted that we had suffered a defeat worse than Pearl Harbor. Out of this disturbed national atmosphere came a political decision to put men on the moon. And we did put people on the moon, and we did it to surpass the Soviets, *not* to settle the question of what the surface of the moon looks like.

We could wonder, given this capsule view of the origins of the space program, whether such a program is necessary today, when the rivalry with the Soviet Union is so diminished. After all, we are spending more money on the space program than the combined budgets of three NSFs and one NIH.

If we did ask that question we would get more than one answer. We would be told, for example, that the Space Program is (a) important science, (b) that it recruits people into science, and (c) that is contributes to civilian technology.

These explanations are all science- and technology-oriented, and they are all somewhat true, or slightly true; but it is clear, at least to me, that they come nowhere near justifying a 14 billion dollar a year price tag on the basis of science and technology goals.

We could also be told, and here I think we are closer to the truth, that the human exploration of space, and perhaps the eventual settling of space by people, is a national goal in itself, quite independent of science. But if it is a national goal, to explore or settle space in this way, then let us articulate this goal, and debate it, rather than obscuring it with scientific justification. And, if we accept this national goal, let us also decide to pursue it at a proper pace, which would not necessarily be the pace appropriate to a race with the Soviets.

In contrast to basic science, space, whatever its rationale, doesn't work, or more accurately it doesn't work or perform some obvious useful function now, in the absence of an intense Soviet-American rivalry. For this reason we need to clarify what we are doing. There is no science that could justify the enormous bill, and if the goal actually is something else, like human exploration of space, let's talk about that and about the proper pace and the right rate of expenditure.

Science in Support of National Goals such as Industrial Competitiveness

In the U.S. in recent years we have graduated from the idea that science alone guarantees industrial leadership to the idea that science and technology plus the rapid commercialization of new ideas are what matter. Innovation is now an important word. *Time* magazine had a special issue on industrial competitiveness. It was entitled "Innovation in America," almost as if innovation and industrial competitiveness were synonyms.

The federal government is moving from a position of supporting only basic science to a position of supporting "generic" or precompetitive technologies. Lists of key technologies abound, coming from both government and private sources. The implication of all these lists is that these are the technologies that are the keys to competitiveness.

A striking example of this emphasis on advanced technology occurred a few years ago when high-temperature superconductivity appeared on the scientific scene. There was a major government reaction. There were public meetings with the President attending to discuss the subject of superconductivity. There was very strong sentiment that, in this area, we couldn't let the Japanese do it to us again. Though somewhat less extreme, there was a similar reaction to the Japanese fifth-generation computer plan, which in fact produced a worldwide, as well as an American, reaction.

Behind all this is the thought that getting new technologies into products is the issue; we have ideas, but others commercialize them. *If new technology commercialization were really the problem*, it would be very convenient, because it would allow us to use a science and technology policy as a substitute for an industrial policy, and industrial policy in a broader sense is and has been a complicated and questionable subject in the U.S.

Unfortunately this new technology view of the problem flies directly in the face of the facts. The U.S. has not had an innovation problem to date, even in a commercialization sense. The industries that make up the balance of payments deficit are textiles, automobiles, semiconductors, and consumer electronics. I will not comment on textiles, as I know nothing about them, but the problems in the other three have had little to do with innovation and everything to do with manufacturing.

These are not industries where we have had ideas and others commercialized them; they are all industries that did commercialize the original ideas and had a strong position in the grown-up industry itself, but later lost that position to competitive products with superior quality, lower manufacturing cost, and to competition having a rapid development cycle leading to rapid incremental improvement in the product.

To date, quality, speed, and manufacturing have been the real strength of the competition, rather than the much more publicized MITI advanced technology efforts, and until we face that reality we are unlikely to make progress.

In this area, as in the others, we need to see a goal — the goal of contributing to American industrial competitiveness through science and technology. We then need, in close cooperation with industry, to discover what science and technology programs will contribute to giving us competitive industry. We need to work back from the competitiveness goal, rather than forward from the latest scientific event. There will be different views and discussions, but I believe a sensible outcome would emerge. The result will likely be a mix of the old and the new, of high tech and of manufacturing technology. But it will be more likely than what we do now to help competitiveness.

In sum, I believe that in all three areas that I have discussed, the individual investigator, megaprojects, and science in the support of industrial competitiveness, we can set goals. If we do this we will be able to set priorities, and if we do not set goals we will not be able to set priorities in a sensible way and in a way that can last.