The U.S. Government's Role in Science & Technology

R. E. GOMORY

The Alfred P. Sloan Foundation

What I have to say about the U.S. government role in science and technology is personal and non-systematic. There will be no budget charts and very few budget numbers.

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 was a fruit of the post World War II period, when the great achievement of scientists during the war, for example those engaged in atomic bomb and radar research, gave both politicians and the public a feeling, and in my opinion a correct one, 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. I am thinking here of 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, or equally of 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.

But like many things that are very successful, success in science has brought its own problems and generated its own misconceptions. One is that scientific leadership is enough, that the rest, for example the design and manufacture of products, industrial leadership etc., that these things are relatively easy and will take care of themselves. Today we know that is not true, or at least things are not that simple.

Another problem, in my opinion, was a natural turning away from industry and its problems, even in the engineering schools, because the

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government money was both freer and more plentiful. And also in the engineering schools, there was in this period a tendency to emphasize science in engineering, to characterize engineering as applied science, which it isn't, and to turn away from the complexities of design and manufacturing, while stressing and awarding prestige to the more scientifically tractable elements of engineering problems.

Today we are learning from the Japanese the lesson that science is not enough and we must do other things if we are to be a significant industrial power—a lesson that was obvious to other nations looking at the U.S. before World War II.

In addition, within the basic science community itself, there are clear problems. There are high rejection rates at the science supporting agencies, such as NIH (the National Institutes of Health) and NSF (The National Science Foundation), 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. All this despite a remarkable record of success.

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. We should fund any good idea because it's worth it.

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.

I will not disguise my own belief that we are witnessing a phenomenon of OVER not UNDER production despite the cries of alarm about the projected shortages of scientists and engineers, but it is also true that is only my belief, we don't know. We have no models, little data and no basis for acting in a rational way. What does happen is much more a political process than a thought out process.

What we would do if we had a decent picture is also unclear. What would our goals be? Is it possible to articulate goals for basic science 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 perhaps, as a country we could have a goal of being world class in most major fields, AND at the same time provide a decent life for those who pursue basic research. Then we could see what it takes to do this, and try to get it. We could then estimate, debate, and work toward such goals. But today we don't have such goals or such a process.

Basic science and the federal government's support of it has really worked. It has undoubtedly benefited the world. We should keep going.

But we should stop flying blind, for the good of the researchers themselves as well as for the rest of the world.

Support of 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.

Of the real type I would list for example, the Superconducting Supercollider, various orbiting telescopes, and other scientific satellites and space probes.

Any megaproject has certain curious elements of natural support that the individual investigator doesn't have. 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.

There is also what I would call support through misunderstanding. Some years ago I visited CERN and asked its director why the various governments that supported CERN did so. He replied that in many cases they thought that power, by which I mean a new source of power, would result. This remark may be a little exaggerated, but much support can be characterized as naive if it is not an outright misunderstanding.

Then there is the support through the on-going actions of a government agency. Some years ago I was fortunate enough to visit one of our space centers. This center launches real scientific space probes that cost a few billion a year. I had the opportunity to hear scientists' thoughts, and how they justified what they did.

Their views were both prosaic and realistic. It was essentially that they could do anything they wanted to if they could get the support of a few key people on congressional committees and at the OMB (Office of Management and Budget). These excellent people were matter of fact about it. They had a lot of good things they wanted to do, and they set about doing them year after year. They dealt with those with whom they needed to deal with to do them. Now this is not exactly peer review, and let me remind you this is billions, not millions, but this is really the way it works.

It would only be fair to observe that the support of individual investigative basic research is aided by this same institutional inertia or institutional autonomy of the agencies 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.

But really the answer to this question, is this the right way to prioritize or spend our science money, depends on the whether or not that money really is available for other uses in science and technology. People will debate endlessly the question of whether the money for megaprojects is extra money, money that eventually will come at least in part out of other science spending, or even, as some will maintain, that it is money that attracts more money, not less, to other parts of science.

Whatever it may have been in the past, I think we are today definitely moving into an era of tight budgets and firm lids on discretionary spending. It will help us, in this new era, to know where our priorities are. The megaprojects will not be free, in my opinion, whatever they may have been in the past. And, in thinking of the future, we should remember the fundamental political appeal of the megaprojects. While support of the individual investigator is somewhere on everyone's list, megaprojects can come out near the top.

Then there is the non-science 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 all added up.

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) it recruits people into science
c) it contributes to civilian technology

These explanations are all science and technology oriented, 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.

We could also be told, and here I think we are closer to the truth, that the manned 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 that was once 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 justification is something else let's talk about that and about its pace and rate of expenditure.

We should remember that megaprojects of both types, because of their out year growth and their history of costing far more than they are originally projected to, may well have a negative effect on the future of more productive science.

Science in Support of National Goals such as Industrial Competitiveness—Weapons—Environment—Energy—Education

I will confine myself to talking about government efforts to support industrial competitiveness, since I know little about the others.

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 synonymous.

The federal government is moving from a position of supporting only basic science support to a position of supporting "generic" or precompetitive technologies. Lists of key technologies abound, coming from both government and private sources (like the Council on Competitiveness). 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 plan, which in fact produced a worldwide as well as an American reaction.

Behind all this is the thought that getting new technologies into product is the issue; we have ideas but others commercialize them. *If it were true* this view would also be very convenient, because it allows one to use a science and technology policy as a substitute for an industrial policy, and industrial policy is a complicated and questionable subject in the U.S., given both the political scene, and more importantly, the history of fundamental abilities and inabilities of the American Government.

Unfortunately this (science and technology) view flies directly in the face of facts: the U.S. has not had an innovation problem to date, even in a commercialization sense. The industries which made 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 in which we have had the ideas and others have commercialized them. These are all industries in which we did commercialize the original ideas and had a strong position in the grown up industry itself, but later lost our position to competitive products with superior quality, lower manufacturing costs, 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.

Connected with this emphasis on science and technology is an outcry about the poor state of the nation's schools, especially the poor test scores in science and mathematics. Once more these scores are taken as a symbol of, or a predictor of, national power. If you don't have good test scores in math and science, you won't have national competitiveness.

Clearly good education helps any country, as does having advanced technology, but the *exclusive* focus on these aspects of the competitiveness problem, or using them as a surrogate for the more complicated reality will, I think, lead to disillusionment in the long run.

Let me illustrate this with the Sputnik example. Some of us can remember the charts we were shown in the Sputnik era about Soviet science and engineering education. About how much calculus the Soviets learned in high school and the large numbers of engineers they produced, both in absolute numbers and per capita. Nevertheless we know now that didn't give them industrial competitiveness.

Today we see the same kinds of numbers for the Germans and the Japanese but never for the Russians. Indeed if the Russians were included, the simple cause and effect relationship that is implied by these comparisons would be shattered and we might have to look elsewhere, or at least look somewhere else in addition, for answers to the competitiveness problem.

It is far from clear what we should do in this complex area of industrial competitiveness, but we will have to be very lucky to make progress with the present set of ideas.

This same comment, that we will have to be very lucky to make progress with the present set of ideas, holds to a greater or lesser extent in all three areas that I have discussed, the support of basic science, the megaprojects, and science and technology for industrial competitiveness.

It is far from clear what we *should* do in any of these areas in the end, but I do think there is ample evidence that we should be thinking hard and in new ways about the U.S. government's role in science and technology.