Understanding the Bush Report – *Harvey Brooks*

The debate that was launched by the original Bush report and its rival report, the Kilgore Plan, has roots that go back to the debate between J. D. Bernal and Michael Polanyi in Britain from the 1930s until the late 1950s. This is perhaps a somewhat over-simplified analogy, but nevertheless worth mentioning.

Then as now, the debate concerned the degree to which it is feasible and desirable to plan the agenda for the national science and technology enterprise in terms of explicit societal or economic goals. Polanyi stressed the need for autonomy and self-governance of the scientific community if it were to contribute most efficiently to societal goals in the long run. His view may be most succinctly summarized in the following quotation from the sociologist of science, Bernard Barber, in something he wrote in the 1960s.

> "However much pure science may eventually be applied to some other social purpose and the construction of conceptual schemes for their own sake, its autonomy in whatever run of time is required for this latter purpose, is the essential condition of any long-run applied effects it may have."
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> (Barber 1962)

In contrast, Bernal, who was strongly influenced by Marxist thought, was impressed with what he saw as the tremendous inefficiencies of autonomous science. He believed that its enormous potential benefits for humanity could only be realized through a publicly discussed and debated flexible plan involving government and many representative elements of society. This same debate essentially has been reflected in all the subsequent debates about national science policy.

It is by now a truism that World War II was a watershed, particularly in the U.S. and, to a lesser extent, in Britain and Europe. For example, in 1935 the U.S. federal government contributed only 13 percent of total national expenditures for research and development, which constituted only 0.35 percent of the national income. By 1962, the federal contribution to this total had risen to nearly 70 percent, with the aggregate being more than 3.3 percent of the national income, an approximately 10 order-of-magnitude increase.

In the 1930s, federally-supported research and development was mostly conducted at in-house, civil-service laboratories, which accounted for about 0.25 percent of the federal budget. This figure rose to 11 percent by 1962, and represented probably more than 35 percent of the federal government's discretionary expenditures.

The imminence of World War II mobilized leaders of American science in advance of American participation in the war. And whereas technical advances in World War I had been generated largely from existing military needs as defined by the military, many of the World War II advances were born in the laboratory, almost as solutions looking for problems. Their military application evolved as military strategy and technology were developed in tandem, with scientists and the military in equal partnership, but with the civilian agency Office of Scientific Research and Development (OSRD) – headed by Vannevar Bush – able to make decisions independent of previously specified military needs. Scientists eventually were able to persuade soldiers to inform them of the general military problems involved, so that the scientists might reach their own conclusions about the kinds of weapons and devices the military would need to meet those problems.

Unlike the situation in World War I, science in World War II was mobilized under civilian tutelage, with the leaders of the scientific community having direct access to the President and to the Congressional Appropriations committees – if necessary, over the heads of the military, although in practice this privilege was seldom exercised.

The experience of World War II had a profound impact on both the political and scientific leadership, and crucially influenced the position of science relative to government after the war. The war-time experience convinced Bush of the importance of an independent role for scientists in an equal partnership with government. It was the fountainhead of his report, *Science: the Endless Frontier* (1945).

The essence of that report was contained in the following eight recommendations and five general principles.

The first recommendation: “Science, by itself, provides no panacea for individual, social, and economic ills. It can be effective in the national welfare only as a member of a team, whether the conditions be peace or war. But without scientific progress no amount of achievement in other directions can insure our health, prosperity, and security as a nation in the modern world.”

Second, “It is clear that if we are to maintain the progress in medicine which has marked the last 25 years, the Government should extend financial support to basic medical research” – that is, the 25 years before the report was written.

Third, "Military preparedness requires a permanent independent, civilian-controlled organization, having close liaison with the Army and Navy, but with funds directly from Congress and with the clear power to initiate military research which will supplement and strengthen that carried on directly under the control of the Army and Navy." It is sometimes said that Bush envisioned that all military research would be conducted under a kind of a overarching Department of Science. That was never envisioned, as this recommendation makes clear.

Fourth: "Basic scientific research is scientific capital. Moreover, we cannot any longer depend upon Europe as a major source of this scientific capital. Clearly, more and better scientific research is one essential to the achievement of our goal of full employment."

That fourth principle most clearly embodies the idea of basic research as the prerequisite for technological innovation. There are two rather different views of this. One is that specific ideas emerging from basic research are the inspiration and source of technological innovation. The other is that the cumulative output of basic research is essentially a resource that can be mined by applied scientists and engineers for the purposes of innovation. It's my view that Bush held much more of the latter view than the direct-event connection.[1]

[1] This was used in a very controversial study called "Project Hindsight." It essentially showed that basic research contributed very little to the development of new weapons systems; however, the study used an event-tree analysis, which I think was a methodology inappropriate to the question.

The fifth recommendation in the Bush report was, "If the colleges, universities, and research institutes are to meet the rapidly increasing demands of industry and Government for new scientific knowledge, their basic research should be strengthened by use of public funds."

Sixth: "To provide coordination of the common scientific activities of these governmental agencies as to policies and budgets, a permanent Science Advisory Board should be created to advise the executive and legislative branches of Government on these matters." This function apparently was originally envisioned for the National Science Board. However, it became unrealistic so long as the National Science Foundation budget constituted such a tiny faction of the total federal support of scientific research, as it did through most of its early history.

The seventh recommendation: "The Government should provide a reasonable number of undergraduate scholarships and graduate fellowships in order to develop scientific talent in American youth. The plans should be designed to attract into science only that proportion of youthful talent appropriate to the needs of science in relation to the other needs of the nation for high abilities." This was a sort of foretaste of the G.I. Bill and was perhaps the most significant and practical initial outcome of the Bush report.

And the final recommendation: "A new agency should be established, therefore, by the Congress, devoted to the support of scientific research and advanced scientific education alone….The agency to administer such funds should be composed of citizens selected only on the basis of their interest in and capacity to promote the work of the agency. They should be persons of broad interest in and understanding of the peculiarities of scientific research and education." This last phrase recurs throughout both the Bush report and through many of the subsequent discussions.

Those were the eight recommendations of the Bush report. There were also five principles which must underlie the program of support for scientific research and education. Bush set these down in the following terms:

First, the new agency “should have a stability of funds so that long-range programs may be undertaken.” Second: “The agency to administer such funds should be composed of citizens selected only on the basis of their interest in and capacity to promote the work of the agency. They should be persons of broad interest in and understanding of the peculiarities of scientific research and education."

Third: "The agency should promote research through contracts or grants to organizations outside the Federal Government. It should not operate any laboratories of its own." This was a pretty flat-footed recommendation, which was followed both in the implementation of the National Science Foundation, and also in the implementation of the Atomic Energy Commission. It was followed to a considerable extent also in the early days of the Defense Department, at least for the support of basic research.

Fourth: "Support of basic research in the public and private colleges, universities, and research institutes must leave the internal control of policy, personnel, and the method and scope of the research to the institutions themselves. This is of the utmost importance.”

And fifth: "While assuring complete independence and freedom for the nature, scope, and methodology of research carried on in the institutions receiving public funds, and while retaining discretion in the allocation of funds among such institutions, the Foundation proposed herein must be responsible to the President and the Congress. Only through such responsibility can we maintain the proper relationship between science and other aspects of a democratic system. The usual controls of audits, reports, budgeting, and the like, should, of course, apply to the administrative and fiscal operations of the Foundation, subject, however, to such adjustments in procedure as are necessary to meet the special requirements of research."

I would like to also to add two other quotes from the Bush report, because I think they explain why he laid such emphasis on universities and independent research institutes.

First, from page 19:

> It is chiefly in these institutions that scientists may work in an atmosphere which is relatively free from the adverse pressure of convention, prejudice, or commercial necessity. At their best they provide the scientific worker with a strong sense of solidarity and security, as well as a substantial degree of personal intellectual freedom. All of these factors are of great importance in the development of new knowledge, since much of new knowledge is certain to arouse opposition because of its tendency to challenge current beliefs or practice.

And then,

> Industry is generally inhibited by preconceived goals, by its own clearly defined standards, and by the constant pressure of commercial necessity. Satisfactory progress in basic science seldom occurs under conditions prevailing in the normal industrial laboratory. There are some notable exceptions, it is true, but even in such cases it is rarely possible to match the universities in respect to the freedom which is so important to scientific discovery.

Bush's observation in this quotation seems even to be supported by the phenomenon which we have seen occurring in the last many years, of the gradual migration to academia of some of the most creative and productive scientists from those exceptional industrial laboratories that Bush apparently had in mind in that statement, such as the Bell Laboratories, the General Electric Research Laboratory, IBM Corporate Laboratory, and several other examples. It's not that these laboratories have not continued to make very important contributions, but apparently, there has been a tendency for a certain amount of migration out of these laboratories, which supports his observation.

Vannevar Bush wrote another report, which is not anywhere near as well-known as *Science: the Endless Frontier*, but is at least as enlightening with respect to Bush's personal view of the relationship between engineering and science, and between pure and applied science. It is called "The Report of the Panel on the McKay Bequest to the President Fellows of Harvard College" (Harvard College 1950). The following two quotes are taken from Section 4, entitled "Present Day Engineering and Applied Science." They clearly express that Bush's views were not quite as purist as has often been implied in recent interpretations:

> The borderline between the engineer and the applied scientist is becoming dim. It has never been clean-cut. An applied scientist is one who renders science useful. An engineer is one who utilizes science in an economic manner for man's benefit...The difference has, in the past, been mainly that the former starts as a scientist and seeks to apply, while the latter begins with the appreciation of a human need and searches out the science by which it can be met...Yet even this difference has been modified. Engineers, those who are really in the forefront of advance, are becoming more entitled to be recognized as scientists in their own right... Applied scientists, under the pressure of war and its aftermath, have often become accomplished engineers as well.

You can see the influence of Bush's war-time experience in that statement.

There was an interesting phenomenon in the World War II scientific effort. It occurred in the radiation lab and the proximity-fuse lab, and was particularly obvious in the Manhattan Project: the leaders of those civilian efforts came, by and large, from backgrounds in nuclear physics. Nuclear physics at that particular time was a subject which involved very much of a cross between science and engineering, since the engineering and apparatus of nuclear physics was a very important part of the whole enterprise. Contrary to the popular wisdom about theoretical scientists, many of the people who led the effort in the radiation lab, the radio-research lab at Harvard, and the Manhattan Project were people who, in their practice of basic science, had experience in many ways quite typical of engineers. That was particularly true at that time in the history of the development of physics.

The second quote provides quite a contrast to some of the statements that have been made about *Science: the Endless Frontier*:

> A science such as physics, or chemistry, or mathematics is not the sum of two discreet parts – one pure, and the other applied. It is an organic whole, with complete interrelationships throughout. There should be no divorcing of applied science from its parent systems...Certainly whatever the organization, there should be a community of interest, a vigorous interchange of ideas and students within the department of mathematics and the applied mathematicians, and the applied mathematicians of whatever stamp who are operating directly in the field of applied science and engineering.

This same principle should apply elsewhere. My view of the relationship between engineering, science, and the research enterprise is that it is divided the into two parts: not science and technology, or pure and applied, but rather opportunity-oriented research and need-oriented research, where "need" refers to social need and "opportunity" refers to both scientific and technological opportunity. These are generally identified with science and technology respectively, but that's not a complete identification. These relations have been profoundly transformed. However, they still represent two parallel streams of intellectual evolution, but with increasingly frequent and more profound cross-fertilization and interdependence. Both agendas have severe limitations when pursued single-mindedly, and these limitations can only be overcome by pursuing both types of agenda in parallel with ever-increasing opportunities for cross-fertilization.

The limitation of the opportunity-oriented approach is that the potential applications of the resulting knowledge are usually spread over a very wide spectrum of societal problems, and highly dispersed in time. Many applications and their timing are unforeseeable when the research is first undertaken. On the other hand, the limitation of focusing too narrowly on the presently formulated or foreseen societal problems lies in the fact that the very definition of these problems may often depend on knowledge not yet discovered.

Also, the knowledge produced by the opportunity-oriented approach tends to be cumulative and can only be created if pursued in the right logical sequence, making it impossible to produce needed knowledge on demand just at the time the need for it first becomes apparent in connection with the solution of the societal problem.

Because of these issues of timing and problem-specificity, the two types of knowledge are most sufficiently pursued in parallel, in an appropriate mix and with continual but deep interchange between the two knowledge streams, each of which is cumulative in its own terms. And, of course, the technological branch is cumulative to just as large an extent as the science branch.

I suspect that the tighter and more frequent the interaction between the two streams of knowledge, the greater the importance of the opportunity-oriented agenda relative to the society-oriented one, even while the latter absorbs and will continue to absorb the far largest fraction of resources.

Not only does the opportunity-oriented agenda more frequently enrich and make more cost-effective the pursuit of the need-oriented agenda, but also the societal agenda will more frequently spin off new intellectual challenges worth pursuing in the opportunity-oriented mode, beyond the needs of the immediate problem, for the sake of their contribution to the conceptual structure of knowledge.

Each of the parallel agendas will increasingly serve as triggering sources for the other in a more symmetrical fashion than has often been appreciated by the inhabitants of either branch of the scientific agenda.

And I might add, the inhabitants of the two branches of the technical agenda are not necessarily distinct classes of people, although they often may be. You find some people, like Edwin Land, who shift back and forth between one agenda and the other.

It is important to make note of the fact that the Bush report did not really recognize the extent to which the scientific agenda – that is to say, the opportunity-oriented research agenda – was often initially triggered by an applied problem, sometimes one that was very narrow initially. This is a legitimate criticism of the Bush report.

It is still important to look at the way such an applied problem is pursued. That is to say, it should be pursued, and ought to be pursued in much greater depth, with much larger ramifications than just the solution of the immediate problem.

An examination of the R&D budget in the U.S. since World War II shows the evolution of science policy during that time. Essentially, it can be divided into three eras. The first era is the Cold War era, which extends and rather abruptly ends around 1966 or '67 so far as R&D is concerned, even though this was the period of the build-up of the Vietnam War. In fact, there was a big de-emphasis on strategic weapon systems during that time.

From the period from 1966 to about 1975, there was an actual fall-off in federal R&D which amounted to about 17 percent in real terms. At the same time, there was a fall-off in university research in the physical sciences, which declined by about 14 percent. And even in the biomedical sciences there was no fall-off, but there was a level-off during that period.

For reasons which are not entirely self-evident, in about 1975 or 1976, there was a resumption of growth in the federal R&D budget, and it was spread over a considerably larger domain. There was also a dramatic increase in energy-oriented R&D from about 1974 to the early 1980s. But the most striking aspect is the rapid rise and continuous rise of privately supported industrial R&D, which continued right through the deep recession of the 1980s.

So, there were really three periods here. The first period was the Cold War period. The second, the period of the dip, might be termed the social-priorities period. During this time, there was an almost doubling of the amount of support for research in the social and behavioral sciences, although it never reached the extent it did in other fields. This was the period of the Great Society program.

It was followed, in the mid-1970s, by considerable disillusionment with the power of the social sciences to attack social problems, and by the gradual resumption of the Cold War military build-up, which began in the second half of the Carter Administration and accelerated during the subsequent Republican administrations.

It is interesting to note that the combined expenditures on defense, space, and nuclear energy never reached the peak, in terms of percentage of GNP, that they had reached in the 1960s. In fact, the build-up was much less rapid than the build-up that had taken place in the early part of the 1960s.

The other characteristic of the period after 1975, although it began considerably earlier and there were even signs of it in the late 1960s, was the increase in interest in economic performance. This was a change from the 1966 to 1975 period, where the priorities were public-sector needs, as formulated in the Great Society program.

After 1975, there was a rapid build-up of public concern about the declining international economic competitiveness of the U.S. especially vis a vis Japan, which became pronounced in the Carter Administration. That period ended in about 1986, and there has been a gradual shift whose exact nature I think we still cannot foresee, but is clearly a part of what is being debated now.

With the surge of the relative private investment in R&D accompanying the unprecedented prosperity of the late-1990's, combined with the growing public and political skepticism about the relative cost-effectiveness of "big government" and tight limits on government spending, a dominant issue of science policy has become the criteria that justify public investment in R&D as opposed to relying on the private sector, if necessary by restructuring incentives so as to induce more private R&D investment. It is generally agreed that there must be some pubic or common good arising out of federal R&D, which cannot be captured by individual firms or even by voluntary associations of individual firms, but just how this public good can be measured, and what is the relative efficiency of private and public spending is a matter of increasingly intense debate. That the economic returns to R&D are large, especially in the longer term, is less and less called into question by the public and politicians, but there is a paradox here. Aggregate returns alone are insufficient to justify public investment in the absence of any showing of a common good that can be quantified sufficiently well to show that it exceeds the sum of the private returns to individual firms. The more tangible and measurable the returns, the more they are likely to be labeled as "corporate welfare" and left to the private sector to support. The more elusive and diffuse they are, the more likely they are to be questioned by skeptics. Closely related to this issue is the optimal allocation of federal R&D spending among universities, non-profit research institutions, and industry.