Science for Society

Cutting-Edge Basic Research in the Service of Public Objectives: A Blueprint for an Intellectually Bold and Socially Beneficial Science Policy

Lewis Branscomb
Gerald Holton
Gerhard Sonnert

Project Manager: Stephen Feinson

Advisory Committee

Report on the November 2000 Conference on Basic Research in the Service of Public Objectives

**Sponsored by
the David and Lucile Packard Foundation
And the Alfred P. Sloan Foundation**

Contributors to the November 2000 Conference on Basic Research in the Service of Public Objectives

Richard Bissell, Executive Director, COSEPUP, National Research Council

Lewis Branscomb, Professor, Emeritus, Harvard University

John Bransford, Professor, Vanderbilt University

D. Allan Bromley, Professor, Yale University

Harvey Brooks, Professor, Emeritus, Harvard University

William Clark, Professor, Harvard University

Rita Colwell, Director, National Science Foundation

Mildred Dresselhaus, Professor, MIT

Jaleh Daie, Director, Science Programs, Packard Foundation

Vernon Ehlers, Member of Congress

David Garman, Chief of Staff, Office of Senator Murkowski

Jack Gibbons, Senior Fellow, National Academy of Engineering;

Ralph Gomory, President, Sloan Foundation

Mary Good, Dean, Information Science and Systems, University of Arkansas

M.R.C. Greenwood, Chancellor, University of California, Santa Cruz

David Guston, Senior Research Scholar, Center for Science, Policy and Outcomes

David Hamburg, President, Emeritus, Carnegie Corporation of NY – *Keynote*

John Holdren, Professor, Harvard University

Elgie Holstein, Special Advisory to the Secretary of Commerce, NOAA

Gerald Holton, Professor, Emeritus, Harvard University

Sarah Horrigan, Program Evaluator, OMB

Richard Klausner, Director, National Cancer Institute

Leon Lederman, Director, Emeritus, Fermi National Laboratory

Shirley Malcom, Director, Education and Human Resources Programs, AAAS

Walter Massey, President, Morehouse College

Richard Nelson, Professor, Columbia University

Robert Palmer, Democratic Staff Director, House Committee on Science

Vivian Pinn, Associate Director, Research on Women's Health, NIH

Paula Rayman, Director, Radcliffe Public Policy Center, Harvard University

Nora Sabelli, Senior Research Fellow, University of Texas

Maxine Singer, President, Carnegie Institution of Washington

Gerhard Sonnert, Research Associate, Harvard University

Warren Washington, Senior Scientist, National Center for Atmospheric Research

Alexandra Wigdor, Deputy Director, CBASSE, National Research Council

Lilian Wu, Research Scientist and Consultant, IBM

TABLE OF CONTENTS

The United States must maintain and improve its pre-eminent position in science and technology in order to advance human understanding of the universe and all it contains, and to improve the lives, health and freedoms of all peoples

The Hon. Vernon Ehlers, *Unlocking our Future: Toward a New National Science Policy*, House Science Committee.

We need a policy that serves the best interests of society and science.

Irving L. Weissman and David Baltimore, *Science*, April 2001

1.0 THE CONCEPT

"Increase the federal investments in selected areas of basic scientific ignorance, where understanding may open new opportunities for society to address its most important concerns"
– Gerald Holton.

"The inspiration that basic research can draw from societal need strengthens its claim on public support in the policy community and from the public to which it responds."
– Donald E. Stokes

1.1 THE POWER OF SCIENCE AND ITS USES

A profound paradox of power and impotence, crying out for a solution, now faces concerned people in every society. On the one hand, there is the unmatched power of basic scientific[1] and technological research, reporting one remarkable advance after another at dizzying speed. On the other hand, individuals and whole societies are plagued by ominous problems that yield all-too-slowly, in part because of persistent ignorance at the fundamental level.

To illustrate: On one side, through astronomical observations and the application of relativity and quantum theory, astrophysicists have been able to describe, in a remarkably quantitative way, the events that are considered to have led to the creation of the universe. Physicists believe they are at the verge of being able to collide the nuclei of heavy atoms into one another at such high energies that the material created in the collision will resemble the stuff from which the entire universe we know exploded in the primordial "big bang." Biologists are now able to map the complex structure of the genome and are on the path to understanding the keys to our personal inheritance and the very mechanisms by which living things operate. These are examples of the extraordinary achievements of the human mind, driven chiefly by curiosity, often in an "Ivory Tower," enabling modern humans to understand in extraordinary detail much about life, nature, and how it all began—and on occasion leading (though usually on a long-range time scale) to valuable spin-off opportunities.

On the other side, there are urgent problems where basic science has not been deployed with the necessary level of dedication, such as reducing national dependence on imported energy, and reducing the threat of global climate change from the growing use of fossil fuels. To be sure, <u>applied</u> research helps, but typically it uses existing sciences and technologies and thus remains often unable to create the fundamentally new sciences or technologies that are necessary for decisive breakthroughs benefiting our economy and our comforts. For example, the US Department of Energy spends many billions each year on nuclear stockpile stewardship and on environmental remediation of contaminated sites. But it has not been able to dedicate sufficient scientific talent to a long-range and well-endowed program of increasing the efficiency of energy use, and finding new, sustainable sources of power that are environmentally more acceptable. Similar analyses might be applied to many other problem areas in which a long-range commitment of our best basic science would make the problems easier to address with the tools of applied research and development.

If more good minds and sufficient funds were available, could some part of basic scientific research contribute more to the achievement of public objectives than it does today? Can it help to solve our most urgent social problems more directly than now? In short, can we construct a sturdier bridge from the Ivory Tower to the sufferings of most of humanity? A positive answer would contribute to solving the painful paradox mentioned at the outset. Our answer, based on historic and recent evidence, is a confident Yes.

An expanded national investment in basic research guided only by intellectual priorities will, as it has in the past, provide many answers that will help advance progress in important national priorities. Continued expansion of this "Newtonian" science is essential. But we take the argument one step farther. A strategic investment in a part of basic research that is clearly designated to address specific national objectives can be expected to make an ever-greater contribution, especially if resources are allocated in pursuit of a well-considered, multi-year strategy.

One kind of evidence for our optimism is the well-known case where public concerns about disease and public health are being addressed by massive investments in publicly funded long-range research. The National Institutes of Health have been extraordinarily successful in showing- and thus convincing the public and their representatives in Congress- that basic studies in molecular and cellular biology are effective strategies in the eventual conquest of disease. This case offers a living demonstration of the power of strategic investments in basic science and basic technology to open new doors to solving the most difficult problems afflicting individuals and society. Equally important, scientific research can inform choices in public policy, so that problems are correctly understood and resources are used in the most effective way.

We also base our confidence on the prior successful federal efforts in our nation's history in fields other than medicine to adopt high-level basic research strategies in service of politically supported goals. Gerhard Sonnert and Harvey Brooks discuss a number of these in the appended paper. One of these, which is discussed in much greater length in a forthcoming book by Sonnert and Gerald Holton[2], was a request President Carter made of Cabinet officers to provide Frank Press, then director of the Office of Science and Technology Policy, with an agenda of basic research goals which they believed would contribute importantly to their departmental or agency missions. This promising initiative culminated in a masterlist of about 80 basic research questions. Other examples of efforts in the same direction were some of the basic research programs supported by the Department of Agriculture and DARPA.

A third source of evidence, of which this Report is a direct product, comes from a two-year examination of the concept and feasibility of giving explicit and conscious attention to a component of science policy, which we have called (for reasons given below) Jeffersonian. This process included a number of meetings with key stakeholders, and culminated in a Conference on Basic Science in the Service of Public Objectives, held in Washington, DC, in November 2000. This forum provided an opportunity for senior scientists and academics, agency leaders, executive branch personnel, politicians and congressional staffers to vet key aspects of a Jeffersonian strategy, and to provide the insights and case discussions found below.

A caveat: We hasten to acknowledge that it would of course be entirely unreasonable to expect basic research to enable significant advances in every problem of importance to the nation. In some cases understanding a problem does not necessarily mean one can find effective, affordable remedies. The remedies that research may allow us to understand depend not only on the conversion of scientific knowledge into practical tools but also on the ability of society's institutions to use these tools and demand more of them. Prevailing social and political conditions influence whether or not available basic and applied knowledge is actually implemented. For example, later in this report we discuss the problem of learning and public education. There has been dramatic progress in research on how people learn, based on progress in cognitive, neurological and behavioral science. But the translation of the basic understanding of learning into how to teach 30 students in a particular classroom lags far behind, in large part because the complex problems of our schools, and of those beyond school, in the lives of students, teachers and parents, inhibit the institutionalization of those particular research findings.

But with this caveat, there is enormous room for a reasonable expectation of positive results from the wider adoption of the research strategy we shall now describe. Indeed, we want to bring to consciousness the opportunity of government agencies to take more responsibility for devising a long-range basic research agenda, in consultation with the scientific community as well as other stakeholders, and for committing the funding to pursue it--along with, and not at the cost of, either appropriate R&D projects for near-term progress on specific problems, or of basic research that aims primarily to sustain the progress of science broadly.

1.2 THE CONCEPT OF USE-INSPIRED BASIC RESEARCH, OR "JEFFERSONIAN SCIENCE"

The introductory quote from the Congressional study led by Congressman Ehlers clearly speaks to the research policy our society should pursue more aggressively: a governmental commitment to "a pre-eminent position in science and technology to advance human understanding of the universe and all it contains, and to improve the lives, health and freedoms of all peoples." These two motivations for public investment in research address both the cultural and the utilitarian justifications for public support for science. When pursued under the first motivation, in the most creative environments by talented people, this research might be called "basic," "fundamental" or "creative"—or "Newtonian Science." When pursued under the second motivation, through the application of already known science to problems on a short-term basis, this research may be called "applied"—or "Baconian Science." When both motivations are present, it can be identified as "use-inspired basic research," after Stokes[3], or "Jeffersonian Science," after Holton and Sonnert[4]. As Lewis Branscomb has pointed out, two (related) distinctions are crucial: First, one must distinguish between the motivations of the research sponsor and of the researcher. Second, one must differentiate between the *why* and the *how* of research. Basic research can be described as a venture into the unknown, as an exploration of some of the secrets of our natural and social universe, guided by the researcher's creativity and ingenuity, and by the scientific method. Once this research process has been initiated, it is of little importance whether the motivation behind it was purely curiosity-driven ("Newtonian") or triggered by the hope for eventual societal benefits ("Jeffersonian").

While some may consider the label "Jeffersonian science" not fully appropriate for political and bureaucratic use, it nevertheless is convenient shorthand for policy scholars, and is reflective of Jefferson's personal commitment to basic science as a tool for public problem solving. The adjectival structure of "Jeffersonian science" suggests that an important component of the motivation for pursuing such a research program is different in nature from the motivation in other essential forms of basic research (such as Newtonian science, where the investment is motivated by the quest for scientific excellence and opportunity regardless of eventual societal uses). The "Jeffersonian" researcher conducts basic research in areas that do have a reasonable chance, in about five to ten years, to ameliorate the scientific ignorance that lies at the heart of a perceived societal problem. This choice also may be made by the research manager in his/her institution, or by the funding agency. [5]

> As Gerald Holton noted in a speech he gave at Harvard University in 1999, "[Jefferson's] motives seemed to me especially well illustrated by his decision to launch the Lewis and Clark expedition, and I reminded the audience that to the Congress, which had to provide the funds, Jefferson presented it "as a venture with commercial potential," but to the Spanish authorities, through whose territory they were to pass, he described it as a purely scientific mission. That was shrewd on his part, but also right. The mixture of both motivations was correct. Jefferson regarded the expedition as a truly scientific survey, but intended also to exploring the area on the map to which the nation's destiny was likely to lead. This was a *dual-purpose style of research*--basic scientific study from which no other payoff may be expected in the short term, but targeted at an area of national importance--and that deserves a name. I thought one would do justice to call it the 'Jeffersonian Research Program'."

The level of creativity and imagination that propels the research itself need not be different in Jeffersonian and Newtonian science, even if the motives for investing in them are. Jeffersonian science is simply a differently motivated way of selecting a basic research problem, and an additional way of legitimizing the funding of science, not a different way of performing it[6]. Speaking at the November 2000 Conference, Dr. Richard Klausner, director of the National Cancer Institute (NCI), found also that President Jefferson's plan for the Lewis and Clark expedition, with its twin goals of gaining basic knowledge and preparing for the needs of the expanding population, provides a strong metaphor for the exploration and discovery model used in the NCI.

In the interest of not being misunderstood on a crucial point, we want to re-emphasize that we do not claim to have invented a new way of doing research[7]. The historical examples cited above and in the rest of this Report show that Jeffersonian research has long existed, but it requires the visibility and conceptualization that it has not received so far, either in the way the public thinks about the issue, or even in the consciousness of many scientists. Furthermore, we do not advocate that Jeffersonian Research should replace basic research -- or applied research, with which it is allied -- within federal science policy. Rather, we argue that the explicit and conscious attention to Jeffersonian Research as part of an integrated federal science policy will benefit all kinds of research -- Newtonian, Baconian, and Jeffersonian. From every perspective, ranging from the purely cultural role of science to national preparedness, even the "purest" scientists, for instance, can properly claim their share of the total support given to basic science. But that total sum can more easily be enlarged by the public perception of what basic research can do for the needs of mankind. Even abstract-minded high-energy physicists have learned the hard way that their funding depends on a generally favorable attitude to science as a whole. Moreover, they too can be proud of the use of the campus cyclotron for cancer treatment and the production of radioisotopes, the use of NMR or synchrocyclotrons for imaging, etc.

1.3 ORGANIZATION OF THE BLUEPRINT

This Report calls attention to a national opportunity, not yet fully seized, to couple the extraordinary creative power of American science to some of the most intractable problems facing society. It explores, through discussion of two existing models and three case examples, the opportunities available to the nation, the processes of governance required for their attainment, the role of the scientific community in that attainment, and the necessity for engaging public communities outside science. The cases involve using creative, imaginative science to make it easier to attain important public goals in health, environment, energy, and education.

After presenting a detailed discussion of two existing models (The National Cancer Institute and the National Science Foundation) and three test cases, this Report outlines the three major components of an emerging Jeffersonian

Science Policy:

- Devising the Agenda -- Publics and Policy: To engage a broad range of interested parties – a diverse community of scientists, other stakeholders committed to solution of public challenges, many Congressional committees, executive agencies, industrial and not-for-profit institutions, policy research and advocacy institutions in 'ownership' of the idea, and to define next steps that might be taken by these bodies on the path toward creating a Jeffersonian Agenda.

- Implementing the Agenda -- Government Processes: To explore how this idea can be made a practical part of budget-making and program management of an expanding component of federally sponsored research, including the need for coordinated strategies across executive agencies and Congressional commitment to long-range research strategies in pursuit of national objectives, and to the stable support necessary for such strategies.

- Enabling the Agenda -- The Scientific Community: To find equally effective means for defending the essential base of opportunistic research whose priorities are guided only by their intrinsic scientific merit (Newtonian science); and to demonstrate, as Maxine Singer noted at the conference, that science must be seen as a ecological system, with interdependencies between ideas and skills, between science and technology, and recognition of a broad range of motivations for investment by government and for performance of research by scientists and engineers. The overall goal is a balanced federal science portfolio of Newtonian, Baconian, and Jeffersonian elements.

2.0 EXISTING MODELS OF JEFFERSONIAN-STYLE RESEARCH

2.1 THE NATIONAL CANCER INSTITUTE (NCI)

Cancer is the second leading cause of death in the United States after heart disease, and most of its forms lead to a protracted illness which just a few decades ago was assumed to be incurable. Over half a million Americans die of cancer each year. There are few public objectives to which science might contribute more compelling than the "war on cancer" launched by President Nixon January 22, 1971.

Incidents of new cancers as well as deaths from all cancers declined slightly between 1990 and 1997 despite the growth of population in the US. Several relatively rare forms of cancer are now subject to effective treatments. And lung cancer driven by smoking, created a rising tide of deaths that has only begun to abate with the decrease in cigarette consumption. But the National Cancer Institute (NCI) has spent $29 billion. Why has there not been more progress?

The reason this problem is so intractable is that cancer is a collective term for over 100 diseases; the biology of cancer is extraordinarily complex. Most important, back in 1971 when the "War on Cancer" was launched, oncology was a relatively new, low-prestige discipline. NIH, in creating the cancer institute, launched a basic scientific attack on the ignorance of cancer cell behavior. The key is molecular genetics, the science that tells us how cells mutate and how the body recognizes those with genetic mistakes that cause uncontrolled growth.

Creating the extraordinary body of scientific knowledge of immunology, genetics and cell biology required the creation of entire new fields of science. But it is the only strategy that gives hope of understanding how and why cancers happen and how their growth might be inhibited. "We haven't reached where we would like to be in cancer -- not because of misplaced strategies, not because of ill-conceived policies -- but because cancer is extraordinarily complex," said Richard Klausner, director of NCI. Overcoming cancer, he said, requires basic research as well as appropriate public policies to take advantage of what is learned[8]. This is a Jeffersonian science strategy, investing in the underlying science to reduce the ignorance that stands in the way of progress toward cancer prevention and cure.

How does the NCI present this strategy to an impatient Congress and public? The first sentence of the published *Budget and Program Plan for the National Cancer Institute for FY 2002* reads: "The National Cancer Institute's goal is to stimulate and support scientific discovery and its application to achieve a future when all cancers are uncommon and easily treated." This sentence states that the NCI's goal is attainable and qualitative: A future in which cancer is no longer the terrible specter of disease that haunts is all today. How is that future to be achieved? Through scientific discovery and its application, making NCI neither a purely research agency nor a clinical medical delivery agency. The word "discovery," rather than "research," suggests research with outputs that are applicable to clinical investigation and practice. Who will make these discoveries? Scientists and doctors supported by the NCI and privately supported scientists whose work is stimulated by NCI activities, thus including the outside world, not just NIH scientists and grantees.

Richard Klausner addressed the challenge of executing a Jeffersonian strategy at the November 2000 conference. He described the planning process at NCI as aimed at getting support for a science approach to NCI missions, noting that the "setting of priorities offers an opportunity to defend the Jeffersonian model. One section of the NCI budget document deals with tools, the other with domains of exploration. They are justified by scientific achievements in a previous period. The third component is disease specific. Progress review groups engage in a 9 - 12 month process of setting priorities for *what we need to know and know how to do* for each disease. The melding of these three things gets agreement on how the first two parts map to the "Baconian" [practical] needs of the disease perspectives. So scientific opportunity can be made consistent with medical needs. Planning and evaluation must be melded together."

In articulating this strategy we noted that NCI uses the words "exploration and discovery" (rather than "research") as characteristic of the kind of technical activities that are likely to bring public benefit. He observed, "We can plan exploration, but we can't plan discovery. Exploration requires tools, data etc. Tools have been too much ignored. Science is too much seen as hypothesis, but tools enable science and science enables tool building. This is broadly understood by the public and reinforces the requirements for exploration." This choice of language may be important to the image NCI conveys to lay audiences. The public often sees "research" and "science" as arcane activities with both positive and negative impacts on society. Exploration is an adventure with either null or positive results, and discovery is almost always seen as a positive addition to human knowledge. [9]

The specific articulation of a societal need as a challenge requiring new knowledge requires that five issues be addressed in the agency's planning:

1) Articulate the societal need as a challenge requiring new knowledge.
2) Articulate science as the discovery process that will create that knowledge.
3) Articulate the connection between discovery and the application of discovery to the societal need.
4) Establish criteria for determining the vehicles and support needs for the explorations required.
5) Address with realism the timelines (5 – 7 years) and the uncertainties of both timelines and plans that are fundamentally dependent on what is discovered.

Klausner also noted that industry plays an important role in their strategy and in garnering support for it. He also notes that once one commits to society's enjoyment of beneficial outcomes from discovery, one must engage the social sciences as well. In fact, he said the social sciences are the NCI's most rapid growing area of science investment.

In using NCI as a possible model for Jeffersonian science, we are not asserting that the model has been validated by objective analysis of the causal and cost-effective linkage between NCI research and the ravages of cancer in the U.S. population. But there can hardly be debate over whether the model has won the support of almost all the groups committed to the fight against cancer: the public support groups, the biomedical research community and most oncology specialists.

The NIH model for funding scientific research contains underlying factors specific to the medical field, and has developed over a long period of time. Nonetheless, as Donald Stokes points out in *Pasteur's Quadrant*, "it is…unreasonable to think that only biomedical research lends itself to the virtues of the NIH model of a scientific agency focused on an area of recognized social need that is able to enlist basic science of the highest caliber from a range of disciplines to develop a fundamental understanding of the phenomena that underlie the problem area, while it also sponsors some pure research and some purely applied research."

2.2 THE NATIONAL SCIENCE FOUNDATION'S APPROACH TO BASIC RESEARCH IN SUPPORT OF NATIONAL GOALS

"Enabling the nation's future through discovery, learning and innovation," Director Rita Colwell's vision for the National Science Foundation (NSF), also comports to the principles in the NCI mission statement. She emphasizes enabling (rather than causing or managing) the future as the goal, and discovery, learning and innovation as the means. "Including innovation reveals our commitment to the public. We strive to move society to a better place." She further said, "The objective of connecting discovery to society is central to our work." Thus the question Walter Massey, then NSF Director, put to his NSB Commission on the Future of NSF in 1992 has now been answered in the affirmative.[10] NSF does accept responsibility for understanding and fostering the chain of events through which the research it supports creates positive outcomes for society.

Like NCI, the NSF strategic goals now encompass people, ideas, and tools. The concept of "people" embraces not only the basic mission in science and engineering education -- furthered at the university level primarily through support of graduate student research and fellowships -- but also the importance of dialog with the American public. NSF reaches some 50 million people through museums and another 100 million through radio, television, and film, although these activities are managed as "public understanding of science" rather than as part of structured strategies tied to specific goals. Like the NCI, NSF's emphasis on tools – the telescopes, ships and databases constituting the infrastructure of modern science – is consistent with the concept of enabling the future.

The emphasis on knowledge production is reflected both in disciplinary research selected by peer review from the ideas of individual scientists [Newtonian science], and in the initiatives which emphasize the opportunities for interdisciplinary research motivated by understandable national objectives [Jeffersonian science]. NSF has mapped new initiatives into FY 2002 (Mathematics) and FY 2003 (Social Behavioral and Economic Sciences). These initiatives, Dr. Colwell said, will "enable NSF to center attention on national and global priorities." "Each of these initiatives cuts across traditional disciplines to bring a broad spectrum of resources to bear on areas of national importance. These areas are self evident."

Dr. Colwell noted that "today we are looking at a total U.S. economy in the order of

$10.4 trillion, yet our estimate is that the United States government spends barely $20 billion on basic research. Using these numbers as a guide, we see that (federally supported) basic research efforts constitute 0.2% of the overall economy. That is the government's investment in the country's future. Out of the $1.9 trillion federal government budget, basic research constitutes 1.1% of the total budget. With this meager amount of federal investment, enormous forces tug at the NSF purse strings. NSF is required to make tough decisions, with the inevitable result that people and discoveries are lost in the process."

Some conferees in the November 2000 meeting pointed to differences in the NSF and NIH approaches, suggesting that NSF might take the concept further. For example, one observed that the process described by the NCI is more than 'talking about science with the public'. NCI has set up a way of listening to different constituencies, and educating them about NIH process, goals, and ways of choosing projects, as well as engaging them in discussions about program priorities. The same cannot yet be said about NSF's approach. In fact, education and education research could be used by NSF to move in the direction of NIH's interaction with the public.

3.0 THREE CASES FOR EXPLORING JEFFERSONIAN SCIENCE

From the three case discussions explored in the November 2000 conference, to which we now turn, we wanted to learn the following:

- Can one select some examples that give us confidence in the value of trying to get a stronger commitment to multi-year explorations that make progress toward a national objective quicker, cheaper, better?

- Are there common features to the processes required to gain consensus support behind such a strategy?

It should be stressed that the choice of issues for the case discussions in no way represents an attempt to preempt the public and political discussions through which national priorities are set, a process to which we dedicate a great deal of discussion below. Rather the issues of education, energy and climate change are currently generating significant attention in the country, and thus lend themselves to as cases to explore the possibilities of Jeffersonian strategies.

3.1 IMPROVING THE QUALITY AND EFFICIENCY OF PUBLIC EDUCATION

> We *recognize a wide consensus that elevates the status of education to that of national defense as a guarantee of national security. Education is also a guarantor of economic prosperity, of exemplary personal health and environmental sanity, of protection against rigid belief systems, e.g. so-called junk science.*
> –Leon Lederman

Education cries out for new approaches using existing and potential results of research, but the return on that research is seriously frustrated by lack of demand from the educational institutions. The case of education raises three key problems that Jeffersonian Research can address: Does research on how people learn and how schools can be more effective exist that, if widely adopted, would permit broad and rapid progress toward an acceptable performance of the nation’s schools? If so, what barriers must be lowered to permit a long-range learning and education research strategy to add value? Can demand be created for the needed research?

In the National Research Council (NRC) book entitled *How People Learn: Brain, Mind, Experience and School*[10], the authors discuss the "convergence of evidence from a number of scientific fields," noting that "the research areas relevant to the science of learning are demonstratively broad, including cognitive development, cognitive science, developmental psychology, neuroscience, anthropology, social psychology, sociology, cross-cultural research, research on learning in subject areas such as science, mathematics, history, and research on effective teaching, pedagogy, and the design of learning environments. New technologies for assessing learning in ways that track the growth of learning, not just cumulative of facts, are needed. Developing effective research methodologies is particularly important for research from this diverse array of disciplines."[11]

At the Conference, John Bransford of Vanderbilt University asserted that current research has produced four requirements for effective education that are well known and verified by research:

a) Students must master substantive content knowledge; problem solving must be based on content mastery.
b) Teachers must also have pedagogical knowledge: Understanding struggles of novices in acquiring content knowledge is important.
c) Teacher quality and capability are essential.
d) Teaching for understanding and not just memory is necessary.

How can one get a cumulative knowledge base on thinking and learning, and make it accessible to practitioners? We need new avenues for capturing the wisdom of practice, and we need a new kind of professional who can bridge the worlds of research and practice.

Alexandra Wigdor defined the issues that control whether demand can be created for this research:

a) How can learning/cognition be incorporated into practice?
b) How to make research on motivation meet classroom needs?
c) How to make school environment support learning?
d) How to make research findings more accessible?
e) Need new forms of collaboration based on teacher/school demand

f) Creating new instruments for expressing demand -- such as National Board for Professional Teaching Standards.

Dr. Nora Sabelli, addressing the question of why research is not used in practice, noted that teachers don't have the time that would be needed to synthesize research, nor the schools have an ethos of experimentation that supports adapting research generated knowledge to local conditions, and that there is a lack of funds for long-term projects to integrate the research knowledge into localized practice. Similarly, there is a lack of infrastructure support that enables this integration of research with practice in effective ways. Sources of this type of funding for research are relatively scarce, and thus no research capacity has evolved at the intermediate (state, metropolitan area) levels, and no or very little private sector research exists on education. There is also the constant issue of public perceptions of education, where the belief that teaching and learning abilities are innate, not subject to development and change, or alternatively that education (and social science) research is no better than common sense, stand in the way of validated, evolutionary improvements in practice.

It can be argued that NIH's success has shifted the balance between medical research and public health research (note, though, that NIH is actively increasing its reliance on social sciences). To avoid a similar imbalance between Newtonian, Jeffersonian and Baconian research in education, other agencies besides NIH and NSF--such as the Department of Education and the States-- would have to have the resources to take up the public engagement aspects of education research implementation.

A conclusion concurred in by Bransford, Wigdor and Sabelli, and strongly supported by Maxine Singer: *Until the business community recognizes the huge size of this market and devises a way to overcome the fragmentation of its demand structure, it is unlikely that there will be an aggregation of demand that can truly benefit from research. This commercial linkage is clearly a key part of the success of NIH and in its defense of linking science to practice.*[12]

David Guston noted that the states have dealt effectively with the transfer of research knowledge to practice in agriculture, where the aggregation of the market is a consequence of the commodity nature of the product. Education looks more like the problem of small manufacturer innovation, where each small firm conceives of itself as having unique problems calling for unique solutions.

Nevertheless, Lewis Branscomb suggested that the agricultural model might be applicable in two of its features: a politically acceptable role for federal agencies in support of research and identification of best practices, and the concept of extension agents, with the resources and authority of state education agencies behind them, working directly with the schools.

3.2 SUSTAINABLE, ENVIRONMENTALLY ACCEPTABLE SOURCES OF ENERGY

The Panel's review of DOE energy R&D activities identified many areas where technological advance could be accelerated if more attention were given to fundamental questions identified in these programs. Examples include better understanding of reactions at the interface of electrodes and electrolytes in fuel cells, the capacity of carbon nanostructures for hydrogen storage, the chemistry and fluid dynamics of CO 2 storage in saline aquifers, the physics of thin-film photovoltaic materials, and many others. The Panel found that linkages between the Basic Energy Sciences (BES) programs (where such issues are investigated) and the applied energy-technology programs (where the findings could be put to use) need to be strengthened in many cases.

While the technology programs do benefit today from the growing body of fundamental knowledge being generated under BES programs, they would benefit much more if BES were to address specific questions identified as important in these programs. The Panel recommends that BES allocate additional resources to support fundamental research activities addressing needs of the technology programs. This could be facilitated by mechanisms such as co-management and co-funding with—or budget sign-off by, or re-routing budgets through—the applied energy-technology programs.

Our recommendation that BES direct some of its resources to serving these needs might raise concerns that the creativity of basic science will be lost if it is constrained by premature thought of practical use, and that applied research invariably drives out pure, if the two are mixed. What is being sought here, however, is not to redirect BES resources to applied research. The technology programs support applied research but give little attention to addressing fundamental questions such as the above. The net effect of this recommendation should be to expand, not diminish, the portfolio of fundamental research activities within the limits of overall budget constraints. In light of the growing interest among policy planners in harnessing science for the technological race in the global economy, the allocation of some BES resources to the development of fundamental research programs that would serve the energy technology programs should add to the political appeal of supporting basic research generally.

Report to the President on Federal Energy R&D for the Challenges of the 21st Century
President's Committee of Advisors on
Science and Technology,
Panel on Energy Research and Development
November 1997

Energy would appear to be the most natural case for a long-range basic research program to create new energy options, but is inhibited by traditional structures in Congress and in the Department of Energy (DOE), and perhaps by the presence of a private sector too heavily invested in current sources and uses of energy, responding to markets that do not reflect future costs.

John Holdren made the case that the strategy to address the U.S. energy problem is to generate better energy options: higher efficiency fossil and biomass fuels, solar photovoltaic, more efficient energy usage, more acceptable sources of fission energy, and other more "long shot" technological alternatives to fossil fuels. The case for giving high priority to energy research is clear: energy R&D declined 3.8 fold from FY 78 to FY97, due to a return to low gas prices, the failure of demonstration projects, budget constraints in Congress, and some political infighting among energy options. Private sector investment in energy R&D also declined some 40%, but far less than public sector investment. Yet energy has many externalities that require comprehensive solutions, such as the increased likelihood of conflict over oil resources in the Middle East, the necessity of changing energy modes to mitigate accelerating climate change, and the cost of controlling air pollution.

Thus energy would appear to be an ideal candidate for a long-range research program devoted to the creation of new technical options. There is a major industrial market for such technology, and relatively few non-market barriers (except for nuclear energy). Yet such a program has yet to mature in the Department of Energy.

To understand this apparent anomaly, consider that energy activities are only $2.2 billion out $18.8 billion in the DOE budget. The DOE Office of Science, has a budget of about $1.5 billion. Mildred Dresselhaus, the former head of the Office of Science, stated that she felt that her job was to make US a leader in physical science and engineering again. This is an admirable goal, given the failure of DOD to keep up funding in those areas, while NIH budgets soared. But it appears that creating a long-range, diversified research strategy to create new energy technology alternatives is not the dominant priority of the Office or of the Department. At the same time, the Department's recently released energy R&D strategy seemed to be quite clear in its Baconian thrusts. What seemed missing is the Jeffersonian strategy.

One possible explanation (aside from the often expressed view that the DOE is not as well managed as it might be) is that in this field, unlike in education, the government, which regulates the energy sector in many ways, has failed to find a mode of collaboration sufficiently attractive to industry to produce the needed demand for government sponsored basic research.

Former associate director of OMB for energy, natural resources and science, Elgie Holstein, observed that all technologies have powerful constituencies. Questions such as "Are fossil fuels yesterday's news or the basis for a better future?" "Should we be spending money on clean coal technology?" "Should we bet on certain non-fossil fuel technologies? Windmills? Nuclear?" contend for answers. Note that the NIH has an analogous problem, because each disease has its dedicated advocates, but there is a difference. The economic interests in health are not organized by disease, but rather by what they provide – hospital care, pharmaceuticals, and medical equipment. Thus each economic sector has an interest in all the diseases. In energy the industries tend to be technology specific. As Holstein concluded, "From the perspective of OMB, one is balancing between strategic investments and demands of competing claimants."

David Garman, a 20-year Hill veteran who has worked for key Energy Committee and subcommittee Chairmen, observed that the members of Congress for whom he works would generally agree with Holdren's recommendations. So why hasn't Congress been more generous? There are structural impediments, such as the fact that committee jurisdictions are split for energy, environment and energy science research. The energy committee sees itself as booster of energy production; the environment committee sees itself as an energy regulator; the science committee has not paid much attention to energy issues outside of climate change. There is also the problem of political support for such research. Typically, the US public wants to spend on energy only when there is a national emergency. The public understands cancer risk and supports biomedical research. Does it equally well understand the risks associated with climate change, air pollution, and import dependency?

3.3 GLOBAL CLIMATE CHANGE

Climate research seems a natural case, and the scientific leadership has correctly chosen regional mitigation as the research focus, thus meeting both key research issues and creating a more attractive and viable basis for a bipartisan research program of appropriate magnitude.

William Clark outlined the revolution in thinking about global climate change, since research began back in the International Geophysical Year (1957). Vostok ice cores showed history was chaotic; our theories did not embrace the cross coupling of geophysical and biological systems, and had no knowledge of human activity intrusion on nature (now known to be comparable to natural cycles). Roger Revelle saw that our use of fossil fuels was a great geophysical experiment. ("We may be in the test tube -- we may *be* the test tube." -- Jane Lubchenko)

Policy-driven, method-driven, and curiosity-driven research motives have merged. In the 1990s scientists found their research more and more driven by policy considerations, but thought their research was being distorted by the "problem of the week." The Global Environment Change program was created by scientists to get some stability against the "problem of the week" (more in Europe than in the US). This morphed into a climate change agenda, which resulted in stable research support in earth systems research. Administration, Congress, and science reached agreement to keep the program growing at levels the community said were needed to get ahead of the curve.

Today the scientific community is saying that we have to address the social issues. What is the role of the scientist, for we are part of the transformation that we are also trying to study? Now the social and natural science parts look for an integrated understanding. The social agendas are increasingly the motivation for setting research priorities. For example, scientists have recently completed an analysis of the impact US of climate change in 20 regions.[13] Researchers found that people weren't interested in climate, but in land use and job building, etc. Integrated looks at multiple environmental stresses are needed, not one stress at a time. Research resources for regionally focused research are inadequate. If it is not national and it is not disciplinary, it sounds like "pork".

Warren Washington made the case that since 1996 a scientific consensus has solidified around the conclusion that human activities are and will be appreciable compared to natural cycles[14]. Data clearly show global temperature rising since mid-1880s, about 0.7 deg Celsius. Serious research problems remain: climate models have improved, but clouds, aerosols and radiation create the biggest questions. There is high uncertainty about rapid changes in ocean and the sea ice aspects of climate. El Nino and Southern Oscillation are hard to predict but have been strong recently. Heavy and extreme precipitation trend will continue. It is expected that this will cause increased flooding which will be hard to deal with. Also because of increased warm in certain regions there is a strong possibility of increased drought periods.

In last couple of years there has been a positive change in Congressional attitude toward R&D funding: Why? Congressmen believe that science has something to do with the lengthy economic expansion, and they do not want to risk stifling it by cutting funding. Furthermore, the high-tech community has become more politically active, especially in financial support of candidates.

Incrementally reducing scientific uncertainty, as a way to make policy easier, is not the way to sell the program, Robert Palmer, senior staff member of the House Science Committee, said. Members of the committee are more interested in regional livability. Maybe environment can be tied in to these current issues, especially with the emphasis on the importance of regional analysis of climate impacts.

Many scientists imagine what policy makers ought to want to know. That approach does not work. What is required is to understand what knowledge system can connect basic research through assessment to policy and back again. At the regional scale, one can put the users and intermediaries into the process more easily, and that is where the program should be moving. Global Change people should declare victory. The mesoscale problem, El Nino etc., is where the fundamentally exciting research is going on. Mitigation research has the value of presenting society with some tools, with the national climate change trend as motivator, to believe something can be done, so when the case for human-driven exacerbation becomes solid it can be accepted and acted on.

4.0 DEVISING THE AGENDA: PUBLICS AND POLICY: ACTIVATING CONSTITUENCIES AND STAKEHOLDERS FOR JEFFERSONIAN RESEARCH

4.1 THE CRUCIAL LINK: PUBLIC SUPPORT AND CURIOSITY-DRIVEN RESEARCH

One of the most important features of the Jeffersonian approach is the conclusion that stable funding for basic research that supports broad societal goals must rest on a consensus of strong, supportive, public constituencies. This approach explicitly acknowledges that in various segments of the public there is confusion and ambivalence about the mission of research in science and technology. As the National Science Board (NSB) reports in *Science and Engineering Indicators*, the U. S. population is still highly favorable toward science as a hope for positive results -- far more than, say, the Japanese public; but they are no longer satisfied with waiting patiently for the uncertain and unpredictable "spin-offs" with which Vannevar Bush justified federal investment in basic research.

This is in part the scientists' fault for failing to communicate effectively the many ways scientific research creates value and opportunity. Neal Lane recently urged scientists to "let people remember" the impact of science and technology. Some additional efforts have started, such as the National Academy's Office of Public Understanding of Science, the NSF Programs with similar aims, the efforts by professional societies, such as AAAS, APS, ACS, etc. Such efforts are needed and no doubt contribute to public understanding of science. And yet, there is a rising call from the public, reflected by their political representatives, to have scientific research and technology perform more obviously for the public good.

> *But, in addition to the members hearing from the scientists and engineers about research, they need to hear from their other constituents about how important science and technology are to their lives. I'm talking about our neighbors, university presidents, students, business leaders, state and local politicians, most of whom know very little about science and technology. Members of congress pay a lot more attention to them than they do to us. In my opinion, we need to adopt a role as "civic scientists" and start paying a lot more attention to them as well.*
> -Neal Lane, The Branscomb Lecture, Harvard University, March 2001

The passage of the Government Performance and Results Act (GPRA) in 1993 reflects just such expectations. The one area of publicly funded scientific research work that seems to satisfy this public expectation is in biomedical research (as reflected in the daily bombardment of news of "breakthroughs" in medicine and biology generally).

The public and the politicians thus remain ambivalent about support for basic research[15]. Among the many symptoms of the present ambivalence, one only has to look at the fact that, according to *Science and Engineering Indicators*, roughly 50 per cent of the population believe that sometimes scientists engage in fraud--which coheres with, and is in part driven by, the delegitimization of science coming from sections of the press and polity, where one routinely finds pronouncements such as "Scientists want only a blank check" to further their careers, and from the sections of academe accusing the scientists of "socially constructing" their work in pursuit of their self interest

The public's ambivalence of course is also reflected to some degree by warnings among our lawmakers. Thus Senator Harry Reid (D., Nevada) warned (April 13, 1999) that many people "seem to see science as a luxury...that can be reduced or abandoned."

The Jeffersonian approach seeks to address this apparent ambivalence in the public support for basic science by crafting strategies that firmly link curiosity-driven research to the goals and concerns of citizens and their political representatives. It harnesses the recognition by many scientists that they are accountable to society for the resources invested in their research, and the conviction that long-term, creative research that creates deeper understandings of problems facing society must be an essential part of a strategy to ameliorate those problems. A Jeffersonian Science Policy will foster new and attractive options for addressing crucial societal goals, and thus decrease the cost and time for making progress while resulting in more favorable outcomes.

Jeffersonian-mode research is, of course, not a new idea, and there is ample evidence to support the notion that a stable, and even expanding base of government financial support can be created through a serious effort to structure fundamental scientific research that is supported by strong public constituencies and stakeholders. At a Nov. 1999 Washington DC workshop, which served as a launching point for the current initiative, then-Director of the National Institutes of Health, Dr. Harold Varmus, acknowledged that the NIH strategy is essentially Jeffersonian, and that this in part accounts for NIH's success in attracting resources to long-range basic programs. He described four broad elements of that strategy, and the pitfalls it must avoid to be successful.

First, the goal (in this case, to address the major illnesses that plague the citizenry) must be simple, clear and self-evident. The NIH institutes are typically named for the disease that motivates much of their research (Heart, Cancer, etc.) Significantly, the Institute for General Medicine has more difficulty in gaining support than those with disease-related names[16]. At the same time when pressed to pursue a *war on cancer*, a metaphor basically Baconian in style, the response has to be twofold: it should, on the one hand, note that progress has been made on many cancers; and, on the other hand, emphasize the many other medical benefits that NIH's Jeffersonian strategy has also given to the public. Thus the metric by which progress is assessed politically must be broader and more flexible than the shorthand banner under which the program advances.

Second, Varmus called attention to the necessity of active public support for the Jeffersonian goal and strategy through interest groups, which are abundant in medicine. The range of potential advocacy groups covers all the interested groups and institutions: including biomedical industry, “patients,” physicians and other health care providers and insurers, research universities, distinguished scientists who themselves are doing purely Newtonian research, and public interest advocacy groups committed to specific elements of the biomedical agenda. The existence of issue-specific public advocacy groups and the goals they adopt for their own interests validate the similar goals adopted by NIH. But these groups must be brought together so that their influence is more supportive rather than fostering competition among “favored” diseases. This is accomplished by means of a large council of citizen groups of which as many as 100 might be represented. In this way, tradeoffs among their competing interests can be subordinated, at least to some extent, to the larger common objective of a larger ‘pie’ of resources for which to compete.

Third, owing to its focus on health, it is easier for NIH than for other agencies to showcase the link between its basic research and benefits to “user” communities. Varmus pointed out that NIH invests $2 billion in clinical research annually, which allows specific examples of new medical capabilities that embody more basic scientific research to be visible in the evening news[17]. This illustrates that a Jeffersonian strategy can be strengthened if it is complemented by a more Baconian approach to the delivery of outcomes. This also helps the sponsoring agency to satisfy the requirements of the Government Performance and Results

Act (GPRA). As Harvey Brooks noted at the November 1999 workshop, some other agencies can do this, too, pointing to research resulting in the Internet, or to the history of agricultural research (with benefits not only to the nation, but worldwide.)

Finally, Varmus noted that the serious intellectual work of building the right basic research strategy and of constantly revising it as the science and problems change is essential, as is a very active and effective ongoing dialogue with political and public interest groups

4.2 SELECTING GOALS TO WHICH SCIENCE CAN BEST CONTRIBUTE

> *We are now freed from the rigid thinking of the Cold War. We should reassess all that we are doing, and the goals that drive our daily work. I have become increasingly convinced that it is no longer sufficient to simply push for more funding for R&D programs. You who create and apply the new knowledge that increasingly shapes our rapidly changing culture must apply yourselves as well to the task of social change, to issues of equity and social stability and to a more fundamental reordering of relationships between individuals, institutions, and sectors of society.*
>
> -Congressman George Brown, PITTCON '97 Conference on Analytical Chemistry and Applied Spectroscopy, March 1997

What are the national purposes to which strategic investments in basic science can most effectively contribute? David Hamburg, in his keynote remarks to the November 2000 conference, identified four groups of goals selected by the 1992 study, led by Guyford Stever, for the Carnegie Corporation's Commission on Science, Technology and Government:

- Quality of life, health and human development
- Sustainable economy
- Environment and natural resources
- Personal, national and international security

A collaboration of public and private efforts is required for progress in all four of these broad goals. Federal agencies are charged with the responsibility for contributing to them. Ultimately the public must set the goals and define its priorities. How is that to be accomplished? Who will set the goals and priorities that determine the allocation of resources for federally funded research?

"Enabling the Future: Linking S&T to National Goals," the title of the Stever report, attempted to answer this question, calling for a National Forum on National Goals. Hamburg noted that scientific bodies such as the National Academies, AAAS, Sigma Xi, or perhaps a new institution, might create the venue for exploring the most promising opportunities for science to contribute to public purposes.

As Maxine Singer points out, defining the public interest is not easy, since both political and scientific inputs are required. The media, she notes, do not do a good job of helping the public understand where science can best contribute. She offered, as a current example, human stem cell research: "Many believe this research is in the public interest, but many Americans reject the idea. Where does the public interest lie? What is the public interest in genetically modified plants? Can anyone say? Some people are concerned about motivation of large corporations, others about organic foods, others about environmental implications, others about international issues. But there is consensus on broad issues: defense (if one is not specific), curing cancer (if you are not talking about stem cells)."

One can imagine a process for selecting public goals and defining the role of Jeffersonian science in addressing them. Suppose focus groups of citizens were asked to make a list of the problems they wanted government to address, listed in order of importance. The list might include curing and preventing disease, finding new, sustainable sources of energy, deterring threats to national security, avoiding the worst consequences of global climate change, improving our schools, achieving social and civil justice. Then imagine that a group of broadly expert researchers were brought in to describe, for each goal, what kind of basic research would be suitable to improve our incomplete understanding of the issues and find new options for dealing with them. A reordered list, according to the combination of how important an issue was thought to be and the likelihood that in time research could make the problem more tractable, would constitute the priority list for public Jeffersonian science investments. In practice, of course, the list of problems is debated every day by politicians who are surrogates for the citizens, and the scientific community struggles to find a way to inject its views on where science can make the most effective contribution into that political discussion.

Three factors make the injection of the scientific view into the lay public's discourse more difficult. First, cynics will say that the equilibrium of political and institutional pressures, not evaluations based on deeper understanding, will determine the priority accorded to public concerns. Second, the scientific community is not in uniform agreement about the efficacy of research in remediation of public concerns. This second factor has two roots: Many of the issues call for applications of the social sciences, with attendant debates about their practical applicability and assertions about their ideological implications. Moreover, there are often serious barriers to the expression of demand for results of social science research and skepticism about its utility, as in the case of public education reform. The third factor arises from the fact that the science research agencies of the federal government, with the exceptions of agriculture and NIH, have little experience in engaging lay publics in dialogue about the contribution of research to public interests. Yet that is just what is required to build the support and ensure the success of Jeffersonian science.

5.0 IMPLEMENTING THE AGENDA: DEVELOPING GOVERNMENT PROCESSES THAT EFFECTIVELY COUPLE BASIC SCIENCE TO IMPORTANT NATIONAL OBJECTIVES.

The second major component of Jeffersonian Science Policy is to couple public constituencies with mechanisms for implementing policy. The framework for science policy-making in the presence of interest groups and institutions must be addressed. Any alternative policy, such as a Jeffersonian one, must meet the requirements of politics and bureaucracy in order to become a meaningful force within the budgetary process.

Total investment in research and development in the United States reached almost $265 billion in the year 2000, an increase of 60% from the level in 1993. But this growth is almost all in the private sector and is development, not basic research. Government support for research and development has shown much less growth (about 11.5%) and was less than half of the level of private investment in 2000. Of this only $23 billion was spent by the federal government on basic research in 2000[18]. Because federal agencies provide the primary support for long-range, basic research in our universities and national laboratories, this public support is crucial for both intellectual progress and stimulating more applied activities, mainly in the private sector.

5.1 BUILDING BIPARTISAN SUPPORT

A first requirement is that any public expenditure that must be sustained over many years must also enjoy bipartisan support. Thus any formulation of Jeffersonian science must be seen by both parties either as a-political or as of equal value to both parties. David Guston, discussing Rep. Ehlers' call for a new science policy at the November 2000 Conference, noted that Rep. Ehlers moved "politics" into the center of the discussion, because Jeffersonian Science may not be as bipartisan as hoped for. It has a more apparent political attraction than Newtonian Science; that is, decision makers will understand its consequences for the creation of and impact on organized interests. And it may compete with more applied Baconian Science. For example, some activist cancer groups might respond to NCI's commitment to Jeffersonian Science by insisting on a higher priority for short term efforts, such as acting on the knowledge of the behavioral and environmental causes of cancer we already possess, not waiting for addressing knowledge gaps in the quest for new scientific discoveries of cancer cures.

Another limitation, noted by former-Science Advisor Jack Gibbons at the Conference, is the limited internal capabilities in the Congress. There are few scientists in Congress. The organization of Congress is highly complex and arbitrary, and many of the important science policy inputs, such as the Office of Technology Assessment and the science division of the Congressional Research Service, are gone. However, the Congress does reflect American optimism about the value of research and exploration. Whereas politicians went through some narrowing of commitment in the middle 1990s, thinking that maybe the private sector might pick up the slack, that tendency is now behind us. Congress is still uneasy, however; federal funds for basic research are perceived as desirable, but given the general impression that beneficial outcomes may be delayed by a decade or more, those research investments lack the urgency to compete with other demands. Is government now increasing expenditures on research that is going farther downstream toward the market, in order to shorten the payback delay? There is unease about this trend too, both regarding inequitable disbursement of funds and uneven geographical distribution of research funding.

In November 1999, then-Science Advisor Neal Lane made the point that even if currently there appears to be strong support for science in Congress, this support is not solid, but vulnerable. If questioned by constituents about why tax money should be spent on science, many politicians will have a hard time making the case convincingly. Here it would make a tremendous difference if the executive branch and/or the science community provided the politicians with more cogent and persuasive rationales for governmental science support.

5.2 CREATING BASIC RESEARCH BUDGETS AND PRESENTING THEM TO CONGRESS

> *A budget must relate to goals, or else there is no basis for optimizing the budget. This needs to drive the definitions.*
> –Richard Bissell

Data on federal basic research (and R&D) is packaged for the congress in three versions:

- · R&D (president's request FY 2001) $85.4 B (Including defense development, testing and evaluation)
- Federal Science and Technology (FS&T) $53.7Billion (National Academies and AAAS)
- 21st Century Research Fund: $42.9 Billion (OMB, including almost all of Federal basic research, over 30% of Federal applied research and about half of Federal non-defense development, know as Federal Science and Technology Budget in FY2002)

These three versions of the FY 2001 budget for research and development activities represent the various ways in which budgets are tracked, with R&D being the broadest and least detailed approach in terms of isolating the type (basic, applied) of research being funded.

FS&T is an attempt to gather together activities leading to new knowledge and new enabling technologies, with the actual numbers being obtained by subtracting large budget items that appear to create relatively little new technical knowledge, such as DOD field-testing and installation of new technologies[19]. Who reads these numbers in Congress? The appropriating committees originally asked for the report that generated the FS&T reconstruction, but the committees are generally inflexible in terms of their budget structures. If the FS&T analysis from the NRC and AAAS is read at all, it is by authorizing committees.

The Office of Management and Budget (OMB) traditionally tracks R&D broken out by basic research, applied research, and development, as well as by agency. It collects the numbers from the agencies before President's budget goes to Congress and presents an R&D analysis after the fact. For FY 1999 the Administration proposed a "Research Fund for America," intending to use the tobacco settlement money. Renamed the 21st Century Fund the next year, it allowed tracking the research budget both internally and in Congress, week by week, because it was defined in terms of line items in the Federal budget submission to Congress. OMB has begun to use it to address the issue of "balance," and to encourage the interesting things that are happening at the boundaries of disciplines. The 21st Century Fund is comprised of most of the basic research, a third of applied, and half of civilian development from the total R&D budget[19].

Are agencies consistent in their definitions? OMB defines basic, applied, development, but agencies interpret definitions differently. For example, NIH considers its entire budget some kind of investment, while NSF excludes some of its funding from research and development (staff of NSF, for example, or the logistical support for Antarctic program).

5.3 INTERAGENCY COORDINATION OF RESEARCH STRATEGIES

Classification of science as basic, applied, and development is problematical, not only on theoretical grounds but as an obstacle to sorting out the distinction between the political motivation for investment in research and the conditions under which it is performed[20]. Thus strategic investment in basic research justified by opportunities to make progress toward national goals might be considered by some as "basic" and by others as "applied." This challenge to the way government agencies categorize their research investments is exacerbated by the fact that basic technological research will play an important role in the pursuit of many national goals. [21]

The Office of Science and Technology Policy and OMB, with advice from PCAST, could establish those long-term goals in budgetary form and monitor agency progress toward them. Bromley noted that PCAST could be a significant mechanism for bringing high-level non-governmental evaluation to these long-range research programs, but this would require a level of time commitment (and budget) more characteristic of PSAC in the 1960s. As Jack Gibbons stated, the added burden on OSTP to oversee the development of these programs would require, more than ever, an increase in the Congressionally budgeted staff for OSTP[22]. Hamburg suggested that this should be done separately from the annual budget, consistent with Allan Bromley's suggestion -- a two-tiered budget process, with a few, multi-year presidential initiatives, plus the regular, evolutionary budget.

Both Bromley and Gibbons saw the White House interagency research agency body – FCCST and NSTC respectively – as a useful management vehicle for the required interagency coordination. Bromley, reviewing his experience in the Bush administration, felt that OSTP, OMB, and the agencies, with FCCST, brought about major changes to enable the "Jeffersonian research" mode. Whichever device is used to formulate interagency programs, and however OSTP and OMB work together to monitor and manage them, a clear requirement is cross-agency coordination of these long-term programs.

5.4 A MACRO BUDGET STRATEGY

The long-term basic research strategies would be established at the level of broad, cross-agency, program objectives. Thus they would form part of the macro-budget strategy. The value of outputs from Jeffersonian strategies would be evaluated at the integrated program level also. Thus when the strategy is being updated, groups of experts would be asked to evaluate what was learned from the totality of the work of the scientific enterprise. But the funds, once allocated to program managers in the agencies, would be distributed under the discipline of peer review, a micro-budget strategy to assure the highest quality in each and every research project. This distinction was clearly seen by Stokes (1997:121). For this reason OSTP and the NSTC (or its equivalent) must play a significant role in strategy setting and evaluation, but individual scientists would not be required to justify their basic research in terms of expected application benefits, whether it were funded under a Jeffersonian or a Newtonian budget allocation. (See section 6.1).

6.0 ENABLING THE AGENDA: THE SCIENTIFIC COMMUNITY– EXPECTATIONS, MOTIVATIONS AND GOALS

"Science is an ecological network not a linear sequence of activities. Science creates tools that change the questions." -- Maxine Singer.

At a time when a new covenant between science and society is being sought, and when disenchanted or aggressively anti-science forces chip away at the moral authority of science itself, the exhibition of the existence and eventual successful results of the Jeffersonian Model should change the general tone in the future. At a lecture at Harvard University, former presidential science advisory Neal Lane described the "science culture barrier…that has to do with our image, the image of the 'white lab coat,' or the 'ivory tower,' or 'cloning ourselves,' or 'curiosity driven,' or 'knowledge for knowledge's sake,' or similar descriptions that are seriously misunderstood."

Lane suggested "the concept of Jeffersonian Science can help us develop a strategy for addressing this image problem, but it will take some serious effort on our parts. There are, of course, two aspects to deal with. One is the need for understanding and consensus within the research community that this is the right way to think about science and even to set research priorities, at least at a general level. And, the other is the need for effective communication of these notions. A dialogue is really what we need, with the larger society."

6.1 A MICRO PROJECT STRATEGY

While it is essential for the scientific community to do a better job of talking about science with the public, with opinion makers and with politicians, the concept of basic science in service to public objectives does NOT (like the Mansfield amendment) mean that the researchers should be expected to defend the relevance of each specific project to the public objectives it might support. As discussed in section 5.3, the responsibility for identifying the national goal by which Jeffersonian science budgets are justified lies with government officials. This forms part of the macro-budget function. Applicants for an NSF grant are, of course, required to defend their proposals. They are asked to indicate how the proposed work will advance scientific understanding and how, also, it might bring value to society and to the institution of science[23]. These responses are an important part of the basis for peer evaluation. Scientists proposing single projects should not be required to describe in detail the public value that justifies the work[24]. They may of course volunteer that information if they wish. The primary job of linking basic science research and public objectives is for the funding agency, for its advisors, and for the lay institutions that identify goals they believe Jeffersonian Science should support.

6.2 HUMAN RESOURCES: EQUITY, SUFFICIENCY AND SOCIETAL INTERESTS

> *"Why does diversity matter in serving public objectives? Public objectives are about us. We need to know what "all of us" means. Our scientific understanding is often limited, and we are uncertain about consequences. It is very useful to have many viewpoints"*
> – Lillian Wu.

> *"Imagine how science might be different if women and minorities dominated science? Would public objectives be different? Would the way science work is done be different?"*
> – Paula Rayman.

The issues a Jeffersonian Science agenda seeks to address go beyond defining research agendas and specifying conditions for their performance. A basic question must be: "What capabilities does the nation require to address the challenges in our society in the coming century?" This phrasing clearly brings in not only the idea of a broader source of priorities for research investment, but the importance of education reform, of reversing the decline of student interest in science, and of the engagement of more women, minorities and handicapped persons in science – in short the balance of human resources required for a truly healthy scientific enterprise

As noted earlier, the public at large does not feel it has a sufficient stake in the scientific enterprise, particularly its more "ivory tower" parts in which linkage with the ultimate solution of societal problems is somewhat indirect and uncertain. This lack of a perceived stake in scientific and technological progress applies particularly to demographic groups that will form a growing fraction of the U.S. population and voters in the next two decades, if not in the longer-term future[25]. It applies as well to women, who will comprise a growing fraction of the full-time work force, including increasingly its upper echelons.

It has been pointed out that our unprecedented prosperity, largely driven by the explosive growth of the high tech sector, would not have been possible without a flood of immigrants educated abroad, particularly from India and China and other Asian countries. This dependence on foreign labor from mainly developing countries creates a very unstable situation that cannot go on indefinitely[26]. Moreover, it is likely to create a serious backlash if there is even a slight slowdown in the economy, since the number of low-tech and service jobs at the bottom of the social ladder has been steadily shrinking, and most of these jobs have been held by disadvantaged minorities or women. These people are voters and form part of the constituency that needs to be convinced that they have a stake in the economic benefits resulting from science. So far the backlash has been directed mainly at the WTO and free trade, but it is only a question of time before it is also directed against technology, and, by association, the science on which it is based.

Publicly supported Jeffersonian-type and even Newtonian type research is responsible for uncovering most of the otherwise unforeseen negative impacts of the application of science and technology, both old and new. Some of these adverse impacts fall differentially on the poor and on minorities. Women as a group are often much more concerned with adverse environmental and health impacts than the rest of the population. Jeffersonian science is an important tool for insuring the responsible use of technology, especially when it is developing very rapidly, and this relates directly to public issues in which minorities and women have a big stake. Consequently, it is important that these groups have spokespeople who are technically literate, and it is furthermore important that members of underrepresented groups are visible parts of the scientific-technical enterprise, so that these groups do not look upon scientists and engineers as “they”, but rather as a part of "us".

> *Key issues for minority participation and basic research in the service of public objectives:*
>
> - *Whether the needs of these groups get reflected in the overall research agenda and human development policies of the country.*
> - *Whether the voices of underrepresented minority scientists are present in key leadership roles at tables where policies are set, resources distributed, decisions made.*
> - *Whether they are part of the process of peer review, helping evaluate the merit of ideas that are proposed.*
> - *Whether problems that may emerge from issues that minorities raise make it onto the table, get funded, are under-supported, get marginalized, contribute to tenure.*
>
> -Shirely Malcom, Conference on Basic Research in the Service of Public Objectives, Nov. 2000

There are three dimensions of the human resource issue:

a) Is the opportunity to participate in finding solutions to the nation’s most pressing problems unfairly denied to a substantial fraction of the population?
b) Can a sufficient number of American scientists be trained without the full participation of women and minorities?

c) Will the expansion of research resources tied to visible societal goals attract the participation of minorities who otherwise would not pursue a scientific career?

At the November 2000 Conference Vivian Pinn pointed out that “the NSF report this year on women in science, mandated in law by Rep. Connie Morella, showed that in 1997 women constituted 23% of the S&E labor force. There was no progress from 1993 to 1997, and in computer science progress has been retrograde.” Paula Rayman observed that in a 1972 conference at the New York Academy of Science on women in science, participants hoped that by the year 2000, women would achieve parity with men. Not even half that goal has been reached in most disciplines. In new fields, like computer science, women made big inroads, but plateaued in the 1980s at 34%, and ever since female participation has been on the decline. In some fields, like medicine and life sciences, women are fairly represented numerically in graduate and professional schools, but remain underrepresented in senior positions, and face continuing obstacles to professional advancement. As Jaleh Daie noted, “the task is greater than any one sector's ability to rectify the deficiencies. We must leverage resources by creating strategic and lasting partnerships between the private, public and NGO sectors. These partnerships are essential to achieving long-term, systemic structural reform. A new forum based on the 4 Cs; Communication, Cooperation, Coordination and consolidation is needed to facilitate and create strategic alliances…we know we have achieved equity when participation of women is unremarkable, natural and taken for granted.

Our conclusion is that to mobilize the indigenous human resources required to address the nation’s critical problem, to ensure that the scientific community and the citizens whom they serve are in tune with each other, and to bring to Jeffersonian science the values and perspectives of the broad spectrum of Americans, the government’s research strategy must have an effective component of a more diverse and more attractive educational system in science and engineering.

7.0 MOVING FORWARD: JEFFERSONIAN RESEARCH STRATEGIES AS A CENTRAL FEATURE OF U.S. SCIENCE POLICY

7.1 LESSONS LEARNED

***Broad Acceptance of the Jeffersonian Science concept*:** The applicability of a Jeffersonian approach is accepted, at least in principle, by a broad spectrum of policy makers, scientists, and politicians. In the two-year history of this project we have experienced a ground swell of support for the idea and very little challenge to it. The question is how to move such an approach to the center of science policy formulation.

***Importance of properly defined national goals*:** Previous and ongoing initiatives have demonstrated broad support for categories of goals, such as quality of life and health, sustainable economy, environmental and natural resources, and security. The process of determining suitable goals and the science to achieve them must take the form of an extended national discussion involving all stakeholders.

Importance of constituents and the public to the formulation of science policy: stable funding for curiosity-driven research that supports broad societal goals must rest on a consensus of strong, supportive, public constituencies.

***Structural change is required in governmental and policy processes*:** Sustained public expenditure and multi-year budgeting, the need for interagency coordination and shared terminology, high-level non-governmental evaluation of long-range research projects, and expanded resources for OSTP should all be part of adapting US science policy to the realities of the 21st Century. There seemed to be a general accord with Bill Bonvillian's observation that "there is a big need to rethink the structure of the science portfolio and the concept -- whatever we call it -- needs serious debate." There is also a need for mission agencies to renew their commitment to their intellectual roots.

***Changes within the science community*:** The scientific community needs to undergo a number of changes in order to positively affect and enhance the nature of US science policy and its ability to meet national challenges:

- Need for improved communication between the scientific community and the public and policy-makers,
- Differentiation between macrolevel policy agenda setting, and microlevel scientific research – individual grant applicants would NOT need to become the principal defenders of the social outcomes side of their research.
- Expanding the base of scientific human resources is crucial to the future of US science policy. Linking social and national issues to science can have a strong impact on attracting women and minorities to what was previously considered predominantly a white male domain.

***There is no one-size-fits-all solution*:** Each national issue requires a unique process for creating the research strategy. The case discussions on education, energy, and global climate change demonstrated the tremendous variance in the adaptation of a Jeffersonian strategy to a given issue.

***Integrating the social sciences*:** There is a need for stronger integration of the social sciences into the process of formulating research strategies linked to societal outcomes.

7.2 MOVING FORWARD WITH JEFFERSONIAN SCIENCE – KEY ISSUES

***Individual and professional society initiative is crucial*:** It is vital to stress that we are not calling for a top down initiative, but rather the active participation of everyone reading this Report. The sources of change identified over the past several years are multiple and varied, and not limited to the higher levels of policymaking and government. Initiatives to create the research strategies we envision might come from groups of scientists, from science agencies in government, or from groups of concerned citizens. All will need to participate in defining the goals and setting the strategies.

***Continued growth of public investments in Newtonian science*,** pursued without regard for applications is essential at the same time that Jeffersonian research strategies are put in place. Neither is a substitute for the other.

A national conversation on the future of science policy must be composed of a series of ongoing stakeholder conversations. These conversations must be designed in a way that encourages not only evaluation within the various groups and communities involved in science policy, but must also cut across traditional boundaries. Scientists must listen to and inform government officials, public constituents must be strongly involved in the formulation of national goals and policies, agencies must represent the importance of the science to politicians.

Measuring the results of Jeffersonian Science is a crucial component to success. David Guston pointed out that the Jeffersonian Science frame begs some critical questions: How do we evaluate -- by what mechanisms and standards -- the results of Jeffersonian Science, and what do we do with the evaluations? Jeffersonian Science invites us to examine the how's of connecting research outputs to social outcomes. We have limited tools for limited assessments, e.g., licensing income for economic benefit. But can we develop others?

Comprehensive analysis of existing models, such as NIH and NSF, will help guide the expansion of Jeffersonian strategies to other national issues and goals. Both the successes and the shortcomings of current efforts can and must inform future policy.

The cases identified in the conference – education, energy and climate change – should be the subject of a broader exploration in order to devise appropriate Jeffersonian strategies. Experts from the relevant fields of science and policy, and the public should be brought together for an ongoing discussion as to how to connect the best science to the most problematic areas of these important national issues. As mentioned above, no single formula will work for all issues – strategies must be crafted on a case-by-case basis.

8.0. CODA

Few will disagree that the achievements of science and technology are going to be preeminent among the determinants of what makes a nation great in the 21st century, as economic potential, military might, and the social fabric of society ever more closely depend on scientific and technological progress. To a large degree, our civilization will only continue to flourish to the extent that science and technology continue to perform in the service of excellence and of the common weal.

One of the obstacles to embracing this truth, and acting upon it, is a certain shortsightedness, an unwillingness, within society at large, and the political system in particular, to wait for delayed gratification. Such an attitude hurts both the achievement of progress toward social goals but also the support for basic scientific research whose potential social benefits, although in many instances very real, often become evident only at a longer view. Our Initiative is intended to ameliorate this problem by strengthening the coupling of basic research and social needs.

To summarize our chief argument: We have proposed an integrated tripartite strategy of federal science policy that adds a third mode to the federal research portfolio, which customarily has been understood in terms of a dichotomy of basic and applied research. To be sure, both applied research that transforms theoretical knowledge into useful products and services, and curiosity-driven basic research that, as the record of the past decades amply demonstrates, has led to unpredictable spin-offs of enormous societal value, will remain vital to a healthy national science enterprise. But the addition of a third mode—basic science that is targeted in an area of important societal objectives, or "Jeffersonian Science"—is likely to bring about specific major advantages over the current system. First, it will enable science to do even more, and more quickly, for society than science is currently being asked to do, and it will thus speed societal progress. Second, it will make the link between basic research and public objectives more obvious, so that the public will have a greater appreciation of the benefits of science, and will be inclined to provide increased and stable support not only for Jeffersonian Science, but for the whole scientific enterprise. And finally yet importantly, it will enhance science's claim to moral authority, which any enterprise, including science, deserves insofar as it is understood to serve the public interest.

These are not radical or untried notions, especially not in American history. As long ago as 1743, Benjamin Franklin sparked the founding of a scientific society "for Promoting Useful Knowledge." And in 1780, John Adams gathered a group of his colleagues to create an organization "to cultivate every art and science which may tend to advance the interest, honor, dignity, and happiness of a free, independent, and virtuous people."

Having entered upon a new century, with its own dangers and opportunities, we remain committed to these same aims, as worthy now as they were then.

NOTES – SCIENCE FOR SOCIETY

1) Basic scientific research is a concept popularized by Vannevar Bush in *Science the Endless Frontier*. Bush believed the creativity of basic science would be lost if it is constrained by premature thought of practical use, a concern that motivated the sometimes challenged distinction between basic and applied research. Many authors prefer "fundamental research" (to characterize its outcomes as contributions to understanding of nature), or "creative research" (to describe the conditions under which it is performed) over "basic research" (emphasizing the curiosity-driven motivation). All three (strongly related) definitions, based on the motivations of the investigator, outcomes, or conditions of research, say nothing about the motivation of the sponsor of the work.

2) Ivory Bridges: Connections Between Science and Society, Cambridge, MA: MIT Press (forthcoming).

3) Some may ask whether the concept of Jeffersonian research is essentially the same as Donald Stokes "use-inspired basic research" or research in Pasteur's Quadrant of Stokes's two dimensional diagram distinguishing "pure basic research," pure applied research" and use-inspired basic research [Pasteur]. Stokes sees Pasteur's quadrant primarily in terms of mixed motives by the investigator – both intrinsic curiosity and extrinsic social value. He recognizes and discusses, of course, public motives in sponsoring such research for its social value. But he does not recognize the need for a conscious strategic plan to pursue a long range, "basic" agenda justified by pursuit of a social goal. That is, of course, the distinction between Jefferson - a public official who created a charter and provided the resources but did not do the research – from Pasteur, who created his own mixed-motives agenda from a variety of resources. Thus the concept of Jeffersonian science takes Stokes's ideas one step further, providing a reasonable justification for using Jefferson as the metaphor rather than Pasteur.

4) Gerald Holton and Gerhard Sonnert, *Issues in Science and Technology*, Fall 1999. They use "Jeffersonian Science" and "Jeffersonian Research" synonymously.

5) The best of the corporate research laboratories such as Bell Labs and the IBM Watson Laboratory have long practiced a management style in which research managers choose fields and recruit research staff with a keen eye to the likely commercial value of the research, relying heavily on self-motivation of the researchers to be both imaginative and productive. Those researchers are, of course, well aware of the needs of the company and do not require much urging to look for opportunities to make a corporate contribution.

6) Baconian research, by contrast, is not only motivated by specific, mission-oriented goals but entails the application (and perhaps extension) of existing knowledge to the achievement of such goals, usually with constrained time tables and resources.

7) From a science policy point of view the place of both Jeffersonian and Newtonian Science in national policy follows directly from the distinction, popularized by Harvey Brooks, between "science for policy" and "policy for science." Jeffersonian science follows logically from the government's need to inform policy on issues facing the nation with the best understanding from science. Newtonian science follows logically from the importance of " policy for science" – the obligation of government to ensure the most vigorous, imaginative, and internationally competitive capabilities in science and technology.

8) "The War on Cancer", *US News and World Report* <www.usnews.com/usnews/issue/cancer.htm>

9) The Commission seriously debated the idea that some part of the NSF activity should merit priority from national goals to which the research might contribute, but the report language was quite muted. National Science Board Commission on the Future of the National Science Foundation: *A Foundation for the 21st Century: A Progressive Framework for the National Science Foundation*

(Washington DC: National Science Board, November 20, 1992.)

10) *How People Learn: Brain, Mind, Experience, and School*, Bransford, John D., Ann L. Brown and Rodney R. Cocking, eds., (National Academy Press, Washington DC) 1999

11) *How People Learn: Brain, Mind, Experience, and School*, p. xxi (executive summary).

12) The growth of interest in distance learning (applied today to post-K12 education) together with the growth of e-commerce might accelerate this possibility, especially since e-commerce allows geographically dispersed fragmented markets to be integrated. E-commerce and similar e-education shift responsibility for funding education from public venues to individual ones. The issue of what will be lost in the process should receive more attention.

13) Climate Change Impacts on the United States: The Potential Consequences of Climate Variability and Change, US Global Change Research Program, Published 2000/2001 (www.usgcrp.gov/usgcrp/Library/nationalassessment/default.htm)

14) US Global Change Research (GCR); NRC, NSF (Envir. Sci and Eng. For 21st Century), IPCC reports.

15) See David Guston, *Between Politics and Science* (Cambridge University Press, 2000)

16) One observer noted "No one dies of 'general medicine'."

17) Thus the New England Journal of Medicine is more effective at conveying the value of biomedical science to the public than are any of the professional journals in biomedical sciences.

18)*National Patterns of R&D Resources: 2000 - Data Update (current to March 2001)*, National Science Foundation,
<http://www.nsf.gov/sbe/srs/nsf01309/start.htm>.

19) Allan Bromley stated that FS&T cut the ground out from under the argument for transferring defense R&D to civil R&D.

20) see Stokes 1997:64-70; Branscomb 1999.

21) The idea of "basic technology research" is explored in L. M. Branscomb, "From Science Policy to Research Policy" in L.M. Branscomb and J. Keller, eds, *Investing in Innovation* (Cambridge MA: MIT Press 1998).

22) Some people have suggested that OSTP should receive an operating budget so it could fund selected, long range research programs directly. Bromley specifically advised against this idea, for which there was no spoken support in the meeting.

23) NSF's Criterion 2 states: What are the broader impacts of the proposed activity? How well does the activity advance discovery and understanding while promoting teaching, training, and learning? How well does the proposed activity broaden the participation of underrepresented groups (e.g., gender, ethnicity, disability, geographic, etc.)? To what extent will it enhance the infrastructure for research and education, such as facilities, instrumentation, networks, and partnerships? Will the results be disseminated broadly to enhance scientific and technological understanding? What may be the benefits of the proposed activity to society? (NSF Important Notice No. 125, Sept. 20, 1999 <http://www.nsf.gov/pubs/1999/iin125/iin125.txt>)

24) During the Vietnam War, scientists supported by the Department of Defense were required by the Mansfield Amendment to document how their research might improve military capability. This question should have been answered by the military agency project managers who had access to the needed information. If application values are to form part of the peer evaluation of project merit,

the peer panels would have to include experts in the field of application, as well as experts in the relevant disciplines.

25) The Y2000 census has revealed that whites are now a minority of 47 percent of the population of California, home of a disproportionate fraction of the nation's scientific and technological capabilities.

26) Stephan-Götz Richter, "The immigration safety valve keeping a lid on inflation," *Foreign Affairs*, Vol. 79, No.2.

APPENDIX A:
THE BASIC-APPLIED DICHOTOMY IN SCIENCE POLICY: LESSONS FROM THE PAST

By Gerhard Sonnert and Harvey Brooks

Prepared for the Conference on Basic Research in the Service of Public Objectives
November 2000, Washington , DC

The dichotomy of basic versus applied science pervades today's thinking about science policy. Transcending it was the key motivation of this Conference ("Basic Research in the Service of Public Objectives," Nov. 28-29, 2000), and the major thrust of that initiative has been properly directed toward the future—how to envision and implement a strong linkage between basic science and the societal good.

Even in a forward-looking enterprise like this, it may be profitable to throw a brief glance back to the past. We hope that the benefits of this historical review will be twofold: It should reveal that the basic-applied dichotomy itself is not inevitable and has been found wanting by many observers. Furthermore, it should show that there have indeed been numerous precursors to our current initiative. They may not have been always successful, but both from their progressive ideas as well as from eventual failures, one might learn what is worth adopting in the collection of statistics and in the formulation of science and technology policy.

Conceptual considerations

Among serious scholars of science policy, few have been wholeheartedly satisfied with the familiar notion of the basic-applied dichotomy—that is, the classification of research into two kinds: "basic research" that has no regard to applicability, and "applied research" that applies already known science for tangible benefits. There have been numerous attempts at re-conceptualizations, for instance Donald Stokes's (1997) concept of four research quadrants. In Stokes's framework, the linkage of basic research and the national interest, which is our goal, appeared in what he called "Pasteur's Quadrant." Moreover, Lewis Branscomb (1995, 1998) appropriately warned that the level of importance that utility considerations have in motivating research does not automatically determine the nature and fundamentality of the research carried out. Branscomb distinguished two somewhat independent dimensions of how and why research is done: the character of the research process itself (ranging from basic to problem-solving) and the motivation of the research sponsor (ranging from knowledge-seeking to concrete benefits). For example, a basic research process, which for Branscomb (1998:115) comprises "intensely intellectual and creative activities with uncertain outcomes and risks, performed in laboratories where the researchers have a lot of freedom to explore and learn," may characterize research projects with no specific expectations of any practical applications, as well as projects that are clearly intended toward application. A large number of observers would agree with Branscomb's (1998:120) verdict that current definitions based on the basic-applied dichotomy "are the source of much of the confusion over the appropriate role of government in the national scientific and technical enterprise."

A few insiders in the federal government have also shared this sentiment. Almost from its inception, some National Science Foundation (NSF) officials on occasion strained against the basic-applied dichotomy. Alan T. Waterman (1965:15), the first NSF director, for instance, suggested a subdivision of basic research into "free research undertaken solely for its scientific promise, and mission–related basic research supported primarily because its results are expected to have immediate and foreseen practical usefulness." In the years that followed, there have been several other attempts to reform the dichotomy or to try out different conceptual schemes. In 1979, NSF organized a conference on the categories of scientific research (NSF 1980a), and about a decade later it undertook a similar effort (NSF 1989). The latter suggested replacing the dichotomy of basic vs. applied research by a trichotomy of fundamental, strategic, and directed research. None of these initiatives made a lasting impact. Although most who studied the issue agreed that the dichotomy was flawed, it just would not die. By the sheer inertia of its existence (and partly through the accumulation of statistics that have been aggregated on its basis [Godin 2000]), it continued to provide the conceptual framework under which data were presented and policy for the support of science was formulated.

What then underlies the resilience of the basic-applied dichotomy, other than the retentive force of the familiar, or perhaps a general human tendency toward over-simplification? We need to start with the observation that the interests of scientists, engineers, and physicians on the one hand, and of politicians and administrators on the other—the two principal groups most involved in government-supported research and the application of its results—may partially converge, but are not identical. A sizable rift exists between the motivations of scientists for doing science, and the motivations of their patrons (in this case the federal government) for paying them to do science. Scientists typically conduct research for the intrinsic intellectual thrill of the process and are less interested in the practical benefits of their work. This predilection is reflected in the science-internal hierarchy of what kind of science is prized most, especially in academe: "Basic science"—curiosity-driven research without regard to applicability—usually carries a higher prestige than "applied science"; and even a certain snobbery of the basic toward the applied scientist can sometimes be observed (Bernal 1967:96, Brooks 1967).

Politicians and government officials, by contrast, are interested in societal and political benefits of research. This is the main reason why government supports science. The political system has typically been somewhat reluctant to invest tax dollars in activities where the ultimate payoff is not at least plausible at the outset. Another motive for politicians to be wary of scientists' requests for federal financing of their basic research is the perceived elitism of research science. Ever since Alexis de Tocqueville's (1862 [first publ. 1835-40]) famed report about the United States in the early 19th century, it has been noted repeatedly that the American political system, as well as American society in general, tends to be highly suspicious of any non-accountable elites. For many politicians, the prospect of supporting a numerically small, fiercely independent, and oddly inscrutable elite of research scientists elicits little enthusiasm—especially if societal or political benefits are not seen as forthcoming.

Owing to an auspicious historical constellation, federal support of basic science—irrespective of applicability, in the way most scientists prefer it—was institutionalized in the years following World War II on the basis of the basic-applied dichotomy. We shall discuss below how this occurred. However, because the interests of politicians and scientists diverge, this governmental support of basic science has typically been precarious. In view of that tenuous situation, many members of the scientific community as well as members of the federal science administration have clung to the old basic-applied dichotomy, perceiving it as the raison d'être of the federal funding of basic science. The clear-cut dichotomy might, in their opinion, be able to prevent any creeping encroachments on the position of basic science, and to forestall a slide down the slippery slope toward the abandonment of curiosity-driven research by the political system.

Through the late 1970s, many politicians who were suspicious of "useless" basic research instead favored supporting applied research with short-term and obvious benefits; but the Reagan Administration provided an additional twist. It was also suspicious of governmental support for applied research. In its view, applied research should better be left to the private sector, because government involvement in that area would only distort "the market," to bad effect.

The concept animating the Conference (for which, as will be explained, a shorthand term might be Jeffersonian Research [Holton 1986, Holton 1998, Holton & Sonnert 1999]) constitutes an ideological middle ground between the two positions. Because it supports the pursuit of basic research, it avoids the arguments leveled against governmental meddling in the development of commercial products and services. But because it also asks that such research be demonstrably located in an area of plausibly achieving national goals, it may as well satisfy those who insist on seeing societal benefits come out of government-sponsored research.

The basic-applied debate and Vannevar Bush

Whereas the distinction between different kinds of research has a long history (Godin 2000), the basic-applied debate took its classic form in the dialogue between Michael Polanyi and J. D. Bernal in pre-World-War-II Britain. In the American context, the two protagonists became Harley Kilgore, a New Deal senator from West Virginia, and Vannevar Bush, an engineer by training who headed the Office of Scientific Research and Development (OSRD) during the war and before that had been president of MIT. Against Bush's efforts to protect basic research, Kilgore emphasized that federally funded science should be harnessed to addressing society's needs. His views were an exposition of what Lewis Branscomb called the "use it or lose it" philosophy of science management[1].

It is ironic but significant that the common notion of the basic-applied dichotomy is in fact a caricature of the relationship between the two sides of the historic argument. Each position in reality was much more complex, and less antagonistic toward its counterpart. It is true that, in comparison with Bernal and Kilgore, Bush emphasized the importance of basic research for which no immediate prospects of applicability were perceived. However, the famous Bush report to President Roosevelt, <u>Science, the Endless Frontier</u>, released in 1945, did not divorce basic science from social benefits completely. On the contrary, one of the ideas was, for instance, that basic science could serve as a resource to be drawn on in the solution of public problems. Basic science would foster development through helping to establish an agenda for what was worth trying in more applied projects. It could do so by predicting what prototypes or policies would be most likely to work. The more profound the theoretical understanding, the more accurate the prediction would become.

Bush envisioned an agency that was organized according to public missions such as health, defense, etc. He was not talking just about pure science, in which only disciplines were relevant. In the Bush panel report to Harvard University President

Conant about what to do with the Gordon McKay bequest (Bush 1950), Bush played down the distinction between basic and applied research, and it seems clear he was thinking more about something like Jeffersonian Research[2].

Yet basic scientists discovered a great deal they liked in Bush's Science, the Endless Frontier. The document was—simplistically—interpreted in much of the scientific community as a charter for pure science with little reference to potential applications. The proposed National Research Foundation, whose priorities scientists would set with minimal regard for applicability, and whose criteria for support would be determined by theoretical understanding and conceptual generality, captured the imagination of many scientists. This heralded governmental support for the basic research they cherished!

Bush indeed clearly felt that too much pressure for applied research, in his famous words, "invariably drives out pure" research (Bush 1945:83), and he feared that not enough basic science would be performed in the United States to provide the "scientific capital [that] creates the fund from which the practical applications of knowledge must be drawn.... [B]asic research is the pacemaker of technological progress (Bush 1945:19)." Bush thus recognized that basic science needed special protection; and he and the other shapers of the postwar federal science system were in a unique position to pull it off. The major contributions science had just made to victory in World War II afforded the prestige and the persuasive force to institutionalize the large-scale support of basic research in the federal science structure. The scientific community latched on to this new arrangement and defended it—and it adopted and maintained the much-simplified basic-applied dichotomy that was perceived to be its conceptual foundation.

Much of the scientific community of the day was convinced that two theorems, working in concert, bridged the two parts of the traditional dichotomy. First, there was a linear assembly line model of innovation (basic research leading to applied research leading to product development). It is commonly attributed to Vannevar Bush, though it is also a somewhat distorted picture of his real views. Second, there was the idea of the unpredictability of the eventual applied spin-offs from basic research. Taken together, these two notions justified governmental support of basic research without an initial evaluation of its potential societal benefits. If basic research is the fountainhead of societal innovation, and if it is unpredictable which basic research will lead when to what (if any) societal benefits, a wide array of basic science projects, so the conclusion, should be sponsored without initial regard for applicability. The argument certainly has merit, but its paints an incomplete picture that should be augmented in at least two ways.

Let us turn to the first theorem. By the time the report entitled Science in the National Interest was released by the Clinton Administration in August 1994, the notion of an assembly-line relationship between the basic and applied research, which were seen as competitors for funds, had been superseded by a different view. The report stated: "We depart here from the Vannevar Bush canon which suggests a competition between basic and applied research. Instead, we acknowledge the intimate relationships among and interdependence of basic research, applied research and technology, and appreciate that progress in any one depends on advances in the others.... (Clinton & Gore 1994:17-18)." The new metaphor for the relation of basic science to development, in the words of the report, is "an eco-system [rather] than a production line."

Another forward-looking initiative to re-think science policy was the report from the House Committee on Science, entitled Unlocking Our Future: Toward a New National Science Policy, which was produced under the leadership of Congressman Vernon Ehlers and issued in September 1998. Far from being separate and distinct, the seemingly initially unrelated pursuits of basic knowledge, technology, or instrument-oriented developments were now understood to be the weaving of a single, tightly woven fabric, of one "seamless web."

The second suggested augmentation of the postwar creed asserts that, beyond the basic-applied dichotomy, there can exist a third kind of research—Jeffersonian Research—that connects curiosity-driven basic research much more closely to societal benefits than the notion of unpredictable spin-offs does.

From early beginnings through World War II

We now briefly review the historical development of the basic-applied dichotomy and of some attempts to overcome it. An obvious watershed in science policy was World War II. During the war and thereafter, the federal government got involved in funding and pursuing science and technology at an unprecedented scale. Nonetheless, it should not be overlooked that there were early roots of a federal engagement in science, reaching all the way back to the founding era of this nation. When the Louisiana Territory was purchased, President Jefferson initiated the Lewis and Clark expedition and gave it a broad charter to explore all aspects of the newly acquired lands—both to bring back new purely scientific knowledge (e.g., plants, customs), and to acquire maps and practical knowledge of the vast unexplored territory into which the U.S. population was bound to expand. This fortuitous combination of basic research and the national interest is the paradigmatic example of "Jeffersonian Science" (Holton 1986). One might think of the U.S. Geological Survey, established 1879, as a more permanent institutionalization of the geological aspects of the Lewis and Clark venture and other earlier expeditions.

In 1830, the Depot of Charts and Instruments was founded, which in 1844 was re-established as the U.S. Naval Observatory. Originally intended to support the Navy's navigational needs, it became active also in wider areas of astronomical observation and time measurement. Another area of science in which the federal government was actively involved from early on was agricultural research. Through the land grant system, the government supported both research in the agricultural disciplines and extension work[3]. The National Institutes of Health trace their beginnings to a laboratory that in 1887 was established within the Marine Hospital Service (MHS). In 1930, the Ransdell Act changed what then was known as the Hygienic Laboratory into the National Institute (singular!) of Health. The National Cancer Institute was established in 1937. During the pioneering days of aeronautics, the government established the National Advisory Committee for Aeronautics (NACA) in 1915, and this organization was to become the nucleus of the much larger National Aeronautics and Space Administration (NASA), formed in 1958. The largest part of these government-sponsored research activities was heavily weighed toward applied science and development. "Basic science" without regard to missions considered to be governmental responsibilities was not supported; most people at that time would probably have condemned any thought of government-funded basic research as a preposterous scheme of mis-allocating public money.

During the New Deal era of the 1930s, the notion of an activist federal government that intervened in many areas of life was ascendant. Support for the idea of government-sponsorship of research grew, as did the concern for making such research relevant to the solution of societal problems. A government-science collaboration of enormous magnitude became reality during World War II. The Manhattan Project was only one of several endeavors to harness basic science for the war. Among the others were the developments of radar, synthetic rubber, and pharmaceuticals. Although the goals of the Office of Scientific Research and Development (OSRD) were heavily on the applied side, the OSRD laboratories were given a great deal of freedom in deciding on their own strategy and specific routes. Moreover, the OSRD contract laboratories in universities were by no means the only sites of the larger effort. The laboratories of the Armed Services and industry contractors were poised to take over where the R&D of the OSRD laboratories left off, and there was close liaison among all parts of the system to achieve the smoothest possible transition from laboratory to production to field trial and back again. Winning the war was of course the primary societal goal behind governmental science-sponsorship during this epoch.

Post-war adjustments

When the war ended, it was generally agreed that one could not go back to the pre-war situation, and that government had to continue playing a major role in supporting scientific research. Yet how exactly this should be done was the matter of hot debate. An early conflict, for example, was over how the legacy of the Manhattan Project should be controlled and what form the Atomic Energy Commission should take.

The Office of Naval Research (ONR) was founded in 1946 to continue work done during the war under the auspices of the Office of Scientific Research and Development (OSRD). The young officers—many of them commissioned reservists who had been scientists or engineers in civilian life before the war, and continued in the Navy for a short time after war's end—in 1946 attempted to turn ONR into a model government foundation. They quite consciously emulated the National Research Foundation proposed in Science, the Endless Frontier, which had been widely publicized and discussed within the general scientific community. Although applicability to problems of potential Navy interest was by no means ignored, it took a back seat relative to "good science" in any major field, whose results were allowed to appear in the usual scientific publications. The system of peer-reviewed project proposals from principal investigators, chiefly in universities, with which we are familiar today, was developed at ONR and served as a model for NSF after that organization was finally established. Indeed the research offices of each of the three military services followed similar lines of development and constituted the principal sources of funding for basic research until the late 1950s. The forerunner function of ONR also became apparent in the selection of the ONR chief scientist, Alan T. Waterman, a former Yale physics professor, as the first director of the National Science Foundation (NSF). He took quite a few staff members with him to NSF.

NSF itself was a relative latecomer on the postwar science scene. Although Vannevar Bush had proposed a National Research Foundation in 1945, several years of (at times heated) debate passed until the National Science Foundation (NSF) was established by legislation in 1950. The eventual outcome was in some ways the result of a compromise between Bush's original ideas and his counterparts' desire to make federal science support more responsible and responsive to the political system. Whereas Bush had proposed a relatively independent organization that was run by scientists themselves, President Truman vetoed such a bill. He insisted on more political accountability for the appointment of the NSF director and thus for the funds to be spent. In the end, NSF was established with an organizational setup acceptable to Truman: The President was to appoint the NSF director as well as the National Science Board (Mazuzan 1994). Nevertheless, it is important to mention that the Presidential appointees were professionals, selected primarily for their scientific qualifications, though not exclusively so.

In the early years of NSF under Waterman, the agency resisted pressures from the Bureau of Budget to develop a federal science policy and evaluate science programs throughout the federal government—in other words, to take on a leading role for science in the federal government and thus become some kind of a surrogate for a Department of S&T. Instead, Waterman emphasized the role of NSF as supporting basic academic research (and science education). This established the identity of NSF as the champion of the "basic" part of the basic-applied dichotomy. At a pragmatic level, one might add that the fledgling NSF probably also lacked the political clout to establish itself as the lead agency over older and larger government units. In any case, the devotion of NSF to the funding of basic research, as understood and cherished by the scientific, and especially academic-scientific, community, is one of the reasons for the durability of the basic-applied dichotomy.

The National Institute of Health (which had come into being earlier) expanded in the immediate postwar era. Following the pattern of the pre-war Cancer Institute, a few additional institutes focusing on other diseases were founded after the war; and, in 1948, the National Heart Act named the umbrella organization for these individual institutes the National Institute<u>s</u> (plural) of Health (NIH). In contrast to NSF, NIH circumvented the traditional basic-applied dichotomy and garnered overwhelming support for its basic research programs by tying them closely to the public interest (i.e., combating disease), without however becoming merely an applied-science agency. The steady growth of the NIH budgets testifies to the success of this agency's Jeffersonian-like strategy.

The "Golden Age"

The launching of Sputnik by the Soviet Union in 1957 was a cataclysmic shock for both the political leadership and large parts of the American public. It led to a swift and dramatic upgrade of the governmental support of the sciences—ushering in what could be called the "Golden Age" of science that arguably lasted at least into the late 1960s, at what time the demands of the Vietnam War became predominant. When the Soviet Union, which had quickly gone from World War II ally to Cold War rival, broke the United States' monopoly on nuclear arms in 1949, this was already cause for some concern. But when Sputnik demonstrated that the USSR had now even moved ahead of the United States in some areas of missile technology, with potentially disastrous consequences for American security, this was felt to be truly alarming. It became a matter of highest national priority, supported by a broad public consensus, to regain the scientific and technological edge.

This global definition of a national objective for government investments in science might be seen as a comprehensive Jeffersonian umbrella extending over the entire government science budget. Another interpretation would be that, instead of a conceptual re-orientation, simply more money was being poured into essentially the old system. Driven by a sense of emergency, the political system started to support (in addition to expanded applied programs) as many "long shots" of basic science as possible, hoping that the more basic research with unpredictable usefulness was funded, the more likely it would become that some beneficial spin-offs would materialize. In any case, large streams of money began flowing into all kinds of science. For the science community these government policies translated into more money for basic research (as well as applied research), and provided ample reason for clinging to the conceptual framework of the basic-applied dichotomy.

New science institutions were set up, such as NASA (out of its precursor, NACA). President Eisenhower created the office of the Presidential Science Adviser as well as the President's Science Advisory Committee (PSAC). Congress formed permanent committees to deal with space and science issues. The Science Adviser additionally became the director of the Office of Science and Technology when President Kennedy established it in 1962.

The existing science units saw their budgets balloon. NSF's annual budget in its first three years had been held to $15 million and had only climbed to $40 million by fiscal year 1958 (pre-Sputnik). Then it jumped to $134 million in one year (Mazuzan 1994), much of it for science education, which was also identified as an area where America had fallen behind her Cold War rival. Based on the National Defense Education Act of 1958, enormous efforts were started to assist science students and young scientists, for instance through student loans and graduate fellowships, and through support of science teaching in elementary and secondary schools. Science education proved particularly congenial to President Johnson's Great Society program of an active government devoted to redressing social inequities. It had the political virtue for the egalitarian-minded President that it helped offset what he considered the elitist distribution of research project funds that resulted from the peer-review process[4]. By their very nature, science education funds could be spread evenly across all states and regions.

Even in the heyday of basic science support in the post-Sputnik era, however, there was periodical grumbling about the wisdom of spending federal money on basic research (cf. Omenn 1985). Already in 1960, the AAAS Committee on Science in the Promotion of Human Welfare described an ongoing debate about governmental support for basic research. "The basic difficulties seem to be the absence of any over-all rationale in the support of science and the overemphasis on projects that give promise of immediate practical results (AAAS 1960:72)."[5]

One of the assignments given to NSF by its founding act was the evaluation of the science programs of other government agencies with a view to formulating a coordinated "science program" for the federal government as a whole. A small, nascent agency such as NSF was very reluctant to undertake this, and in 1962 President Kennedy turned to Jerome Wiesner, his Science Adviser, for the task. Wiesner in turn asked Hugh Loweth of the Bureau of the Budget (BoB) to bring together a government-wide task force consisting of the principal science officers of each of the federal agencies supporting science. Wiesner also recruited Harvey Brooks to co-chair this task force as an outside adviser (as a member of the President's Science Advisory Committee [PSAC]).

It turned out that these science officers had never gotten together in such a way before, having instead negotiated individually with BoB on behalf of their respective agencies, but never in the light of a government-wide science plan. This was a time when there were mutterings in Congress (e.g., by Senator Humphrey) about the need for a Department of Science. The discussion was useful, but fell far short of coming up with a unified, coherent plan, or even agreeing that such a plan was necessary or desirable. The "science budget" was dominated by the "big ticket" items, such as the burgeoning Apollo spacecraft program and the first of the ballistic missile programs. Projected R&D budgets tended to fall off a cliff as soon as astronauts were to land on the moon at the end of the decade. At the same time, the NIH program was expanding so rapidly that it dwarfed all the other basic and applied research programs. Perhaps the most useful function of the task force was to make some of the health professionals aware of new developments in the physical sciences that might have applications in biology and medicine. It did not, as some may have hoped, lay the groundwork for a Department of Science, or even suggest that such might make sense.

Soon after this rather unproductive OST initiative, the Committee on Science and Astronautics of the House of Representatives undertook an attempt to help devise a sound rationale for the federal support of science. In December 1963, it contracted with the National Academy of Sciences—in the "first contract ever entered into by Congress and the Academy (NAS 1965:v)"—to conduct a thorough study of the matter. At that point, the National Academy had already started a project on its own. Upon request of its membership at the annual meeting in April 1963, the Academy[6] undertook a study, funded by the Ford Foundation, that resulted in the 1964 report entitled Federal Support of Basic Research in Institutions of Higher Learning. For the House Committee, the Academy then produced two substantial reports on the topic, Basic Research and National Goals in 1965, and Applied Science and Technological Progress in 1967[7]; but these efforts could not forestall the crunch of research funding in the late 1960s.

As long as funding for all aspects of science grew, it was easy to overlook the potential conflict between the two parts of the basic-applied dichotomy (as well as between egalitarian and elitist tendencies). Yet toward the later years of the Johnson Administration, the pie stopped getting larger, partly owing to the growing costs of the Vietnam War. At the same time politicians demanded a tighter control of basic research and expressed a greater preference for applied research. Concluding a review process that had started in 1965, the Daddario-Kennedy amendment of the NSF basic law in 1968 subjected the agency to greater control (or greater accountability, depending on one's point of view), established the social sciences as a designated field of support, and authorized the support of applied research as well as basic research. In this period, thus, pressures favoring applied research over basic research began mounting, especially as the end of rapid growth came into view. President Johnson conspicuously emphasized the importance of tangible, short-term (and political) benefits when he declared during a visit to the National Institutes of Health (NIH) in 1966 that "Presidents ... need to show more interest in what the specific results of research are—in their lifetime and in their administration ... (cit. by Omenn 1985:1110)."

When, in 1966, the Office of Naval Research sponsored a convocation on "Research in the Service of National Purpose" to celebrate its twentieth anniversary, signs that the Golden Age was waning were discernible (Brooks 1966:33). A few months later, the Defense Department released a report from Project Hindsight (Sherwin & Isenson 1967, cf. Greenberg 1966) which had ostensibly examined the scientific and technological innovations that advanced the development of a number of sophisticated weapon systems, and concluded that only 0.3% of these innovations came from "undirected science." Because this study was apt to fuel criticism of the scientists' favorite doctrine of "unpredictable spin-offs," it created quite a stir in the scientific community (e.g., Science, 154 [2 Dec. 1966]:1123, 155 [13 Jan. 1967]:150, 157 [29 Sept. 1967]:1512).[8]

Rise of applied research and Jeffersonian attempts

As the "Golden Age" ended, the realities became harsher and more difficult. Although there were numerous attempts at reforming science policy and at re-defining the relationship of science and politics, the system as a whole remained pretty much the same. Yet, especially since the end of the Cold War, the need for a fundamental re-thinking of the system, which to a large part has been based on the premise of the basic-applied dichotomy, has become more apparent. This has been exacerbated by the inability or unwillingness in Congress and the Executive Branch to commit to a reasonable increase in funding for research (other than for NIH). The time now seems to be ripe for an explicitly Jeffersonian initiative. To some extent, history during the past three decades has been preparing for this change.

Partly to react to the increasing pressure on NSF to be more responsive to its explicit applied research mission under the Daddario-Kennedy amendment, NSF introduced a new program called Research Applied to National Needs (RANN), which lasted from 1969[9] to 1978 (Mazuzan 1994). As incentive, the Office of Management and Budget (OMB) promised NSF a budget increase of $100 million, half for the new applied program, and half to take over projects that the three Services had to relinquish because of the 1970 Mansfield amendment. The amendment prohibited the Defense Department from funding any research not directly related to military function or operation, and directed NSF to conduct the more basic research programs that up to that point were run by the military.

From its start, RANN differed from the traditional NSF programs. It was organized around designated (i.e., societally defined) problems, and it attempted to link up industry with academic research from the beginning. RANN's chief aim was to encourage and accelerate the application of existing basic scientific knowledge to a wide range of potential uses. Criticism of this new program abounded, the fiercest opposition coming from the science community, which feared an erosion of support for basic or "pure" research. The reorganization of the NSF's applied research programs in 1978 created a division of Integrated Basic Research (IBR) as part of the new Directorate for Applied Science and Research Applications. Unlike the older, applied-research activity of RANN, the division of IBR was formed to provide "support for basic research that has a high relevance to major problems" in selected topic areas in the basic research directorates. This could be interpreted as a move toward a more Jeffersonian approach.

The same ideas pervaded the Tenth Annual Report of the National Science Board of the NSF, released on August 2, 1978, which was entitled Basic Research in the Mission Agencies: Agency Perspectives on the Conduct and Support of Basic Research (NSB 1978). The report contained a collection of basic research topics that were perceived to respond to a national need and thus to merit government support. Sixteen agencies, ranging from the Department of Agriculture to the NSF, and from the Department of Housing and Urban Development to the Veterans' Administration, had contributed basic research questions whose solution, in their view, would benefit their missions.[10]

The Press-Carter Initiative

One of the less well-known episodes of creating a Jeffersonian element within the federal research structure, that is, of attempting to link basic research to national objectives, occurred in late 1977 and early 1978, at the beginning of the Carter Administration. We considered it worthwhile to do some thorough research on this relatively obscure episode, because it holds potentially valuable lessons for transcending the basic-applied dichotomy. A brief summary of the events can be found in Holton & Sonnert (1999), and a more detailed account is forthcoming (Sonnert & Holton [in press]).

With Frank Press, the President's Science Adviser, as the driving force, the government agencies were asked to propose basic science research questions whose solutions they thought would help the federal government significantly in fulfilling its mission. This poll led to the assembly of a "master list" of more than 80 research questions, which is appended to this paper. Further iterations would have been necessary to coordinate the various suggestions and set priorities. But the initiative stalled before completion. It was overshadowed by other, more pressing concerns and finally became obsolete with Carter's electoral defeat in 1980. Regardless of the disappointing end of the initiative, we think that the master list that was produced is an astounding document, which at first glance contains many forward-looking questions. This is a testament to the quality of scientific expertise available in the government agencies at that time. The survey could not have succeeded to the extent it did without the groundwork that previous Science Advisers, especially Jerome Wiesner and George Kistiakowsky, had laid by injecting a great amount of scientific expertise into the executive arm of the federal government. It will be an interesting exercise to for a judgment about the quality of the whole master list, and we intend to submit it to panels of experts to see how the questions would appear today in the wisdom of hindsight. But already there are a number of lessons one can draw from the Press-Carter Initiative:

☐ Allies are important. The alliance of Frank Press and the Office of Science and Technology Policy (OSTP) with the Office of Management and Budget (OMB) was crucial in getting the attention and cooperation of the other government agencies. Moreover, the involvement of the President himself of course lent legitimacy to the activity.

☐ The overall goal should be an integrated science policy containing all elements of research. In particular, the initiative to establish a Jeffersonian-type research mode in federal government must go hand-in-hand with a strong assurance of the continued support of basic science programs that do not fall under the new heading. During the Press-Carter Initiative, the "mission agencies" in general were more enthusiastic about the envisaged coupling of basic research and the national interest than were NSF and NASA. The latter organizations seemed to drag their feet because they were wary of what they perceived as an attack on basic science support. This, as well as the embattled history of RANN, show that it is essential to protect the interests of the basic scientists in any reform attempt.

☐ A key condition for success appears to be to learn from and cooperate with NIH, the agency that has probably been most successful in convincing the public and politicians to support a science program that links the public interest (in this case, curing diseases) and basic research. In the 1970s, NIH was somewhat reluctant to associate itself with the Press-Carter Initiative, but this stance may have changed. It is perhaps an encouraging sign that Harold

Varmus, who until recently served as the director of the National Institutes of Health (NIH), made a point of acknowledging the close association of biomedical advances with progress in the whole spectrum of basic sciences, often outside the fields of biology and medicine.

- The bottom-up, inductive type of process in the Press-Carter Initiative appeared feasible. As mentioned, the initiative generated a list of about 80 questions (which would have had to be organized and whittled down). This process may be an interesting alternative to a top-down process that would start with the determination of a very small number of key problems and then, in a deductive manner, would divide these problems into a larger number of sub-problems. The advantage of the Press procedure seems to be that its decentralized approach allows individual units of government a greater measure of initial input and generates a larger number of interesting questions.

Conclusion

While the history of the basic-applied dichotomy in American science policy has been at the center of this paper, we should at least mention several other closely related issues that are of importance for implementing a Jeffersonian Research strategy.

First, a Jeffersonian approach points to the need for visible political accountability in the setting of S&T priorities in the budget process. How can this be distinguished from the "politicization" of decisions about science, and of scientific and engineering advice about public policy problems? Such a perceived "politicization" could be deleterious because a great deal of the credit scientists can claim for their statements rests on their audience's assumption that these statements are objective, or at least not politically biased. To what degree can expertise be strictly "apolitical", and what criteria can be used to decide how apolitical it is? In any case, it appears to be beneficial if the agenda of public objectives is set explicitly in the political realm and not by the scientific community alone (although of course with its input).

The scientists in government agencies play a pivotal role here that is hard to overemphasize. Making the agenda-setting decisions part of the political process shields the scientists from being viewed as self-serving schemers who try to extract the maximum funding for their own research agenda. Once the priorities are set by a political process (i.e., externally), the granting of awards could then proceed according to science-internal criteria and, at that stage, would be isolated from the necessity of demonstrating societal relevance for individual research projects.

A second issue is that of organization, which has two aspects. First, the question is to what extent the government should conduct research on its own (e.g., in national laboratories and with staff scientists) versus using a contract and research grant system in which academic or other scientists perform research projects. The American solution has been to delegate a great deal of research to non-government investigators. This has certainly strengthened an independent academic science culture that favors basic research without primary regard to applicability.[11] Second, there is the question of how the science and technology effort should best be organized within the government. Should science and technology be centralized in a separate Department of S&T (as is the case in many industrialized countries), or should the S&T activities, ranging from R&D to scientific advice, be distributed among various agencies according to their societal mission (such as transportation, economic growth, environmental protection, energy supply, etc.)? It is unclear how the establishment of a Department of S&T would affect the basic-applied dichotomy and the chances for overcoming it.

Finally, one should not underestimate the pragmatism of the many smart people, in federal science policy as well as in research science, who often have found ways to conceive and fund research projects that are at the same time basic and closely coupled to public objectives. Jeffersonian-type arguments are already being made from time to time and from case to case; that is, problems of societal importance are used, quite legitimately, to justify federal support of basic science projects. For instance, current NSF-sponsored research in atmospheric chemistry and climate modeling is linked to the issue of global warming, and Department of Energy support for plasma physics is justified as providing the basis for a reasonable though long-range path toward controlled fusion as an energy source. What seems to be missing at present is an overarching philosophical rationale and institutional legitimation of Jeffersonian science within the total federal research effort. This applies especially in areas where the classification of research according to recognized scientific disciplines differs most from its classification according to social purpose. A prime example is physical science research in relation to medicine, which often reaps great benefits from the development and exploitation of new instrumentation and measuring techniques derived from the physical sciences.

Many science administrators and policy makers have, in a sense, been laboring to fit round pegs into square holes. Imagine how much easier things might become if the form of the holes was officially changed to round. Rather than working around the dichotomy that is enshrined in the administrative structures of the federal government as well as in the mindset of some politicians and administrators, they could rely on an explicitly acknowledged and structurally sustained rationale of basic science in the service of public objectives—as a complement to the more traditional types of both basic and applied science within federal science policy. Such a rationale would be conceptually sound as well as easily understandable by the public.

REFERENCES

AAAS Committee on Science in the Promotion of Human Welfare (1960). Science and human welfare. Science, 132, (July 8, 1960), 68-73.

Bernal, J. D. (1967). The Social Function of Science. Cambridge, MA: MIT Press. [First publ. 1939]

Branscomb, L. M. (1995). Public funding of scientific research: Policy criteria for investigator discretion, sponsor's intent, and accountability for outcomes. In Vannevar Bush II: Science for the 21st Century, Proceedings of a Sigma Xi Forum, Research Triangle Park, NC: The Scientific Research Society, 147-171.

Branscomb, L. M. (1998). From science policy to research policy. In Branscomb, L. M., & Keller, J. H. (Eds.), Investing in Innovation: Creating a Research Innovation Policy That Works, Cambridge, MA: MIT Press, 112-139.

Branscomb, L. M. (1999). The false dichotomy: Scientific creativity and utility. Issues in Science and Technology, 16(1) (Fall 1999), 66-72.

Brooks, H. (1966). Basic science and agency mission. In Weyl, F. J. (Ed.), Research in the Service of National Purpose: Proceedings of the Office of Naval Research Vicennial Convocation, Washington: U.S. Government Printing Office, 33-47.

Brooks, H. (1967). Applied research: Definitions, concepts, themes. In National Academy of Science, Applied Science and Technological Progress, Washington, DC: National Academy of Science, 21-55.

Brooks, H. (1980). Basic and applied research. In National Science Foundation, Categories of Scientific Research, Washington: National Science Foundation, 14-18.

Brooks, H. (1973). The Physical Sciences: Bellwether of Science Policy. In Shannon, J. A. (Ed.), Science and the Evolution of Public Policy, New York: Rockefeller University Press, 105-134.

Brooks, H. (1994). The relationship between science and technology. Research Policy, 23, 477-486.

Bush, V. (1945). Science, the Endless Frontier. Washington, DC: U.S. Government Printing Office. [Reprinted 1960].

Bush, V. (1950). Report of the Panel on the Gordon McKay Bequest Chaired by Vannevar Bush to the President and Fellows of Harvard College. Cambridge, MA.

Clinton, W. J., & Gore, A. (1994). Science in the National Interest. Washington, D.C.: Executive Office of the President, Office of Science and Technology Policy.

Comroe, J. H., & Dripps, R. D. (1978). The Top Ten Medical Advances in Cardiovascular-Pulmonary Medicine and Surgery between 1945 and 1975, How They Came About: Final Report. 2 vols. Bethesda, MD: U.S. Dept. of Health, Education, and Welfare, Public Health Service, National Institutes of Health.

Godin, B. (2000). Measuring science: Is there "basic research" without statistics? Paper No. 3 of the Project on the History and Sociology of S&T Statistics, Montreal: Observatoire des sciences et des technologies.

Greenberg, D. S. (1966). "Hindsight": DOD study examines return on investment in research. Science, 155 (18 Nov.), 872-873.

Holton, G. (1986). The Advancement of Science, and Its Burdens. New York: Cambridge University Press. [2nd edition: Cambridge, MA: Harvard University Press, 1998].

Holton, G. (1998). What kinds of science are worth supporting? A new look, and a new mode. In The Great Ideas Today, Chicago: Encyclopaedia Britannica, 106-136.

Holton, G., Chang, H., & Jurkowitz, E. (1996). How a scientific discovery is made: A case history. American Scientist, 84, 364-375.

Holton, G., & Sonnert, G. (1999). A vision of Jeffersonian Science. Issues in Science and Technology, 16(1) (Fall 1999), 61-65.

Illinois Institute of Technology Research Institute (1968). Technology in Retrospect and Critical Events in Science. NSF contract NSF-C535. Chicago: IIT Research Institute.

Mazuzan, G. T. (1994). The National Science Foundation: A brief history. NSF document 88-16. Arlington, VA: National Science Foundation.

National Academy of Sciences (NAS) (1965). Basic Research and National Goals. A Report to the Committee on Science and Astronautics, U.S. House of Representatives. Washington, DC: U.S. Government Printing Office.

National Science Board (NSB) (1978). Basic Research in the Mission Agencies: Agency Perspectives on the Conduct and Support of Basic Research. Washington, DC: U. S. Government Printing Office.

National Science Foundation (NSF) (1980a). Categories of Scientific Research. Washington: National Science Foundation.

National Science Foundation (NSF) (1980b). The Five-Year Outlook: Problems, Opportunities and Constraints in Science and Technology. Washington, DC: U. S. Government Printing Office.

National Science Foundation (1989). Report of the Task Force on R&D Taxonomy. Washington: National Science Foundation.

Omenn, G. S. (1985). Basic research as an investment in the future. American Journal of Roentgenology, 145, 1109-1112.

Sherwin, C. W., & Isenson, R. S. (1967). Project Hindsight: A Defense Department study of the utility of research. Science, 156 (23 June), 1571-1577.

Sonnert G., with the assistance of G. Holton (in press). Ivory Bridges: Connections Between Science and Society. Cambridge, MA: MIT Press.

Stokes, D. E. (1982). Perceptions of the nature of basic and applied science in the United States. In Gerstenfeld, A. (Ed.), Science Policy Perspectives: USA-Japan, New York: Academic Press, 1-18.

Stokes, D. E. (1997). Pasteur's Quadrant: Basic Science and Technological Innovation. Washington, DC: Brookings Institution.

Tocqueville, A. de (1862). Democracy in America. Trans. H. Reeve. 2 vols. Cambridge, MA: Sever and Francis. [orig. publ. in France in 1835-1840]

Waterman, A. T. (1965). The changing environment of science. Science, 147, 13-18.

NOTES

1) We owe this term to a personal communication from Lewis Branscomb.

2) In fact, Bush was himself an engineer more than a scientist, and in the McKay bequest report, he was writing about engineering and applied science. Jeffersonian Science is the kind of science engineers tend to do in academic engineering schools.

3) The legal basis was the Land-Grant Act of 1862, also known as "Morrill-Act" after its sponsor, Representative Justin S. Morrill of Vermont.

4) Following the Johnson Administration's egalitarian impulse, the Centers of Excellence program, which NSF started in 1964, excluded the top twenty academic institutions from its funding and concentrated on a geographically balanced selection of second tier institutions.

5) The President's Science Advisory Committee (PSAC) addressed the issue of basic research in its statement entitled Scientific Progress, the Universities, and the Federal Government, which was released in November 1960.

6) Within the National Academy of Sciences, the task of producing this report, as well as the other NAS reports mentioned, was assigned to the Committee on Science and Public Policy, which was chaired during the first phase of this work, until 1965, by George Kistiakowsky, and then, until 1971, by Harvey Brooks.

7) In 1969, a third report, entitled Technology: Processes of Assessment and Choice, was issued. This report became the blueprint for the Office of Technology Assessment.

8) In 1968, an NSF-sponsored report entitled Technology in Retrospect and Critical Events in Science, came to quite opposite conclusions (Illinois Institute of Technology Research Institute 1968). Examining the history of five breakthrough innovations, it found that, in each case, about 70% of the key papers that contributed to one of these innovations could be classified as reports of basic research. Ten years later, during the Carter Administration, a medical study (Comroe & Dripps 1978) similarly supported the view that basic research plays a crucial role in propelling applied technology. This study examined the top ten advances of cardiovascular and pulmonary medicine and surgery between 1945 and 1975 and found that 42% of the research articles that contained key contributions to these advances reported research whose goal was unrelated to those later clinical breakthroughs. For a more recent study on this subject, see Holton, Chang & Jurkowitz (1996).

9) RANN at first started in the form of a precursor program called Interdisciplinary Research Relevant to Problems of Our Society (IRRPOS), which after two years was expanded into RANN proper.

10) Further momentum was added two years later, in 1980, with the release of the two-volume NSF report The Five-Year Outlook: Problems Opportunities and Constraints in Science and Technology (NSF 1980b). Again, long and detailed lists of fundamental research that must be encouraged were compiled, lists that showed considerable overlap with those prepared in the earlier report.

11) A parallel issue would concern industry-sponsored university research, though with the additional complication of intellectual property rights and resulting conflicts of interest.

PRESS INITIATIVE—MASTER LIST OF QUESTIONS

Source: OSTP

Editorial Note: The brackets at the end of each item indicate its origin (i.e., Press: part of Frank Press's original memorandum; DA: Department of Agriculture; DoD: Department of Defense; DoE: Department of Energy; S: State Department; NASA). The four questions submitted by the Department of Transportation [DoT], but not included in the list have been added, in brackets, to the Engineering, Computer and Material Sciences sub-heading. The questions were unnumbered in the original; the numbering provided here was added for convenience.

Astronomy and Astrophysics
[1] What is the nature of the universe? How did it originate? Is it expanding, contracting or in a steady state? How large and how old is it? [NASA]

[2] Is there intelligent life elsewhere in the universe? [NASA (part of original item)]

[3] What are the matter and energy mechanisms of stars—-quasars, pulsars, black holes? [NASA]

[4] What is the nature of a solar flare? How is the energy stored and how is it released? [NASAI

[5] How do planets evolve and what are the common processes that shape the environments of the Earth and the planets? [NASA]

[6] How does the material pervading the universe collect to form complex organic molecules, stars, and galaxies? Research in this area can provide increased understanding of fundamental natural laws and the origins of the universe. [Press]

Biology and Microbiology
[7] Can we discover anti-viral agents to combat viral diseases? The development of such drugs would have as large an effect on mankind as did the discovery of antibiotics. [DoD]

[8] What are the mechanisms by which cells repair damage to their genetic material? This information will provide a better understanding of how the cells minimize mutations as a result of normal and imposed environmental stress. [DoE]

[9] How do cells change during growth and development? Advances and understanding in this area should provide insights into the development of cell specialization and, perhaps, the aging process. [Press]

[10] What are the molecular mechanisms by which genes are regulated to produce specialized products, and what new information is required to exploit the new DNA recombinant technology? This work may lead to improved knowledge of gene action. [Press]

[11] Can microbiological research develop organisms which can convert crude organic materials, such as common cellulose, into livestock feed? The ability to convert common cellulose to feedstock would significantly increase the availability of high-grade animal protein for human consumption. [S]

[12] What predisposing factors govern cellular differentiation and function in plants and animals? Successful research directed towards this question can provide an understanding in plants of factors responsible for drought tolerance and winter hardiness and in animals the mechanisms governing the development of fat and lean tissue. [DA]

[13] What are the mechanisms by which hormonal substances regulate growth and reproduction in plants and animals? Answers to this vital question could help solve many perplexing problems, e.g., conception and embryonic mortality in animals and control of post-harvest ripening of fruits and vegetables. [DA]

[14] In our eco-system affecting man and animals, how do microorganisms gain resistance to antimicrobial drugs and what mechanisms affect the maintenance and transfer of such resistance? Research to provide an understanding of bacterial resistance to drugs used in their control is essential for the protection of human and animal health. [DA]

[15] What are mechanisms within body cells which provide immunity to disease? Research on how cell-mediated immunity strengthens and relates to other known mechanisms is needed to more adequately protect humans and animals from disease. [DA]

[16] How can genetic improvement of crops for improved performance under stress conditions be accelerated? Research is needed to identify, more rapidly, useful gene sources for increasing photosynthetic efficiency and resistance to environmental stress. [DA]

[17] What are the physical and biochemical factors associated with secondary cambial differentiation? The secondary cambium of a tree divides to form identical cells which are capable of becoming either phloem or xylem cells. Studies at the North Central Experiment Station are directed toward identifying the physical factors and biochemical signals which direct cambial development and differentiation. Such information will provide essential clues on the formation of wood. [DA]

[18] How can utilization of the forest resource be enhanced through manipulations at the level of the plant cell, and through single-cell biodegradation? Tree cells can be stimulated to produce oleoresins, natural biocides, specific carbohydrates, and organic acids. Cell morphology such as fiber length can be altered to affect paper properties. Single-cell protein, hydrocarbons, acids, vitamins, steroids, and alcohols can be produced through biodegradation of tree components. [DA]

[19] Can the microbiology of the gastrointestinal tract of man and animals be controlled? Research on this important question is needed to understand the contribution of microbial activity to general health and its effect upon nutrient utilization. [DA]

[20] What are the quantitative differences between minimum human requirements for nutrients and those amounts needed for optimum physical, behavioral and mental functions? Research in this area will contribute to the attainment of maximum physical fitness and a longer, more vigorous, productive life. [DA]

Chemistry and Biochemistry
[21] Combustion is older than recorded history, yet it is poorly understood in scientific terms. It is important that better understanding be achieved for all aspects of combustion, in order that our fossil fuels can be used with maximum efficiency and minimum adverse impact on the environment. [DoE]

[22] To what extent can laser-induced chemistry be used as a practical, synthetic tool? Research in this area could lead to processes for preparing pure products with a low energy input and low environmental side effects. [DoE]

[23] For many applications, solar energy is impractical because sunshine is intermittent, and energy storage is wasteful and expensive. Basic research is needed to develop ways in which sunlight can produce storable fuels. One possibility is to mimic but improve on photosynthetic processes, with emphasis on increased efficiency and products simpler than carbohydrates. Another approach is the use of sunlight to promote reactions which decompose water to hydrogen and oxygen. [DoE]

[24] The liquefaction of coal is currently done by converting the complex coal structure to simple molecules, then re-combining these into appropriate fuels. The process is capital intensive and energy wasteful. Research is needed on means to transform the coal into useful liquid fuels by a more direct route. This will involve much greater insight into the structure of coal and its reactions during the transformation process. [DoE]

[25] How do catalysts work? Research on this question can lead to more economical ways to produce hydrogen and to convert coal to useful liquids and gases. [DoE]

[26] What is the chemical basis of life? Where and how did it originate? Is a carbon-based chemistry a prerequisite for life? Does gravity play a significant role in the development and maintenance of life? [NASA]

[27] Can simple chemical reactions be discovered that will generate visible radiation? The results of research on this question may lead to inexpensive lasers for communication and industrial uses. [Press]

[28] Can new homogeneous catalysts be prepared that will catalyze chemical processes important to the chemical industry? Research in this area could make it possible to make specific molecules needed in industrial processing techniques with minimum energy expenditure and without the creation of unwanted molecules that may pollute the environment. [Press]

[29] How do enzymes work? This research should help discover how enzymes selectively catalyze and control the chemical reactions carried out by living systems. The results of this research should extend knowledge on how to synthesize molecules in living cells. [Press]

[30] What mechanisms of herbicidal action, at the cellular level, are responsible for weed-killing effectiveness? Understanding these mechanisms is essential to improving technologies for reducing the $6 billion annual crop losses caused by weeds. [DA]

[31] To what degree can conventional chemical pesticides be replaced by novel chemicals such as pheromones and insect growth regulators for forest insect pest suppression? Development of such chemicals would provide means of protecting the timber resource with minimal adverse environmental effects. [DA]

<u>Earth, Oceans and Atmospheric Sciences</u>
[32] At what rate will atmospheric carbon dioxide concentrations increase as a result of increased use of fossil fuels? What effect will increasing carbon dioxide levels have on climate? How will this change the global social, economic and political structure? How might the impact be ameliorated? [DoE]

[33] Can a predictive capability be developed regarding geochemical transport processes in the accessible regions of the earth's crust? Successful research directed toward this question would have major impact on expansion of the Nation's resource base, and would be of vital importance in resolving waste (nuclear and non-nuclear) problems. [DoE]

[34] What is the nature of climate? What are the processes that control climate? How far into the future can you predict it? Is our climate warming or cooling? How far in advance can you predict weather, climate? Is there a relationship between climate and solar activity and, if so, what is the physical connection? [NASA]

[35] What are the physical processes that govern climate? Greater understanding of climate could aid in the prediction of climate changes and allow time for measures to offset their impact. [Press]

[36] To what extent is the stratospheric ozone affected by contamination of long-lived, man-made chemicals? The results of this research are important to man's survival and to the future of major industries. [Press]

[37] What is the petroleum potential of the continental slopes and the adjacent ocean floors beneath deeper waters? This work is helping to identify the resource potential of the ocean's floor beyond the OCS. [Press]

[38] How do organisms in the deep sea influence the productivity of the ocean? How will they react to sea floor dumping and mining activities? Answers to these questions will aid in assessing the future of the ocean as an important food source and should also provide baseline data on contamination of the sea. [Press]

[39] Can research into the processes by which mineral deposits were formed in the earth's crust be sufficiently aided by deep ocean floor investigations so that mineral resources can be more efficiently located on land <u>or</u> sea-bed? Research which would improve the success-rate of exploratory efforts could be of considerable advantage. [S]

[40] What improvement in understanding of oceanic and atmospheric effects on climate can be gained by increased use of sophisticated technology, such as satellites, in observing air/sea interactions? Air/sea interaction, is particularly Important in pursuing the promise of regional seasonal climate prediction and in determining the role of the ocean as the major absorber of atmospheric carbon dioxide (with implications for the fossil-fuel energy future). [S]

[41] What physical processes govern the interaction between high energy plumes and the ambient atmosphere? Research in this area is needed to improve air pollution models and forest fire forecasts. [DA]

<u>Economics</u>
[42] What is the economic and technical potential for saving energy in the processing and marketing of agricultural commodities? [DA]

[43] What are the potentials for per capita energy saving and improved levels of living for alternative sizes and population densities of communities in the United States? [DA]

[44] Despite continued long-term real economic growth in the United States, why are many rural areas chronically depressed? [DA]

[45] What changes in policy at the Federal, state and local level can be designed to increase job opportunities in rural areas? A team research approach could provide a guide for changes in policy and more effective use of rural development funds. [DA]

[46] What are the effects on farm income and consumer prices of environmental rules that pertain to farming? What environmental benefits result from such restrictions on farmers? [DA]
[47] What is the potential for microbial production of useful complex organic compounds including food products? Economic microbial processes for producing many complex organic chemicals from waste products appear feasible. [DA]

[48] What are the individual and cumulative impacts of public domestic feeding programs on recipients and the Nation's economy? The annual level of current Federal programs is morethan $7 billion. This research will facilitate analysis of alternative policy proposals. [DA]

[49] How and how much is the instability of food and fiber product prices accelerating wage-price inflation and so handicapping real national economic growth? What gains in real economic growth would result from alternative price stabilizing mechanisms? What are the distributional effects of alternative economic gains and losses? [DA]

[50] Since the production time frame for timber is much longer than for most agricultural crops, the economic consequences from trade policies in timber products may not be fully apparent for decades. Better economic methodologies are needed for assessing the gross national product, social welfare, and capital formation In developingcountries. [DA]

Engineering, Computer and Material Sciences
[51] Can man-machine interfaces be made so simple as to allow real time translation by untrained personnel? Such developments would not only provide improved communications between the nations, but also have a profound change in our daily life. [DoD]

[52] How can productivity be enhanced by automation and artificial intelligence? With limited trained manpower supply in some areas and the saturation of productivity in others, it is extremely important for the nation to develop methods which will permit continual increases in productivity. [DoD]

[53] Can new materials such as ceramics be developed to replace metals in high temperature situations? For example, use of ceramics in turbine blades will permit greatly increased operating temperatures, reduction of size, increase in efficiency, and reduction in fuel consumption. [DoD]

[54] The economic and predictable fracture of rock is of critical importance to energy production. Obvious examples are drilling for new resources, mining of coal, oil shale and uranium, and releasing natural gas from low permeability formations. Research is needed on the mechanical behavior of rocks, in order to improve our understanding of them as engineering materials. [DoE]

[55] Can new materials be developed which would be less dependent on critical or strategic elements? The obvious example of a benefit to be derived from research on this area is the possible substitution for Cr in steels. [DoE]

[56] Computer models of physical and socio-economic processes are needed to guide, and often to replace, experimentation. Advances in analytical and numerical techniques and in computer hardware are required to simulate these processes more effectively. [DoE]

[57] How do cracks initiate and propagate in materials? This research should provide information needed to develop structural materials that resist corrosion and failure under stress. [Press]

[58] How can structures be designed and constructed to be both economical and earthquake resistant? In addition to reductions in life loss and personal injury, implementation of improved design procedures is expected to reduce losses to buildings alone by an average of $250 million per year. [Press]

Note: The following four questions (59-62) were not part of the original document, which left out the submissions by the Department of Transportation.

[59] [How can the development of improved construction materials impact the cost of construction? Examples include soil stabilization and development of improved structural concretes. In FY 1976 and the Transition Quarter, DoT grants for construction totaled $7.86 billion. A 15% reduction in construction costs could have saved over $1 billion in this time period alone. DoT]

[60] [What limitations, if any, exist on the development of full performance electric and hydrogen-powered automobiles, trucks and buses? areas of concern here include the development of improved batteries and lightweight, reliable and maintainable hydrogen storage tanks. The basic discipline area of interest is materials. The need for continued transportation energy research is obvious. DoT]

[61] [What are the implications of the advent of inexpensive, large capacity microprocessors on the decentralized control of largescale systems in general and transportation networks in particular? I is necessary to expand our efforts in the area of modern control theory in order to understand the feasibility of and benefits from decentralized control. A situation now exists where the hardware state-of-the-art is advancing faster than our ability to employ it optimally. DoT]

[62] [What are the long term impacts of major changes in the Nation's transportation system on the economic and social environment? How do improvements in transportation affect urban and regional form, the distribution of cities and the economic and production processes? Research on the development of formal methods and analytical tools such as large scale network analyses will be required to better understand the interaction of the major parameters and to more accurately predict the consequences of government policy decisions. DoT]

Environmental and Ecological Sciences
[63] Can specific bioprocessing methods be designed for removing and degrading toxic pollutants in industrial process and waste water? The benefits would be reduction of such agents to innocuous gases, production of chemical feed stock, and improvement of water quality. [DoE]

[64] What are the ultimate carrying capacities of the terrestrial biosphere? [NASA]

[65] What ecological factors and life-cycle phenomena govern insect dispersion and population explosions? Research on this question can lead to the development of innovative pest management technology to supplement current biological, cultural, and chemical control measures. [DA]

[66] To what extent does nitrogen use in agriculture affect the ozone layer and what are the costs and environmental benefits of reduced nitrogen applications? [DA]

[67] What is the chemical composition of precipitation and dry particulate matter and how does it vary with season and location? This information

will provide baseline data for atmospheric input to nutrient cycling and can relate to both point and non-point sources of air pollution. [DA]

[68] What factors influence susceptibility of harvested plant and animal products to post-harvest losses? In many parts of the world, such losses represent 30 to 50 percent of the food supply. Research can promote development of the technology required to preserve quality and protect against losses from rodents and insects. [DA]

[69] How can the environmental stress tolerance of current crops and grasslands be improved? Utilization of as much as 40 percent of the world's noncultivated but potentially productive land is limited because of severe weather aberrations and other stress conditions. [DA]

General (Interdisciplinary)

[70] To what extent can the occurrences of natural hazards such as fire, flood, earthquake, and pestilence be foreseen sufficiently in advance to permit mitigation of their effects? The problems of prediction and of mitigation are different for each hazard, but for each, research offers promise of reducing human and physical costs. [S]

[71] What are the economic, technical, and public health impacts of restricting antibiotics and other additives in animal feeds? Adverse impacts may more than offset direct benefits of feed additive bans. [DA]

[72] To what extent are agricultural chemicals transmitted to the Nation's waterways and what are the most cost-effective ways of reducing this pollution? [DA]

[73] How will geoclimatic changes from increasing carbon dioxide levels and particulate loads in the atmosphere impact agricultural productivity? Research is needed to determine the influence of such changes on temperature, rainfall and other climatic variables that could significantly influence the agricultural potential of different regions. [DA]

[74] What factors most influence the distribution of foods and so relate to human health? Research to help answer this question is needed prior to public nutrition programs. Such research could indirectly enable reduced health costs. [DA]

[75] What are the economic potentials for expanding food production by (a) land and water development, and (b) application of new technology? This research would show the capability of the U.S. and other countries to meet the rising world demand for food. [DA]

[76] Can productive self-sustaining systems be developed to utilize biological wastes? Research in this area could provide a means of improving comporting of organic wastes and preventing soil, water and air pollution with potential for yielding energy and other useful products. [DA]

Physics and Biophysics

[77] Can materials be found that exhibit superconductivity at room temperature? Such a discovery would be extremely important to our energy needs as well as revolutionize all technology using electrical energy. [DoD and DoE]

[78] Are there fundamental building blocks in nature? Some recent advances have been made which indicate that even the subnuclear "particles" are not fundamental and further research is necessary to uncover the secrets of the nucleus. [DoD]

[79] How can considerations of second law efficiencies be incorporated into energy strategies? Energy should be valued not by its amount alone, but also by its thermodynamic quality. A significant reassessment of energy economics may be in order. [DoE]

[80] How are the fundamental forces of nature related? Four types are currently known: nuclear (strong), electromagnetic, radioactive (weak) and gravitational. Only electromagnetism is well understood; the rest defy us to master them. [DoE]

[81] Does an "island of stability" beyond the current periodic table or "abnormal" states of nuclear matter exist? These speculations can be tested and if found could have important consequences for nuclear energy production. [DoE]

[82] What is the nature of gravity? Are there gravity waves, and if they exist, how do they propagate and at what velocity? [NASA]

[83] What is the nature of matter? Why is matter and charge quantized? [NASA]

[84] What are the limits for communications use of the channel capacity in the visible spectrum? Progress in this area could significantly expand the capacity of optical communication systems, and since these systems use glass fibers instead of copper, their use would result in tremendous monetary and resource savings. [Press]

[85] Can microwave technology or other alternative sources of energy be safely and effectively used to process and preserve food? Food processing and preservation account for nearly 5% of the nation's consumption of fossil energy. Research could provide alternative less costly energy sources and methodology. [DA]

Social, Psychological and Behavioral Sciences

[86] What is the nature of intelligence [NASA (part of original item)]

[87] How do we think? [NASA (part of original item)]

[88] What are the individual and cumulative effects of government regulation on domestic productivity? This research will provide a sound technical basis for assessing the benefits and cost of proposed, as well as existing, government regulations. [Press]

[89] What are the factors controlling cognitive development? For example, how can the large number of component processes involved in reading and understanding a paragraph be characterized? Research on this question should provide new knowledge on the processes involved in reading and comprehending text. Such work is important on providing a basis for improving the techniques for teaching people to read and comprehend. [Press]

[90] What are the mechanisms responsible for sensory signal processing, neural membrane phenomena, and distinct chemical operations of nerve junctions? Research in these areas will extend knowledge of perception, behavior, and the chemical functioning of the nervous system. [Press]

[91] What are the factors—social, economic, political, and cultural—which govern population growth? High population growth rates in the developing countries impose an economic burden which too often exceed the gains made by development. Social and biomedical research on safe, efficacious, and culturally acceptable contraceptives would therefore be of great benefit. [S]

APPENDIX B:
WHAT IS THE IMPERATIVE FOR BASIC SCIENCE THAT SERVES NATIONAL NEEDS?

by Gerald Holton

Based on a presentation at the Conference on Basic Research in the Service of Public Objectives, November 2000, Washington, DC.

A major focus of discussion in Washington and in academe is how to strengthen the conduct and support of basic research in science and technology. It is a timely effort: the federal support for basic research has dropped precipitately during the last decade (to about 0.6% of GNP, back to where it was in 1953), the U.S. population remains, to our shame, dangerously ignorant of the sciences (for example, 70% of the nation's colleges do not require even one hour of science or mathematics for graduation), and the true champions of basic research support in the House and Senate of Congress are still few in number. More ominously, the world has entered a new phase of history, with potential instabilities before us, including global change, energy, literacy and learning, the threats of wars, poverty and the spread of disease--among many possible examples. Many of these are relevant to scientific studies; but all, known or yet unsuspected, will greatly determine the life of our children and generations beyond. Just as our nation's civilization has been shaped in large part by the extraordinary powers of the sciences and technologies, the phase of history which we have entered will surely also be formed by the findings and tools to be developed by scientists and their near colleagues, by how their work is to be encouraged and conducted.

Thus a main point of the vision I wish to sketch is this: The scientific part within our total cultural spectrum can do, and now must do, far more to serve the needs of this nation and humankind, and needs a corresponding expansion in terms of human resources and financial support. True, the sciences as a whole have risen to glorious heights in the twentieth century, despite many shortcomings and persistent obstacles. In addition, economists have found that the social return on the federal investment in science and technology has been between 30% and 60%. In the darkest middle-part of World War II, the sciences and technologies even helped the Allied forces to rescue Western civilization itself. Now, in our battle for a more secure future, the sciences, more mature and numerous, are even more capable of great achievements for the public good.

How? Many of us believe the first step is to widen the common understanding of what the sciences now can do. It is imperative to cut the chains impeding some of today's sciences, to let them help more effectively with the public needs before us. By widening of the common understanding of science's powers, I mean specifically to encourage much more intensively *a style of basic research that locates itself in areas of ignorance of how to meet societal needs*. It is a mode of research which I have thought and written about for some time, using as a convenient shorthand the term "Jeffersonian Research." That notion is neither radical nor untried. For example, much of the basic research now supported by the National Institutes of Health is chosen and pursued in this mode, with wide approval in Congress and among the public. The White House report on science in 1994, and Rep. Vernon Ehlers' report of 1998 contain similar proposals. A fascinating attempt to institutionalize this mode across all executive departments and agencies was made under the direction of Frank Press during the Carter administration. In the last decades there were also a few other federal initiatives along that line. But what many of the past attempts have lacked is an explicit verbalization of the overarching rationale and the institutional legitimating of Jeffersonian Research within science policy. This is what I shall consider here. A proper start is to summarize more precisely what characterizes the two current main research styles as commonly perceived, and how a third one I select for greater attention differs from the others.

Newtonian vs. Baconian mode of research

Among the familiar research styles are two modes of basic research, well established and utterly needed to be adequately supported in the total range of efforts. One mode is primarily curiosity-driven basic research, without the expectation of any but perhaps long-term social benefits, apart from the important one of increasing of scientific understanding itself. The other mode is that part of R&D pursued in the reasonable hope that a fairly early harvest would result, for use and practice beyond the originating laboratory. In popular parlance, the difference between the two is ivory tower versus quick payoff, or pure versus mission-oriented, or the craving for omniscience versus that for omnipotence, or, for shorthand use, the Newtonian mode versus the Baconian.

At first glance, those two modes seem antithetical, even antagonistic, and invite choosing one over the other. For example, as to the support of basic research, Senator Harry Reid warned in April 2000 that many people are now beginning "to see science as a luxury that can be reduced or abandoned." On the other hand, federal support for applied science research has been attacked as "corporate welfare"; and similarly, one still hears occasionally Vannevar Bush's remark, made in passing in his grand Report of July 1945, that "there is a perverse law governing research:...applied research invariably drives out pure."

If one looks at these two modes in turn, and arrays historic examples of scientific results in two opposite columns, deep differences seem to persist. In the column on the left, that of curiosity-driven achievements, we would find for example Galileo's telescopic discovery of the moons of Jupiter; Newton's Principia; Faraday's discovery of generating electricity by moving conductors in magnetic fields; Johann Gregor Mendel's report of his experimental results of artificial plant hybridization and Thomas H. Morgan's research on fruit flies; Roentgen's discovery of x rays; Einstein's brief paper of November 1905, ending with the speculation that "The mass of a body is a measure of its energy content," and the vaguely expressed hope that "It is not beyond possibility that this theory may be tested." The story continues in the latest, astonishing reports, in journals such as Science and Nature.

The second, right-hand column, presenting mission-oriented or Baconian-mode results, would be equally long. It might start with the casting of a monstrous canon of new design at the command of Sultan Mechmed II in the 14th century, which breached the walls of Byzantium; in our century it would feature the inevitable transistor, antibiotics, the Genome project, or the discovery by Müller and Bednorz of high-temperature superconductivity found in a substance nobody else thought worth investigating.

A closer look at these apparently divergent listings, left and right, reveals that they have four important commonalties. First of all, each has its own charisma. I need only say that the fascination of the public with, say, astronomy on one hand and mechanical and electronic devices on the other are reminders of those ancient, benign enchantments with science and technology that are built into most souls from the beginning.

Second, both modes are part of a whole seamless web or eco-system. Galileo's telescope depended on the practical results of ancient glass-making technology; Newton's calculations required data supplied by map-making expeditions; and so forth. Conversely, Müller and Bednorz's work rested crucially on basic results in thermodynamics, crystallography, quantum mechanics, as well as the applications of low-temperature technology, and of some thermometry first developed in the 19th century. There is much borrowing of ideas and sharing of instruments. Thus, both these research modes are essential for each other's well being.

Third: Basic research done without an explicit mission does of course sometimes result in unforeseen, extraordinarily useful applications, although usually with considerable time delay. Faraday's discoveries were the basis of electric generators and motors built decades later. Mendel's and Morgan's labors eventually became part of the intellectual ancestry of brilliant biotechnology industries. One of Einstein's papers of 1916 was to be the basis for the ubiquitous laser. NMR was morphed and transformed into MRI. The roots of the computer and the Internet can be traced similarly.

The possibility of such eventual, but not foreseeable, spin-offs from basic research has undergirded the public's affection with basic science so far. These were explicit in Vannevar Bush's Report when he wrote to President Roosevelt, without committing to a timetable of achievements: "Scientific progress is one essential key to our security as a nation, to our better health, to more jobs, to a higher standard of living, and to our cultural progress."

A last, fourth kind of similarity between the Newtonian and Baconian modes of research is both exciting and troublesome. An advance in either one can undermine an old worldview, and help in the ascent of a new one. Galileo's finding of the moons of Jupiter presaged the final acceptance of the Copernican view of the universe, and of our place in it; and similar tectonic shifts began with Darwin's lonely studies of finch beaks. The whole pseudo-scientific alibi for racism had to be abandoned, thanks to the findings of anthropologists and geneticists. In each case where a new worldview and new rights asserted themselves, vigorous resistance had to be overcome, and may, to some degree, exist to this day. It explains why science and technology are now feared or derogated by some parts of academe, and why the public may be quietly troubled by the possibility of another such revolutionary earthquake. Therefore I see part of the imperative for an expanded vision of science policy that it may put the needed respect of the populace and policy makers on a sounder basis.

The Jeffersonian mode of research

The Baconian mode generally applies known science to a known need; the Newtonians pursue science regardless of needs. Both must of course continue to flourish, not least because all modes interact. But research in the Jeffersonian mode, by contrast, places itself on an uncharted area on the map of science which, if the expedition succeeds, may reasonably soon have a bearing on a persistent national or global problem. It is in a sense a combined mode, and the label I chose for it reflects the fact that Thomas Jefferson himself saw two intertwined goals for science--not only the full understanding of nature, which he treasured, but in addition what he called simply "the freedom and happiness of mankind."

It is not difficult to imagine intentionally targeted basic science research projects where, with less uncertainty and less time delay than from Newtonian research, one can reasonably hope to find a key to alleviate specific, well recognized societal dysfunctions. For example, much remains to be done in cognitive psychology; the biophysics and biochemistry involved in the process of conception; the neurophysiology of hearing and sight; molecular transport across membranes; or the physics of nanodimensional structures, to name a few. The results of such basic work may plausibly be expected, on a reasonable timetable, to give us a better grasp of complex social tasks such as, respectively, childhood education, family planning, improved quality of life for handicapped people, the design of food plants that can use brackish water, and improved communication devices. Other examples with plausible societal significance, many now in progress, could include marine biology resources and related environmental and ecological goals; further research on imaging of the brain; studies situated between understanding behavior and mental illness on the one hand, and neurophysiology on the other; and research on the remaining social and psychological obstacles that still stand in the way of greater participation and diversity, not least in careers in science and technology.

I am not saying that this terminology, Jeffersonian research, is the only acceptable one. Of course not. It is merely a convenient shorthand term, analogous to the widely accepted one using the names of Newton and Bacon. "Basic Research in the Service of Public Objectives," although longer, is one of many equally good terms. But Jefferson himself pointed to the existence of this third mode, when he announced the twin aims for the support of the Lewis and Clark exploration. He wrote that its purpose was "to extend the boundaries of science, <u>and</u> to present to [the citizens of this nation] their knowledge of this vast and fertile country which their [children] are destined to fill...."

Jefferson, who declared himself most happy when engaged in some scientific pursuit, was delighted to receive from the explorers the samples of new fauna and flora, the notes on Indian languages, the geographical maps, and the like. But he also knew that such scientific information would help to prepare America's future on its vast new territory. Out beyond the frontier, there had to be, as Jefferson put it, "room enough for our descendants to the hundredth and thousandth generation." In just this way do I see the need for some portion of our sciences to be dedicated explicitly to the preparation, beyond our own time horizon, of the fortunes of our descendants.

In the Louisiana Purchase and the Lewis and Clark expedition, we see also examples grasped initially by only a few visionaries, to change the opportunities latent in history, as if by a quantum jump. The institutionalization of the Jeffersonian mode of research in all fields, as part of a new mandate for science, parallel to that for National Security, would again take a brave act of political will, expressed not least in enlarging the whole pie of federally-funded support for the entire spectrum of science and technology--just at a time when we may afford it.

In one of the meetings arranged on this topic, Dr. Harold Varmus, then of NIH, noted that it would be important to get active public support through the kind of council-of-citizens group he used at NIH, and also that the delivery of outcomes is now of concern to every sponsoring agency as a result of the Government Performance and Results Act (GPRA). Dr. Rita Colwell reminded us that the National Science Foundation has recently developed increasing constituency support, for example, by forming the NSF Council for Public Research. Other participants thought that the availability of explicitly Jeffersonian research might greatly appeal to potential science researchers now not sufficiently attracted to the other, more visible current modes. As Dr. Walter E. Massey remarked: "I am particularly attracted to the argument that tying research to broad and meaningful national goals may make science more attractive to women and minorities....I think that showing how scientific research can be related to visible societal goals can be a strong attraction to many students who might otherwise not consider scientific careers."

Finally there is a need to face the rarely asked question, "In what consists the moral authority for the pursuit of science and technology in the first place?" In my view the answer consists of several interlocking components: the <u>imperative to excellence</u> of the enterprise; <u>internal accountability</u>, which centers on the ethical imperatives in the conduct and use of research; <u>external accountability</u>, including explaining to the lay public and funding authorities what is being achieved and how; <u>identity preservation</u>, including identifying what is science and what is not, where the limits are, and fighting against unjustified external attacks or misrepresentation, as well as internal enemies such as arrogance and scientism. The most important component of the moral authority of science is the last component on such a list, <u>obligation to the larger community</u>, or, in short, coupling science and technology to the wider interests and needs of the country and the world. As Representative George E.

Brown once said: “We must have a research system that arches, bends, and devolves with society’s goals…I consider it a moral imperative to enlist science and technology in a campaign for a more productive and humane society.” Representative Brown did not need to evoke Jefferson explicitly, but there was an echo in his words.

I know it will not be easy to make this vision a functioning imperative, alongside, and with the same power as, the existing imperatives for lively curiosity-driven and mission-oriented research. Like the latter two, basic research in the service of urgent public objectives will need administrative help and funding through various administrative agencies, although no single "home," any more than, say, applied research is the captive of only one agency. However, the enlarging of basic research in the nation's interest will need, at least in its early stages, a congenial institution that agrees to serve as champion, teacher, information center, facilitator.

A search for that will not be easy. But this should not dissuade us. Jefferson himself gives us courage. In 1812, a dark period in the nation’s history, he writes to John Adams: "I have given up newspapers in exchange for Tacitus and Thucydides, for Newton and Euclid; and I find myself much the happier." But Jefferson's native optimism prevails all the same, an optimism we can share as we gather allies. He tells John Adams: "I do believe we shall continue to grow, to multiply and prosper, until we exhibit an association, powerful, wise and happy, beyond what has yet been seen by men."

REFERENCES

L. M. Branscomb, “The False Dichotomy: Scientific Creativity and Utility,” Issues, Vol. XVI, No. 1 (Fall 1999), 66-72.

L. M. Branscomb, "From Science Policy to Research Policy," in Investing in Innovation: Creating a Research and Innovation Policy that Works, ed. L. M. Branscomb and J. H. Keller (Cambridge, Mass.: MIT Press, 1998).

Board on Sustainable Development, National Research Council, Our Common Journey: A Transition Toward Sustainability (Washington, D.C.: National Academy Press, 1999).

H. Brooks, "The Changing Structure of the U.S. Research System," in H. Brooks and R. Schmitt, Current Science and Technology Issues: Two Perspectives, Occasional Paper No. 1 (Washington, D.C.: Graduate Program in Science, Technology, and Public Policy, George Washington University, 1985), 17-72.

Committee on Science, Engineering, and Public Policy (COSEPUP), Science, Technology, and the Federal Government: National Goals for a New Era (Washington, D.C.: National Academy Press, 1993).

David Hart, Forged Consensus: Science, Technology, and Economic Policy in the United Sates, 1921-1953 (Princeton, N.J.: Princeton University Press, 1998).

G. Holton, The Advancement of Science, and Its Burdens (New York: Cambridge University Press, 1986) (second edition: Cambridge, Mass.: Harvard University Press, 1998), chapter 14.

G. Holton, "On the Jeffersonian Research Program," in G. Holton, Science and Anti-Science (Cambridge, Mass.: Harvard University Press, 1993), chapter 4.

G. Holton, "What Kinds of Science Are Worth Supporting? A New Look, and a New Mode," in The Great Ideas Today (Chicago, Ill.: Encyclopaedia Britannica, 1998), 106-136.

G. Holton, H. Chang, and E. Jurkowitz, "How a Scientific Discovery is Made: A Case History," American Scientist 84 (1996): 364-375.

House Committee on Science, Unlocking Our Future: Toward a New National Science Policy (the "Ehlers Report") (Washington, D.C.: September 1998).

APPENDIX C:
JEFFERSONIAN RESEARCH AND THE NATIONAL CANCER INSTITUTE

By Richard Klausner

Presented at the Conference on Basic Research in the Service of Public Objectives, November 2000, Washington, DC.

While it has been stated that NIH provides us with a model for "Jeffersonian strategy," there remains a constant tension between medical/public health need and scientific opportunity. The Baconian impulse that plays out in questions about dollars spent vs. a quantitative measure of burden of disease, calculated disease by disease, is strong and persistent. The notion of Jeffersonian vision of achieving societal goals via support for basic research is attractive for many reasons, not the least of which is that it accurately reflects how biomedical research has been successfully accomplished and successfully supported. I agree with the conclusion of the last meeting of this group that the phrase "Jeffersonian Research," while a useful shorthand, many not be the best way to refer to the public description of how to integrate support basic science and its infrastructure with achieving societal goals. On the other hand, that language that we use is important and I like the reference to Lewis and Clark's Corps of Discovery as a powerful historical antecedent to publicly supported science. That early 19th Century enterprise offers us two terms that I will refer to that help frame how we can pragmatically approach a Jeffersonian model of publicly funded science: exploration and discovery.

Great and specific goals are expected of the National Cancer Institute (NCI) – to reduce the burden of cancer. The public interaction with the NCI is intense because of the prevalence and burden of cancer, the fear that this set of diseases invokes and the growing engagement of the public, particularly the concerned and immediately affected public, in the scrutiny and support of NCI.

Support for a Jeffersonian model requires that we clearly articulate that we currently lack both the knowledge and tools needed to eradicate cancer, that cancer is an incredible puzzle, that we can only hope to solve this puzzle through discovery, and that the very nature of discovery entails uncertainty, and surprises. Given that premise, we need to direct our attention, as managers of these enterprises, to the exploration processes that allow discovery. While creating their Corps of Discovery, Jefferson and Lewis focused their planning on two aspects of the exploration: the exploration vehicle (transportation, tools, personnel, supplies…) and the exploration domain (the uncharted territory of the Louisiana Purchase). I will return to these two components of exploration in planning the science enterprise of the National Cancer Program.

In directing the NCI for the past 51/2 years, the clearest manifestations of the questioning of our philosophical model of science in service of society has been in the question put to NCI, and to the whole NIH, as to how we plan and set priorities.

On one level, this question asks how we determine needs for programs, budget and other resources and how resource choices can be both accounted and accountable.

On a deeper level, the setting of priorities forces the formulations of the structural details of a science funding enterprise – grants, contracts, centers, training, infrastructure, etc., on the one hand, and on the other hand, it forces a formulation along categories of research, whose taxonomy and inter-relations are complex and changing. For example, basic vs. applied; basic vs. clinical vs. population research; science vs. technology, etc.

On the deepest level, the setting and articulation of priorities lays out the Jeffersonian landscape and offers the opportunity to define and defend the philosophical underpinnings of the enterprise. So, of the different aspects of this Jeffersonian model being discussed here, I would like to focus on planning and illustrate how we have attempted to shape a planning process to articulate, to drive and to gain support for a science-based National Cancer Program. The planning process and the products of that process provide both the opportunity to articulate Jeffersonian science principles and to convincingly link those principles to how the institution and supported enterprise actually functions. The planning process should:

1. Articulate the societal need as a challenge requiring new knowledge.
2. Articulate science as the discovery process that will create that knowledge.
3. Articulate the connection between discovery and the application of discovery to the societal need.
4. Establish criteria and processes for determining the vehicles, the domains and the support needs for the exploratory activities that are required for discovery.
5. Address with realism the timelines (5-7 years) and the uncertainties of both time lines and plans that are fundamentally dependent upon discovery.

In undertaking planning, we must confront the potential oxymoron of science planning. We can plan exploration but not discovery. It is in reinforcing this central premise that it is useful to return to the inspiration of Jefferson's Corps of Discovery. In the planning for exploration, we must address manpower needs, infrastructure and support systems, the tools of both exploration and discovery, and even the products (communication, documentation, measurements, collections, etc.). At the NCI, we have used the distinction between the vehicles and the domains of exploration to drive both review and planning and to organize the creation of many new funding initiatives. The goal is both discovery and the application of discovery. The means of discovery is the capacity to explore that involves both designing vehicles of exploration and formulating the domains of our exploration. New explorations range from genomics, to models of human disease, to the linkages of biology and chemistry to explore molecular targets, of physics and biology to measure and image, to bioinformatics, to epedemiology, to clinical trials. These provide a conceptual framework for the discovery process that is driven within these exploratory domains and through these exploration vehicles by the processes and ethos of Newtonian science.

The issue of tools, in the broadest sense, emerges as a prominent component of exploration and discovery. Tool building is an underused element in articulating Jeffersonian science. It has been too much pushed to the periphery of biomedical science and this reinforced, at least at the NIH, by an overly restrictive view of science as hypothesis testing, a view that does not help in supporting a Jeffersonian vision. Science is rather a process of inquiry that includes, but is not limit to hypothesis testing. We inquire via a wide range of tools, the tools both limiting and often determining the boundaries and successes of inquiry. Tools enable science and science enables tool building. I have found that speaking of the value of the building, dissemination and use of tools is broadly understood by the public and Congress and reinforces the exploration-discovery-application continuum that can define a Jeffersonian science enterprise. Attention to the importance of tools emphasizes that to be successful, such an enterprise must recognize that it is the infrastructure for exploration and the freedom to explore that connects basic science to achieving societal goals. The question of tools, where they come from and how they are used, also serves to link all of the sciences and demands attention to fundamental knowledge in physics, chemistry, materials science and mathematics/computational science as critical to progress in biology and medicine.

How has all of this played out in the NCI planning process and how have we addressed the tension I alluded to earlier between scientific opportunity and medical needs? Our planning processes have three components, two of which comprise the annual budget document called the NCI By-pass Budget, submitted directly to the President. That document employs a scientific opportunity approach that emphasizes the vehicles for and the domains of exploration.

One section, called the NCI challenge, addresses the vehicles for exploration. It argues for the rationale behind, the size, and the structure of the major vehicles of exploration including:

- Investigator initiated research
- Consortia, networks and centers
- · Informatics and information flow
- Clinical trials
- Training, education and career development
- And others

The second section, called Extraordinary Opportunities for Investment, describes new and promising domains of exploration. They illustrate how the possibilities of exploration are created by often unexpected development in knowledge and tools. These domains currently include:

- Genes and Environment
- Molecular Targets
- Cancer Imaging
- Molecular Signature
- Cancer Communications

Against this backdrop of programs aimed at exploration, driven by the nature and opportunities of science, we add a third planning component – disease specific progress review groups (or PRGs). Every three months, a group of outside scientists, clinicians and consumers are organized to begin a 9-12 month process of producing a report to the Institute that formulates and prioritizes what we currently believe we need to know, need to understand and need to be able to do in order to address the issues of a particular cancer. To me, the most interesting part of this process and the aspect most relevant to this discussion of Jeffersonian science, is the process of mapping the recommendations against the plans, programs and priorities established through the scientific opportunity-based planning processes of the By-pass Budget. We do this in a very detailed and quantitative way and we do it with the members of the outside review group. The result is an open discussion of the extent to which a scientific enterprise driven by notions of exploration and discovery actually addresses the somewhat Baconian charge to disease-specific planning groups. In every case to date, about 85% of the recommendations map to our vehicles and domains of exploration that were established by addressing scientific opportunity. Importantly, the public demonstration of this mapping process reinforces rather than challenges the essential role of discovery in making progress against particular cancers. At the end, reports of the PRGs are widely advertised and posted on the NCI website along with the mapping to NCI vehicles and domains of exploration. They serve as much to inform and challenge the research community as to charge the NCI. Our role in implementation is then primarily to guide investigators to utilize the vehicles and domains of exploration, manifested by dozens of funding structures, to pursue the scientific community's goals of discovery.

In summary, I believe a Jeffersonian vision of science can sustain public support and confidence without compromising the nature and ethos of science. Essential to this vision is that it must actually describe how our Federally supported science enterprises work. That requires planning, implementation and evaluation processes that reflect and reinforce the vision. While I have only referred superficially to the planning process, the core of all three of these processes must be an explicit formulation of the nature of science and of the nature of the connection between science and societal goals. I cannot emphasize enough the importance of making this vision explicit and accessible and clearly linked to the planning, implementation and evaluation processes that turn that vision into action. The relationship betweens societal needs that motivate public investment and the successful conduct of science in a way that ultimately addresses those needs will continue to be questioned. The tension between scientific opportunity and societal needs is not easily resolved but rather must be continuously engaged. Finally, the different Federal science enterprises need to learn from each other how to address this tension if we are to best formulate this Jeffersonian model that I think will have much resonance with the public, our politicians, and the scientific community.

APPENDIX D:
HUMAN RESOURCES DIVERSITY AND THE FUTURE OF SCIENCE: THE CASE OF UNDERREPRESENTED MINORITIES

By Shirley Malcom

Presented at the Conference on Basic Research in the Service of Public Objectives, November 2000, Washington, DC.

The last several times I heard the late Congressman George Brown speak it was before audiences largely made up of scientists. At those tie he played the role that one friend of mine has described as being a loving critic and a critical lover. While embracing the wonders of science and the hard work and commitment of those who do the science he challenged the community to do more – to take on the hard tasks of putting science to work on behalf of humankind, to direct energies and attention to problems that beset us, to connect with and educate the public who needs to understand – the public who is the patron of the research.

There was always tremendous respect for his words. I'm not sure how much sympathy there was for his point of view: and I don't know how much understanding there was of the implications of what he said. Where discussions were occasioned among those doing basic or fundamental research there was sometimes a "wink and a nod."

Basic research, they argued, has little to do with responding to today's problem or crisis: so it is hard to see how a plea for relevance or application of this knowledge has any bearing. Or does it?

The first thing that those of us who work on these problems have to understand is that the public is really many publics, with many different problems and needs. As we seek to apply knowledge to solve our problems we realize that not all problems are solved by direct assault. We can't predict where key pieces will come from. We must invest in "just finding out" because we want to know. And the public pays for this. We do this work on their behalf – "just to find out." Of course, there is the obligation that comes with accepting this support to share what we know, not only with our colleagues but also with our patrons.

Basic research is sometimes said to be "curiosity driven." But then the natural question arises – whose curiosity? Based on what experiences? What interests?

What concerns? What issues? Reflecting what history? What problems stir us? What challenges?

When we look at the scientific community of this country, the community of knowledge seekers, we must take notice of the fact that it looks very different from the overall population of this country. At a time when groups that collectively represent underrepresented minorities (African Americans, Latinos and American Indians) are about a quarter of the resident US population they are a much smaller fraction of the science and engineering communities. These are young populations (who comprises 31% of 5-17 year olds), growing populations who will become a larger and larger fraction of America. Already in states such as California, Texas and New Mexico these groups are no longer a small minority.

Underrepresented minorities received 15% of science and engineering (S/E) bachelor's degrees in 1997 and 11% of S/E master's degrees. In 1998 they received 5% of S/E PhD's in natural sciences and engineering. And over time the discrepancy will increase if the S/E degrees remain flat as the population becomes more and more composed of groups now in the category of underrepresented minorities.

I can argue from purely human resources and economic development grounds that we need to develop these talent pools.

But this conference is not about workforce – it is about basic research in the service of public objectives. Here the arguments for minority participation get expanded to:

- Whether the needs of these groups get reflected in the overall research agenda and human development policies of the country.
- Whether the voices of underrepresented minority scientists are present in key leadership roles at tables where policies are set, resources distributed, decisions made.
- Whether they are part of the process of peer review, helping evaluate the merit of ideas that are proposed.
- Whether problems that may emerge from issues that minorities raise make it onto the table, get funded, are under-supported, get marginalized, contribute to tenure.

Perhaps in many area of science their interest may matter less, as has been argued by my colleagues in mathematics and physics. But the role of “voice” in priority setting is important, independent of field – figuring out how to distribute a pool of support that is smaller than our appetite for it (so many questions, so little money).

And certainly in areas of health, environment, human systems and interactions and the basic sciences that under gird these, there can be little doubt of the vested and shared interests that lie in greater minority participation. It is in the national interest that we promote inclusiveness. And even in the places where direct impact cannot be easily discerned or where the problems of study have little immediate bearing there are strong arguments to be made for the added and different perspectives, talents, values, and attitudes that greater participation can bring.

The numbers of underrepresented minority PhDs will not change unless and until the faculty currently involved in research in our major universities deliberately work to change them. We create new scientists through an apprentice process. Our system, envied by much of the world, is based on the simultaneous production of research and the next generations of researchers. Yet few within our community have embraced the challenge of nurturing minority talent. And many departments, flush with federal research dollars, seem unable to simultaneously conduct research and educate underrepresented minority students, undertaking a task clearly in the national interest. Many choose instead to recruit students from other countries rather than address a serious national interest. In an analysis published in our newsletter, *Making Strides*, we compared federal funding patterns and minority graduate enrollment. We found many institutions that failed to heed the words from the gospel of Luke, that to whom much is given, much is expected.

Several years ago the NSF made its expectations explicit within its strategic plan in terms of the integration of research and education. Subsequently the criteria for review of proposals were modified to undergird those expectations – reflecting the need to expand our definitions of merit beyond more narrow definitions of quality.

“As you spend your patrons money on the best research you can do, are you doing the best you can to build a future for research – (an infrastructure so to speak) to support education, human resources, development of a talent base that is diverse and reflects the face of America, to respond to the patrons’ need to understand the importance and value of what you do?”

It is not clear that these rules, though put in place, are being applied – or even if it is understood why they are in place. As the population and electorate become increasingly minority the research community must be concerned about participation, communication and legitimacy.

As the population becomes more diverse how do we attract the next generation of scientists and engineers without relying on foreign sources – sources inherently unreliable with the changing of political and economic circumstances in young people' home countries.

As the science draws talent and ideas from diverse sources the research is enriched. As the questions relate to diverse populations the research is informed.

Some of the most pressing and troubling questions in science today relate to the disparities in health outcomes of American minorities compared with the majority population. Even where we are able to control for income we still see differences. What accounts for them? Differences in lifestyle? In treatment? In reaction and response of physicians? Or are the differences environmental? Genomic? Physiologic? And if physiologic, in response to what? How do the same PSA titer levels mean different things for white males and black males? Will the same questions be asked if the researcher is oblivious or indifferent to differences because he does not see it in his lab or look for it in his work? We can legitimately argue about the need for a separate center to focus on minority health. We can likely say that had these health concerns been addressed in the context of the regular research enterprise we would likely not have had nor likely need a separate center.

The ability of the science community to determine the structure of research funding likely also depends on our responsiveness to the needs of these publics. Because funding for research comes through a political process, the process will in fact be subject to electorate pressure. Where the science community is unresponsive to legitimate needs to build science capacity throughout the country, for varying populations, for institutions committed and active in serving diverse populations, for specific areas of concern we lose control of the process of rational decision making regarding support for science.

We fool ourselves if we underestimate the human resources dynamic, the intellectual, policy, political, ethical and moral issues associated with underrepresented minorities in science.