Beyond the Endless Frontier – *Michael Crow*

In this article, I'm going to present recommendations for specific changes in the original design that Vannevar Bush gave us 50 years ago in *Science: The Endless Frontier*. This may put me a long distance out on a limb, and I do it with some trepidation, realizing that those who have done this before who are not eminent Nobelists usually have been butchered shortly afterwards.

My premise is that Vannevar Bush's design is not flawed in any serious way. Rather, it is so seriously outdated that it appears completely flawed. To update Bush's design, I have approached Vannevar Bush as if he were a software engineer who laid out the program for the conduct of science in the United States some 60 years ago. I tried to consider it from the perspective of what the design principles were that Bush put into his software code.

There were seven such principles. One was political autonomy. Bush's design parameters separated the scientific enterprise as much as possible from political processes. In practice, there are varying degrees of separation, but autonomy was one of the design parameters.

A second design principle was self-regulation by scientists. Scientists, like the Marine Corps and major league baseball, and any elite group for that matter, were supposed to discipline themselves, set up mechanisms to control their culture, and so forth.

The third of Bush's design principles was a focus on science for science's sake as well as for problem-solving. This principle has been distorted by many people who think that Bush's principal design parameter was science for science's sake. These critics are wrong. Science was both for fundamental discovery and for specific problem-solving.

Fourth, because of both his background as a professor at MIT and his time as dean as well as president of the Carnegie Institution, all of Bush's design parameters are built around a strong academic model of individual achievement. The focus is on the individual – both the individual discipline and individual scientists.

In his last three design parameters, Bush called for scientists to be accountable for achieving national security from an economic, military, and health perspective. Rather than specific accountability, project by project, discipline by discipline, or field by field, he called for scientists and their outputs to be measured in terms of general accountability. Success was to be determined by national achievement.

Sixth, Bush called for a national science organization. That is, he proposed concentrating basic research in a single area. He didn't call it the National Science Foundation. He and his panel had other names for it, but it was to be a single, major, basic research agency.

Lastly, he called for amazingly small budgets. I'm not sure if this was a political calculation on his part and those that were working with him, but the budgets that he called for were very small.

I've taken each of these seven design parameters and, thinking like a software engineer, I have looked at each of them from the perspective of how it could be improved, enhanced, or in some way made better.

Design parameter #1: political autonomy. It may sound like a strange response to this recommendation, but we should establish an institutional mechanism for forecasting our long-term national science and technology needs. This should be a rigorous, ongoing, continuous process that fills a current void.

One of the reasons that political autonomy isn't working for the scientific community is because nobody in the general population knows where they're going or why they're going there. And if they get there, how or when they got there. That is why we need a process that would generate a science and technology roadmap so that everyone can see where scientists are headed and why, and what that means in terms of implementation.

I am not suggesting that we replace the Office of Technology Assessment, which had its own problems. Rather, Congress should establish a means by which a national science and technology roadmap can be developed. A good example of this process has been carried out

the last few years at the National Institute for Future Technologies in Japan, which conducts an exercise to plot the direction of national movement.

Second, the Office of Science and Technology Policy, regardless of the administration in power, must look to this roadmap and either follow it or explain why they're not following it. If done well, mission agencies can and should build their agendas around it. I know this sounds a little bit foreign, but I am looking for something concrete that people can think about.

Design parameter #2: self-regulation by scientists. On this parameter, I have three specific recommendations. One, spend a measurable percentage of all national science assets on educating the public about science and research. We are doing only half of this now. We're trying to educate about science, but we are failing to educate about research.

Second, and this is very controversial, develop a science court for internal discipline and conflict resolution. Bush made no account for this. The numbers of conflicts, questions, and debates, are only going to increase in the years ahead. If we do not develop some type of a mechanism, we will not be able to deal with the political backlash that will occur because we don't have the kind of checks and balances in the system that one would think we ought to have.

My third recommendation is to broaden the criteria for peer review to include the potential for considering broader social profit. Social profit is a poorly-defined term, but suffice it to say that it's an amalgamation of all those things not related to science. If peer review processes on a project and program level do not find a way to begin to include social profit as part of the decision-making process, the notion of self-regulation by scientists will have to be significantly modified at some point. It's under attack right now.

Design parameter #3: science for science's sake as well as for problem-solving. I think one of the barriers that we have to this is incessant fighting, discussing, and arguing over the definition of basic and applied research. The National Science Foundation is a basic research agency. The Environmental Protection Agency is not. We ought to do basic

research here and not there. It's the old adage that my work is basic, and so therefore I can't explain it; and you just ought to fund it, because you're too uninformaed to understand it anyway.

We are going to have to define these terms once and for all, and there has been a major attempt to do this in the Press report (*Allocating Federal Funds for Science and Technology* Press, 1995). Second, we need to evaluate projects with regard to their purpose, realizing that the type of research – basic, applied or what-have-you – relates to the function of the mission agencies. I suggest that all government agencies have the possibility of doing basic research, applied research, and technology development in support of their missions. This is something that should be better understood and better organized. That is, we should bring discipline to an undisciplined process. Lastly, consider all projects and program areas as equal, regardless of their scientific focus or technical objectives.

One of the ideas that permeates the American university setting is that if you go through a Ph.D. program and you're then hired by another academic institution, that's great. If you get a job in industry, that's good. If you get a job somewhere other than those two places, that's not so good. There is a hierarchy in which basic research is the highest order function and all other functions are somehow lesser. I suggest that we find a mechanism wherein all research, all projects, are equal. This goes back to Bush's design.

Design parameter #4: a strong academic model of individual achievement. This parameter has led to a number of problems: barriers between disciplines, difficulty moving in new directions – a whole range of things.

We should develop new, team-funding mechanisms and expand the recognition mechanisms for team participation. We don't have that in the national labs. We have that in industrial labs, but not in academia. We should work toward the evaluation of scientists by discipline and by group. For example, what is the field of chemistry contributing, and to whom?

There is another consideration that goes beyond individuals and individual departments. These are what I call star groups, groups that have the capacity to make significant

achievements. We need to find a mechanism beyond the individual model of trying to disperse resources to a large number of people in equal amounts. We should find a way to provide significant funding to these star groups.

Design parameter #5: general accountability. I think there should be a significant evaluation of agency research programs based on their success or failure to attain particular pre-defined goals or objectives.

If we know why we're moving in a particular direction, people should have some understanding of our logic. They will be able to see how or if an agency's programs contribute to moving toward a defined goal or objective. This may sound a lot like central planning, but it's not. I do not aim to differentiate projects based on an artificial modality. I'm talking about a way to determine, down to each and every individual project, the ability of a project to make progress towards a pre-determined goal or objective beyond merely the scientific goal or objective.

Looking at general accountability, this means that the White House Office of Science and Technology Policy and not the Congress – which would probably do this separately – would have a map. They would establish annual, five-year, and ten-year objectives for national science and technology investment. We don't do that now; we just talk about it. We put together the Council on Science and Technology, which has not been that effective.

We have to drive the process by the precursor step, which is constructing the scientific and technological map by asking where the science might take us. Then, following that mapping activity, decide upon a strategy or plan. Instead, what we do now is spend about 90 percent of our science budget on implementation and ten percent on planning, thinking, strategizing, and so forth.

What does this mean in a research agency? It means that U.S. government research agencies that are funding research projects to industry, academia, or laboratories and that don't have an elaborate mechanism for evaluating the progress of their research programs according to

a national strategy and national R&D map are wasting money, since they don't have the means to evaluate whether or not they're making systematic progress.

They certainly can know whether scientists have won the Nobel Prize, but otherwise, progress is difficult to determine. We do not have sufficient or appropriate measurement tools today, but we need them. Developing tools of assessment is going to require some new mechanisms, some new thinking, and some new cooperation between social scientists and others that have the capacity to interact with scientists.

Design parameter #6: a single basic research agency. This is a bad idea because there are basic research questions that are linked to all of the agencies' missions. What you can have is a single basic research agency like the National Science Foundation which has a specific role. This agency is in charge of building the foundation, knowledge, and research tools to support the research activities of the other mission agencies of the government.

What does that mean? At a research agency, it means rethinking budget and planning models to define their roles as producers of foundation knowledge, basic knowledge, or specific solutions to problems. Some agencies are working on specific solutions to problems. One of the agencies might be working on foundation knowledge. Those planning and budgeting processes need to be linked together.

Design parameter #7: limited resources. Bush emphasized both in the words and their undercurrent, and in the class of individuals he had participating in the process to build *Science: the Endless Frontier* as a report, that limited resources should only be allocated to the best science.

It has been argued that one of the reasons to spend resources at as many institutions as possible is to enable a bell curve distribution of scientists, such that somewhere in the middle or on the right side of the curve, someone is going to be very successful. And therefore you need to have as many participants as possible.

I don't think that is a logical argument to sell to the public. Instead, one has to argue for two things: first, concentrate resources in the fields of greatest importance, linked specifically to their individual mission. Second, and perhaps controversially, dramatically increase the size of average grants – more funding for fewer groups – making the competition even more intense, in order to separate groups that have the capacity to compete on a world class basis from those that do not.

Clearly, we have moved beyond the parameters of Vannevar Bush's science policy design. The complexity of interactions in today's arena calls for equal complexity in the design of our policy apparatus, analysis, and planning. I have suggested science and technology roadmaps to address the outdated notion of political autonomy. Pubic education, science courts, and peer review reform will help to modify scientists' self-regulation. Looking more closely at the purpose of research and developing tools of assessment will increase accountability. We need to increasingly work towards linking scientific research to societal outcomes and Vannevar Bush's design does not facilitate this goal.