7   Fifty Years of Science and Technology Policy in Ten Minutes

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This chapter takes a quick look at the past 25 years of science and technology policy issues in order to try to say something coherent about the next 25.

I will begin with a few recurring themes in science and technology policy over the past 25 years. These themes hold considerable promise for defining the agenda of the AAAS Colloquium on Science and Technology Policy when it meets 25 years from now (in the year 2025). I will then speculate about some specific topics that could be on the agenda in 2025. I will conclude with a suggestion for the agenda for 2001.

Recurring Themes

First, a key enduring theme is allocating scarce funds across fields, problems, and types of performing institutions. This theme is closely related to the theme of interagency coordination of research activities and to the theme of setting priorities for science.

A second enduring theme is ensuring the integrity of the corpus of scientific and technical knowledge. This includes specific issues like the adequacy and functioning of the peer-review system; managing fraud in science; and dealing with pseudo-science, junk science, and, most important, self-delusion in science.

Another perennial theme is enhancing access to science for everyone, along with improving public understanding of science. This is both a continuing problem and a continuing opportunity.

Yet another recurring theme is balancing the essential openness of the scientific system with the critical protection of strategic scientific and technical assets. This emerged as a serious issue during World War II and continued throughout the Cold War. Today, the specifics have changed but the theme is the same: balancing the openness of the university with the need for corporate control of information in a competitive world based on new technology, taking advantage of the networked society while ensuring that information assets can be protected.

The problems appearing under these enduring themes are never solved. They recur repeatedly in slightly different forms, and each generation of policymakers has to address them anew. They are the endemic issues that define the essential nature of what science and technology policy is, both as a field of inquiry and as a field of practice.

The 2025 Agenda

Since only the specifics of these perennial issues change (while their fundamental character remains the same), I can predict with reasonable assurance some of the main themes of the 2025 AAAS Colloquium on Science and Technology Policy. These themes will be familiar: allocating scarce resources and setting national priorities, ensuring the integrity of science, enhancing access to science for all, and balancing openness with the protection of strategic assets. I can not imagine that these problems will go away, and we could have a session on any one of them in 1976, 2000, or 2025.

I will now offer some more speculative suggestions of issues that might be on the agenda in 2025. One subject for the 2025 agenda might be, "What's Next for the National Aeronautics and Space Administration (NASA)?" By 2025, we will probably have sent people to Mars and returned them safely. "What's next?" has been the perennial NASA question since we went to the Moon in 1969. I expect it will come up again after our trip to Mars, with all the attendant anguish that this question always raises.

New directions for medical and health care research will also be on the agenda. We will be focusing more on lessons from social science research to learn how to manage the growing numbers of the very old—those 110 and older.

Another topic for 2025 will be the debates in Congress over repealing the last remnants of obsolete 20th century legislation protecting personal privacy. Everyone at the 2025 AAAS Colloquium will wonder at the naivete of the folks in 2000 who tried to strengthen privacy protection in the face of the emerging ubiquitous Internet. By 2025, there will be nothing private left to protect. Everyone will know or be able to find out anything at all about anyone at all.

By 2025, and perhaps well before that, officials from the National Institute of Mental Health will be on the agenda to discuss their new programs of research on Internet depression. This syndrome will have been identified as resulting from the collapse of the fantasies that, in the Internet era, everyone is connected to everyone else and everyone is in control of himself or herself. We will have discovered that neither fantasy is or can be true—that connection with everyone lies beyond human cognitive powers, and that networked information systems can be used as well for centralized as for individualized control. Mental health research and practice will face a whole new set of challenges that are unlikely to be addressed successfully by chemical means.

In 2025, finding money in the federal budget for research is going to be a real challenge. We will, of course, have the cost of supporting all those 110-year-olds. And we will have to make enormous investments in our energy-conserving infrastructure and in the means to manage the effects of global warming. The big story in Congress will be the tremendous cost of the Great American Coastal Seawall that we will be building from Boston, Massachusetts, to Brownsville, Texas, to hold back the rising ocean.

Finally, the centerpiece of the 2025 Colloquium will be the reports of progress on the frantic efforts in university and government laboratories around the world to locate the source of that three-day burst of coherent radiation that originated beyond the solar system, and reached us in late 2024. It included the proofs of all of Euclid's geometry. Where did the signal come from and how can we connect to those who sent it?

The 2001 Agenda

I would like to return from my predictions for 2025, to end on a more serious observation about the immediate future. One of the things that has struck me over the past 25 years is the number of times that

we have needed a research and development program to address a critical national problem and we have not had an appropriate place to put it. We have made numerous attempts to create such a place, but without long-term success. For example, in the late 1960s and early 1970s, the National Science Foundation created, first, IRPOS (Interdisciplinary Research on Problems of Our Society) and, later, RANN (Research Applied to National Needs). In the 1980s there was interest in creating a "civilian DARPA" (Defense Advanced Research Projects Agency). Also, in the 1980s, Representative George Brown, Jr. (D-CA) was interested in a National Technology Foundation. In the early 1990s, we set up the Critical Technologies Institute, which was intended to tell us what the emerging needs were, if not where the relevant research could get done. But we still do not have any place to go.

Why is the lack of an applied research organization a problem? Let me give some examples of the sorts of issues it might address. Today, computer and information security is an example of a critical national problem for which there is no home for research. Academic scientists and engineers have no place to go for support for research on this problem. Another important "homeless" research issue is understanding and managing the societal effects of the Internet. We desperately need to get research underway to understand the issues that will develop as the Internet becomes more a part of our daily lives. But there is no place to go for support for such research.

We have needed a national applied research organization for a long time. In the 1950s, we needed a place to fund research on desalinization of water. In the 1960s, the problems were developing new methods of housing construction and upgrading the technology of mature industries. In the 1980s, the problem was dealing with inefficient manufacturing systems. Fortunately, the people who did the studies leading to the book, *The Machine That Changed the World*[1], found support at the Sloan Foundation and in the auto industry itself. Without that support and their study, American manufacturing would not have enjoyed its present resurgence. The government was nearly absent in the support of studies and research to improve our manufacturing systems.

Throughout the last 50 years, we have not been able to create an enduring operation that can identify and seriously support research applied to the emerging problems of our society. Addressing this key issue ought to be high on the science and technology policy agenda of the new President and the new Congress, and it should be a cornerstone of

the agenda for the AAAS Colloquium on Science and Technology Policy in 2001.

Endnote

1. Womack, J. P., Jones, D. T., and Roos, D. *The Machine That Changed the World:* Rawson Associates; New York, NY. 1990.