

SCIENCE POLICY PRESENT: WHERE IS THE FRONTIER?

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I. Does Science Policy Matter?

To paraphrase Tolstoi: All rich countries are alike. The general alikeness of affluent nations can be seen in such economic and social indicators as gross domestic product (GDP) per capita, GDP growth rates, macroeconomic structure, fertility and other demographic trends, literacy rates, school enrollments, and measures of overall well-being, such as the Human Development Index.¹ One amazing similarity among OECD (Organization for Economic Cooperation and Development) nations seems to be the percentage of the population whose income is insufficient to lift them out of poverty. Table 1 shows that, for about two-thirds of a selection of affluent nations in 1991, poverty rates prior to income redistribution programs lie between 20 and 25 percent. Of perhaps equal interest, although only tangential to my comments today, the redistribution policies of all of these nations save one—the U.S.—lead to actual poverty rates of about five percent or less.²

Table 1: Relative Poverty Rates (%), circa 1991³

	Post-tax/ transfer relative poverty	Pre-tax/ transfer relative poverty	HDI (1998)
Australia	6.4	21.3	0.929
Belgium	2.2	23.9	0.925
Canada	5.6	21.6	0.935
Denmark	3.5	23.9	0.911
Finland	2.3	9.8	0.917
France	4.8	27.5	0.917
Germany	2.4	14.1	0.911
Ireland	4.7	25.8	0.907
Italy	5	21.8	0.903
Netherlands	4.3	20.5	0.925
Norway	1.7	9.3	0.934
Sweden	3.8	20.6	0.926
Switzerland	4.3	12.8	0.915
United Kingdom	5.3	25.7	0.918
United States	11.7	21	0.929

It is axiomatic that science and technology (S&T) are crucial drivers of economic growth. But, whereas national research and development (R&D) investments relative to GDP are generally similar among affluent nations—typically in the range of two percent for non-defense expenditures—what we might call meso- and micro-scale R&D policies, and the

¹ E.g., UNDP, *Human Development Report* 1999. (New York: Oxford University Press, 1999).

² Kenworthy, Lane, *Do Social-Welfare Policies Reduce Poverty? A Cross-National Assessment*. Luxembourg Income Study Working Paper No. 188, Maxwell School of Citizenship and Public Affairs, Syracuse University, September 1998.

³ *ibid*, United Nations Development Programme, *Human Development Report 2000* (New York: Oxford University Press, 2000).

resulting structures of national R&D enterprises, vary significantly from nation to nation. Public funding for R&D varies considerably, from Japan, whose R&D budget is less than 20 percent government money, to France, at over forty percent. (Table 2a) At a somewhat finer scale, the differences are still more apparent. The most obvious illustration of this diversity is biomedical science, which in the U.S. commands more than 40 percent of the total federal nondefense R&D budget, as compared to four percent in Japan and Germany, seven percent in France, and 24 percent in the U.K. Similarly, Japan devotes more than 20 percent of its civilian R&D to energy, while the U.S. spends about three percent, Germany four, France around seven; UK one. The U.S. spends 24 percent of its civilian R&D on space; France, 15; Germany five; etc.⁴ (Table 2b)

Japan	19%
U.S.	32%
U.K.	32%
Germany	34%
France	41%

	Energy	Health	Space
U.S.	2.8%	42.0%	24.2%
Japan	21.4%	4.2%	6.7%
Germany	3.9%	3.8%	5.3%
France	6.6%	7.3%	15.2%
U.K.	1.1%	23.3%	4.3%

In fact, such numbers don't tell the whole story, because European nations and Japan distribute large chunks of their federal R&D dollars in the form of block grants to universities, who then have great discretion in allocating among various programs. This points out that both within and between nations there is an enormous diversity of policy models used to determine meso-scale R&D priorities and to translate those priorities into actual expenditures. In some ways the U.S. is more decentralized and more diverse than most other affluent nations, with policy-making and funding housed in many federal agencies, and budgetary authority residing in many Congressional committees. At the same time, in the U.S., there is a tighter linkage between specific agency missions and funding allocation to universities than in many other OECD nations. And of course the decision processes for disbursement of funds in universities and national laboratories (as well as the relative roles of different types of R&D performing institutions) varies greatly from nation to nation and within nations. The role of peer review, smart managers, earmarks, block grants, equity policies (e.g., NSF's EPSCOR program; German efforts to support institutions in the east); are all highly variable and reflect different institutional

⁴ National Science Board, *Science and Engineering Indicators – 2000* (Arlington, VA: National Science Foundation, 2000 (NSB-00-1).

⁵ *ibid*

⁶ *ibid*

and national histories, politics, and cultures. Human resources are also variable, with the proportion of scientists and engineers at over nine per 1000 in Japan, eight in the U.S., six in France and Germany, and five in the U.K.⁷ And of course the role of R&D in "industrial policies" has varied greatly over time and varies greatly between nations and between sectors. The 1991 OTA report *Federally Funded Research: Decisions for a Decade* summed up the situation: "While there may be certain universality in science, this does not carry over to science policy."⁸

Of course it is these very details that make up the nuts and bolts of what science and technology policy is all about, and much emotion and energy is invested in promoting policies that favor one approach, priority, or program over another. But given the great diversity in meso- and micro-scale science policies both within and between affluent nations, and given the relative similarity of macroeconomic and socioeconomic profiles of these same nations, I can see no reason to believe that there is a strong linkage between specific national science policies and general national scale economic performance. Of course the U.S. has a strong aerospace industry and a strong pharmaceutical industry, in part because of R&D policy priorities over the past fifty years, but the U.S. still has a 20 percent pre-tax poverty rate, average life expectancy in the mid-70s, GDP per capita above \$20,000, Human Development Index above 0.9, etc. etc., just like other affluent nations. So, while federally sponsored science and technology are obviously causal contributors to economic welfare, and while science and technology policies of some sort are necessary to ensure such contributions in the future, there is little reason to imagine that particular policy models and choices make much of a difference to broad economic outcomes—there seems to be a diverse range of options that work well—indeed, this diversity may itself be a component of success.

Standard debates about science policy are important in that they keep the policy process focused on the value of science, and they allow for various policy options to be exercised and tested repeatedly over time. But particular decisions about priorities, administration, institutional and sectoral arrangements, even levels and allocations of funding, are not very important determinants of long-term macroeconomic performance of affluent nations. From this perspective, the machinations of science policy—the constant stream of conferences and reports; the dozens of committees and working groups; the lobbying and legislation—are best viewed as metabolic byproducts of an ongoing struggle for influence and funding among various political actors such as members of Congress, Administration bureaucrats, corporate lobbyists, college presidents, and practicing scientists. The significance of this struggle is largely political and internal to the R&D enterprise—it is not a debate over the economic future of the nation, despite continual grandiose claims to the contrary. We are largely engaged in science politics, not science policy. Or, to adopt a Kuhnian perspective, this is normal science policy.

Of course federal science and technology are expected to do more than support the economy. They are aimed at ensuring the provision of sufficient quantities and quality of

⁷ *ibid*

⁸ U.S. Congress, Office of Technology Assessment, *Federally Funded Research: Decisions for a Decade* (Washington, DC: Government Printing Office, 1991). p. 272.

food and water; of protecting the environment from people, and protecting people from the environment; of improving health; and of course of protecting national security. But again, affluent nations display an approximation of parity in levels of achievement of most of these goals (which is not to say that they have all been achieved), so I can see no reason to think that R&D policy itself has been a crucial determinant of national performance in these and related areas, or that the marginal changes in R&D policies and funding levels that are the subject of most R&D policy debates are nearly as important to the outside world as they are to the R&D community itself.

By way of illustration let me point out that no area of research in the U.S. is the subject of more contempt than agriculture, because funding is less dependent on peer review, research is mostly carried out at lower-prestige universities, and overall it has the stench of practicality all over it. Yet agriculture science has delivered continued growth of knowledge and continued benefits to society that are absolutely crucial to our survival. Science seems to be a pretty robust practice, however you choose to administer it. (Loren Graham made a similar argument based on his studies of science in the USSR.⁹) It's robustness derives in large part because anyone with any sense knows we can't live without it.

Now this session is supposed to be about the S&T information system in the context of new frontiers of S&T policy. I don't quite know what the S&T information system is, but if you look at the information that is generated about the S&T enterprise, and if you look at how that information is used in S&T policy dialogue, you cannot help but observe that most of it is directed at understanding and enhancing inputs into the system. For example, NSF's semiannual *Science and Engineering Indicators*, by far the most comprehensive and important source of information on the R&D enterprise as a whole, contains copious data about priorities, disciplines, sectors and performers, artificial or non-existent categories like basic vs. applied, and, increasingly, the science education system and workforce. These data reflect inputs to the enterprise, or moving parts within the enterprise, and they are mostly expressed in terms of dollars spent. The few output data are directly tied to internal performance of the system—for example, articles published, degrees conferred, or patents granted. The *Indicators* both reflect and support the tendency to talk about the enterprise in terms of quantified inputs and outputs, and to assume that the societal consequences of R&D are somehow contained, homunculus-like, in the numbers. The similarity between nations with different approaches to science policy suggests that these details of internal structure are not very strongly felt in the real world. When the Superconducting Super Collider died, some physicists foretold the end of science or at least the end of civilization, but it didn't turn out that way; it was just the end of their jobs. This is the old frontier; we have colonized it, given it life, rendered it productive. Like a wilderness transformed into a nice rich lawn, it wants a bit of watering and trimming from time to time, but, from a policy perspective, it's a pretty low maintenance operation.

⁹ Graham, Loren R., *Science in Russia and the Soviet Union* (Cambridge, England: Cambridge University Press, 1993), p. 198.

All of which raises the question of why we care about publicly funded science to begin with. If science is an end in itself, then the ongoing tussle about the internal structure of the enterprise—a tussle we honor with the term "science policy"—certainly seems more than adequate to support the delivery of continued vitality. If, in addition, science is a crucial input to the economic welfare of modern nations, then at least we can say that science policy seems to be doing no harm.

But if science is a means to other desired ends—to the myriad outcomes that contribute to a high quality of life and the capacity to maintain that high quality for future generations—then the role of science policy and the metrics of success become much more difficult to assess. Now we are talking about more than just economic growth—we are talking about applying science as a tool aimed at directly improving our well-being. For the remainder of this talk, I will focus on four obstacles to achieving particular desired outcomes with an input-driven science policy.

II. Problems of Input-Driven Science, Part 1: Solving the Wrong Problem

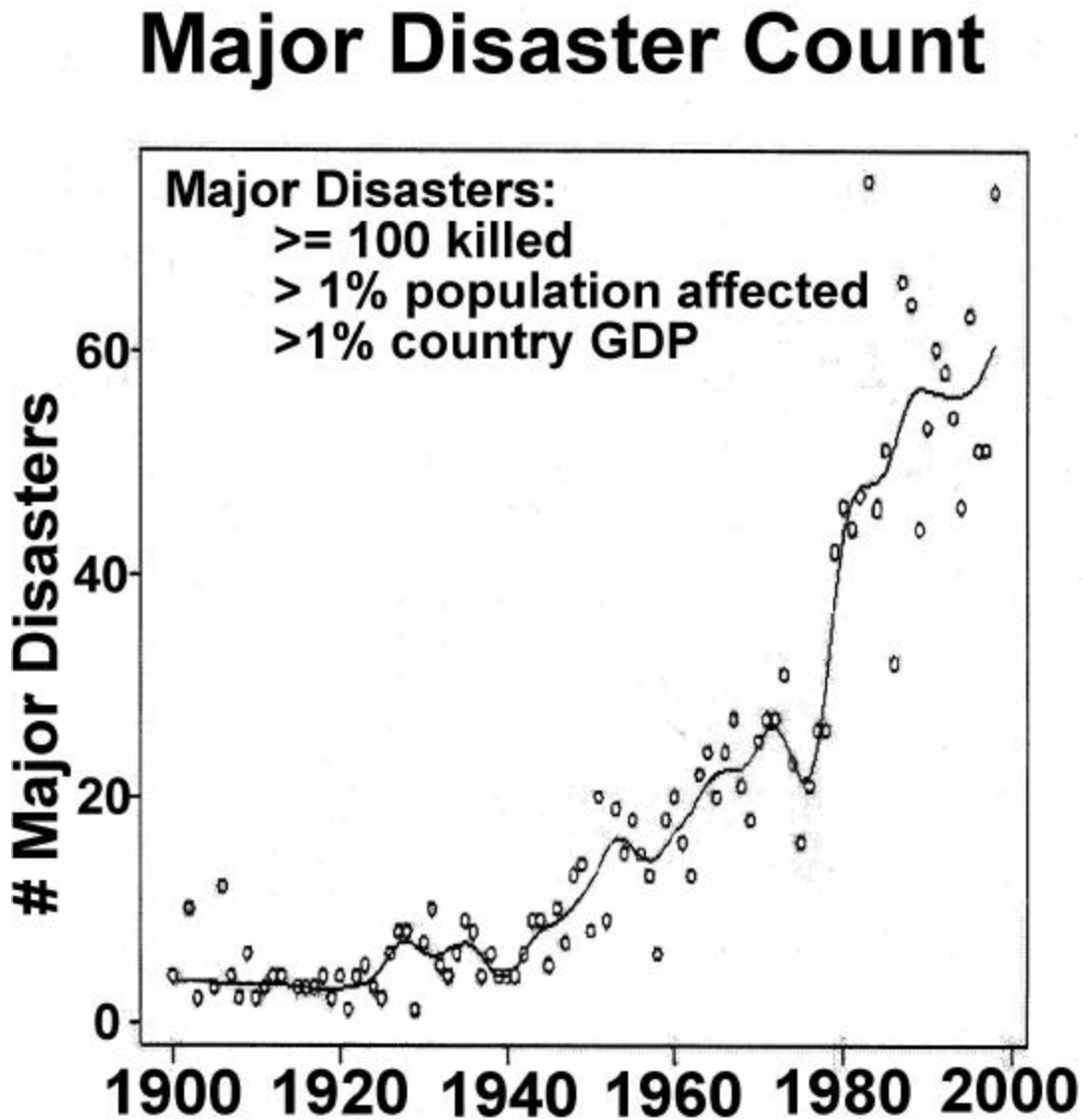
One perspective on this problem is highlighted in our approach to the threat of global climate change. The nature of this threat, of course, is not climate change itself, but the impacts of a changing climate on society and the environment. Yet research has focused almost exclusively on understanding the basic physical, chemical, and biological processes and causes of climate change, and on predicting the future of climate change. This might be a sensible approach if the best prospect for dealing with climate change was to control the behavior of the climate. But in fact there is little or no prospect of either assessing or exerting such control. It also might be a sensible approach if the vulnerability of society and the biosphere to a changing climate was mostly determined by climate conditions themselves. Yet this is clearly not the case. For example, the rapid rise in the number, societal impacts, and economic costs of natural disasters in the 20th century (Figure 1) has nothing to do with climate change, and everything to do with demographic and development patterns of a human population that has quadrupled in the past 100 years. The U.S. climate change research agenda—a very considerable effort at just under two billion dollars a year—may be motivated by the threat of a changing climate on society and the environment, but it can contribute little knowledge to the desired outcome of preparing for and meeting that threat.¹⁰

Research priorities for climate change grew from a science policy process focused strongly on criteria of scientific and technological opportunity, disciplinary hierarchy, and historical funding patterns—that is, factors internal to the enterprise. In particular, these priorities reflect the availability of technological tools to monitor earth systems and to manipulate huge sets of data, and some compelling scientific and philosophical questions relating to humanity's capacity to understand and manage complex systems such as the atmosphere and biosphere. Efforts to connect the climate research agenda to specified societal outcomes, such as reduction of vulnerability to climate and weather,

¹⁰ Sarewitz, D., and Pielke Jr., R., Breaking the Global Warming Gridlock, *The Atlantic Monthly*, July 2000, pp. 54-64. Pielke Jr., R., Klein, R., and Sarewitz, D., Turning the Big Knob: An Evaluation of the Use of Energy Policy to Modulate Future Climate Impacts, Energy and Environment (in press).

and management of water resources, have been afterthoughts at best, both modestly funded—typically at a level of about five percent of total program support—and poorly integrated into larger programs.

Figure 1: Changes in disasters over time¹¹



¹¹ OFDA/CRED International Disaster Database, available at: www.md.ucl.ac.be/cred

III. Problems of Input-Driven Science, Part 2: Health Science Versus Health

Over the past decade or so, the most significant change in U.S. science policy has been the increasing dominance of biomedical R&D in the civilian federal portfolio. The nature of this change is a clear illustration of the current operation of R&D policy process that I have laid out so far. Since, 1990, health research has increased by more than 50 percent in real terms, while civilian R&D as a whole has increased by only 25 percent. The National Institutes of Health now consumes 42 percent of US federal civilian R&D expenditures and fertilizes a potent and rapidly growing sector of the economy. How potent?: in the second quarter of 2000, 20 of the 25 best performing mutual funds focused on biomedical stocks. Somewhere out there is the next Microsoft. In 1997, sales in the pharmaceutical and biotechnology sectors were almost \$100 billion.¹² Another reflection—albeit an indirect one—of the economic importance of biomedical research is the oft-cited fact that the U.S. spends about 14 percent of total GDP on health care (about \$4200 per person), compared to 11 percent for Germany (\$2700), seven percent for Japan (\$2400), and six percent for the U.K. (\$1300).¹³

If the goal of the last decade's increasing focus on health research has been to fuel economic growth, then this approach to science policy seems to have been successful. On the other hand, if the goal has been to improve health, then this approach has been a relative failure. The World Health Organization recently announced that the U.S. ranks 24th in the world in "health attainment," and 37th in overall health system performance, despite the well-known fact that the U.S. spends more on biomedical R&D and health care, by any measures, than any other nation.¹⁴ (Table 3) (To be fair, these numbers are slightly exaggerated because the list includes countries like Monaco, Andorra, and San Marino. This would be like saying that health levels in Beverly Hills are higher than in the U.S. But Greece and Spain are also ahead, despite their considerably weaker economies.)

Table 3: Health Indicators¹⁵

Health Attainment, 1999 (Disability-adjusted life expectancy)	Health Expenditure as % of GDP
1. Japan (74.5)	7.1
3. France (73.1)	9.8
5. Spain (72.8)	8.0
7. Greece (72.5)	8.0
12. Canada (72.0)	8.6
14. UK (71.7)	5.8
20. Finland (70.5)	7.6
22. Germany (70.4)	10.5
24. U.S. (70.0)	13.7
29. Portugal (69.3)	8.2
33. Cuba (68.4)	6.3

¹² Exceptional Returns: The Economic Value of America's Investment in Medical Research (New York: Funding First, May 2000).

¹³ World Health Organization, *The World Health Report 2000* (Geneva: WHO, 2000).

Now one might respond to this observation by asserting that science and science policy cannot be blamed for faults in the political and economic system, for the greed of the insurance industry and the failure of Congress to enact meaningful health care reform. But the flip side of this argument is that, because many other nations have been able to achieve comparable or better levels of public health without a similar commitment to biomedical R&D or expensive high technology medical care, we should have no reason to assume or argue that more biomedical research will result in better public health. Another way to think about the problem is to ask the following: if our goals are to increase years of healthy life (where the U.S. ranks 24th—4.5 years less than first-ranking Japan), and to create both equitable access to, and equitable outcomes from, our national health system, how would we structure our R&D portfolio to best contribute to these goals?

Now interestingly enough, these are precisely the goals set out in the Department of Health and Human Services report "Healthy People 2010," and it is worth asking what the connections are between these explicit goals and the priority-setting process and research portfolio at the National Institutes of Health. And the answer is: very little. Because, while NIH seeks in some ways to allocate dollars to disease research based in part on the relative impact of various diseases on society,¹⁶ they do not make allocations on the basis of what type of research is likely to yield the best public health outcome—rather, they allocate on the basis of what type of science is likely to yield the best scientific outcomes.

This is a crucial point, because here the fundamental flaw of modern science policy rears its head: the confusion of ends and means; the conflation of scientific outputs and societal outcomes. NIH claims to use five criteria for allocating its fiscal resources:¹⁷

- Public health needs
- Scientific quality
- Probability of scientific success
- A diverse research portfolio
- Strong infrastructure and human resources

Four of the five of these criteria are explicitly internal to the research enterprise. The fifth—public health needs—is implicitly internal to the enterprise, because it governs the distribution of inputs, without saying anything about outputs—that is, about public health outcomes. These criteria are all about means—science—not about ends—health. The case of breast cancer is illustrative. Public activism has led to increased awareness and research expenditures on breast cancer—a good thing. But to what extent do

¹⁴ *ibid*

¹⁵ *ibid*

¹⁶ Gross, C., Anderson, G., and Powe, N., The Relation Between Funding by the National Institutes of Health and the Burden of Disease, *New England Journal of Medicine* 340 (24): 1881-1887, June 17, 1999.

¹⁷ Working Group on Priority Setting, Setting Research Priorities at the National Institutes of Health (Washington, DC: National Institutes of Health, September 1997), available at: www.nih.gov/news/ResPriority/priority.htm

research priorities reflect an analysis and balancing of the range of connections between research inputs and health outcomes? Consider two reasonably well-established facts. The first is that mortality from breast cancer among Japanese women in Japan is between five and ten times less than among American women living in America—or women of Japanese descent living in America. This fact has been known for many years.¹⁸ The second fact is that mutations in the BRCA 1 gene, which occurs in between 0.1 and 0.5 percent of women (and confers about an 80 percent probability of breast cancer) is responsible for about five percent of all breast cancers.¹⁹ So if we could prevent every woman with the BRCA 1 gene from contracting breast cancer (right now this is accomplished through prophylactic mastectomy), we would reduce incidence by five percent, whereas if we could replicate Japanese rates of breast cancer in the U.S., we'd reduce mortality by 80 or 90 percent. While the latter challenge may be more difficult from many perspectives, a significantly lower degree of success would still yield significantly higher benefits for the population as a whole. On the other hand, it would require a major reprioritization of research, with more emphasis on environmental, cultural, dietary, and behavioral questions of cancer prevention, relatively less emphasis on molecular genetics and biochemical mechanisms, and, not incidentally, less potential for contributing to the profitability of the pharmaceutical and biotechnology industries. In this context, the molecular biologist Robert Pollack observes that "the current emphasis on the genes responsible for a tumor [is] an interesting sidelight to the real problem of cancer."²⁰

IV. Problems of Input-Driven Science, Part 3: Structural Violence

An even more troubling problem has to do with justice and equity. It is well known that 40 million or so people in the U.S. don't have medical insurance. Public health disparities between socioeconomic and ethnic groups are significant and discouraging; most conspicuous is the 6-year difference in life expectancy between white and African Americans.²¹ The orgy of self-congratulation following the completion of the draft genome map was of course accompanied by visionary promises of improved human health, but here is what Washington Post columnist William Raspberry had to say: "The rich will have their lives improved and extended. The poor won't. . . . Gene mapping will make possible huge improvements in health outcomes—for those who can pay the price. Both the improvements and the cost will increase with each new medical discovery . . . And so will the gap between rich and non-rich."²² This gap is much more than an economic issue: it is a reflection of our society's views about who deserves to be healthy. And this fundamental question of justice is also a question of science policy.

¹⁸ E.g., Wynder, E., Fujita, Y., Harris, R., Hirayama, T., and Hiyama, T., Comparative Epidemiology of Cancer Between the United States and Japan, *Cancer* 67(3): 746-763, February 1, 1991.

¹⁹ Roberts, L., Zeroing in on a Breast Cancer Susceptibility Gene, *Science* 259: 622-625, January 29, 1993; Easton DF, Ford D, Bishop DT (1995) Breast and ovarian cancer incidence in BRCA1-mutation carriers. Breast Cancer Linkage Consortium. *Am J Hum Genet* 56:265-71.

²⁰ Pollack, R., *The Missing Moment* (Boston: Houghton Mifflin, 1999) p. 173.

²¹ Nemecek, S., Unequal Health, *Scientific American*, January 1999, 40-41.

²² Raspberry, William, Genetic Side Effects, *The Washington Post*, June 30, 2000, p. A31.

Biomedical research illustrates the political and moral dilemmas that arise from a science policy process that focuses on the internal workings of the R&D enterprise, rather than the connections between the enterprise and a range of desirable societal outcomes. On the one hand, health inequities in the U.S. are not caused by biomedical R&D priorities. On the other, these inequities are likely to be exacerbated by current science priorities, because research outputs will preferentially benefit those who already have better health and who are positioned to make best use of the health care delivery system. Are we prepared to accept this role for NIH?, that is, the role of strengthening the inequitable distribution of health benefits in the nation? Of course, "inequitable distribution" is a euphemism. Another, more expressive term, is "structural violence," which expresses the fact that maldistribution of public health benefits is a cause of premature death just as certainly as is armed violence.²³ Science policy could ask: how can S&T reduce structural violence? The answer might imply a far different allocation of resources than reflected in the current enterprise.

While this problem illustrates a thorny and highly ambiguous problem for science policy on the domestic front, some clarity can be achieved with an international perspective. Figure 2 shows a curve of disability-adjusted life expectancy plotted against average GDP per capita for all nations. The graph displays several striking trends. First, this is a curve with two parts, one very steep, where increasing longevity is strongly correlated with increasing GDP per capita, and another which is rather flat, where more money doesn't correlate very closely with greater life expectancy. Now of course the vast preponderance of the world's biomedical research dollars are aimed at diseases suffered by people on this flat part of the curve, especially over toward the right side. This accounts for about 1.3 billion people and on the order of 12 percent of the global burden of disease.²⁴ Yet of course the sensitivity of this curve to increasing investments is very small—not much bang for the buck. And even when a problem is shared by both arms of the curve—e.g., AIDS—the interventions that are useful for people living in the flats may be irrelevant to those living on the slope.

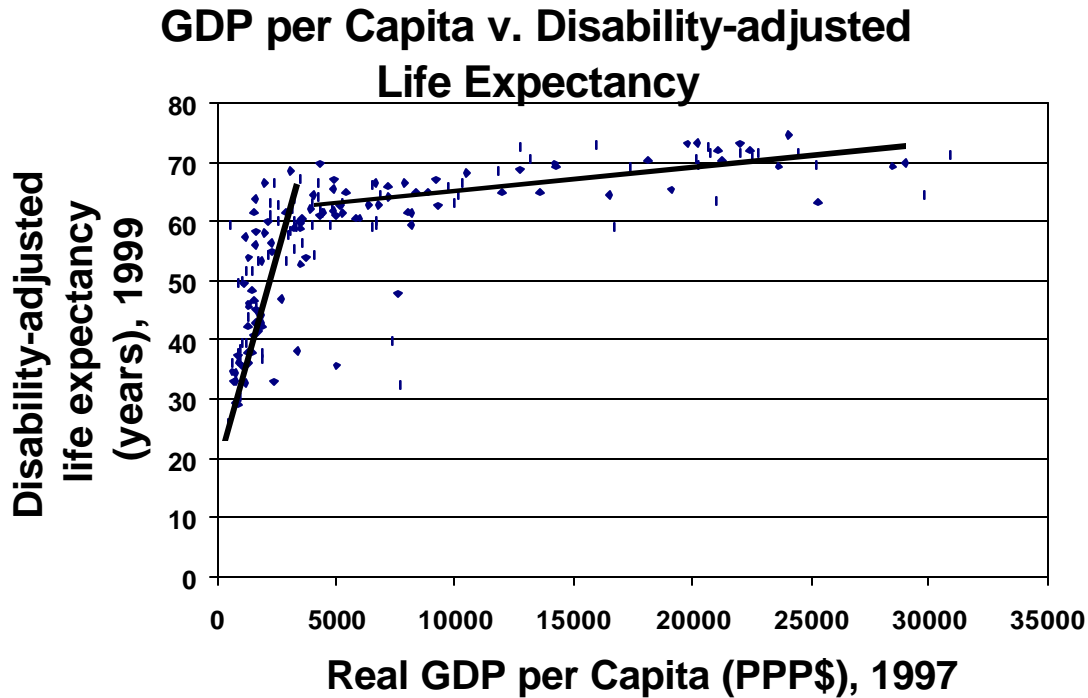
On the other hand, the steep part of the curve is where medical research and medical intervention can make a huge difference, especially given the small investments now made on the health problems that dominate this large majority of the human population. And, in terms of bang for the buck, it's important to note the positive feedback between good health and strong economies. Healthy people can work hard, and they have a reason to work hard. They also have a reason to invest in education, and to save for the future: because they have a future. The arrow of causation between health and wealth clearly points in both directions on this steep part of the curve.²⁵ But the necessary medical research and medical intervention is of a different type than what now dominates the biomedical research agenda. Significant realignments of research priorities would be

²³ Kohler, G., and Alcock, N., An Empirical Table of Structural Violence, *J. of Peace Research* 13(4): 343-356, 1976. I thank R. Rhodes for exposing me to this literature.

²⁴ The Global Burden of Disease: Executive Summary, available at: www.hsph.harvard.edu/organizations/bdu/summary.html

²⁵ Bloom, D., and Canning, D., The Health and Wealth of Nations, *Science* 287, February 18, 2000, pp. 1207-1209.

Figure 2: Health vs. Wealth²⁶



required to reduce health-related structural violence, both in the U.S. and globally. We are finally beginning to see some recognition of this need among governments and international organizations.

The examples of public health and climate change illustrate why input-driven science policy is often not well aligned with societal needs—even if those needs are explicitly invoked to justify the science in the first place. The principal goal of science policy has always been to ensure a healthy science enterprise as judged by criteria internal to science, and that is what we have created. Lacking, to a surprising degree, has been an analytical function to support the design of R&D programs in light of the specific needs and capabilities of society. Such a function would not need to do a better job predicting or controlling the course of science. It would need to develop appropriate information to guide science resource allocations. For example, in developing a more equitable, outcomes-oriented cancer research portfolio, Robert Pollack suggests that an "agenda for basic research would begin with a planetary review of differences in the incidence of various cancers, because some regions and cultures are hot spots for some cancers, while in others the same cancers are exceedingly rare. From this international effort, governments and companies worldwide would have the information necessary to plan a planetary strategy for the prevention of cancer: planetwide optima for low-mutagen food, air, and water and clear guidelines for behavior that would, together, assure the lowest possible frequency of avoidable cancers."²⁷

²⁶ World Health Organization, *World Health Report 2000* (Geneva: WHO, 2000).

²⁷ Pollock op. cit. p. 173.

V. Rational Science-Policy Design and the Big Taboo

Outcomes-oriented research agendas must themselves be based on rigorous approaches to problem definition and characterization. This is harder than simply saying: "let's fund the best science and that will lead us to the best outcome." Science policy design must itself be seen as a target for research, rather a process for applying bromides about "scientific excellence." Scientific outcomes do not emerge fully formed from the laboratory; they are created by evolving interactions between the outputs of research and the needs and capabilities of society. The fact is that in many areas of science, we lack sufficient information and knowledge about these interactions to design truly rational research programs. So we fall back on what we know how to do best: talk about inputs to the system, and assume that we will get the outputs we need and desire.

Input-driven science policy works pretty well in the economic realm because the marketplace doesn't much care what types of products it purveys, so long as there is a steady stream of innovation. Moreover, the very operation of the marketplace provides the type of feedback that research planners can use to enhance research strategies. Private sector R&D managers have strong incentives to listen to market signals, and to incorporate them into foresight planning processes that have become central to the development of corporate research portfolios. But in the absence of a market and market incentives, developing sufficient information for publicly funded, outcomes-based science policy requires a willful public commitment to research in support of policy design—a commitment that is simply not given much priority by science policy makers in the U.S. today.

This discussion begins to imply something quite uncomfortable. It is often said that science and technology are now advancing at rates far beyond the response capacities of our institutions of governance and culture. Ongoing front-page controversies over genetically modified organisms are a stark example of this discordance, as are debates surrounding the manipulation and patenting of human genetic material, and the protection of privacy in a world of increasingly open and rapid information exchange.

In a controversial and widely read article in *Wired* magazine called "Why the Future Doesn't Need Us," Sun Microsystems chief scientist Bill Joy argued that emerging technologies in genetics, nanotechnology, and robotics may create tangible threats to the future of humanity. Noting that in the next few decades personal computers will be a million times more powerful than they are today, Joy writes: "As this enormous computing power is combined with the manipulative advances of the physical sciences and the new, deep understandings in genetics, enormous transformative power is being unleashed. These combinations open up the opportunity to completely redesign the world, for better or worse: The replicating and evolving processes that have been confined to the natural world are about to become realms of human endeavor."²⁸

²⁸ Joy, Bill, Why the Future Doesn't Need Us, *Wired*, April 2000.

Bill Joy is no Luddite, and whatever one may think about his specific prognostications, his general point is unavoidable: the power of science and technology will continue to accelerate; the ability of humans and their institutions to understand and respond to the changes wrought by science and technology will remain more or less the same. The biggest and perhaps most important question for science policy in the coming century has been completely taboo up until now: What types of science shall we not do. Knowledge cannot be unlearned. When the person sitting across the table from you is a clone, the opening for reasoned dialogue about the morality and wisdom of cloning will slam shut.

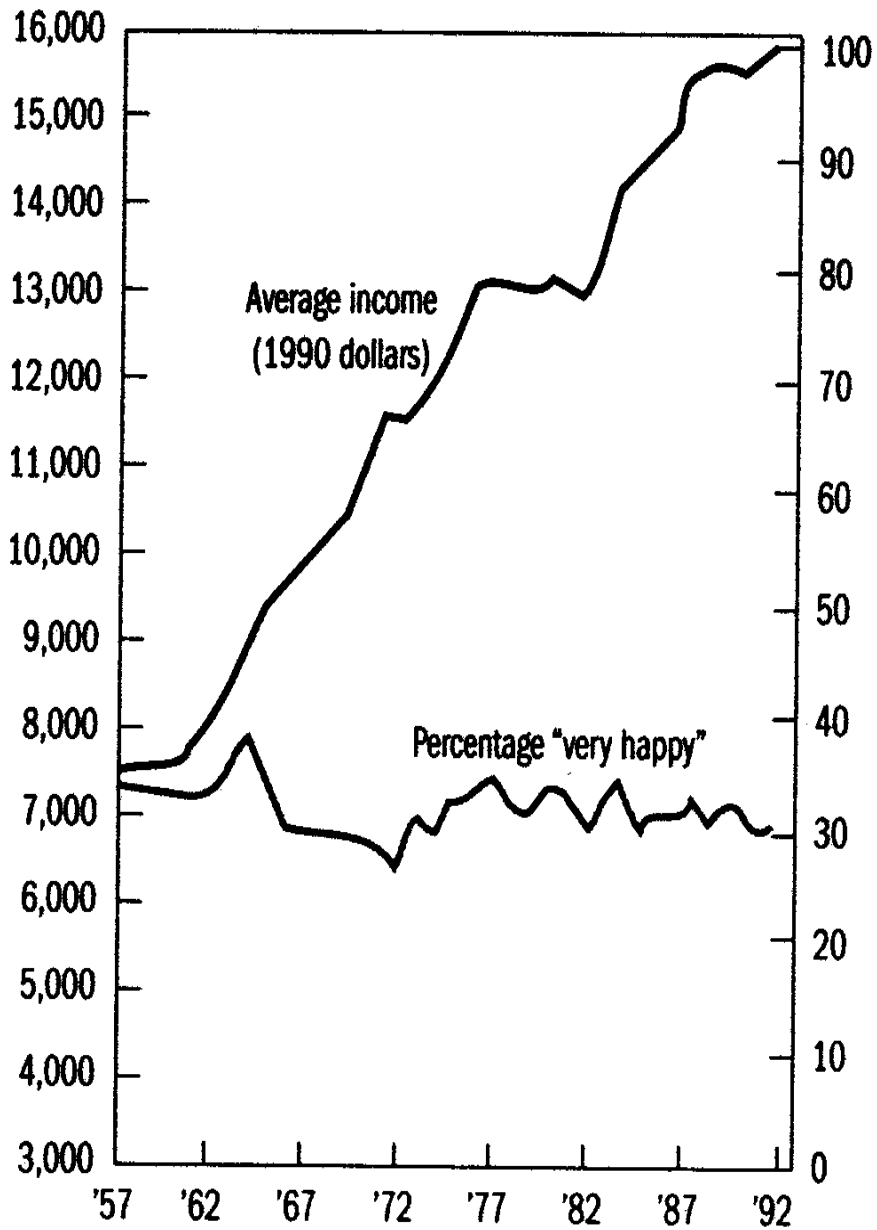
The trepidation with which I raise this issue is testimony to the hegemony of our current, input-driven view of science and technology policy. Why wouldn't we want to consider slowing or stopping or not embarking on certain lines of research because we just don't understand enough about their implications? Our one example of such a decision—the voluntary moratorium on recombinant DNA research undertaken by molecular biologists in the mid 1970s—looks in retrospect like a quaint aberration.

A standard response to this line of argument is that science and technology have been transforming society for centuries, and you either climb aboard or get left behind. But to suggest that the stirrup, the printing press, the cotton gin, the internal combustion engine, the electric power grid—all of which did profoundly transform the world—are equivalent in their transformational potency to what we are creating today is nonsense. The only possible analog we have to today's emerging technologies is nuclear weapons, and many physicists did indeed fight unsuccessfully to put that genie back in its bottle. Lest we forget, the U.S. and Soviet Union came within a hair's breath of a cataclysmic nuclear war during the Cuban missile crisis. We were lucky, not smart.

A final point on this question: affluence has been achieved by nations constituting about 15 percent of the human population. These are also the nations who fund and perform most of the world's science. More absolute affluence for the fortunate 15 percent may well be a political necessity, but the connection between affluence itself and quality of life is less than obvious. (Figure 3) For the most part, science policy—or, more precisely, science politics—largely exists to serve the political need of stimulating more economic growth for those who are already well off. If this is the case, then arguing against a precautionary approach to certain types of knowledge generation makes no sense. There is much more to lose than to gain. The idea that we might choose not to pursue some types of R&D—even if they are scientifically ripe—dignifies the principle that human values should not be sacrificed to the impersonal advance of science under the impetus of the marketplace.

I have argued that science policy as currently practiced in the U.S. is now on autopilot; it operates largely as struggle over allocation of resources, well-suited for supporting the general process of economic growth, but less able to contribute to particular societal outcomes. I have raised four challenges facing science policy today that are not amenable to solution through our current approach. First is the tendency of an input-driven policy model to define problems in scientific rather than societal terms, as revealed in the case of climate change research. Second, and related to the first problem,

Figure 3: The “Happiness” Trend²⁹



► Average U.S. per capita income vs. percentage of U.S. population which considers itself “very happy,” 1957-1992.

²⁹ Myers, D.G., *The Pursuit of Happiness: Who is Happy, and Why* (New York: Oxford University Press, 2000).

is the tendency to conflate progress in science with progress in society, as displayed by the biomedical research enterprise. Third is an apparent increase in the differential appropriability of science, which leads in turn to exacerbated inequities in health outcomes, and most likely in a spectrum of other outcomes as well, including environmental, economic, and cultural. And finally is the question of deciding whether we ought not to pursue some types of knowledge at all, while we seek to better understand how we might manage and respond to that knowledge.

There are no theoretical obstacles to taking bold steps toward meeting any of these challenges. The obstacles are political and psychological. The idea and practice of input-driven science policy is so deeply internalized in the minds of both scientists and science policy makers—and so broadly accepted by the U.S. public—that alternative mental models are instinctively labeled as anti-science and rejected. Yet there are signs of change. Researchers in the human dimensions of climate change are beginning to understand how to generate knowledge that improves the capacity of people and organizations to respond to a dynamic climate. (Incredibly enough, the National Research Council has actually issued a report on this very subject.³⁰) Organizations such as the World Health Organization, the International Red Cross, and United Nations Development Programme are generating information about the structure of health, wealth, and human development that provides a foundation for setting outcomes-based priorities and measuring progress. The idea of public participation in science priority-setting is very slowly gaining ground. Controversies over the ethical, political, and environmental implications of a range of techniques and technologies have begun to stimulate a public discourse that bears some vestiges of thoughtfulness on both sides. These types of activities can nourish an outcomes-oriented approach to science policy whose success is measured in terms increasing quality of life, rather than increasing size of R&D budgets.

³⁰ Panel on the Human Dimensions of Seasonal-to-Interannual Climate Variability, *Making Climate Forecasts Matter* (Washington, DC: National Academy Press, 1999).