Essays of an Information Scientist, Vol:4, p.285-289, 1979-80 Current Contents, #40, p.5-9, October 1, 1979

Current Comments

How Can We Prove the Value of Basic Research?

Number 40 October 1, 1979

I probably don't need to convince *Current Contents*® readers that basic research is important. Many of you will no doubt agree that it is vital to pursue basic research, not only because it increases knowledge, but also because unexpected benefits often spring from it. For the past decade, we have experienced special problems in the funding of basic research in the US. Federal support of basic research has been declining over the past 10 years. According to the National Science Board,[1] federal support increased about 4.3% a year from 1968 to 1976. But in terms of constant dollars it declined an average of 1.8% a year. There's been a slight upward trend since 1975, but basic research obligations are still about 5% lower than 1968 expenditures. From 1979 to 1980, however, the National Science Foundation estimates that support increased 9%. But since the 1979 inflation rate is about 13%, basic research will continue to have trouble holding its ground.

Even in the heyday of federal support for basic research there were always areas of neglect. But in recent years basic research was given a particularly low priority.[2] Last year's budget was under 0.7% of total federal spending. Science journalist Daniel S. Greenberg calls this "the sort of sum that the Defense Department might fritter away in collective overruns on a major missile system."[3] This dismal economic climate coexists with what *Christian Science Monitor* science editor Robert Cowen calls a "fashionable anti-intellectualism" among the public and Congress that bodes ill for future support of basic research.[4]

The interest that I've expressed in the past[5,6] in drumming up support for basic research was rekindled recently by *Biomedical Scientists and Public Policy,*[7] a collection of papers about some social and political aspects of biomedical progress. It is edited by my friend H. Hugh Fudenberg, chairman of the Medical University of South Carolina's Department of Basic and Clinical Immunology, and Vijaya L. Melnick of the University of the District of Columbia's Department of Biology. I was reminded to read the book when I recently visited Hugh in South Carolina. I always wanted to see Charleston, the setting of and inspiration for Dubose Heyward's play *Porgy and Bess,* which George Gershwin later set to music. I also made the trip to speak on the question, "Can significant medical research be recognized?"[8]

Some of the articles in the Fudenberg-Melnick book are relevant to that topic, though not all of them deal with basic research. (The contents page is reprinted in Table 1.) Those that discuss basic research define it differently. Perhaps all of the definitions are covered by those given in Lewis Thomas' paper, "On the planning of science." (p. 67-75)[7] (The paper is a reprint from a 1975 book.[9]) Thomas describes *applied research* as "the kind of scientific activity that you must engage in when you are entirely certain how an experiment, or a chain of experiments, is going to turn

Table 1: Contents page of *Biomedical Scientists and Public Policy*. H. Hugh Fudenberg & Vijaya L. Melnick, eds. New York and London: Plenum Press, 1978. 238 p. $17.95.

out." But in *basic research*, by Thomas' definition, the outcome is uncertain, and "the shock, and the surprise, come when the experiment *does* turn out as you hoped." This recalls Wernher von Braun's quip that "basic research is what I am doing when I don't know what I am doing."[10]

One of the most impressive chapters of the Fudenberg-Melnick book considers the problem of how to recognize important basic research. (p. 15-33)[7] It was written by Julius H. Comroe Jr., director of the University of California's Pulmonary Research Institute, and Robert D. Dripps, who was the University of Pennsylvania's vice president of health affairs until his death in 1973. Comroe put Dripps' name on this paper, originally published in *Science* in 1976,[11] because Dripps worked on the 6-year study from 1970 to 1973. The paper is based on a two-volume report[12] available for $9.75 from Public Inquiries and Report Branch, National Institutes of Health, Building 31, Room 5A03, Bethesda, MD 20014.

Comroe and Dripps were disenchanted with the conventional defense of basic research. They correctly asserted that most people rely on anecdotal evidence to argue that basic research eventually pays off. For example, Roentgen's basic research on physics problems inadvertently led to the discovery of x-rays. One could mention numerous similar examples. Comroe and Dripps wanted a more objective justification for supporting basic research. Furthermore, they were stimulated by a 1966 Defense Department report entitled "Project Hindsight."[13] This study concluded that 20 of the most important military weapons came about as a result of *applied* research. Comroe and Dripps questioned the methodology of the study. More importantly, they feared that the conclusions of the military study would be transferred to the biomedical sector.

Comroe and Dripps decided to find out how much basic research really contributed to biomedical science. They asked over 100 specialists to vote on the top 10 advances in cardiovascular-pulmonary medicine and surgery that occurred between 1945 and 1975. Table 2 lists the 10 advances.

The next step was to identify the "essential bodies of knowledge" required for each advance. For example, before successful open-heart surgery became possible, scientists needed electrocardiography, cardiac catheterization, blood typing, blood preservation, blood banks, anticoagulants, antibiotics and many other devices, substances, and techniques. Comroe and Dripps counted 137 "essential bodies of knowledge" that preceded the 10 advances.

Table 2: The top 10 clinical advances in cardiovascular and pulmonary medicine and surgery in the last 30 years, quoted from: Julius H. Comroe, Jr. and Robert D. Dripps. Scientific basis for the support of biomedical science. (Fudenberg H H & Melnick V L, eds.). *Biomedical scientists and public policy.* New York and London: Plenum Press, 1978. p. 15-33.

Cardiac surgery (including open-heart repair of congenital defects and replacement of diseased valves)
Vascular surgery (including repair or bypass of obstructions or other lesions in aorta, coronary, cerebral, renal, and limb arteries)
Drug treatment of hypertension
Medical treatment of coronary insufficiency (myocardial ischemia)
Cardiac resuscitation, defibrillation, "cardioversion" and pacing in patients with cardiac arrest, slow hearts, or serious arrhythmias
Oral diuretics (in treatment of patients with congestive heart failure or hypertension)
Intensive cardiovascular and respiratory care units (including those for postoperative care, coronary care, respiratory failure, and disorders of newborn)
Chemotherapy and antibiotics (including prevention of acute rheumatic fever and treatment of tuberculosis, pneumonias, and cardiovascular syphilis)
New diagnostic methods (for earlier and more accurate diagnosis of disease of cardiovascular and pulmonary-respiratory systems)
Prevention of poliomyelitis (especially of respiratory paralysis due to polio)

They then sought to identify the key articles that contributed to these bodies of knowledge. The knowledge needed for the advances has been evolving since ancient times, so it was obviously impossible to examine every paper of possible relevance. The authors did, however, manage to examine 4,000 articles! They found 2,500 that were important to the bodies of knowledge involved. They also named 529 "key articles." Key or milestone articles were the articles that affected the direction of future research, and reported new discoveries or hypotheses essential for the development of the areas of knowledge.

Comroe and Dripps concluded that 61.7% of the key articles reported basic research. Other kinds of research, including applied or clinical research, accounted for 21.2% of the key articles. Development and engineering accounted for 15.3%. Review articles or those synthesizing the data of others accounted for 1.8%. The data showed "that clinical advance requires different types of research and development and not one to the exclusion of another." But, since basic research seems to result in almost twice as many key discoveries as the other kinds of R&D combined, it ought to be funded generously.

I was impressed with the grand scale of this work when I first saw it, but didn't have the time to give it the attention it deserved. Joshua Lederberg, during a discussion of the various kinds of reviewing scientists do, pointed out to me that the study is a work of incomparable scholarship and will be important for future historiographers.[14]

I will have more to say about Comroe's contributions to scholarship in the future. It will suffice here to quote the conclusion of the paper. It recommends:

> That an independent, highly competent group be established, with ample long-term support to conduct and support retrospective and prospective research on the nature of scientific discovery, to analyze the causes of long and short lags between discovery and clinical application and to support and test means of decreasing long lags, and to evaluate present and proposed mechanisms for the support of biomedical research and development. (p. 33)[7]

My only disagreement is that such a group should not limit itself to biomedical research. I think we need its counterpart in the physical and social sciences as well. Nor would a study of the impact of humanities research be irrelevant. I regret to report that Comroe and Dripps' recommendation has not yet been adopted. It puzzles me that the Office of Technology Assessment in Washington hasn't done studies like this.

The Comroe study sought to avoid the usual "let-me-give-you-an-example" approach to defending basic research. In his chapter, "Informing the public: fiscal returns of biomedical research," Hugh Fudenberg shows another objective way to make the case for research. (p. 35-48)[7] (This paper is an expanded version of an earlier one.[15]) Fudenberg begins with the pessimistic but reasonable assumption that Congress and the public can no longer be swayed by vague arguments about how future medical discoveries could save *lives* or improve the quality of life for some. So he argues that the *money* spent for basic research today actually saves money in the long run. He bases his argument on examples of the basic research funded by the National Institute of Allergy and Infectious Diseases (NIAID).

Fudenberg argues that basic research in the mid-1950s on feedback control of antibody synthesis led to the near-eradication in the early 1960s of Rh hemolytic disease in newborns. In the past this disease caused infant and fetal mortality. Many victims who lived suffered brain damage, which meant many of them spent their lives in institutions. The eradication of the disease, Fudenberg estimates, saves $60 million a year in the US and 10 times that throughout the world.

Before the early 1970s, Fudenberg notes, symptom-free blood donors often passed serum hepatitis on to people who received transfusions. But NIAID basic research on antigens serendipitously led to ways to screen blood donors for serum hepatitis. Estimated yearly savings: $100 million. Fudenberg adds that a vaccine introduced in 1963, the result of basic research, prevented birth defects in fetuses whose mothers contracted measles. Estimated savings: $180 million yearly. And J.F. Enders' and coworkers' 1949 basic research on virus propagation eventually led to the 1955 development of the polio vaccine. Estimated yearly savings: $2 billion. Fudenberg estimates that, overall, the $33 million or so that NIAID spends on basic research each year results in savings of over $3 billion a year.

Some may say that his arguments are oversimplified. Perhaps it is true that these estimates cannot be taken at face value. Certainly many factors, inflation among them, must be accounted for. But Fudenberg's argument does dramatize the idea that basic research has an economic value that is not always obvious.

Thus, Fudenberg regards as "fiscal irresponsibility" the fact that only 17% of the National Institutes of Health (NIH) budget is earmarked for basic research. Perhaps his most dramatic statement is his attack on the Office of Management and Budget (OMB). Urging that funding of applied research does not occur at the expense of basic research, he says: "Had funds been awarded according to OMB criteria in the late 1940s in an attempt to conquer polio, we would now probably have the world's best respirator and polio would still be with us." (p. 47)[7]

To boost funding for basic biomedical research, Fudenberg recommended in 1973 that a National Foundation for Biomedical Research be established.[16] Such a non-profit organization would collect and disseminate information on the costs and benefits of biomedical research. It would be funded by members and scientific societies. Unfortunately, this has yet to materialize. The same is true of Vijaya and Daniel Melnick's proposal to establish a Washington area liaison office for biologists to keep scientists informed of Congressional action. It would also provide non-partisan, independent evaluations of government programs and proposed legislation. This organization would aid support for all types of research simply by making scientists more aware of what is going on. (p. 11-12)[7]

In singling out these chapters I don't mean to slight the other papers in this fine book. Many of them touch on basic research. Szybalski discusses it in the context of recombinant DNA research and asks how DNA research regulation might affect scientific freedom. Marston, Cooper and Fullarton, and Maldonado discuss it against the backgrounds of health education, policy, and funding. My failure to comment at length on these and the other chapters doesn't mean I think the issues discussed are unimportant.

I hope it's clear from my summaries that parts of *Biomedical Scientists and Public Policy* provide a useful look at basic research in the US. However, I think a chapter on the impact of US basic research on *other* countries might have been relevant also. Fudenberg estimates a 100 to 1 financial payoff from NIAID basic research in the US. On a worldwide scale, the cost-benefit ratio of US basic research is probably closer to 200 to 1. Certainly other countries benefit from US basic research as well as their own. For all its shortcomings, the US has repeatedly fostered an impressive percentage of the world's basic research discoveries. The US dominance of most-cited lists[17] and Nobel prizes bears this out. Would it be so outrageous, then, for foreign governments to help support US basic research? Undoubtedly direct funding of US scientists by other countries would face insurmountable political problems. Not the smallest of those problems is the fact that scientists in many other countries feel they are not adequately supported by their own governments.

We really need an internationally funded organization similar to the NIH or the National Science Foundation. Such an International Science Foundation could support basic research by any qualified investigator in any nation. We know enough to be able to identify those scientists throughout the world capable of making important discoveries. The nations of the world should find a way to give them the moral and complete financial support they need. I also think it would be relevant to build up the archive of case studies that Comroe and Fudenberg have pioneered, so that every citizen would recognize the sheer folly of letting our best scientists squander their time in a continuing quest for adequate support.

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*Reprinted in **Garfield E.** *Essays of an information scientist.* Philadelphia: ISI Press, 1977. 2 v.