Letters

Manpower in Biomedical Science

Information provided in the editorial by Dael Wolfle and in News and Comment by Greenberg concerning the federal budget for science (5 Feb., pp. 561, 582) heightens the fears and frustrations of those who are concerned about the growing manpower crisis in biomedical sciences. Wolfle notes that the President's request calls for increases in biomedical research of 51 percent for NSF, 8 percent for NIH, and 5 percent for NASA and, further, that since 1955 the R&D budget has grown, on the average, 20 percent per year. R&D programs, by and large, consume manpower. Support for training the people necessary to mount R&D programs not only started late but has increased at a distressingly slow rate, according to the findings of every responsible survey of manpower needs made in the last decade. Senate Report No. 1460 (on Departments of Labor and HEW Appropriations Bill, 1965) states:

> Testimony made it abundantly clear to the committee that research manpower is a broad area of program need for which adequate provision is not now being made. A greater effort to increase the pool of research manpower is . . . probably the most urgent program need now faced in health research.

Under the President's request, NIH training programs—presently far too modest, with the possible exception of those of the Institute of Mental Health—can be expanded little or not at all; most of the 8-percent increment requested by the President will be dissipated by increased costs. Implementation of the ambitious programs proposed in the so-called DeBakey report ["A National Program to Conquer Heart Disease, Cancer and Stroke," *Report of the President's Commission on Heart Disease, Cancer and Stroke* (Government Printing Office, Washington, 1964)] will put significant additional strains on the manpower pool. If adequate support for training additional manpower is not forthcoming *now* (remember it takes from 7 to 10 years to produce a competently educated researcher), expanding federal research and service programs will contribute substantially to the impairment, if not the ruination, of many institutions desperately trying to obtain, retain, and train biomedical scientists. For this they need training grants. It would appear from the Senate report cited above that Congress is more perceptive than the administration; its recommendation that "the Department [of HEW] take a more realistic view of its obligations to provide an ample supply of trained manpower for research, education, and service" is to be commended. Training is less spectacular than R&D, but it is a conservation activity essential to the protection of the resources which alone offer hope of conquering heart disease, cancer, and stroke.

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Research and Purpose

I doubt if anyone has given more constructive thought to the role of basic research in this country than has Alan Waterman, nor with more results. Yet his introduction of the term "free basic research" into the discussion ("The changing environment of science," 1 Jan., p. 13) could do a disservice to science and scientists. There are three points I want to make.

1) The problem arises from the oft-stated desire of university scientists for "free" funds to do research with. This is nonsense. Money is something that is given in exchange for goods or services. There are no "free" funds legally available to scientists or to anyone else. It is not logical, nor will it long remain economically useful, to urge the allocation of federal appropriations to scientists without the requirement of accountability.

2) What makes research basic is not the objective or lack of one. As Allen Astin has stated it, basic research is "characterized only by the intensity or depth of the inquiry" (in *Symposium on Basic Research*, D. Wolfle, Ed., AAAS, 1959, p. 144). Research upon the structure of matter, the cure of cancer, the mechanism of photosynthesis, the development of lubricants, can be basic or not to the degree that it is done well, that new concepts are developed and their correctness established, and that new avenues of study are opened up. The idea that quality of endeavor is the important and distinguishing characteristic is not limited to science. It is expressed in everyday terms in an old song: "It ain't what you do, it's the way that you do it, that's what gets results" (Oliver and Young, copyright 1939, Leeds Music, New York).

3) Is it important to have an understood and clearly stated objective for a research endeavor? I believe it is. Waterman cites Archimedes, Galileo, Newton, Jenner, and Pasteur, noting that each worked upon problems of technological importance. He could have come much further into the present. It would be no compliment to Calvin to say that his work on photosynthesis did not have a clearly defined objective, or to Woodward, or to Ziegler, or to the Nobel prize winners of the coming years. It has been my experience that all research workers of great ability have clearly defined objectives, and that they will expound them at the drop of a hat. It is the ineffective research worker who often has no well-defined objective and who speaks obscurely of obtaining basic information, of "contributing to knowledge."

Some time ago, I suggested a statement that summarizes the points I am trying to make: "It is no handicap to good research to have a purpose in mind."

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With respect to Paul Klopsteg's editorial, "Justifying basic research," and Alan Waterman's related article, "The changing environment of science" (both in the 1 January issue), let me, as a social-scientist observer of science, allude to the difficulties that representatives of science seem to have in reaching agreement on how to justify basic research.

In the 1963 hearings of the House Select Committee on Government Research, Leland Haworth warned that in arguing basic research as the foundation on which all technology rests (a point often made in justification) "it is necessary to understand that it is usually in the broad sense that this is so. . . . Scientists . . . have fallen into the trap of trying to illustrate the ultimate utilitarian value of basic research by giving examples where a single fundamental experiment has had an important practical impact." And one often hears scientists complain about congressional lack of understanding when a legislator asks what "practical results" are to be expected from a projected expenditure on basic research. Yet scientists persist in inviting such an expectation.

Witness, for example, Paul M. Gross's testimony before the House Subcommittee on Science, Research, and Development, given within a month of Haworth's strictures before the other committee: "Let me cite," said Gross, "a single concrete example as evidence of the value of basic research . . . [a] paper published in the *Journal of Economic Entomology* in 1951 [on] 'Experiments with screw-worm flies sterilized by x-rays.'" He then went on to describe how the experiments led to the eradication of screw-worm flies in Florida, where these insects had been causing the death of millions of dollars worth of cattle annually [see *Science* **142**, 647 (1963)]. "The annual savings to the livestock industry of Florida alone would pay many times over not only for this but for much other basic research."

While it may in general be true, as Gross also said, that "basic research has been leading with increasing rapidity to applied research that has been of widespread benefit," it is nevertheless the case that, once a scientist goes on record as agreeing to justify work on sterilized screw-worm flies in terms of its economic utility, he is inviting congressmen to expect similar justifications for such items as those ridiculed in an earlier 1963 hearing: the revision of the classification of earthworms, the systematics of heliconine butterflies, and a study of resistance to persuasion —which a legislator said he thought was a question settled by Adam, Eve, and the apple.

There is apparently a thin line between asking for basic-research funds because scientists are "curious about nature" and basing requests upon specific utilities, but it is a line that needs further exploration and explanation. I wonder if adequate studies are being done on the relation of basic applied science to technology, so that it might be possible to make some meaningful statements about the *general* relationship, including, perhaps, the average length of time between a basic discovery and its technological application, and whether this time is in fact declining. If this is possible, strained claims for basic research might be avoided, as well as further repetition of that hybrid-corn story to which the congressman (in Klopsteg's editorial) objected.

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. . . As a culture, we have prided ourselves on our "practical nature" and on Yankee inventiveness. These ideas are pleasant to contemplate and are seldom questioned. Historians of American science have not, however, been able to establish any unusual capacity for inventiveness or practicality in the American record. They have more readily established the origins of the cultural commitment to our contemporary and special concept of "utility."

It is generally agreed that this concept is a heritage from the upright and demanding religious views of the New England forefathers, who left us with the Puritan ethic of useful work. However, the operational significance of the early Puritan concept of utility differs greatly from that of the concept widely held in this country today. Utility as early Americans viewed it was an integral part of the Puritan religion—blended with their theology and the science they used to support it. The Puritans saw nature and the cosmos as the unchanging product of the original creation. All nature had been designed by the Creator and was operated with providential utility to benefit man. Man himself was part of this orderly scheme and had a moral responsibility to acquire new knowledge of nature and to seek to understand the divine utility of natural phenomena as part of his daily life. Through such knowledge he could better know the Creator. Thus the Puritan concept of utility was part of an open-ended, ever-expanding system which gave highest honor to pursuit of new knowledge.

Charles Morton wrote in the *Compendium Physicae*, "'Tis natural theology, that men should be industrious in natural philosophy." Beauty and utility in nature were as one. As Perry Miller says in *The New England Mind*, beauty was "the perfection and congruence of one thing with another." Following such an integrated conception of beauty and utility, men were expected and encouraged to pursue new knowledge and to explore natural phenomena. Their zeal in this is at least comparable to that associated with basic research today.

The "New England mind" with such a philosophical bent was not concerned with the "practical" as we know it, but the Puritan search for specific utility has remained with us as a habit of mind, although now far removed from the original theological context. Puritan utility had greater significance for man's soul than for his body. Our contemporary social interpretation of utility reverses the order of emphasis. We must recognize, nevertheless, that many men who base their decisions on this limiting concept of utility, which restricts itself to what can be measurably directed toward economic service or gain, do so out of moral conviction. We must help them to comprehend that contemporary investment and support for basic research, the pursuit of new knowledge in an expanding system, is a valid and necessary enterprise; that basic research has proved most productive when not restricted to a narrow mission; and that its pursuit today is fundamental to economic and social survival even though we of this moment can only speculate about what may have utility in tomorrow's world.

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Productivity Measure Disputed

Fleming's figures on the number of American papers per billion R&D dollars (Letters, 25 Dec. 1964, p. 1636) are undoubtedly weighted by D dollars that build hardware, not papers. The experience of the Air Force Office of Scientific Research with $140 million spent for the support of truly basic research during the period 1959 through 1963 shows an average cost of $18,600 for the 8000 books, journal articles, symposium proceedings, and technical reports that resulted. This cost seems to be in accordance with similar figures quoted elsewhere. By Fleming's figures, we should have produced only 32!

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