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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