tested all reacted readily to retine. In Johnsson's early experiments they reacted even more strongly than the L929 fibroblasts. So if retine is involved in oncogenesis, it must be the ability to produce retine, rather than the ability to react with it, which is lost. Its excessive enzymic destruction by glyoxalase may also be considered. Since retine acts equally by mouth and by injection, and does so in very small quantities, and seems to be, in therapeutic doses, devoid of untoward side effects, some of these possibilities can be tested on patients without causing inconvenience, once the substance is available.

Closer study of the *R/P* quotient and of the absolute concentrations in various age groups, animal species, and various organs which have a different cancer incidence may help to clear problems of oncogenesis, but will have to wait for the development of a reliable micromethod for the estimation of these regulators. With the knowledge of the active group and the relations to the SH groups, such a method may be actually in sight.

The mechanism of action of retine and promine is unknown. It may be connected with the SH system, or nucleic acids, or something else. But in spite of all these incertitudes it seems likely that better knowledge of these substances will open a new alley for an attack on cancer and some of the fundamental problems of cellular biology.

References and Notes

1. M. Abercrombie and J. E. M. Heaysman, *Exptl. Cell Res.* **6**, 293 (1954); ———, *Nature* **174**, 697 (1954); M. Abercrombie and H. H. Karthauser, *Exptl. Cell Res.* **13**, 276 (1957); M. Abercrombie and E. J. Ambrose, *ibid.* **15**, 332 (1958); ———, *Cancer Res.* **22**, 525 (1962).
2. A. Szent-Györgyi, *Bioenergetics* (Academic Press, New York, 1957), pp. 120–137; *Introduction to a Submolecular Biology* (Academic Press, New York, 1960), p. 125.
3. A. Hegyeli, J. A. McLaughlin, A. Szent-Györgyi, *Proc. Nat. Acad. Sci. U.S.* **49**, 230 (1963); ———, *Science* **142**, 1571 (1963); A. Szent-Györgyi, A. Hegyeli, J. A. McLaughlin, *Proc. Nat. Acad. Sci. U.S.* **49**, 878 (1963); ———, *Science* **140**, 1391 (1963).
4. M. R. Schmeer, *Science* **144**, 413 (1964).
5. M. S. Parshley, *Cancer Res.* **25**, 387 (1965); *Excerpta Med.* **15**, 662 (1961); *ibid.* **16**, 814 (1962).
6. C. T. Bahner, L. R. Barclay, G. Biggerstaff, D. E. Bilancio, G. W. Blac, M. Close, M. M. Isenberg, E. Pace, *J. Am. Chem. Soc.* **75**, 4838 (1953).
7. A. Furst, B. L. Freelander, F. A. French, H. Gross, D. Demsher, *Proc. Am. Assoc. Cancer Res.* **2**, 204 (1957); F. A. French and B. L. Freelander, *Cancer Res.* **18**, 172, 1290 (1958); B. L. Freelander and F. A. French, *ibid.*, pp. 360, 1286.
8. K. Lohman, *Biochem. Z.* **254**, 332 (1932).
9. E. Racker, *Biochim. Biophys. Acta* **9**, 577 (1952).
10. E. M. Crook and K. Law, *Biochem. J.* **52**, 492 (1952).
11. M. Jowett and J. H. Quastel, *ibid.* **27**, 486 (1932).
12. F. G. Hopkins and E. J. Morgan, *ibid.* **42**, 23 (1948).
13. D. R. Coman, *Cancer Res.* **4**, 625 (1944); *ibid.* **20**, 1202 (1960).
14. The underlying research was sponsored by the National Institutes of Health (Grant GM-10383).

Some Current Problems of Government Science Policy

What should be the balance between expenditures on pure and on applied science, and who should set it?

Harold Orlans

In the fall of 1963, much concern was evident in the scientific community about the course that several congressional committees would take in their inquiries into federal research and development programs; and the concern of interested parties is always evident at the time of the President's budget message and subsequent appropriations hearings in Congress. Now that the Select Committee on Government Research has completed its work and the House Subcommittee on Science, Research, and Development has finished its round of hearings and reports, I believe it would be generally conceded that the committee members and their staffs did an excellent and constructive job. Both committees—particularly the select committee chaired by Representative Carl Elliott of Alabama—broke new ground. It is not necessary to agree with every one of their recommendations to acknowledge that, under severe time pressure, they asked trenchant questions and gathered and published fresh and insightful information about the nation's gargantuan research-and-development enterprise. However, the fact that this special congressional effort was required to bring to light current and comprehensive statistics on such matters as the geographical distribution of federal R&D funds and the amount received by leading universities and companies suggests that the executive agencies responsible for informing the public about these expenditures had not been doing their job adequately. Let us hope that in the future these agencies maintain the standards of fuller and more timely reporting which have now been set with the assistance of Congress; for we can hardly expect to have either good current policies or adequate consideration of desirable new policies without comprehensive, timely, *and public* information about existing R&D programs.

The author is a member of the senior staff of the Brookings Institution, Washington, D.C. This article is the text of a talk given 26 April 1965 at the Tenth Institute on Research Administration of American University, Washington, D.C.

As the rate of increase of federal R&D expenditures has been declining and as the volume of expenditures in major agencies like the Department of Defense, the National Aeronautics and Space Administration, and the Atomic Energy Commission has leveled off or declined, a major issue of public policy—and of public and private conflict within many agencies and their constituencies—has been posed: how much of the pie should go to basic research? Or, to put the matter another way, how much should go for research at universities, and how much for research and development in industry?

The Doctrine of the Sparrow

The answer of academic scientists is not entirely surprising: more should go to them. With a monotony that bespeaks a unison more than an originality of thought, they and their spokesmen in Washington argue that there is no danger of spending too much on basic research; that all "competent" university scientists should be supported to do work of their own choice; that science is an indivisible whole and all fields merit support equally (although some fields merit support more equally than others); that, while the results of any particular basic research project are unpredictable, it is not merely probable but virtually certain that the results of *all* basic research will yield a value greater than their cost. Some of the more ardent advocates of pure science even assert that the results of *any*—or *almost* any—pure research will *certainly* be rewarding, scientifically and socially, conceding only that one cannot predict precisely where or when the reward will be found or who will receive it. This may be termed a contemporary scientific version of the doctrine of the sparrow or the falling leaf—that no harm, no matter how slight, can befall a living thing without serving a higher moral purpose. As purposelessness and futility are thus vanquished in theology, if not in life, so, in the current eschatology of research, error, triviality, and important findings whose importance is unrecognized all equally serve the higher purposes of science. Thus, Alan Waterman, former director of the National Science Foundation, has declared that "The results of such [basic] research, in competent hands, are never without value. Even when no breakthroughs appear, the total effort always brings a possible breakthrough closer"; and he has spoken of "the statistical evidence [which was not, however, further identified] that most of the body of science ultimately achieves practical utility" (*1*). The fascinating justification of heavy federal expenditures on high-energy physics recently advanced by 30 distinguished physicists also dances delicately along the line of statistical likelihood—variously appraised as certain, probable, unlikely, and "not impossible" (*2*)—that these expenditures will yield a significant practical return. I do not doubt that they will yield *some* practical return: this one expects from the work of oafs, let alone that of brilliant men. The critical question—and I wish only to submit it, not to answer it—is: Will it yield a return commensurate with its cost, or greater than the return that can be anticipated from a comparable investment in other fields of science and technology (not to mention other areas of human endeavor)?

An Inconsistency

A striking inconsistency is apparent in the logic of many analyses of federal R&D policies. At times, the relation between the amount of money the federal government spends for basic research and the amount it spends for development is stressed, as when it is said that basic research expenditures are "only" x percent of the total R&D expenditures, whereas development expenditures are nx percent; therefore, it is argued, if economies are needed, the larger rather than the smaller amount should be cut, or carefully "scrutinized" (why not scrutinize both?). However, at other times, it is stressed that basic research should not be compared with development. Thus, it is, of late, increasingly contended that the basic-research expenditures of an agency should not compete with its expenditures on the development of new technology (which should compete instead with expenditures for the procurement and maintenance of existing technology, and other operating needs).

What, then, if anything, *should* basic research expenditures compete with? The answers to this question are frustratingly vague: indeed, no really satisfactory answer has yet been given, although there has been no lack of adventitious suggestions, ranging from expenditures on gambling or tobacco to some arbitrary percentage of the gross national product—all of which are proposed on the condition that they allow adequate scope for expansion. However, one significant suggestion was offered recently by the President's science adviser, Donald Hornig, in a letter to Senator Pastore, chairman of the Joint Committee on Atomic Energy, in which he stated that "the level and character of support for high energy physics must be determined and periodically reassessed in the context of . . . the overall national science program (rather than in relation to the applied research and development programs of the AEC) . . ." (*3*).

To my mind, there is still a good deal of usefulness in comparing expenditures on basic research with those on applied research and development, if only because these sums draw to one type of activity or the other men of comparable training—and I mean by this not only Ph.D.'s and Nobel prizewinners but the more numerous serfs of scientific and technical fiefdoms with mere master's and bachelor's degrees, and their auxiliary corps of glassblowers, machinists, secretaries, accountants, and groundkeepers. Although it was undoubtedly justifiable, immediately after the war, to complain that government expenditures on basic research were but a small percentage—evidently no more, and probably less, than 6 percent—of the $1 billion then spent on R&D, the 11 percent devoted to basic research in 1964 and the more than 14 percent proposed for 1966 out of some $14.6 billion (excluding capital plant and facilities) seem, on the face of it, not utterly, irredeemably, and tragically inadequate. Indeed, if the proportion of government funds going into university research were to be slightly reduced while that going into various forms of more direct aid to higher education were to be correspondingly increased, diverting a number of professors from laboratories to lecture rooms for another hour or two a week, the average quality of both education and research might well be enhanced.

If there is a portion of the R&D spectrum where national expenditures now appear patently inadequate to meet national and international needs, surely it is no longer the realms of pure science but those areas of obsolescent or inefficient civilian technology, at home and abroad, in which the prospect of private profit has been too dim to elicit enough private capital to ensure technical progress, while public expenditures have been blocked by the difficulty of devising a political formula acceptable to the major parties concerned. When the $7-million program of assistance for civilian technology proposed by the Department of Commerce a couple of years ago was rejected by Congress, it was noted in Sweden that their government was spending more—absolutely, not relatively—than the United States government on such programs. Surely, no R&D task merits greater priority today than the search for politically viable ways of utilizing engineers and scientists no longer required for military work to render industries like housing, transportation, textiles, and coal more efficient; to reduce the pollution of air, water, and soil; to improve our systems of education, medical care, and local government; and to raise the standards of living in impoverished areas of this and other nations. It is strange how much money and ingenuity are devoted to searching for indirect, accidental, and even surreptitious benefits of academic, military, and space research and development, and how little to R&D programs of direct and evident social and economic utility. Have we become so muscle-bound politically that, like Primo Carnera, we can display our strength but not use it where it is obviously needed?

A National Budget for Basic Research?

To return to Hornig's very interesting statement that the level of support for high-energy physics "must be determined . . . in the context of . . . *the* overall national science program . . ."—and the definite article which I have italicized is not the least interesting part of this statement, since it alludes to something which simply does not exist. What is advocated here for one field of science must, in principle, be applied to any and every other clearly recognized field. It appears, in short, that the president's science adviser is advocating the preparation of a national or at least a federal budget for all fields of basic scientific research. A number of other signs point in the same direction: the greater separation of government-wide expenditures on development and on research in Special Analysis H of the 1966 federal budget; the energetic and not entirely noncompetitive efforts by committees of the National Academy of Sciences to define and project desirable budgetary levels for various fields of science; and particularly the attempt by an *ad hoc* committee appointed by Academy President Frederick Seitz to grapple with two difficult but inescapable questions about scientific allocations posed by Representative Daddario's Subcommittee on Science, Research, and Development (*4*):

> 1. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?
>
> 2. What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?

Although some may feel that the Academy committee has dodged its assignment, rather than confronted it squarely, by answering these questions in the form of 15 separate essays written by individual committee members, it is nonetheless gratifying to see the questions being seriously faced at last, and the resultant document (*5*) is a significant contribution to the thin but growing literature on the aggravating problem of scientific choice.

The new efforts of the National Science Foundation to examine the same problem of scientific allocations should also be noted; these were reported recently by Foundation Director Leland Haworth (*6*):

> The Foundation . . . plans to give additional emphasis to the compilation and analysis of data which bear specifically on the question of relative total levels of support and measures of apparent needs. . . . Thus, we hope eventually to be able to cite fairly precise figures relative to the average amount of total research support available to academic scientists, by field of science, and to augment such data with judgments from competent people in the various fields on the question of reasonable ranges of support levels for each discipline. . . . The problem of making interfield priority judgments should become more manageable if somewhat more complete information on a field-by-field basis can be made available.

The establishment of such a central data bank on federal research grants and contracts is to be commended and should materially assist the rational allocation of scientific expenditures by both public and private agencies—provided that the raw data are not husbanded and used by one camp or another as a weapon in its struggle for a share of limited funds, but are made freely available to all to enlarge our knowledge of national allocations to—and returns from—various fields of science. Too often in the past certain data relevant to public policies have been regarded as proprietary and released only in politically convenient tabulations. Let us, again, hope that the agencies responsible for formulating federal policies for science will adopt the same principle of the fullest possible disclosure of data that is universally accepted with regard to the data of science itself.

No one observing the Washington scene can, however, be so deluded as to believe that key decisions always are or can be made in public and based solely on considerations known to the public. The inner councils of government are always, to some extent, obscure; the passage of time gradually enlarges the public record of private deliberations while reducing both its authenticity and its relevance to future decisions; and available records of the process of decision in major scientific programs are sparse indeed. Except for such information and insight as can be gleaned from congressional hearings, evidence is not generally taken in public; deliberations proceed behind the necessary or convenient cloak of executive or legislative privilege, and the final pronouncement commonly resembles a brief for one side more than a dispassionate examination of available alternatives.

The Composition of the Jury

In this situation where verdicts are reached in private, the composition of the jury assumes a special importance: it provides, in fact, the principal visible assurance that justice is being done. The composition and method of selection of important scientific policy groups therefore merits continuing public scrutiny and discussion. Social scientists have managed to secure representation on an enlarged section of the National Research Council and an occasional appointment to the National Science Board, but none has yet been selected for the President's Science Advisory Committee. Engineers have been so dissatisfied with their status in the National Academy of Sciences that they have formed an academy of their own. The composition of the President's Science Advisory Committee was perhaps adequate to its earlier responsibilities of advising upon the value of proposed weapons systems. However, as the committee's responsibilities have broadened to the formulation of general government policies for science and technology, and as the machinery for implementing its advice has been strengthened, the committee's credentials for performing these larger tasks should be periodically reassessed. The geographic concentration of its members has fortunately been broadened by the latest round of appointments, but the addition of a few more members from industry and a few from selected fields of social science would strengthen the committee's competence to deal with some of the problems which it now faces.

Finally, a few words about what is sometimes regarded as the missing link in the establishment of national policies for science and technology: Congress. Congress has been quicker to see, and to act upon, deficiencies in the executive's formulation and coordination of R&D policies than to remedy its own deficiencies. There is a clear need for improved mechanisms within Congress, comparable to those which have been developed in recent years within the executive, for handling the flow of scientific programs and budgets on a basis that is broadly consistent and campatible with the national interest. The appointment of a new unit in the Legislative Reference Service of the Library of Congress to provide information on scientific and technological programs and policies, the continuing work of the Daddario subcommittee, and the establishment of the new permanent Subcommittee on Research and Technical Programs of the House Committee on Government Operations, under the chairmanship of Representative Henry Reuss of Wisconsin, indicate a recognition of the problem. Is it too sanguine to foresee further Congressional steps to define national rather than sectional goals for science and technology and to enlarge the authority of Congress as a whole in the making of science policies?

Summary

The problems of government science policy I have noted are not exactly new, but each has, I believe, acquired a new degree of urgency from the pressure of events: How much should be spent on basic research and how much on civilian technology? How can reasonable allocations be made among various fields of science? Who is to make these allocations, in the executive and in Congress? The degree to which we can, by objective research and perceptive analysis, accommodate the accidents of history and politics to the changing needs of science, industry, and society will determine the degree to which we can serve not the interests of those groups and individuals (both scientists and politicians) who happen to be in positions of power, but the present needs of the nation.

References and Notes

1. A. Waterman, Director's Statement, in *Nat. Sci. Found. Ann. Rept.* **8**, xii (1958); *Science* **147**, 16 (1965). In contrast, John Jewkes argues that "the overwhelming mass of scientific thought and observation in the Western world was never designed to bring, has not brought, and is highly unlikely ever to bring the slightest improvement in material standards of living" [*Economic Journal* **70**, 14 (Mar. 1960)].
2. See R. Sachs, H. Primakoff, H. A. Bethe, and J. R. Oppenheimer, all in *The Nature of Matter: Purposes of High Energy Physics*, L. C. L. Yuan, Ed. (Brookhaven National Laboratory, Upton, N.Y., Jan. 1965); see a → *Science* **147**, 1548 (1965).
3. From letter, dated 27 March 1964, from Donald Hornig to Senator John Pastore, in *High Energy Physics Program* (Joint Committee on Atomic Energy, Feb. 1965), p. 57.
4. *Basic Research and National Goals* (Committee on Science and Astronautics, House of Representatives, Mar. 1965), p. 1.
5. *Basic Research and National Goals*, report of the Committee on Science and Public Policy, National Academy of Sciences (U.S. Government Printing Office, Washington, D.C., April 1965).
6. L. Haworth, "Director's statement," in *Nat. Sci. Found. Ann. Rept.* **14**, xxv (1964).

News and Comment

Defector's Odyssey: Personal Look at Soviet-Bloc Science Provided by High-Ranking German Physicist

The Senate Internal Security Subcommittee last week issued one of the more bizarre prose productions of the Cold War, a 94-page document entitled "Nuclear Scientist Defects to United States."*

The work opens with a foreword in which Senator James O. Eastland (D–Miss.) says he feels the testimony therein "will be of considerable interest to Members of the Senate and to the scientific community and all thoughtful Americans." It closes with an index headed by the generous notation that the "subcommittee attaches no significance to the mere fact of the appearance of the name of an individual or an organization in this index." And between these two statements lies approximately 4 hours and 30 minutes of a closed-session colloquy between Committee Counsel J. G. Sourwine and Heinz Barwich, an East German who was director of the Institute for Nuclear Research, in Rossendorf, East Germany, and former deputy director of the Institute for Nuclear Research, at Dubna, near Moscow.

When Barwich defected last September, while attending the U.N. Atoms for Peace conference in Geneva, the Associated Press reported that "Western sources considered him the greatest prize in two decades of nuclear intrigue." And the implication was conveyed that to top the Barwich coup, the Russians would have to pick up at least two AEC Commissioners and a member of the Joint Chiefs of Staff. The spooky past of East-West intrigue, nuclear and otherwise, impels the outsider to caution in judging security matters that governments select for public display. But on the basis of the published Barwich-Sourwine dialogue, and other information, it would appear that if Barwich is the "greatest prize in two decades of nuclear intrigue," the cost-effectiveness ratio of this intrigue is appalling. Barwich himself, though occupying a highly important place in the early stages of the Soviet weapon program, states flatly that his association with secret research ended in 1952, and that thereafter he was engaged in work that was aboveboard and generally known to the West.

One measure of the Soviet evaluation of his knowledge may be seen in the fact that, though Barwich revealed his political doubts by openly opposing a Soviet position at the 1960 Pugwash meeting, he was still permitted to travel outside the Soviet bloc. The Russians, who reportedly keep many of their top military and space researchers out of sight of the West, apparently didn't see much hazard in letting him move about. One reason may have been that his present wife and two children were residing in East Germany, but they, too, managed to make it to the West, although, according to Barwich, two children by an earlier marriage were caught and imprisoned. In any case, the Barwich tale, taken at face value, seems to have little if any military significance, but it does offer a sad chronicle of a talented and obviously ambitious scientist seeking to make his way in an at-

* Available for 30 cents, U.S. Government Printing Office, Washington, D.C. 20402.