system by creating new means of publicizing and discovering information and by allowing other means to atrophy through lack of use.

Such stability as there is in the process of dissemination seems to be related to the long lag in journal publication and to the common assumption among psychologists that journal publication is the normal outlet for research findings. These factors probably sustain the present general form of the process. The system of informal dissemination, the amount of effort devoted to obtaining information prior to journal publication, and the size of the audience that seeks immediate access to the findings when they finally appear in journal form would seem almost certain to change if the publication lag were greatly shortened or if journal publication were less widely sought.

Notes

1. Extended treatments of the results of this research appear as volume 1, Reports of the American Psychological Association's Project on Scientific Information Exchange in Psychology (December 1963). A limited number of copies are available from the American Psychological Association, 1200 17th Street, NW, Washington, D.C.
2. The time intervals are actually medians of data reported in one or more of the Project's studies. There are often large deviations about these medians and, as can be seen in the original reports, certain of these deviations are associated with interesting phenomena.
3. The work reported here was supported by grants G-18494 and its continuation GN-281, which were made to the American Psychological Association as part of the program of the Office of Science Information Service of the National Science Foundation. The research was planned and executed by the staff of the Project on Scientific Information Exchange in Psychology which includes (in addition to the authors) Bertita E. Compton, Madelyn J. Miller, Margit Siegmann, and Kazuo Tomita. The Project's advisory panel reviewed research plans and findings. Its members include Raymond A. Bauer, Dorwin Cartwright, Kenneth E. Clark, John G. Darley, Quinn McNemar, Donald W. Taylor, and Arthur H. Brayfield (*ex officio*).

News and Comment

Oceanography: Cost-Effectiveness Technique Employed To Support Case for Basic Research Program

A few years ago, when congressmen first began to ask hard questions about the justification for federal support of basic research, the scientist in the witness chair would generally offer little more than poetic reverie about the virtues of the quest for knowledge. When it was plain that this didn't fully satisfy the questioners, the answers began to take on a more practical tinge: basic research in health, agriculture, and other fields, it was pointed out, paid off in visible economic returns. This was understandably more digestible, and, as a result, the leaders of the various scientific disciplines began thinking hard about the justifications they could offer for sizable public support of their work.

The most advanced product of such thinking has now come forth in the form of a report by the Committee on Oceanography of the National Academy of Sciences–National Research Council. Titled "Economic Benefits from Oceanographic Research,"* it is a compelling document that might be subtitled, "Two Will Get You Seven."

Quite convincingly, it makes the case that investment in oceanography will pay off handsomely, and, since the oceans conceal such interesting things as fish, minerals, and Russian submarines, it does this in a fashion that could not be even remotely approached by its predecessors in this genre: the Report of the Panel on High Energy Physics (sponsored last year by the Atomic Energy Commission and the White House Office of Science and Technology), and the Academy's report, last month, on a 10-year program in ground-based astronomy. But perhaps the most significant aspect of the new report is not the solid case that it makes for investing in oceanography; rather, its principal significance may be that it represents the first attempt at a comprehensive application of the cost-effectiveness technique to a field of basic research.

The committee that wrote the report did not simply state the obvious—that oceanographic research can be expected to produce an attractive economic return; rather, while repeatedly emphasizing the uncertainties inherent in its projections, it sought to calculate the financial returns that might reasonably be expected from such research. And it came to the happy conclusion that annual nondefense expenditures of $165 million over the next 10 to 15 years (the current figure is $138 million and an annual growth of 10 percent seems to have found Congress' favor) could be an "essential component" in saving $3 billion a year, principally through conservation practices, and in adding annual production of about another $3 billion.

To arrive at this conclusion, the committee not only took up the obvious matter of fish (estimating that $50 million in marine research and development could double the $1 billion that fishery industries products now add to the gross national product); and the obvious matter of minerals (estimating, for example, that $50 million worth of research in that field could lead to large-scale mining of marine manganese worth $125 million a year); it went even farther afield and estimated that oceanography's contributions to weather forecasting could produce substantial savings for cattle and hog producers.

States the report: "The farm value of cattle and hog production in 1962 was $9 billion. Weather-produced variations in the size of the crops of corn, oats, and hay have serious economic effects for livestock producers, as do changes from year to year in the productivity of permanent pastures and range lands, caused by variations in seasonal rainfall. Significant savings would be obtained if the farmers could plan how to feed and dispose of their stock on the basis of reliable long-range weather forecasts. A five-percent saving would amount to $450 million."

Similarly, it pointed out that in 1962 "the value of potato production was roughly $500 million and of fruits including grapes perhaps $2 billion. . . . A five-percent gain through better planning and production" might be anticipated from better weather forecasts.

And it even went so far as to attempt extremely cautious estimates of the economic value of contributions that oceanographic research might make to cleaning up and conserving coastal waters for such purposes as

* Publication 1228, 50 pages, $2, Printing and Publishing Office, National Academy of Sciences–National Research Council, Washington, D.C. 20418.

bathing, scuba diving, and sailing. "It is difficult," it pointed out, "to place a monetary value on the availability to man of sufficient and adequate recreational facilities," but "available fragmentary information" suggests annual expenditures of $2 billion for recreational use of the sea, with an annual growth rate of at least 5 percent. If $10 million a year were invested in developing and preserving the nearshore area for holiday use, it went on, "it would not be unreasonable to consider that at least 10 percent of the average annual increase in the gross value of marine recreation will result from such research."

Some of these attempts to place a price tag on the fruits of basic research are, first of all, likely to strike some people as preposterous, and, secondly, to produce a mixed reaction in a public that has been nurtured on the line that basic research is so intrinsically valuable that it needs no justification. Commenting editorially on the Academy report, the Washington *Post* sourly suggested that "the oceanographers are on the wrong track"; that "anarchy would be the result" if cost-effectiveness were to become the yardstick for federal support of basic research. It warned that if cost-effectiveness were to prevail, the oceanographers "may find themselves badly outflanked" by disciplines that could promise a better payoff, and it went on to ask, "What about pure research where there is no immediate bang for a buck? No one challenges its value or necessity, yet where would this research stand on a cost/effectiveness chart?" (The answer to the *Post*'s question might be that the high-energy physicists would be driven to such speculations as these: Probing into the heart of the nucleus may lead to the development of antigravity techniques, with attendant savings of an estimated $6.8 million lost annually in broken dinnerware. Similarly, the ground-based astronomers could point out that their spectacular-looking research tools could become tourist attractions; this, as a matter of fact, was what the Small Business Administration had in mind when it authorized a loan for a motel in the vicinity of the 600-foot radio astronomy dish once planned for Sugar Grove, W.Va.)

But the stuff for whimsy that the oceanographers provide should not conceal the fact that the question "What good is it?" is becoming increasingly more powerful as basic research annually asks Congress to approve larger and larger budgets. And, as the various disciplines are unwillingly placed in competition for what comes under the lump heading of federal research and development funds, it isn't only congressmen who are concerned about such rapidly growing fields as oceanography. (In fiscal 1958, oceanography received $24 million; the budget projected by the Interagency Committee on Oceanography calls for $350 million by 1972.) Last March, for example, Frederick Seitz, President of the National Academy of Sciences, said in an address to the Optical Society: "I am particularly concerned that the maintenance of the ships, yards, and docks associated with this field [oceanography] will eventually absorb an enormous amount of the money that might better be used in new fields of science." Seitz, of course, was not invoking a strictly utilitarian concept to justify support for particular lines of research, but his concern and the concern voiced by congressmen with the costs of high-energy physics both arise from the same imponderable: when funds are not available for all promising proposals, how do you work out priorities in basic research?

If the answer had to be based on a precise, theoretical formulation, the decision-makers would probably quit or be driven out by the losers. Who is to say whether molecular biology should take precedence over low-temperature physics? The answer, of course, is that in this country, at least, science's financial problems are a long way from necessitating either-or decisions. It is stimulating to ponder which you would rather have, a radio telescope or a new chemistry building, but the fact is that the federal budget is sufficiently plump and Congress is sufficiently proscience to assure that most promising proposals will be funded. Disappointed grant applicants no doubt take another view of the matter. But, together with the military and the farmers, the scientific community has vastly outdistanced other segments of American society in gaining access to the federal treasury. And, as one looks back over the past few years, the period when the rate of growth for research and development began to level off, it is perhaps noteworthy that the accelerator proposed by the Midwestern Universities Research Association was the only major research expenditure to be killed off for largely financial reasons. Tight funds have unquestionably created problems and stresses throughout the scientific community, but this is a condition that prevails in and out of science, and no one has yet been so bold as to suggest that research, of all federally supported activities, should work with blank checks.

Although the current spate of reports on the needs of various disciplines may have been mainly inspired by exaggerated fears of budgetary problems, they are undoubtedly serving many useful functions. It is difficult to see how anything but good can result from having informed representatives of each discipline look into the future and estimate needs and opportunities. The physical plant that is now required for many fields of research is so costly, and the construction lead times are so great, that a hit-or-miss approach no longer suffices. Furthermore, with financial pressures providing a backdrop, it is likely, though not inevitable, that reflections on the future will be more knowledgeable and sophisticated than they might be in a financially unrestricted atmosphere. And, finally, the reports that have been produced recently have helped educate the public to the importance and the financial needs of the research it is asked to support. Quite possibly the oceanographers went overboard by producing something that resembles a stock prospectus, but the questions of social utility and economic payoff intrude themselves when large public funds are involved; and, while it may be unbecoming to exploit the matter excessively, it is something that likewise cannot be altogether ignored.—D. S. GREENBERG

> "While total research and development funds were only about 3 percent of the gross national product in fiscal year 1963, the rapid growth of expenditures for research and development in relation to the growth of the nation's total economy cannot continue indefinitely. Thus, it is important to attempt to evaluate, in economic terms, the potential benefits to be expected from given expenditures on scientific research and development, so that they may be compared with benefits that might be expected to accrue from alternative expenditures."—From "Economic Benefits from Oceanographic Research."