26 June 1970, Volume 168, Number 3939 **SCIENCE**

Future Prospects for Physical Oceanography

Are present plans for expanded oceanographic research designed to solve basic scientific problems?

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Marine Commission Report

This is a time of much planning in the marine sciences. Within the United States alone, three major 10-year plans have been issued, and proposals have blossomed in France, Japan, and the U.S.S.R. A major report has also been issued by Unesco, and even regional interests (such as those around the Gulf of Mexico) have been quick to make 10-year plans of their own. The primary aim of all the U.S. plans is to provide an impetus for expanding research programs in the ocean sciences at a time when government funding of science is being curbed. A long summer of luxuriant growth of marine science under the Democratic Administration appears to have been followed by the first frosts under the present Administration. The distinguishing feature of these reports is their wide scope; they cover practically all civil marine activity, all branches of ocean engineering and scientific study, and all kinds of ocean industry—such as oil-well technology, transportation, deep-sea mining, and various legal and legislative questions. Because of their all-inclusiveness, it requires a bit of digging to isolate the portions of the plans that pertain solely to physical oceanography. Being tied together with all other marine activity is both a strength and a weakness for physical oceanography: it is a strength because it documents the potential social usefulness of physical oceanographic knowledge (and justifies the cost); it is a weakness because it tends to subordinate formulation of specific critical scientific problems to those that are less clearly defined but that on the surface appear to be politically attractive.

The Commission on Marine Science, Engineering, and Resources is a distinguished body chaired by Julius A. Stratton, chairman of the Ford Foundation and former president of the Massachusetts Institute of Technology. The Commission report (*1*), addressed to the President and the members of the (U.S.) Congress, is mainly concerned with justifying the creation of a new governmental administrative arm, the National Oceanic and Atmospheric Agency (NOAA). The report succeeds in giving a truly astonishing, encyclopedic, and convincing account of the importance of the oceans to our nation and the world. In the portion of the report that concerns physical oceanography (*1*, pp. 173–198), the principal goal proposed is "the development of a system for monitoring and predicting the state of the oceans and the atmosphere. . . ." It is also clearly pointed out that the key to the development of such a system lies in our "ability to deploy present technological capabilities effectively" and to develop the necessary scientific basis for such a predicting system. The Commission report does not pursue the basic scientific questions pertinent to the development of such a system but goes on to describe various types of hardware—for example, submersibles, man-in-the-sea devices, satellite applications, and buoys.

Science and Environment, volume 1 of the panel reports (*2*) that accompany the parent report, is more explicit. It recognizes that any effort to begin a system of marine "environmental prediction is science-limited." It goes on to make specific recommendations: "extensive field experiments should be conducted to describe physical processes associated with ocean fluctuations. . . ." and "the Nation should undertake a series of systematic investigations into the oceans' current systems to study their dynamics through cooperative field investigations, marshalling at one time multiple ship, buoy, and aircraft arrays, as well as an expanded effort in the theoretical and mathematical modelling of such systems."

Most physical oceanographers will agree that the chief concern of their field is to understand the actual hydrodynamical mechanisms at work and the ways in which they interact through the range of space and time scales found in the ocean. They will also agree that, until the nature of this machinery is clarified and in some sense ordered quantitatively, predictive schemes will, at best, be empirical. Moreover, most physical oceanographers will agree that present knowledge is very incomplete and that they do not know how they can attack this formidable problem with present ideas, observational techniques, or level of funding. Some candid oceanographers will even acknowledge that their own productiveness has been a result of skill in discovering tractable side problems on which they could make individual progress but that they have been skirting problems of the ocean circulation—the problems that are central and that therefore so clearly demand a level

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of organized effort of measurement and interpretation far greater than any individual scientist can muster. The habit of skirmishing with ocean dynamics, instead of doing battle, is so entrenched that it is not surprising that the response by the federal agencies and by the scientific community to the recommendations of the Commission has been, as we shall see below, rather wide of the mark.

Federal Planning Guide: MAREP

While the Commission's recommendations were being formulated, the many agencies within the federal government published a *Federal Planning Guide for Marine Environmental Prediction* (MAREP). This document "recommends an intensified program to develop major scientific possibilities that now can be clearly foreseen; relates the planned program for the future to programs now under way; and identifies new initiatives that must be undertaken to supply the new and expanded environmental prediction services required by the nation's citizens for industry, recreation, commerce and defense."

The first chapter entitled "Marine environmental prediction," describes the need for such predictions and mentions related meteorological programs, problems of radio-frequency allocation, and the benefits that will purportedly accrue from expanded federal funding. The chapter is well sprinkled with startling photos of ships being driven ashore by high winds and of resort communities being gutted by storm waves. It also contains a paragraph about science:

> Scientifically, each group has learned much about its respective sphere—the atmosphere and oceanic hydrosphere, their complex structures, interactions, and motions. This knowledge has been applied in formulation of mathematical and small scale operational models to simulate both static and dynamic features of the gaseous and liquid fluids, and, insofar as possible, their interactions at various space and time scales. Electronic computers now make it possible to formulate advanced numerical models for atmospheric and oceanic predictions. However, effective prediction with elaborate models can be achieved only when the atmosphere, the ocean, and the biota therein are considered in the same model.

Now it is undoubtedly true that elaborate numerical models of the atmosphere do exist and do successfully predict future states of the atmosphere. Although some steady-state ocean models have been constructed, their formulation is based on very shaky assumptions. As interesting as these ocean models are, they fall short of being able to predict the variability of the ocean. The implication in the paragraph above, that oceanographers have advanced numerical models that are ready to begin predicting, is very misleading. Therefore, the preoccupation of chapter 2 in the Planning Guide with "systems concepts," "data acquisition system," "best mix of platforms," "communications systems," "arrangement functions," and so forth, seems somewhat premature. One might as well have debated the proper color for the paper on which telegrams should be printed before the telegraph had been invented.

Chapter 2 does contain a disarming paragraph:

> A number of unsolved problems remain. These prevent any detailed formulation of a conceptual design or framework for the total Marine Environmental Prediction Program. Some problems require an earlier solution than others.

Indeed they do! We should first find out how the machinery of the ocean works.

Chapter 3 explains the strategy by which the Interagency Committee expects its goals to be achieved. It gives a list of 28 specific "targets." Examples are:

> Improve ability to predict temporal and spatial distribution of biological fouling and biological sound production for national defense and industry applications.
>
> Automate the storage, retrieval and display of historical environmental data.
>
> Initiate estuarine flushing forecast services in 1970, and extend service to eight estuaries by 1975.

It is reassuring to note that each of these targets is already a subject of study by some groups and projects within the federal government. According to the budget summaries (*3*, p. 136), these particular governmental projects cost $37 million in fiscal year 1969. The Planning Guide promises implementation of the 28 targets by 1975, at which time the annual budget will be trebled. The only one of the 28 "targets" that seems related to the problem of understanding the general circulation of the ocean reads:

> Accelerate research on the dynamics of the oceans as a foundation for the formulation of more complete and effective numerical prediction models.

The federal research project now in process that is mentioned as having some relation to reaching this "target" is Project E-3 (Priority Two): "Formulation of mathematical models which simulate large-scale dynamic processes of the oceans"—which currently consists of the modest efforts to construct steady-state models (mentioned before), some sort of statistical military model for the Navy, and a very modest effort of the Coast Guard to predict ice drift. These efforts are to be level-funded at $2 million per year.

By contrast, items listed under such bureaucratic headings as "program planning and coordination" and "systems design" (an example of which is "conduct aggressive coordination to standardize oceanographic data format and reporting codes") are accorded Priority One and are budgeted for approximately $5 million per year.

By far the most generous budgeting in the Planning Guide has been reserved for "new technology development and implementation," under which programs such as the Ocean Data Buoy Project (U.S. Coast Guard) and the satellite-sensing and interrogation systems will be expanded from $4 million per year (1969) to $74 million per year (1975). We do not doubt that measurement systems costing sums like these will be necessary to unravel the puzzle of the general circulation, but, from what one can gather from reading the Planning Guide, the development of these instrument systems will also require parallel support for a carefully planned program of scientific investigation of the ocean's mechanics, which will stipulate in part the properties and deployment of these systems.

Chapters 4 and 5 constitute a useful description of the various activities in which the various agencies are now engaged. It is an impressive list of services, but it is difficult to discern how much of this effort is actually scientific oceanography.

Unless some very definite provision is made to wed projects such as the National Data Buoy Project firmly to the Commission's recommendation for "a series of systematic investigations into the oceans' current systems to study their dynamics . . . ," technological equipment such as buoys may be largely tied up in routine weather forecasting and be unavailable, or inappropriately designed and located, for basic oceanic research. To illustrate, the elaborate data-disseminating systems such as the Fleet Numerical Weather Central at Monterey have made little impact on research in physical oceanography. They do not, for example, provide surface temperature data that are sufficiently dense or sufficiently accurate to be of much use in the Gulf Stream studies at Woods Hole—even though this region is one in which the Monterey printouts are dense. However important it may be for various government agencies to expand their present programs and services, it may be hoped that they will not do so without considering if their expansion increases their contribution to science.

The idea of strewing moored buoys throughout the ocean is not new. When in 1707 the three-decker H.M.S. *Association* sank off the Scilly Isles, owing to a navigational error, and the British naval hero Sir Cloudesley Shovell was found on the beach strangled by looters, the government was moved to offer a reward of £20,000 for a means of determining longitude. Lucasian professor of mathematics William Whiston and a colleague, Humphry Ditton, circulated a proposal (*4*) to moor hulks in deep water along the world's shipping lanes and to equip them with rockets as time signals. They did not neglect to suggest that they might have qualified to win the £20,000:

> Which Reward, whether we have any just Claim to, in whole or in part, we do hereby intirely submit to the Sagacity and Justice of those eminent Persons whom the Legislature has been pleas'd to intrust with the Tryal, Experiment, Judgement, and Determination of all such Proposals. We conclude all with our hearty wishes as *Men*, that this our design may tend to the common Benefit of Mankind; as *Britains*, that it may tend particularly to the Honour and Advantage of this our Native Country; and as *Christians*, that it may tend to the Propagation of our Holy Religion, in its original Purity, throughout the World.

The reward, however, went to a humble bicycle-maker, who 60 years later made a better marine chronometer, and all that remains in memory of the scheme to strew the ocean with buoys is an indecent poem ("On the Longitude") by Jonathan Swift.

Academies' Report

The third major U.S. report on the future of marine sciences that contains material of interest to physical oceanographers is *An Oceanic Quest—The International Decade of Ocean Exploration* (*5*), which was composed by a joint committee of the National Academy of Sciences and the National Academy of Engineering, chaired by Warren Wooster and William Shoupp. This report was requested by the National Council on Marine Resources and Engineering Development after President Johnson proposed the decade in a speech in March 1968, and it was issued in mid-May 1969. In the meantime the idea of U.S. commitment to an international decade seemed to have cooled somewhat, but early this year an Office for the International Decade was set up in NSF with a budget of $15 million. Whether "An Oceanic Quest" will join the snows of yesteryear remains to be seen, but it does present a coherent and specific program for expanded scientific work. Moreover, it is not so heavily weighted toward support of current programs as is the Planning Guide, and it represents the views of a fair section of the scientific community, for it was put together from preliminary drafts by many hands. It covers all aspects of the marine sciences but very little engineering, and, of course, it contains its share of geopolitical peroration and social justifications.

The portion concerned with physical oceanography is to be found in the chapter entitled "Two fluids: physical and environmental prediction" (*5*, pp. 61–84). It reflects certain pressures brought upon the committee to emphasize the socially useful, and for that reason words like "prediction" are used where "understanding" might be more appropriate. Nevertheless, it states quite clearly that oceanographers, unlike meteorologists, are not ready to formulate a global monitoring or prediction system and that this long-term goal, although it may be worthwhile, must be deferred until preliminary studies have been made. Certainly, oceanographers must follow up every opportunity that exists under the planned global meteorological observation programs [World Weather Watch (*6*) and Global Atmospheric Research Program (*7*)] to obtain information about the ocean. Where oceanic weather buoys are installed, where merchant ships are equipped, where oceanic islands are employed for global meteorological programs, useful oceanic information can also be obtained, even if only in shallow surface layers. Much can be learned by collaboration with meteorologists in their programs. For example, J. Bjerknes has discovered remarkable long-period changes in the properties of surface waters over large regions of the equatorial Pacific; these changes may have important bearing on long-term weather prediction, and they unquestionably call for a more extensive net of sea-surface and upper-air observing stations in the central Pacific.

Oceanographers can certainly benefit from a modest increase in the number of points in the ocean where weather ships or research vessels take regular time series of hydrographic stations, such as are already under way at station PAPA in the Gulf of Alaska and PANULIRUS off Bermuda. Oceanographers can even urge support for simple coast surveys and instrumentation (such as tide gauges) for underdeveloped countries without endangering their souls. But the deployment of points of measurement appropriate for study of the atmosphere is unlikely to be suitable for fundamental oceanic studies, and oceanographers will need to design their own field experiments.

"An Oceanic Quest" recommends that initial attempts to measure the deep ocean synoptically and to model it numerically be confined to certain limited regions of the ocean in the following order: (i) the equatorial Pacific, (ii) the Arabian Sea, and (iii) an upwelling area. From a practical point of view, it might seem that the order should be reversed, because the significant periods involved in the equatorial Pacific seem to exceed 1 year, in the Arabian Sea are practically annual, and in upwelling regions are several weeks. Moreover, the areas involved are largest for the equatorial Pacific and are smallest for the upwelling regions. Furthermore, there is an upwelling region conveniently located off the coast of California. A program with limited resources and short duration might be designed to study and model the dynamics of the California upwelling region. But even this program would strain the capabilities and resources of the oceanographic community. Let us envisage a network of at least 50 buoys, maintained in various configurations off the coast for a 2-year period. Then let us consider that it costs the Woods Hole Oceanographic Institution over $500,000 a year to maintain a project centered about a single mooring site south of Long Island. The magnitude of such an enterprise then becomes evident. The need is not only for money but for an administrative apparatus to carry out the operations. Even the U.S. Coast Guard's National Data Buoy Project does not anticipate having more than 40 deep-sea buoys by 1975, and certainly most of them will not be available for fundamental oceanographic research.

A program in the Arabian Sea based on what a single resident research vessel could accomplish with conventional instrumentation in a period of 5 years could doubtless shed considerable light on the response of the oceanic circulation to the annual rhythm of the monsoons. Such a program would be stimulating to physical oceanographers and could provide fodder for theoretical consideration (*8*). It would not, however, reveal the essential dynamical features of the circulation, because a single ship simply cannot measure the relevant variables densely enough in time or space to delineate the role of the medium-scale eddy processes. Similarly, the widely spaced observational net appropriate to a climatological study of the long-period variability of the equatorial Pacific would fail to determine most essential parameters of the dynamics.

"An Oceanic Quest" also recommends the establishment of several heavily instrumented islands for the purpose of determining the propagational characteristics of high-frequency (gravity-inertial range) motions in the mid-ocean. Fairly dense arrays of thermistors, pressure gauges, current meters, and so forth, are needed for this purpose, and it seems that it might be more economical to link them by submarine cable to an island instead of trying to maintain them in true mid-ocean. However, it may turn out that a manned buoy would be superior to an island. Experiments with the ocean telescope at Bermuda (operated by the Massachusetts Institute of Technology and the Office of Naval Research) and with fields of buoys surrounding the Bouée Laboratoire in the Mediterranean are already providing information on how elaborate the arrays of sensors or "antennae" will have to be in order to get "well-tuned" signals from the high-frequency energy-propagation processes in the ocean.

The "Quest" report recommends pilot studies to discover the capabilities of various new techniques, such as free-fall current measurers and bottom-mounted pressure gauges; and, of course, it gives lip service to the need for complete world coverage of deep-water temperature, salinity, and oxygen data by conventional means. More exciting is its espousal of a plan (called GEOSECS), advanced by a group of geochemists led by Harmon Craig and Wallace Broecker, which would run three long traverses from the Antarctic to the northern limit of each major ocean basin. On each traverse there would be systematic sampling for as many chemical substances as possible, from surface to bottom. In contrast to the very tentative nature of other proposed future work in the oceans, the GEOSECS program has won the full endorsement of the geochemical community, and preliminary funding has been obtained.

On the three long traverses, a total of 120 stations will be occupied. At each station, vertical profiles of fifty 30-liter water samples will be taken, and at alternate stations twenty 1000-liter samples will be collected. Vertical spacing of these samples will be guided by continuous temperature, salinity, and oxygen profiles. In addition to the sea-water sampling, gases and particulate matter will be extracted from the air over the sea, and interstitial waters and mineral matter will be obtained from sediment cores. The properties that are subject to change during storage will be measured, and the trace particulate and dissolved radioisotopes sought in the 1000-liter samples will be extracted on shipboard. The remaining work on the samples will be done in the laboratories of the participating scientists.

Among the properties to be measured are (i) the natural radionuclides ^{14}C, ^{226}Ra, ^{228}Ra, ^{32}Si, ^{222}Rn, and ^{7}Be; (ii) the man-made radionuclides ^{90}Sr, ^{137}Cs, ^{3}H, and ^{14}C; (iii) the chemical properties pH, alkalinity, $\Sigma\ CO_2$, pCO_2, NO_3, PO_4, SiO_2, O_2, trace metals, trace gases, and dissolved organics; (iv) the stable isotopic ratios $^{13}C/^{12}C$ and $^{18}O/^{16}O$; and (v) the amount and composition of the suspended particulate matter.

Sixteen months at sea will be required for the collection of samples. An additional 2 years will be required to complete the laboratory analyses. It is sobering to realize that even this very limited program will strain the resources of the U.S. geochemical community. If the GEOSECS program can be carried out as planned, it should provide a very useful set of data for physical oceanographers to use in computing the roles of advection and diffusive transfer processes in the general scheme of the circulation of the ocean.

The prospects for other portions of the U.S. national program do not look so bright. If the membership of the President's newly appointed (10 October 1969) Task Force on Oceanography is any indication, it will largely concern itself with assessing the potential of the ocean as a field of profitable exploitation for private industry.

The Russian Plan

The Russian "Ten-Year Program for World Ocean Studies" calls for very heavy commitment of ships (*9*, pp. 9–12) to make many winter and summer oceanographic surveys over a period of 3 to 4 years in many parts of the world's oceans, including the Antilles current, the California current, the area off Mindanao, and the Galapagos Islands. The programs may engage 25 to 60 vessels. The plan also calls for certain "perennial hydrological observations" on standard sections at fixed geographical localities, evidently for the purpose of measuring the local variability of ocean currents. The program is a very ambitious one, which covers most of the named currents in the world's oceans. Sections are to be made seasonally for 6 to 11 years. This part of the Russian proposal would presumably involve international cooperation because it would require an extremely large fleet of ships. Since measurements would be made by conventional means, they could be carried out by regular naval hydrographic survey vessels or by fisheries research vessels following routine instructions. Whether this approach can really determine much about the true variability of ocean currents is open to some question (*10*), but it would, of course, give a much better idea of the mean state of these currents than we have today.

The section in the Russian plan that recommends T-shaped and X-shaped arrays of moored current meters (with at least 13 moorings per array and with nine or more current meters per mooring) is more interesting scientifically than is repetition of standard sections. If the Russian oceanographers are able to entertain imminent deployment of arrays such as these, it would seem that they lead the world in this technology and are thus in a position to obtain much new and useful scientific information about the extraordinarily complex field of motion in the sea. The Gulf Stream and the Japan current do not seem to be the best regions for initial work; they are too atypical of most of the deep ocean, and they are hard on moorings. The Russian proposal puts primary emphasis on the North Atlantic —a region that is almost neglected in "An Oceanic Quest."

The Institute of Oceanology of the Russian Academy of Sciences has scheduled an experiment with 20 buoys, each with 15 current meters, to be placed in a 180-km square near the Cape Verde Islands during the first half of 1970. Thus the stage is being set for some fundamental dynamical experiments in the ocean. The Russian current meter itself needs to be improved, and other variables besides the velocity need to be measured; but the main problem—that of maintaining arrays of moorings at sea—is being solved. Indeed, these Russian developments are so hopeful that they provide the basis for what this reviewer regards as a truly significant international experiment.

The Russian 10-year program is a compromise between two rather sharply divided schools of opinion: on the one hand, the pragmatic ship operators who recommend rather routine surveys of a conventional variety and, on the other hand, a new generation of scientifically motivated oceanographers who are seeking to establish new ways of studying the ocean. Considering where the power so often lies in such a contest, I believe the scientists are doing remarkably well.

International Plans:

Ponza Report and IGOSS

Attempts are also being made through the Intergovernmental Oceanographic Commission (IOC) of the United Nations in Paris to formulate a long-term program for international cooperative studies of the ocean. In May 1969, a group of administrators and scientists met for a week on the Mediterranean Island of Ponza. There were representatives present nominated by the Advisory Committee on Marine Resources Research of the Food and Agriculture Organization, by the Scientific Committee on Oceanic Research of the International Council of Scientific Unions (ICSU), and by the World Meteorological Organization. A report entitled *Global Ocean Research* (*11*) was issued in June; we shall call it the "Ponza Report." The report was to have been prepared with reference to the following guidelines: (i) What are the most important oceanic research problems that should receive particular attention in the near future? (ii) What types of research programs can best contribute to solving these problems? (iii) In what geographical areas of the world's oceans will increased research efforts make the best contributions toward solving these problems? (iv) What kinds of supporting facilities, services, and manpower will be needed to carry out these programs? (v) How can results of the above exploration and research programs best contribute to various peaceful uses of the ocean, its floor, and its resources? (vi) How can ocean exploration and research best contribute to the particular needs of the developing nations? (vii) How can increased ocean research activities by the developing countries contribute to their social and economic development?

The Ponza Report, as it finally emerged under the chairmanship of Cyril E. Lucas, director of the Marine Laboratory in Aberdeen, Scotland, recommends a massive effort in marine geology, a global monitoring system for marine pollution, and a biological program oriented toward fisheries development; it also contains a section on "ocean circulation and ocean-atmosphere interaction" (*11*, pp. 3–9). Possibly because many members of the working group on oceanography were meteorologists acquainted with the World Weather Watch program, the highest priority in this section was placed on the types of research that might someday lead to the eventual development of an ocean-atmosphere predictive model. It is believed that, although the predictability of "weather" may be limited to 1 or 2 weeks by the very nature of the large-scale hydrodynamical processes (no matter how dense the observations or how detailed the numerical simulation), certain longer climatological predictions might be possible if the ocean can be included realistically in the models. Thus, there is considerable emphasis in the Ponza Report on systematizing and increasing the gathering of near-surface ocean data on a synoptic basis, although this information may have no immediate scientific utility.

There is, of course, considerable danger in this idea of rapid expansion of routine measurement programs. The idea of an Integrated Global Ocean Station System (IGOSS) under the sponsorship of the IOC has been eagerly supported by the bureaucrats of various governments. After all, anyone can sprinkle dots on a map of the world and call it a plan for future measurement; one can also seek assignable radio frequencies, delve into the legal aspects of maintaining deep-sea stations, and so on, even though the strategy has no clear scientific design. It is exhilarating to play such games. Thus, most of the effort expended in the IOC-sponsored cooperative studies of the Kuroshio is not really designed to reveal the time-dependent meander structure of the Kuroshio current but is actually just a gathering of a "useful" amount of background data by using conventional hydrographic stations over the whole northwestern Pacific. The Ponza Report recommends expansion of this effort to include the whole North Pacific. It amounts to committing eight ships to 20 years of full-time routine survey. Such a survey could doubtless employ ships that would otherwise be unemployed, but is this the best use for them? In some respects, the Ponza Report and "An Oceanic Quest" are similar: they both emphasize need for study of an upwelling area (but in the Ponza Report it is recognized that there may be a basically simpler upwelling regime at some place other than the California current). The Arabian Sea emerges as a region of high priority because it has potentially rich fisheries and because it is so extremely variable; hence, the monsoonal circulations might yield to measurement with only one or two resident ships. The Ponza Report recommends regional studies of the Brazil current, the deep-water flows over sills in the Atlantic, and the eastern boundary regions on the west coast of Africa. One of the interesting features of the Ponza Report is its emphasis on the desirability, for scientific reasons, of carrying out certain field studies cooperatively:

> In the study of shorter period phenomena in the ocean, such as detailed turbulent development of the seasonal thermocline, studies of internal waves and mixing processes, dynamics of the thermocline, etc., many new and sophisticated instrumentation schemes need to be tried in the ocean. It is necessary to provide facilities in a few places where the intensive trials of such measuring techniques may open up possibilities of measuring phenomena and processes previously little understood. For example, we can anticipate

intensive networks of moorings, internal wave antennae, arrays of bottom-pressure gauges, scientific acoustic ranges, etc.

Such installations will be quite expensive, and it seems to us that it would be a good policy in the case of new measuring techniques to encourage scientists to install them in a common facility, in the same limited portion of ocean water, because it seems certain that the information obtained by one system of instrumentation will be very useful in the interpretation of the results obtained from other similar installations. Thus the properties of internal waves observed on a mid-depth array of temperature sensors could be independently observed on a line of pressure gauges deployed along the bottom. The value of both sets of observations would be greatly enhanced if they were made in the same locality. The more sophisticated the apparatus, the more essential this interplay of installations becomes.

A large permanently moored barge, or group of barges, moored in the ocean could serve a host of related experiments. In addition, or alternatively, a suitably located mid-ocean island could be used. Criteria for selecting of such an island include access to deep water nearby and space for large shops and other installations.

Whether IOC can materially advance the scientific objectives outlined in the Ponza Report is difficult to judge, but its attempt to improve upon the Ponza Report by producing in plenary session a grotesque draft outline of a "comprehensive outline of the scope of the long-term and expanded program of oceanic exploration and research" contains such an exhaustive listing of marine activities that it is little more than a hastily produced catalog. Perhaps oceanographers everywhere are expected to be comforted by finding their work listed in "the yellow pages."

It would seem more important to identify specific programs of highest priority and to advocate them. In one sense, IOC seems to have done so, as its determined advancement of IGOSS shows. IGOSS is not a scientific program. It is a future system for reporting data internationally, which is meant to serve needs of immediate prediction. But it provides a field upon which the many nonscientists attached to ocean science may disport and may exhibit their ingenuity in diplomatic circumlocution. If this type of political activity should come to preoccupy IOC, it is evident that marine scientists will need to find another international forum.

In the meantime, IOC finds itself increasingly committed to fostering, through IGOSS, such projects as increasing the number of weather-ship stations by 19, of which seven would be in regions south of 40°S. The effect would be to preempt at least 50 ocean-going vessels for the purpose of routine data gathering. Moreover, the recommendation that by 1971 we begin to set out 310 automatic data buoys, which would permanently cover the world's oceans at a spacing of 1000 kilometers and would require a world fleet of 20 deep-sea buoy tenders, seems to be on the fringe of madness. Certainly no clear scientific objective is served by so heavy an investment of technology. One can only hope that economic realities will prevent the realization of such runaway dreams of conquering the ocean.

The scientist finds that there is something Kafka-like in his relationship to programs like the (U.S.) National Data Buoy Project and the (U.N.) IGOSS. They do not appear to be aimed at any clearly defined scientific problem; nevertheless, the scientist finds himself incessantly being asked to "identify" scientific problems related to the design and use of systems for the monitoring and prediction of oceanic conditions and to provide advice, often in a fairly detailed way, on many questions that seem so premature, or so poorly posed, that they reveal a deep underlying ignorance of how scientific work is conducted. For example, one questionnaire asks the scientist: "At what depths should temperature and salinity be measured? and with what precision? How frequently should these observations be made?" Surely the answer depends on the particular phenomenon that is being investigated. Every oceanographer varies his technique of observation, and his choice is dependent on the particular study he is making.

There is no one answer to questions like these. Of course, a definite answer would delight someone trying to plan a global monitoring system. There is mounting pressure on oceanographers to come up with a program that can make use of instruments deployed routinely over the world's oceans. There is a deep division between those who want to establish a large-scale operational system in the ocean and those who want to understand the phenomena occurring in the ocean. Both camps recognize that there will have to be a major engineering effort launched to develop this understanding. But the organizers think in terms of a fairly rigid and permanent observing network, and the scientists think in terms of a varied series of experiments. Organizers are seeking the necessary funding and are trying to provide the necessary management; scientists, in a confused flurry of conflicting plans, are trying to respond to the questions and demands that are directed toward them. One wonders whether there is a true dialog: the words are often the same on both sides, but the meaning seems strangely different. The only thing that seems to keep the discussion going is the organizers' need for a scientific "imprimatur" and the scientists' knowledge that major research programs in the future will need both management and money.

Summary

It seems that no oceanographic problem has yet been formulated that can justify a data-gathering system on a global scale involving several hundred widely dispersed buoys. We can, however, envisage a carefully steered evolution of large-scale experiments in the ocean, which would substantially increase understanding of the ocean and which would require an equivalent, large investment of money and effort. To insist on setting up a routine global system at this time would be to mock the scientific advisers. But the course recommended by scientists will require experts at the helm. Can we have much confidence in the guidance of the leaders of the National Data Buoy Project or of IGOSS?

The physical scientist does not see the ocean primarily as a source of wealth or as a jumble of geographic curiosities. He sees it as a hydrodynamic phenomenon: larger than his laboratory, smaller than a star. To fathom its behavior, he needs detailed data on the velocity field in the ocean, from the microscales of mixing processes within the thermocline up to scales as large as the gyres of the general circulation. He needs special studies of the "Swallow eddies" within these gyres in order to determine whether the general oceanic circulation is dominated by eddies, as the general circulation of the atmosphere is known to be dominated by larger planetary waves. Is the dynamics of the ocean similar to or fundamentally different from that of the atmosphere? Only measurement will tell, and it will have to be mea-

surement at a level of technological sophistication quite beyond the present level and beyond that proposed for routine monitoring. Access to somebody's monitoring system, or to a few years of ship time, will not suffice to obtain the kind of information the scientist needs about the oceanic velocity field. Some carefully designed measurement programs are going to be needed—on a scale larger than an oceanographic institution can manage but smaller than the space program. To be useful scientifically, these programs will have to give first priority to questions of hydrodynamics. To date there is little indication that they will do so.

References and Notes

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Man's Oxygen Reserves

Claims that this important resource is in danger of serious depletion are not at all valid.

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In almost all grocery lists of man's environmental problems is found an item regarding oxygen supply. Fortunately for mankind, the supply is not vanishing as some have predicted. There are hundreds of other ways that we will hazard the future of our descendants before we make a small dent in our oxygen supply. A few basic facts will make clear why this is the case.

First of all, each square meter of earth surface is covered by 60,000 moles of oxygen gas (*1*). Plants living in both the ocean and on land produce annually about 8 moles of oxygen per square meter of earth surface (*2*). Animals and bacteria destroy virtually all of the products of this photosynthetic activity; hence they devour an amount of oxygen nearly identical to that generated by plants. If we use the rate at which organic carbon enters the sediments of the ocean as a measure of the amount of the photosynthetic product preserved each year we find that it is about 3×10^{-3} mole of carbon per square meter per year (*3*). Thus animals and bacteria are destroying all but 4 parts in 10,000 of the oxygen generated each year. The net annual oxygen production corresponds to about 1 part in 15 million of the oxygen present in the atmosphere. In all likelihood even this small amount of oxygen is being destroyed through the oxidation of the reduced carbon, iron, and sulfur being exposed each year to weathering processes. Thus, in its natural state the oxygen content of our atmosphere is exceedingly well buffered and virtually immune to change on a short time scale (that is, 100 to 1000 years).

Man has recovered altogether about 10^{16} moles of fossil carbon from the earth's sedimentary rocks (*4*). The fuels bearing this carbon have been combusted as a source of energy. The carbon dioxide produced as a by-product of this enterprise is equal in amount to 18 percent of the carbon dioxide contained in our atmosphere (*5*). Roughly 2 moles of atmospheric oxygen was required to liberate each mole of this carbon dioxide from its fossil fuel source. By so doing we have used up only 7 out of every 10,000 oxygen molecules available to us (*6*). If we continue to burn chemical fuels at our currently accelerating rate (5 percent per year), then by the year 2000 we shall have consumed only about 0.2 percent of the available oxygen (20 molecules in every 10,000) (*7*). If we were to burn all known fossil fuel reserves we would use less than 3 percent of the available oxygen. Clearly a general depletion of the atmospheric oxygen supply via the consumption of fossil fuels is not possible in the foreseeable future.

Even in a large urban center oxygen depletion is a second-order problem. For examples, auto exhausts contain about one molecule of carbon monoxide for each ten molecules of carbon dioxide (*8*). Continuous exposure to carbon monoxide contents of 100 parts per million creates serious physiological problems (*9*). If automobiles account for 50 percent of the total oxygen demand in an urban area, carbon monoxide would reach the critical level before the oxygen content of the air had dropped by 2 percent (*10*).

There has been considerable reference to man's alteration of photosynthetic rates and the resulting change in the oxygen content of the atmosphere. From the above it should be clear that the oxygen supply is immune to such changes. The extreme case makes this

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