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SOME FUTURE TRENDS IN OCEAN RESEARCH AS SEEN BY THE GEOPHYSICS BRANCH OF THE OFFICE OF NAVAL RESEARCH

HUGH J. McLELLAN
Geophysics Branch, Office of Naval Research
Washington, D.C.

Every oceanographer in this country has had as an article of faith, belief in the existence of a money barrel in Washington, labelled ONR, maintained for the benefit and promotion of the affairs of those people engaged in activities loosely classified as "oceanography".

For seven years I tested this dogma myself and found that, if not a demonstrable truth, it was at least a faith one could live by. What more can one ask of any faith? Not content with worshipping from afar, I was led to become a servant in the temple. At a very disturbing time it turned out. Things are changing around the money barrel and lamentations and wailing have been heard from the far reaches of our land.

Now we all expect our work to change, in fact we wouldn't have it otherwise. It is a far different matter to tamper with an article of faith. Even though everyone interpreted the dogma in his own way to start with—the forthright statement that things are different from what they were believed to be constitutes heresy. Anyone guilty of one heresy is clearly capable of any heresy. Therefore one can make up his own heretical proclamations and impute them to the heretic and have this imputation generally believed.

Let me review some of the things that are being said of us:

Item: ONR is no longer going to support basic research in oceanography.

Item: It has to be of direct application to planned weapons systems or ONR is no longer interested.

Item: ONR is cutting off support of graduate students in oceanography.

Item: (From directors stomping into our office) "You are trying to run the research program of my institution."

Item: (From an October 1 Chapman report) ONR is going towards research with a purely military payout and therefore to classified research only.

To my best knowledge these five statements at least are false. I want it clearly understood, though, that by selecting only these five, I don't admit the truth of any or all other statements of this sort.

I am going to try to tell you what the situation really is and some of the things we feel may happen in the next few years.

Let us review the bidding.

1. ONR's mission has been, and is, to have basic research done that will lead to an improved capability for our forces to be defensive or offensive at sea. As a spender of public funds, ONR has an obligation to buy the best product at the best price. There is no getting around the argument that it is impossible to predict which basic research will pay off. Nonetheless, with any real limitation on funds, the responsibility for deciding how the funds shall be apportioned among the possible efforts is inescapable. Such decision making is the task of those public servants assigned to work in the office. A citizen who thinks this task is being ill done should agitate to have the decision makers replaced by more competent individuals from among the citizenry.

The first decision that had to be made was what portions of our resources should go to physics, mathematics, psychology, geography, oceanography, etc. Difficult decisions of this type have been made for many years, and oceanography has fared the best by far. Other fields have had no substantial increases for years, many good programs have been completely sacrificed and ocean science has grown rapidly. The reason for this is implicit in the title of a speech given by the Chief of Naval Research to the recent Navy Underwater Sound Symposium. It was: "Prospects in Oceanography—Central Science of the Navy".

Now, with the limitations that your Congress in its wisdom, and Mr. MacNamara impose, we are entering an era where hard decisions must be made as to how the effort should be split within oceanography.

Fortunately, this point was not reached before other sizable sources of public support have appeared in agencies which have quite different missions than has ONR.

2. With a few minor exceptions, ONR has never made *grants* to institutions for research in oceanography. However, we have had a policy of broad contracts covering most of the types of work at an institution, and have relied heavily upon the directors to decide how best to use our resources. They have proposed what they wish to do and we have contracted with them to do it. We have given them as much latitude as possible and, in most cases, this has resulted in maximum mileage for our dollars.

We are not displeased.

However, within the last two months the director of one of our larger contracts referred in a letter to a very high Navy official to an "annual grant for research" from the Geophysics Branch—and we have concrete reasons to believe that others thought of our contracts as Institutional Grants which they had a right to expect would be renewed annually and that we had no right to interfere concerning the way the funds were spent. This must change quickly if our usefulness to the oceanographic community is to continue.

The solution is not, in my mind, for us to staff up with a large group of hard-nosed project officers. The solution lies in our finding a way to work more closely with the directors in designing the kind of program that, to the extent we can divine, best fulfills the Navy's need for research in this area.

3. ONR has no mission specifically to promote education in oceanography or other fields. Nevertheless, our office has in the past made research support commitments to universities that made it possible for them to set up teaching programs. We have supported, and now are supporting, a large fraction of the country's graduate students in oceanography through part-time employment on our contracts. We are extremely proud of the role we have played here. But, consider the rational!

More well-trained scientists were absolutely essential for the research our office felt the Navy required to be done. Students have never been paid for being students but for assisting their professors in their research and for carrying out the very productive research that was an essential part of this graduate education. Thus, education in oceanography has developed to its present state largely through Navy sponsorship. Meanwhile, the Navy has gotten a return for its money that can easily be demonstrated to have been a good return.

NSF has entered the scene with a mission in both basic research and education. We gladly defer to this agency the responsibility for looking after the well being of the academic institutions. This is not a matter of choice but necessity. We know that the need for continuity in support is recognized within NSF and hope that they will soon learn how to provide it. Numbers of small one-year grants based upon the collective whim of review committees is not the method.

One of the real dangers we face today is that the institutions to which we give broad support will sell their best packages to NSF and we will end up having our funding go to the supporting roles and to the expensive efforts that are not given good reviews by NSF panels, whose members hope to feed themselves from the same trough. We would, in effect, supply all the bread for the sandwich while NSF provides the ham or cheese to go between. I think you must agree that, in light of our mission, this is an unacceptable way to dissipate our resources even though on a national basis the results might be excellent.

Let me say here emphatically that it is not our intent to change our role precipitously. We feel a real responsibility to the institutions that have grown up with our encouragement and which, by and large, have served the Navy as well as it has served them.

We must, however, gradually back out of the position of supporting institutions because they are, or aspire to be, centers of oceanic research.

What then do we feel should be ONR's role in oceanography? Remember that it must be a role that is defensible within the Navy's total R&D effort.

We must have a definable, balanced program that we can demonstrate is as good as, and as agressive as, the program of any other agency, and that looks ahead to Navy problems in a way the others don't.

We must support new departures in the field. This incidentally is easier for us (provided we are not broke) than for other agencies, because we, in our office, can make arbitrary decisions without applying to the reasoned deliberations of those who have become respectable through application of current or classical approaches.

We must keep a broad-fronted attack going on the ocean—supporting some work even in areas that seem to have no conceivable bearing on naval operations—ours is a basic research program and if there is any single characteristic that makes research basic it is that you don't know all the implications. A recent example that could be cited is research into dissolved organics in sea water. Two years ago we would have had difficulty in showing any naval relevance besides surface slicks. Today we know enough that some serious applied research could be justified.

We tend to be optimistic about a continuing role for ONR in oceanography. If we can get organized to the point that we can tell those to whom we report precisely what activities we are supporting, and why, our ability to continue support will be retained. If we can identify new things that really should be done, I'm sure we can get resources and do them.

To illustrate our optimism we began this summer, in the face of what then looked like a 15% funding decrease, to hold discussions with oceanographers about types of endeavor that need to be initiated. We realized that this meant looking for projects in "Big Science" since everyone already has money to do little science that needs doing.

For the first go it appeared that Physical Oceanography is the part of the field most ripe for improvement and we are pursuing our search for the most important things we can help with.

The task which originally faced physical oceanography was that of describing the "steady state" field of physical properties and evaluating the ordered motion of the waters. It was early found that, through the geostrophic assumption, a relationship between the field of motion and the field of mass could be formulated which gave first order agreement with the few available direct current measurements.

There followed an era where careful measurements were made of temperature and salinity to a precision that permitted accurate estimates of relative density. By assuming a "depth of no horizontal motion" the flow field for the upper layers of most parts of the world ocean has been mapped. By invoking continuity for heat and salt, and adding information on nonconservative constituents, a gross picture of the deep circulation has been added.

This task has been well done. Critical observers have shown the extent that the mass field departs from the stationary and, in some cases, have advanced physical reasons for observed fluctuations. The necessary fiction of a "depth of no horizontal motion" which the geostrophic assumption demands has been called into question so that, although the surface circulation is well described, the mass transport of certain major currents may be uncertain by a factor of two.

Beginning in earnest about twenty years ago theories of currents in a stratified fluid on a rotating earth controlled by wind stress, friction and inertia have been developed and refined. These can adequately explain the major features but suffer because adequate observations are not available to check refined features of the models. Indeed the feature of geostrophy, which is retained to some extent in most theoretical models, has yet to be subjected to a quantitative check in the deep open ocean.

Theory has also suggested a complexity of small to medium scale motions that could be excited by impressed external forces, by inertial instabilities in shear flow, etc. Some of these should be predictable from the physics of the system and the boundary conditions, while others represent the degradation of ordered motion through the process we term turbulence and, if predictable, are predictable only in a statistical sense. What few pertinent observations are at hand confirm only that oceanic motion in detail is at least as complex as theory would indicate.

The obvious task now facing the physical oceanographer is to devise and use systems of instrumentation which will yield a true picture of motion in the important scales. This will allow for the refinement of theoretical models and hopefully an advance toward the goal of prediction.

Four types of experiments are under discussion within the scientific community.

The first looks to the gross behavior of circulation within an entire ocean basin. In each basin the surface circulation is dominated by a large anticyclonic gyre, wind driven, with marked intensification along the western boundary. These anticyclonic circulations are perturbed in respect to intensity and to local geographic position of their boundaries. These perturbations give rise, especially in the eastern parts of the basins, to major departures of temperature structure from the climatological mean. The questions to be asked are: What is the frequency and magnitude of the variations?; How are variations in one part of the system related time wise to variations in other parts?; and What is the underlying cause of the perturbations? Required for the answer are long time series of observations from fixed stations around the periphery of the gyre in its minimum and maximum expected extent. For a first effort temperature observations alone might suffice and the network might omit the region of the strong western boundary current. Such an operation has been proposed for the North Pacific.

A second type of experiment under consideration would concentrate on a western boundary current where there has been the greatest amount of theoretical study. The Gulf Stream north of 32°N would be a logical starting point. The experiment would require a three dimensional array of temperature, salinity and current sensors across and along the stream and time series of observations from them. The questions to be asked are: What is the nature of the flow in space and in time?; To what extent can geostrophic computation be trusted to yield flow information?; What augmentation in mass transport and momentum takes place down stream?; and What are the fluxes of heat, momentum, mass, and vorticity down stream and cross stream? This sort of study would have maximum impact upon theoretical studies but would present severe technological problems in mooring instruments in such a strong current.

A third type of experiment asks: What are the temporal and spatial scales within which significant amounts of kinetic energy are found in the ocean?; How is the energy distributed through the spectrum of scales and periods?; Can this knowledge lead to a formulation of the physical laws, and evaluation of controlling parameters, governing the motion? The initial experiments would go to a comparatively quiet part of the ocean away from strong known currents and, after setting lower limits for the significant space scales and upper limits for the frequencies containing significant energy, set a three dimensional array of current, temperature, and salinity sensors to record for a sufficiently long period that spectra and cross spectra could be computed. Once sufficient information was collected on this background the experiment would be moved or expanded into regions with well defined currents.

The fourth type would take new instrumentation which has been yielding direct measurements of mass transport for the Florida Current into the open ocean. The first problem is to achieve precise relative navigation beyond VHF radio range from shore. This is being investigated and there is promise that buoy mounted systems may work. Direct measurements in major currents would yield a model of the ocean circulation with more reliable numerical values.

Experiments of these types cannot be embarked upon until firm, scientifically valid, plans are worked out; the technological feasibility of carrying them out has been assured; and a nucleus of talented scientists has been identified who will dedicate a significant fraction of their lives to the prosecution. Nevertheless, this is the direction to which we believe one must look for the next major increase in understanding the ocean as a physical system.

In conclusion I would like to quote the final passage from the Chief of Naval Research's speech referred to earlier.

> "To those who compare our efforts in science with our efforts in space, the comparison is apparently odious. The fact remains that the Navy

tries hard to do the most it can with the financial and other resources it gets by permission of your representatives and mine. You need not fear that our approach to ocean science will be less than it could be; in fact, it will be all that the law allows. The law allows enough for a dynamic program and that is what we intend to have—*period.*"

DISCUSSION

Wooster: How would ONR funds for oceanography be divided between "big science" and "little science", and how would this ratio compare with the ratio of publishing scientists involved in the two "sizes"? Back of this question is my fear that a large proportion of ONR support would go to the "big science" projects which might involve only a small number of scientists, the rest of us withering on the vine.

McLellan: This would be hard to predict. Certainly one could not think of cutting out all of the small, isolated efforts since many of the very productive scientists in any field can only function this way. "Size" is just not a valid basis for decision. Neither is "number of publishing scientists" or "pages of published material generated." What you fear is recognized and is a valid point of concern. On the other side of the coin one might fear that we would fail to get on with the job because everyone is comfortable doing what was very productive ten years ago.

Laevastu: If the results of recent works on problems of currents and variability, especially those by O. Saelen (Norway) and by NATO's La Spezia laboratory, have not been taken into consideration in planning the relatively expensive current and variability studies, they should be, as these works seem to answer to a considerable extent the questions and problems raised in the proposed studies.