RECENT ADVANCES IN OUR UNDERSTANDING OF THE CIRCULATION PROBLEM AND THEIR IMPLICATIONS[1]

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When I was first asked to prepare a paper for this program, although it was obviously an honor, I hesitated to accept, for it seemed to me doubtful that a regular staff member of the Woods Hole Oceanographic Institution should be asked to express his views. After all, one of the chief purposes of this Convocation was to gain the benefit of your collective advice as to future scientific policy. The occasion of the dedication of the new laboratory was to be used to give us a new start and some encouragement to embark on novel, and perhaps more fruitful, directions. For nearly 25 years I have been intimately associated with our efforts to advance the circulation problem. I have written or helped to write many annual reports. What could I possibly say here that would be new or helpful to the main purpose of these meetings?

However, after considerable hesitation I came around to the idea that at least one paper in this series might well review the factors on which our present program is based and might try to set forth our plans and expectations for the future. If we are to gain the benefit of sound advice, those who have been asked to help us should at least know what we now think the problems are and how, until now, we have been planning to meet them.

Under the term circulation problem I include a wide variety of phenomena, both large scale and small scale. It has been emphasized many times that the movements of the water constitute the central theme in oceanography and that we are justified in considering this as a separate and somewhat specialized field of science. Few aspects of oceanography can be successfully divorced from the consequences of water movements, either the slow ones or the swift ones.

In 1931, when the Atlantis became available for full time operation, the underlying reasoning of the geostrophic equation was just becoming generally understood. It seemed that here at last

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was a way to make physical oceanography three dimensional and at the same time put it more nearly on a synoptic basis. When our field work was planned, our first idea was to follow the highly successful lead of the International Ice Patrol Service and compute water movements from networks of discrete observations of temperature and salinity. If we could only occupy a large enough number of hydrographic stations, we could then solve for the water movements over a considerable area. In those days the circulation problem seemed just a matter of patience, hard work at sea, and arithmetic. Because the Gulf Stream was near at hand and because it was obviously a major factor in the circulation of the North Atlantic, we began collecting temperature and salinity profiles across this exceptionally swift current when the ATLANTIS was ready. We had little comprehension of the magnitude of the task and of the limitations of the then available observational techniques.

The first few Gulf Stream profiles seemed to agree rather well with the simplifying assumptions of the geostrophic equation. Computed surface velocities on the whole agreed with available navigational experience, but even after several years of sailing back and forth across the Gulf Stream we had gained little comprehension of how crude our navigation was. The interpolation between the points of observation of temperature and salinity smoothed out the picture of the current system to about the same degree as the necessary averaging of the surface current, as observed by celestial navigation. In neither case were we able to see any of the fine-scale structure that today makes the circulation problem seem so much more complex.

It is amusing to note that in 1934, after about three years' work in deep water in the western North Atlantic, we considered that the ATLANTIS had gathered enough data on the Gulf Stream System and that we should resume systematic surveys of the Gulf of Maine, where the interests of biologists were traditionally centered. It was a rude shock when, after nearly two years of monthly surveys of the Gulf of Maine area, the physical oceanographers here could not even write one paper that added anything of significance to what Dr. Bigelow had worked out on the basis of much less systematic and more widely spaced observations. Although we came to realize in this way that the circulation problem in shallow water is an extremely difficult one, we still thought that in deep water the movements were broad and relatively steady and that they could be generally explained on the basis of the then available theory.

Thus, in 1937, the ATLANTIS returned to making Gulf Stream profiles, this time on the line between Montauk Point and Bermuda. Internal waves became somewhat of a worry, but if enough stations were occupied these could be smoothed out. We began to encounter more and more "eddies" on either side of the Gulf Stream. Usually these involved transports equivalent to the Gulf Stream, but sometimes they were considerably weaker. Where do eddies leave off and internal waves begin? This became the doubt which indicated that all was not well.

At this point World War II intervened, and for four or five years the circulation problem was neglected. On the other hand, during the war years we acquired several new techniques, but, perhaps more important, we attracted some new people. As most of you know, after the war, under the leadership of Mr. F. C. Fuglister, an entirely new approach to the problems of the Gulf Stream System was developed. By exploiting the capabilities of the bathythermograph and of Loran navigation it became possible to gain measurements of the surface velocity and at the same time observe approximately the temperature distribution in three dimensions, at least down to moderate depths. At once the Gulf Stream became surprisingly narrow and swift. The theoreticians, led by Mr. Henry Stommel, quickly advanced a plausible explanation. The effects of the change of the Coriolis force with latitude were for the first time included in the theory. But after a year or so of further field work to the eastward, it became evident that the currents were not only narrow and streaky in the west, where they have a northward component, but that they also retain these characteristics in flowing from west to east.

It soon became evident also that for the type of survey so successfully initiated by Mr. Fuglister a single ship was inadequate. To gain a picture of the large scale pattern of the Gulf Stream System, several vessels should be used simultaneously. Operation Cabot was organized and that more or less terminated a stage in our thinking. It is the nearest approach to synoptic deep sea oceanography that has yet been attempted, but it is now abundantly clear that even six ships are too few to gain a clear picture of a phenomenon as large as the Gulf Stream.

In retrospect, Operation Cabot had some other serious deficiencies. It was planned with a preconceived idea of what would be encountered. It concentrated on what might be called "Gulf Stream Number One," and only one of the ships obtained a few observations in the overlapping "jetlet." From this and other experiences, Fuglister's multiple current hypothesis was born. It would require a survey considerably more elaborate and extensive than Operation Cabot to demonstrate conclusively the existence of the sort of current system that he has postulated. A major question today is whether or not such a survey should soon be attempted. If so, are we ready for it and should we again survey a considerable area of the Gulf Stream System or should we focus attention on some other major current? After all, the Gulf Stream may turn out to be a special case. There is growing evidence that the North Atlantic is not a typical ocean. The reconnaissance phase of North Atlantic physical oceanography can be considered completed. Should we continue to study these relatively well explored currents or should we turn our attention to a contrasting region?

Opportunities for multi-ship surveys will not come frequently. We should plan the next one with the greatest of care. Were precise navigation available, there would be little doubt in my mind that the next major effort should be in the Antarctic Circumpolar Current, for I believe that the so-called convergences are nothing more than overlapping jet streams. Yet, without something comparable to Loran navigation, a co-operative survey in southern latitudes might fail. Nevertheless, this is surely the only ocean current system that is relatively free of continental boundaries.

Since Operation Cabot, we have been casting about for new techniques that would be helpful in gaining a better understanding of Gulf Stream phenomena and we have also had a good look at the currents in the trade wind latitudes. I will elaborate on some of the new techniques, but the best that can be said of our efforts in low latitudes during 1952 and 1953 is that they were inconclusive. For lack of precise navigation, the surveys were too open to yield a unique solution. Mr. Fuglister's principle that the current always appears to follow the ship was once again in operation. How to contour temperature data gained in the course of widespread surveys is still a major problem in physical oceanography. The most direct and simple solution to the contouring problem may yield most misleading results, as he showed recently.

Several years ago Dr. Willem Malkus made a direct attack on the old problem of how velocity varies with depth. How deep is the Gulf Stream and how does the velocity profile change in the downstream direction and how does it differ in landward and seaward meanders? He has evolved a variety of devices which we call bathypitotmeters. The development stage has just about been completed. New tactics have evolved, and the first cruise using these new techniques has just terminated.

It is too early to predict what will come out of these new methods, but at least it can be said that they should help to fill a long felt need. Dr. Malkus has set himself the problem of observing how the velocity structure in a given water mass changes with time. He simply drifts with the current, observing the crosscurrent components, the path of the axis of the current, and the velocity-depth changes. For the remainder of this summer we plan to monitor the Gulf Stream almost continuously in these and other ways. What happens to the end of the current after a meander has broken off? How long does the resulting eddy persist? These are critical questions and I do not see how to proceed until they are answered.

Meanwhile, the hope that a plane can monitor the broad synoptic situation while a ship examines a limited part of it in three dimensions is being partially realized. Dr. William S. Richardson has greatly improved the radiation thermometer, first devised by Stommel and Parsons. He has flown it many miles, and what he sees of the temperature pattern of the sea surface adds greatly to our present difficulties. While the ships have shown that the Gulf Stream is discontinuous on a large scale, the plane seems to indicate that, at the surface at least, it is also discontinuous on a small scale.

As most of you know, a few years ago Mr. W. S. von Arx took a new and radical tack. Our vessels are confronted with the difficulty that the ocean is too big. He proposed to make a small one. He started very small indeed, for his first model was no larger than a good sized salad bowl. He is now planning one 24 feet in diameter. It would be feasible to operate a model 100 feet or so in diameter and still spend less money than that consumed by a single vessel. There is little doubt today that model techniques in the earth sciences are embarking on a rapid development. The model integrates much more easily and more cheaply than electronic computors, and it is much more fun to watch.

The future of the circulation problem would be bright were it not for the uncertainty that all important factors operating in the prototype could be included in a reasonably sized model. If the model has to be 100 yards in diameter rather than 100 feet, then we are out of luck. Meanwhile, the model technique will clearly be helpful to the development of somewhat simplified theories of oceanic circulation, and it should also show the critical areas in the southern hemisphere where field observations should be concentrated.

It is my belief that the model technique constitutes an essential link between field observations and theory. Only by using all three approaches simultaneously and wisely are we likely to succeed.

Now, what can be said about the future of theories? We have some very good and satisfactory theory that is based on the idea that the average winds over the ocean provide most of the energy necessary to maintain the horizontal currents. Theories that include the obvious major effects of seasonal production of large amounts of surface water of different densities are just being born. Theories that can deal simultaneously with thermo-haline processes and a variable wind field have yet to be devised, although it is clear that in the ocean we have currents that range all the way from pure wind currents to currents deriving all of their energy from density differences. Most currents appear to be of an intermediate character, and it may be many years before we can assign values to the relative importance of these two causes of the circulation.

Meanwhile one very important lesson has been learned in shallow water. After the war, when interest was developing in estuarine circulation, rapid progress was made because in general a continuous record of one factor was available, namely the freshwater inflow. The analysis of the shallow water surveys had to account for the varying contribution of energy from the river. We were no longer dealing with spot observations alone. The spot observations had to be so analyzed as to transmit a measured amount of fresh water out of the estuary. Undoubtedly our deep water observations could be made to yield more convincing results if they too had to allow for some continuingly measured variable.

Several recent efforts in this direction have produced encouraging results. Mr. Wertheim's electrodes on either side of the Florida Straits presumably give a record of the energy available to the beginnings of the Gulf Stream. Changes in the initial energy should be reflected in future happenings downstream. We are dealing with a system that has both short period and gradual variations. Our attempts to analyze synoptic data must admit to this fact and should be directed so as to advance our ability to predict what will happen off New England, for example, following radical changes in the Florida Straits.

In this respect I believe we are somewhat ahead of meteorolcgy, for we have stumbled on means of getting continuous measurements of the variations in energy available to parts of the marine system. I feel that a considerable effort to gain continuous data on oceanic circulation is well justified and will yield increasingly valuable results, especially after really long records become available.

The long term approach is really the responsibility of the institution rather than of the individual researchers on its staff. Few of us have the patience to wait for accumulation of even a 10 year record. The Woods Hole Oceanographic Institution has now been in existence nearly 25 years, yet today we do not even have a continuous record of winter temperature in Woods Hole Harbor for the whole of this period.

Only recently, through Mr. Stommel's enthusiasm, has the idea of a continuing oceanographic observatory been born. No doubt he will soon turn his attention to other matters, yet it is clear that some organized effort to secure long series of data will in time pay big dividends. More of our present effort should be directed toward a selection of critical places where continuous observations will yield the most significant results and also toward methods of devising inexpensive means of recording a number of types of information simultaneously.

Too few oceanographic instruments that can obtain long continuing records comparable to those of a tide gauge ever become fully developed. The inventor, in satisfying the needs of his particular research problem, nurses the instrument so long as it is critical to his particular study and then puts it on the shelf. We have no organization at present to refine the design of such instruments and to put them to work gathering long series of data that may become of great interest to future investigators. This has been a serious deficiency in our past program and I hope that something can be done about it in the future. The idea that an oceanographic laboratory should maintain a few permanent points of observation in the ocean seems to me a sound one and it is high time that suitable equipment was developed.

To summarize: our experience indicates that it will be a long time before we are able to develop adequate synoptic pictures of the circulation pattern of considerable areas of the ocean in three dimensions. The more detailed our surveys, the more difficult the problem has become. The hope is that models can be developed that can be trusted. Our ships, supplemented by buoys and submerged recorders of various kinds, can observe what goes on at a given spot or in a given water mass as a function of time, and perhaps the model can interpolate between these few points of continuous observation. The outstanding characteristic of oceanic circulation is that it is variable over a very wide time scale. It varies significantly from week to week, from month to month, from year to year, and probably over periods of 100 years or so as Mr. L. V. Worthington has recently pointed out. Combining observations widely separated in space and time usually obscures the very factors we are trying to understand.

We still have exploratory survey work to carry out in large areas, especially in the southern hemisphere. Before this is undertaken on any considerable scale I feel that we should first develop a means of precise navigation. To survey new areas by using conventional techniques perhaps would seem satisfactory to a geographer. It would fill in areas that are now more or less blank on our charts. But so far as the circulation problem is concerned, I doubt that we would learn much that is new, except possibly through multi-ship operations. Meanwhile, awaiting a better and more universal means of navigation, I believe that we should continue to study the limited areas where Loran navigation at least is available.

When one looks back on the history of the circulation problem, advances in our understanding of a large scale phenomenon has always depended on major improvements in navigation. Maury's contributions depended on the chronometer coming into general use. He developed the climatological, two dimensional ocean that was the starting point of modern oceanography. The next hundred years were consumed in trying to understand a three dimensional, statistical ocean. Only when Loran navigation became available did we begin to appreciate the pitfalls of the averaging and interpolating procedure. We need something much better than Loran and we need to have all parts of the system under our control. If any of you doubt this statement I suggest that you spend a few hours aboard the ATLANTIS while she is tied up to the dock. You will find that Woods Hole moves in sudden and unpredictable ways as some technician at Halifax or Nantucket decides that it is advisable to alter the timing between the master and the slave station.

At present our only means of proving out either developments in theory or in models is through navigation. Until we have improved navigation by another order of magnitude we will remain in danger of confusing the average ocean with the one that exists at any one time. In this respect meteorologists are ahead of us, although in the long run we have an advantage, for our system changes so much more slowly.