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proceedings of the symposium

LEVEL II

ASPECTS OF DEEP-SEA RESEARCH

February 29 - March 1, 1956

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PROCEEDINGS OF THE SYMPOSIUM

ON

ASPECTS OF DEEP-SEA RESEARCH

Held at the

National Academy of Sciences-National Research Council
Washington, D. C.
February 29 - March 1, 1956

(10) William S. von Arx, Editor

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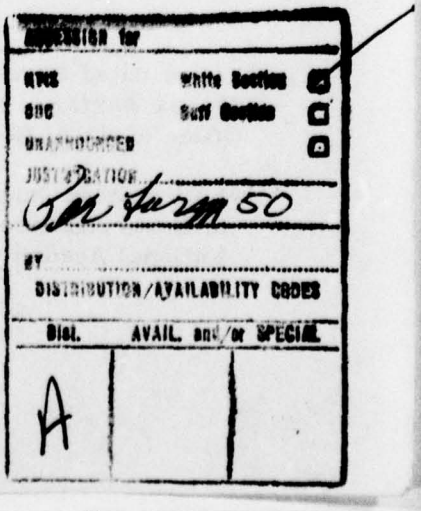
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ABSTRACT

This document represents the proceedings of a symposium sponsored jointly by the Office of Naval Research and the Committee on Undersea Warfare of the National Academy of Sciences-National Research Council and held February 29 - March 1, 1957.

Participants were drawn from various United States and foreign laboratories and the 25 papers presented dealwith physical and chemical oceanographic problems relating to the deep sea. Also included are discussions of the papers and a summary of the symposium which draws attention to a number of factors important in more fully opening up the deep regions of the sea to scientific study.

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FOREWORD

Of recent years there has been an expanding interest in the depths of the oceans. Technological advances have added urgency to the exploration of these vast uncharted and relatively inaccessible regions and improvements in instrumentation have provided better ways of obtaining the desired information. Scientists from other disciplines have been attracted by some of the borderline problems of oceanography, and the Navy has become increasingly conscious of the oceans in depth.

The growing importance of the oceans' depths both in terms of research and of military operations led the Committee on Undersea Warfare to consider the present state of and future requirements for knowledge in this area of endeavor. Subsequently the Committee proposed that the Office of Naval Research join with the National Academy of Sciences-National Research Council in sponsoring a small symposium on deep-sea research, the general objectives of which would be as follows:*

a. To review the state of our present scientific knowledge concerning the deep sea, directing attention to the deficiencies and gaps,

b. To consider the variety and implications of information now lacking about the deep sea,

c. To discuss the problems involved in obtaining the desired information and consider and evaluate possible solutions to such problems, and

d. To focus the attention of diverse organizations and activities having common or overlapping interests in deep-sea research on the possible need for and desirability of a comprehensive, unified, cooperative program of research which will include areas of investigation and exploration of the oceans believed to be particularly significant to civilian and military exploitation.

The Office of Naval Research agreed with the desirability of holding such a symposium and planning got underway.** A program committee was formed consisting of Dr. D. W. Prichard, Chesapeake Bay Institute of the Johns Hopkins University, Chairman; J. A. Knauss, Scripps Institution of Oceanography; A. E. Maxwell, Office of Naval Research; and G. W. Wood, Committee on Undersea Warfare.

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*Letter dated November 28, 1955 from: Executive Secretary, Committee on Undersea Warfare, National Academy of Sciences-National Research Council to Office of Naval Research, Code 466.

**Letter dated January 31, 1956 from: Head, Undersea Warfare Branch, Office of Naval Research to Executive Secretary, Committee on Undersea Warfare, National Academy of Sciences-National Research Council, Ser 2305.

In assembling a well-balanced program, it was difficult to retain sufficient scope to encompass the important problems of deep-sea research while restricting the subject sufficiently to allow informal discussion. Many subjects having direct bearing on the deep ocean could not be included in the program; for example, biological aspects of various phenomena occurring at both the surface and bottom boundaries. However, the gaps in the discussion represented by these omissions do not necessarily mean that the subjects are regarded as being of lesser importance in the general problem of deep-sea research.

Thus the scope of the symposium was limited to problems in physical and chemical oceanography particular to the deep sea which, for practical purposes, was considered to be those parts of the open ocean beneath the main thermocline. These are the regions which remain largely unexplored and about which knowledge and understanding are meager except in very general terms.

In addition to the planning of the program, considerable attention was given to the composition of the audience. Many of the participants were not actively engaged in oceanographic research per se, but they contributed much to the discussions and the interchange of information between this peripheral group and the oceanographer was considered to be most fruitful.

During the course of the symposium, a resolution was introduced urging a national program aimed at providing means whereby men and their instruments could be transported to great depths in the ocean. There was unanimous agreement on the resolution and following the meeting it was transmitted by the Committee on Undersea Warfare to the President of the National Academy of Sciences and the Chief of Naval Research.

These proceedings were assembled and edited by Dr. W. S. von Arx, with the assistance of A. E. Maxwell, G. W. Wood and the staff of the Committee on Undersea Warfare.

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1. REMARKS ON DEEP-SEA RESEARCH

John D. Isaacs
Scripps Institution of Oceanography
La Jolla, California

I am happy to have the opportunity to join this well-conceived and timely discussion of deep sea research. A mere census of members of this meeting adequately attests to the importance and interest in the subject.

If I were to speak extemporaneously, I could probably cover about half the ground in five or six times the allotted period, so if you will please bear with me, I will read my remarks.

From an inspection of the program, I gather that much will be said about what the physical oceanographer is doing and plans to do and about things he does well. I wish to devote most of my discussion to things he is not doing or, in my opinion at least, not doing very well, and I hope that this launches this symposium in a good argumentative mood.

One deficiency which the oceanographer shares with many of his fraternity is in his communication with his fellow man.

Oceanographers are something of a breed apart from students of other provinces. They spend longer periods of time away from land than have any other members of their species since the days of sailing ships. Despite this fact, no serious degree of subspeciation or mutation has yet occurred. Oceanographers sometimes fall prey to motion sickness, they beget their progeny ashore like others of the species, and their offspring display a surprising tendency to become newspaper reporters, physicians, ballet dancers, and to go into business. In view of this, we still must regard the problems of mankind as our own, particularly problems in the area where we possess special understanding, that is, in science.

The burgeoning of science has been so startling in the last decades that few are not still dazzled, and these few hold highly divergent opinions on the developing position of the scientist in society. That the scientist will emerge as an extremely powerful element and as a new élite is thought by some to be likely, but the regard in which he will be held by the popular mind remains in doubt. One could ponder with profit the prescience of St. John: "Marvel not, my brethren, if the world hate you."

I believe consideration of this is of vital concern to all, and, I can only urge that we consider our position and our "product," our communication and understanding.

I am willing to accept the thesis that man's "betterment" are those processes that enrich his life, and broaden his understanding and his experience. But we, as scientists, tend to be snobbish in this and to hold in higher regard understanding and experience which only scientists or the highly informed can

appreciate, and to deprecate experience in our field that is more general. I believe that this higher regard is justified, but that the snobbery is not. Too often there is apathy or active obstruction to the flow of knowledge from the science through its application to engineering and industry. And here I define industry as that human activity that makes the earth available for the enrichment of her inhabitants. This may be by the catching of fish, the creation of beaches, the writing of popular books on science, or the wresting of manganese from the sea floor. If we do not believe in helping the dredging engineer or the sport-fisherman understand the environment which he invades and cry only "desecrator," we do not believe in man or, at least, not in teleology and the direction man is going.

I do not attempt to detract from the accomplishment of those who can encompass an ocean in their understanding, I plead for compassion on those who have not encompassed a tide pool. Let us not draw away from them that strive to bring the bounties of the earth to the enrichment of mankind, though the "product" is a "popular" one, nor let us cut the flow of understanding between science and mankind, for if science does not give man adequately rewarding experience he will look elsewhere.

If this smacks of homily, I am sorry; if of admonition, I can only say that I recently have seen two of the world's greatest leaders in engineering turn bitterly from the established disciplines and say, in effect: "We have no brotherhood with scientists." That there is much to be said on both sides of this question does not detract from its tragic implications.

I have said that our communication with our fellow men is defective, even that with closely allied groups such as engineers.

Now, before passing on to pleasanter subjects, and while I am making a long face about communication and rapport, I will say that communication even between students of marine science is poor. For example, I decry the tendency of the models of the physical oceanographer to dominate and limit the field of deep sea oceanography, or perhaps, I would state it better if I were merely to object to the fact that the physical oceanographic models are manifestly incomplete in themselves and for the interrelated purposes of other workers in the marine sciences. For example, Johnson's larvae of Emerita blithely pass across the streamlines of the California current and produce only a momentary irritation. Biologists are likely to slap down a chart of the distribution of some plankton upon the isotherms or isohalines, often on a different scale, and both the physical oceanographer and the biologist comment briefly upon any similarities that may be noted and consider that they have been broad and understanding in an interdisciplinary matter. Yet one of the salient points to be understood is how and by what mechanism does a population of plankton maintain itself in a great current, permitting the current to drift through it almost as an island cloud-cap maintains itself against the winds.

Can we afford not to make every effort to incorporate such truths into our physical models? For example, following the suggestion of Johnson, Hardy, and others, transients of flow or internal waves, undetected and unaccommodated on our present models, would, if "rectified" by vertical migrants, in some places speed them on their way and in others permit them to move against the current.

Or possibly fine countercurrent structure, similarly undetected and unaccommodated, may dominate the distribution. Whatever the mechanism, I believe that by study of biological and physical relationships new understanding will ensue, transport and inoculation mechanisms will be revealed in the sea which constitute paths and barriers as clearly defined as any terrestrial ones. And we also will discover that shifts in such paths and barriers will be shown to be responsible for major changes in the populations. The red tide, for example, possibly is only the response of a motile phototropic organism to surface convergence. I believe that we will find the classically presumed barriers of temperature and salinity to be much less important than supposed.

Let me call to your attention several diagrams from a manuscript by Hedgpeth and Menzies:

The first diagram (Figure 1.1) is a chart of the distribution of Sardinia and Sardinops versus temperatures. Occurrences are shown in heavy outline. It will be noted that sardines exist across almost the entire range of oceanic temperatures, yet these fish have a very limited distribution. Fish, of course, are adaptable.

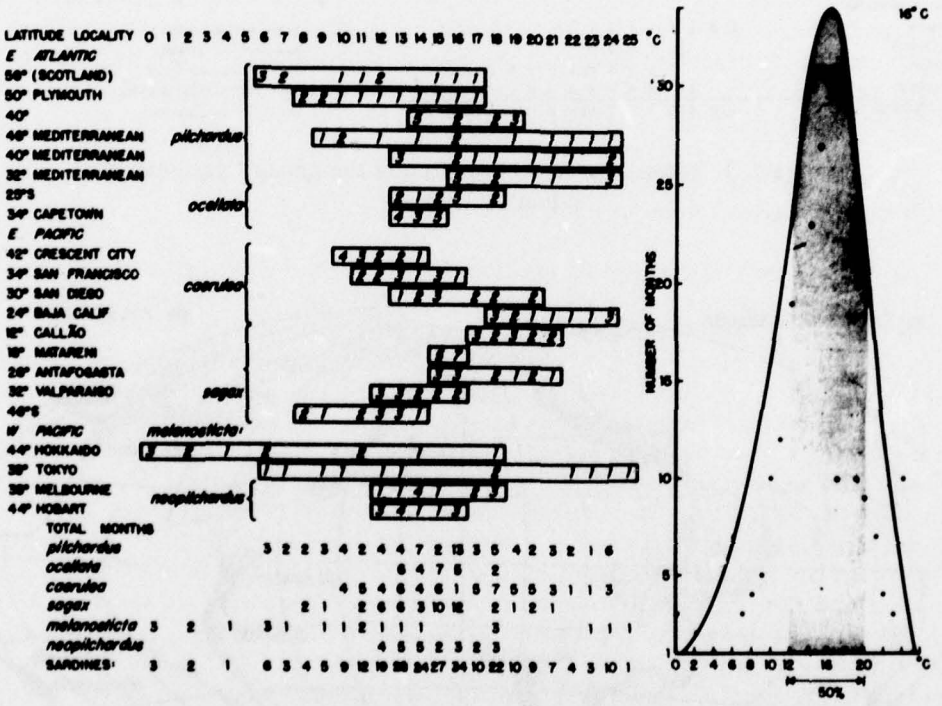


Figure 1.1 Temperature distribution of the sardines (Sardina and Sardinops) (Monthly means)

Figure 1.2 is a similar diagram of Nyctiphanes, a euphausiid. You will note here also the wide thermal range of occurrence. This euphausiid also is very limited in distribution. These limitations are displayed in Figure 1.3.

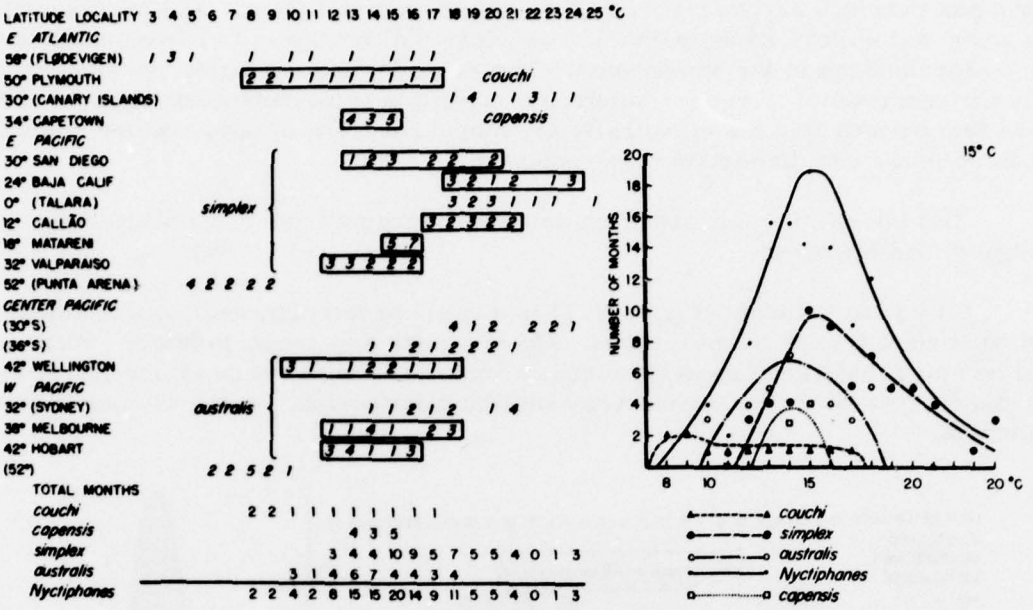


Figure 1.2 Temperature distribution of the genus Nyctiphanes (Monthly means)

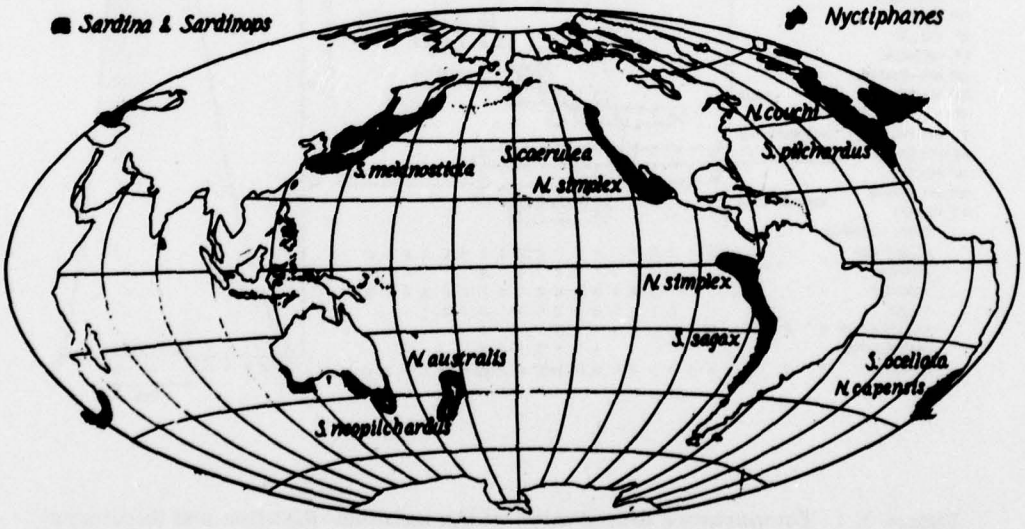


Figure 1.3 Geographical distribution of Sardina, Sardinops and Nyctiphanes

In order to accept any thermal limitation on either of these creatures it is necessary to assume some special mechanism such as thermal limitations of breeding ranges, as assumed by Hedgpeth and Menzies. Temperature certainly does not constitute a simple barrier, and much more powerful controls undoubtedly exist.

Much of the above statements also are applicable to marine chemistry and sedimentology. I believe that rapid progress in physical oceanography will be made only when the model is centrally adapted to critical observations in any field, and not when the observations are included in the model only as appendages or exceptions or entirely neglected.

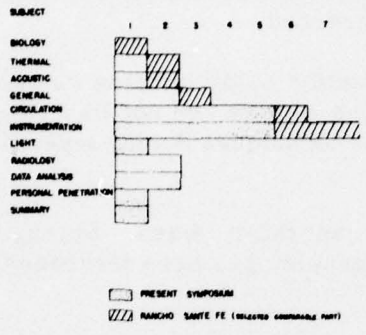


Figure 1.4 Histogram of papers

In conclusion, Figure 1.4 shows a comparison of the subjects of this symposium with those given at Rancho Santa Fe. You will see that radioactivity, light, and the personal penetration of the oceans by the oceanographer are the conspicuous new subjects of the present meeting. These are powerful approaches. If we add stable-isotope investigations, the geochemical approach, and close interrelationship between physical, biological and geological studies, I will venture to predict that the next decade will witness both a more intimate and vastly broader understanding of the sea. Fine structure, age, and deep circulation will be understood. The occurrence of sand layers in the deep sediments, long-period overturn, and climatic variations will be elucidated, and powerful new perspectives as astonishing as colliding radio-emitting galaxies will be attained beneath the surface of the sea. If we communicate well with other marine scientists, indeed with scientists, engineers and many others, we will be rewarded with a wealth of understanding. I urge you, in our turn, let us share these riches well.

DISCUSSION:

MR. VINE: There is one item on the histogram (Figure 1.4) which I think is fairly important, and that is the summary paper. Summaries were not given at Rancho Santa Fe. This is something we all look forward to because I am sure it will engender more argument than any one single paper.

2. ALONG WHAT PATHS IS PHYSICAL OCEANOGRAPHY LIKELY TO PROCEED

Columbus O'D. Iselin
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

It seemed to me that much of my assigned topic was very adequately covered last summer in a little essay by Mr. Henry Stommel, which I believe most of you have seen, entitled, "On the Present Status of Our Physical Knowledge of the Deep Ocean." The papers to follow, including one of his, will attempt to enlarge on various aspects of this general topic, and outline several promising paths ahead, especially insofar as hardware is concerned.

What I would like to do is inject a note of caution early in these discussions, and to point out some of the practical reasons why we are not likely to make many considerable advances by means of new techniques during a period as short as in the next ten years or so.

I have been involved with oceanography for just thirty years. During this time the effort, in terms of money, facilities, or people, has been increased by at least a factor of ten.

The fact that during the International Geophysical Year about 60 research vessels and perhaps 500 people will be at sea is really remarkable, yet even this great effort is not likely to alter greatly our general understanding of the situation. There is an immense amount of more or less routine descriptive oceanography still to be done.

What has been happening recently, and I believe this trend will continue, is that we are becoming much less certain that we can describe the system that we are trying to understand. If one has been attempting to teach physical oceanography, as I have, this is abundantly clear. It is much more difficult today to prepare a constructive lecture on ocean currents than it was, say, 15 years ago. We can easily criticize the old lectures on the general circulation of the ocean, but we have little new positive information to add to them. Only in somewhat more restricted subjects, such as surface waves or acoustical transmission or submarine geophysics have major advances been made.

Why are some of us who study the North Atlantic circulation somewhat pessimistic? For one thing, we have begun to grasp the full complexities of the problem. We have completed the exploratory stage in a few areas at least. We see the need for new methods and new equipment, but if the past is any guide, these do not evolve quickly. We are beginning to think in terms of synoptic situations, but for the most part our long accumulated data are barely able to describe the average conditions.

We are worried by the fact that our particular ocean does not seem to be like other oceans, which are, to be sure, much less completely described. We find that in the North Atlantic circulation problem, we cannot ignore the ex-

changes with the South Atlantic, and the endurance of our ships is inadequate for the exploration of this ocean so devoid of island ports.

But supposing we had capable ships: could we make any considerable advance over what the METEOR achieved nearly 30 years ago? We plan to have a try at this in selected areas during the next two winters, but lacking LORAN navigation I doubt that we will have more than marginal success. Without frequent navigational fixes it seems rather hopeless to study the swifter currents. Thus we will concentrate our attention on the deep water masses, both next winter and during the International Geophysical Year.

Convenient, long-range, precise navigation remains the number one need in physical oceanography in my opinion. Without it, our ability to describe the near-surface currents over any considerable area will proceed only slowly. Meanwhile there is much to be learned about the slower deep movements through available techniques, and I believe that several of the papers which follow will adequately stress this point.

So much effort is going into new navigational techniques at present that I believe oceanographers can safely leave this part of their problem to others. We have some special problems in navigation which we can deal with ourselves, for example, precise navigation relative to buoys. The larger problem seems to be in safe hands, so we must just be patient, but we will probably have to wait more than ten years.

Assuming that means will soon be found for knowing how the ship is moving relative to the bottom or to some fixed stations ashore, the oceanographer has the more special problem of observing the change of velocity with depth. This is being attacked in a number of different ways and these efforts should be continued at high priority. However, in the end we need a solution that does not require stopping the ship, and which has an accuracy of at least a tenth of a knot or so. In principle at least this should be achievable acoustically by observing the doppler shift of volume reverberation, but progress in this direction has been discouragingly slow. Problems of classification seem to be the main obstacle, although it is also clear that relatively expensive, high-powered equipment would probably be involved, especially on the existing noisy ships. We should miss no opportunities to remind our acoustical friends that to be able to measure, from a moving ship, the change of speed and direction of flow as a function of depth would constitute a major advance in oceanography. Whoever accompishes this will find himself immortalized in the oceanographic community.

Meanwhile, of course, several other promising schemes are being tried that require stopping the ship or slowing down to towing speed. Before too much time and effort are invested in these less elegant solutions, I feel that we should request the acousticians to give us a clear statement of the probable limitations of doppler techniques. How much will a fully engineered system cost and how long will we have to wait for it? What will be the accuracy of the depth scale?

The measurement of very slow motions deeper down in the ocean, although at first glance a more difficult problem, I believe will be solved first. Below the main thermocline, relatively few observations will probably suffice, and the time

and effort to obtain them can be easily justified, but to describe the motions in the upper 500 to 1000 meters of the water column will require literally millions of observations, and if we are to attack synoptic situations the ships will have to proceed at all practical speeds.

Thus we come to our next major problem: how to employ the ship most wisely, how to maneuver so as to gain the most effective coverage of an area. The people on the ship cannot know what lies ahead. What seemed a logical solution to this problem was tried out a few years ago. Let an airplane develop the two dimensional, surface current pattern ahead of the ship so that the people on board can concentrate on the third dimension. This still seems a promising solution, at least in regions where relatively swift currents prevail, but it is beset by several practical difficulties, at least so far as available airplanes are concerned. Even if the plane had the necessary endurance, the people in it do not. The plane is accumulating information faster then it can be digested and, in addition, a formidable navigational problem is presented. Again we are faced with waiting for technical developments in another field. Just as the earliest oceanographers had to wait for somebody to devise a chronometer, so we are waiting for a world-wide radio navigational system, for refinements in sonar and for better, slow, long-range airplanes. Such developments take time and military considerations control the priorities.

In the meantime, what can be done to advance the science of physical oceanography within the reach of available funds and our own capabilities? The papers which follow will discuss the more promising possibilities, but in my opinion few substitutes will be developed during the next ten years or so for the widely used reversing thermometer and the Nansen bottle.

I will limit my concluding remarks to a few potential developments which seem to have been left off the program, judging from the abstracts that have been submitted.

First we need better cables, especially long cables having electrical leads. Almost every kind of deep-sea oceanography would become easier and more efficient if we did not have to retrieve the instrument to read the result or if we could provide the instrument at the bottom of the cable with electrical power.

Again the technology involved is largely outside the area of our special competence. The market for such cables appears to be small, although in fact there may well be a host of non-scientific applications. This is an area in which I believe the Bureau of Ships could be of great assistance to oceanography.

Can we agree on what types of cables we want, and on their electrical characteristics? Have we sufficiently explored the existing manufacturing facilities? Could oil well logging cable be strengthened to satisfy most of our requirements? Is buoyant cable a practical idea? Are faired cables being neglected?

Second, many of our cable problems would vanish if the ship could be stabilized and maneuvered at very low speeds. For several other reasons these would be desirable achievements. During the last year two pre-design studies have produced somewhat conflicting results. The Bureau of Ships has indicated

that stabilization and maneuverability at low speeds can be achieved in a specially designed ship. M. Rosenblatt and Son, faced with designing these characteristics into an existing hull, say that the penalty will be too high in cost, weight and space. It is heartening to note that naval architects are becoming interested in our design problems, but again the demand is so limited that progress is likely to be slow.

Given ideal ships, improved cables, better airplanes, adequate navigation, etc., much could no doubt be achieved, but we are not going to get these things quickly, nor can we do much more than urge that they be developed. In the foreseeable future, oceanography will continue to be plagued by the hostility of its environment and the vast areas to be covered. In high latitudes it is, for the most part, too rough in winter to make field work profitable. In summer in the western North Atlantic too frequently cruises become abortive due to hurricanes. The spring and autumn are awkward periods due to the university calendar. Thus the demand for ship time is very uneven seasonally.

The fisheries people are on the whole only interested in the shallow seas and these, because of tidal currents, are by far the most difficult to understand through established techniques. In Europe the pattern of long but infrequent voyages has become established. More frequent but shorter voyages, such as is the custom here, are equally unacceptable to our wives, and perhaps also to ourselves.

It is no wonder then that we have begun to think in terms of buoys, radio telemetering and instruments with shore-connected cables. These are highly attractive potential developments in oceanography. Several of the papers to follow will extol their virtues. Such developments will help us no doubt, for clearly we must learn to gain continuous information. We have been struggling for too long with spot observations, separated too widely both in time and in space. However, during the next ten years or so the main burden will continue to be borne by ships, imperfect though they are, and those who are willing to sail in them, imperfect though they are. Effective oceanography at a desk on shore is still some distance away, for we have yet to gain a clear picture of the physical characteristics of the sea in all three dimensions and over wide enough areas.

3. THE THERMAL STRUCTURE IN THE DEEP SEA

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Mr. Isaacs said that we must share our knowledge with the rest of mankind, and I would like to talk here more about sharing our knowledge just among ourselves, among oceanographers. Really, I think the title of my speech, "The Thermal Structure in the Deep Sea" is not entirely suitable. I will talk primarily about thermal observations.

The thermal structure in the deep sea, or rather, our knowledge of that structure, has always been one of our most important tools for the study of ocean circulation. We are here to discuss ways and means of increasing our understanding of this circulation. It is evident, therefore, that we do not believe that our present knowledge of the thermal structure has given us the answers to our problems. Should we, therefore, drop our investigations of the thermal structure and turn to newer and perhaps more fascinating lines of endeavor? Before we answer this question we should know something about the present state of our knowledge of the deep sea thermal structure.

To begin with, we have much more data on the subsurface thermal conditions than we have for any other physical characteristic of the deep sea. We have - I don't know how many observations. They are mostly in the Hydrographic Office. I believe Dr. Lyman could tell us exactly how many there are.

(Dr. Lyman, from the floor, replied there were three-quarters of a million.)

At Woods Hole we have virtually all of the subsurface data from the Atlantic Ocean on three by five inch cards. These data occupy about 100 cubic feet of office space and they represent the thermal structure of 350 million cubic kilometers of ocean. But let us beware of falling into the trap that usually lies in the path of anyone discussing thermal data from the oceans, namely, to start by talking about the great number of observations. Since we are dealing with the oceans of the world and a dynamic, not static, characteristic of these huge bodies of water, any numbers we might use really do not have much meaning. The meaning lies not in the number of observations or the space they occupy, but in their quality and the manner in which they are distributed in space and time over the oceans.

It is rather difficult to say anything, in a general way, about quality of data. There is one rather simple criterion that we might use, and that is to note whether or not the data have been published. As a rule of thumb I would say the most reliable are published data accompanying a scientific paper. Unfortunately there are at the present time rather few scientists interested in spending much time collecting temperature data and publishing the results. The least reliable data are, of course, those which no scientist is willing to publish under his name.

Today, we must, by and large, depend on laboratories and institutions to

publish temperature data collected as part of large programs. There is an ever-present danger of turning this job of collecting and publishing these data into a routine chore. Under these circumstances the quality of the data is bound to suffer. One of the difficulties, as I see it, is that we are apt to turn these chores over to what we call nowadays; observers, technicians, and clerks, and then expect these people to turn out published data worthy of our institutions, while they themselves remain anonymous. This practice develops little incentive to painstaking effort.

I think one rather important question is: what are we going to do in this respect when we come to deal with the International Geophysical Year, which may produce data in great quantities?

Before going on to discuss the space-time distribution of our data I want it understood that whenever I refer to unpublished data I do not necessarily mean classified data. The classification of temperature data from the deep sea is a problem which I will not discuss but I hope that the matter will be brought up later.

This brings me then to the distribution of available subsurface temperature data.

The South Atlantic is the only ocean that has ever been thoroughly and systematically surveyed by one group of scientists during one particular period with the objective of understanding the thermal structure of the entire body of water. I know of no set of data that can compare in this respect with that published in the METEOR Expedition volumes in 1936. A set of data that covers a longer period but a much smaller area is the excellent U. S. Coast Guard Ice Patrol observations in the Grand Banks area of the North Atlantic. For the rest, few if any sizable deep-sea areas have been studied in anything like a thorough manner. Considering the climate in the Antarctic, the DISCOVERY efforts deserve high praise. But here, I think, the problems of the area are too great to be adequately dealt with by one vessel.

The North Atlantic as a whole has been dealt with in a spotty and erratic fashion, but at least it has been dealth with. This cannot be said for the Indian Ocean or the South Pacific. The North Pacific is being given a good going over at the present time, and I am looking forward to the publication of more and more interesting data from that ocean.

We can say then, that as far as the distribution of observations go, we have the gross picture of subsurface thermal conditions as they were in the South Atlantic in the period 1926-28. We know the varying picture of the thermal structure during the spring of the year in the Labrador Current. For the rest, our data although quite concentrated in certain small areas, are too erratic to be subject to successful statistical treatment.

I would like to mention here that we stand a good chance of continuing the spotty survey technique in the coming International Geophysical Year. No orderly or complete survey is going to be made of any of the world's oceans as far as I know. A relatively small portion of the North Atlantic is going to be studied in

some detail. For my part, I would rather see this area of study extended, say toward the south, than to have the two meridional sections in the South Atlantic as presently planned.

I must not forget to say a word about the scarcity of deep observations. Except for the previously mentioned METEOR Expedition in the South Atlantic there are very few sets of observations that extend down below about 2000 meters. We believe that the range of temperatures below 2000 meters is small, but on the other hand whatever changes do occur there, either with time or space, are of great importance to the theme of this symposium.

When we discuss the distribution or ocean coverage of data, we must remember this third space dimension. Because it takes less time and requires less equipment to obtain information near or at the surface, we naturally have accumulated a great preponderance of surface data. This then brings me back to the subject of publishing data. And this, of course, brings me to the bathythermograph.

The two by five by ten foot file of data in my office does very little good unless the information contained therein is made available to all those who might be interested. Most of this file is made up of bathythermograph data, none of which have been published. Perhaps not all of the these data collected in the last fifteen years are worth publishing; perhaps they cannot be published in their entirety but must be reduced in some manner; or perhaps the information can only be published in the form of charts or atlases. But in any case, I feel strongly that these data must be published in some form. I feel that there is some urgency to get this done in order that we can clear the decks, as it were, and prepare ourselves to handle the flood of such data that should result from the International Geophysical Year surveys.

I published a chart of the temperatures at a depth of 200 meters in the North Atlantic which was based primarily on bathythermograph data. Although many thousands of observations were studied and we produced the first ocean-wide chart since the METEOR Atlas was published twenty years ago, the result was a very small reproduction measuring $4\frac{1}{2}$ x $5\frac{1}{2}$ inches. Details were obscured, and the usefulness of this chart is limited. In the future if we publish such charts I hope that we will make them large enough to be effective.

Figure 3.1 is a poor copy of Wust's chart of the average 200 meter temperature in the North Atlantic taken from the METEOR Expedition Atlas. Maybe it was at this point that the law of diminishing returns set in. This chart, together with numerous others for greater depths from the METEOR Atlas, thoroughly described the thermal conditions in the Atlantic Ocean. These charts, together with two charts for the 200 and 400 meter temperatures, published by Schott in 1935, pretty well cover all the world oceans. And that is about all we have today to refer to when we want to look at published information about the subsurface temperatures of the ocean.

Figure 3.2 shows the 200 meter temperature picture as we published it in 1954. The picture has been extended into the Gulf of Mexico, the Mediterranean, the Labrador Sea and the Norwegian Sea. There is no spectacular difference be-

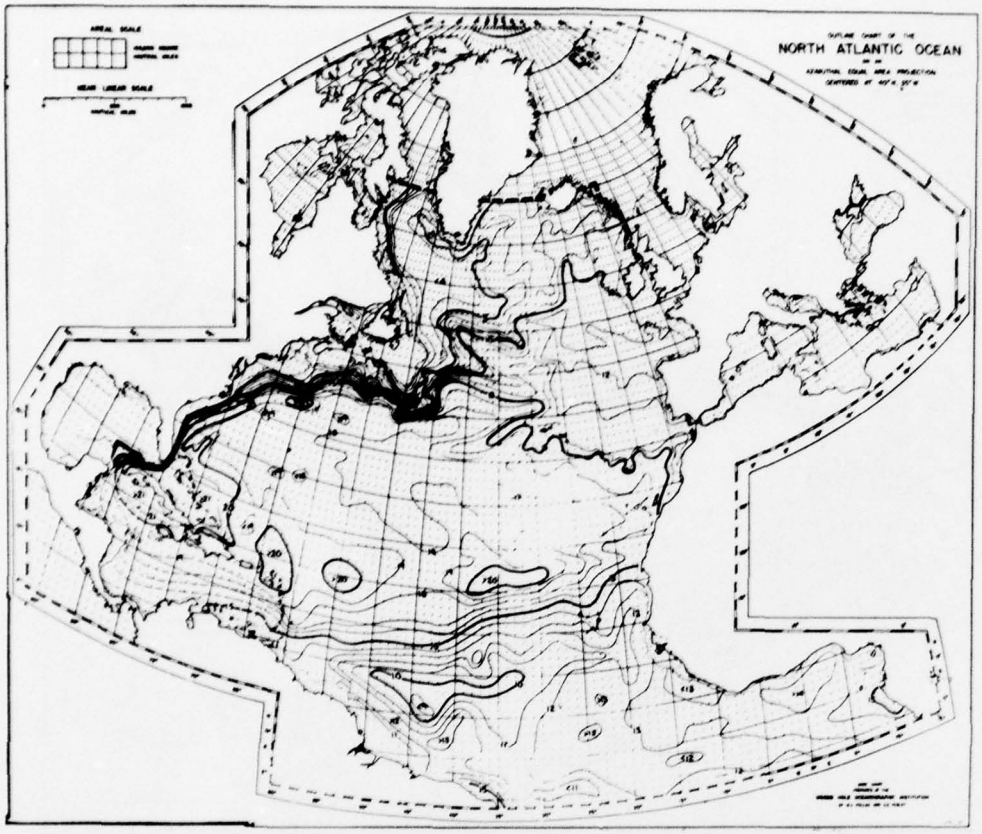


Figure 3.1 Average temperature (°C) at 200 meters (after Wust's chart in Meteor Atlas, 1936)

tween this and the previous chart, and we could hardly expect there to be. There are, however, numerous important differences in detail. We do not have the time to go into the significance of these changes here, but it would appear that the final chart on the thermal conditions at this depth has not yet been made. (Figure 3.3 shows us why.)

If we take, for instance, the 18 degree line at the start of the Gulf Stream, and the 18 degree line toward the east: it follows various paths eastward, comes quite far back, then across the ocean and back, and across the ocean again. Does this have any significance?

There is an interesting point in this connection: if you have such a curve in the middle of the ocean where there are relatively small amounts of data, anyone would agree that you could draw a smooth line if you are trying to give information about the thermal structure. But in the western North Atlantic if such a thing occurs right at the coast, we do not allow such smoothing because we know in this case this is an effective representation of the Gulf Stream.

The conditions near the coast we expect to be somewhat special. We cannot tell whether these curves should be smooth or whether we could smooth them, or how. So I would rather like to be assured that if charts of this type were produced again, that they be done in really good size, about that of the original of

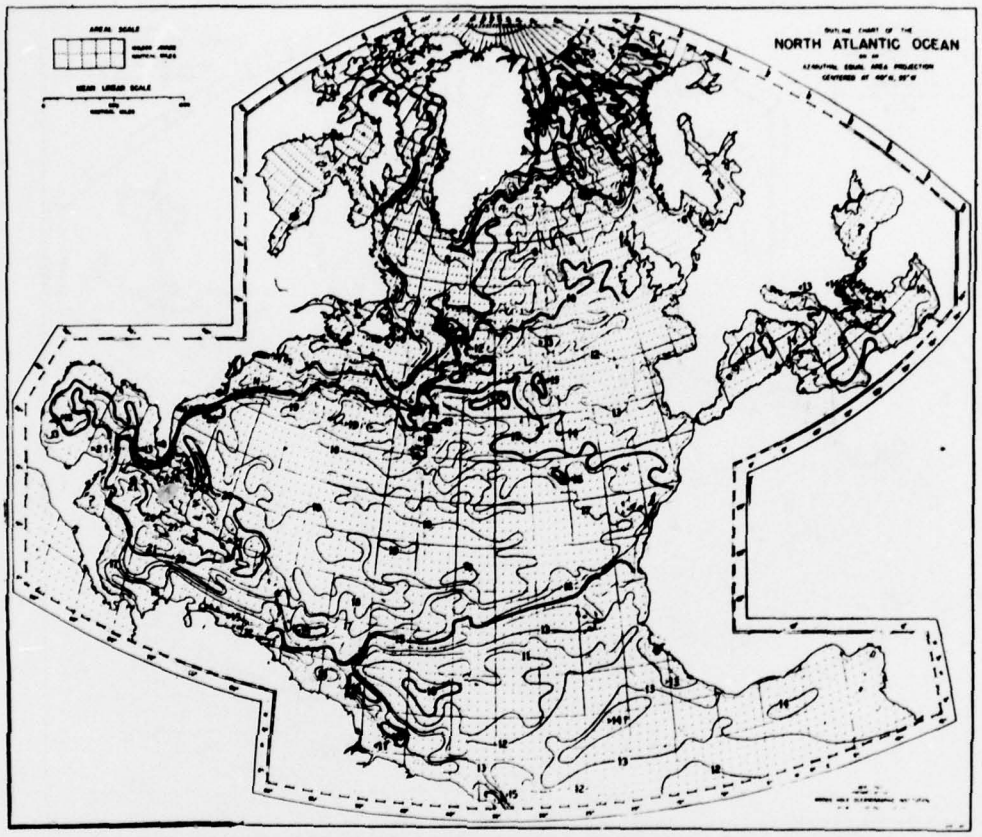


Figure 3.2 Average temperature (°C) at 200 meters (Woods Hole Oceanographic Institution, 1954)

this chart, in color, and the lines drawn on them to be smooth. Those in Figure 3.2 have not been smoothed. If a chart like this is based on the average temperature, then underneath in each square containing data we might put the average temperature in some sort of printing that would be covered by the color at a distance, but clearly visible at close range so that you could see just what somebody was trying to put over on you when he drew the chart.

Figure 3.3 shows that even after all these many thousands of new observations are added to Wust's, we can hardly be talking about average conditions in some parts of the North Atlantic. This chart shows the distribution of the observations used to produce Figure 3.2. In certain parts of the oceans we show average conditions, in others we show the results of a few isolated observations, and for the rest we are theorizing. Remember that this is the ocean that we always point to as being the most thoroughly covered by observations.

I believe that one of the most important steps that we can take during the next few years is to see that the available data on the thermal structure gets published. We must determine the best means of publishing data obtained with the bathythermograph. We should produce new charts of reasonable size. We must be careful to see that the standards of accuracy are maintained at a high level and that we do not mistake quantity for quality. We can begin by publishing some papers on the subject of the mixed layer.

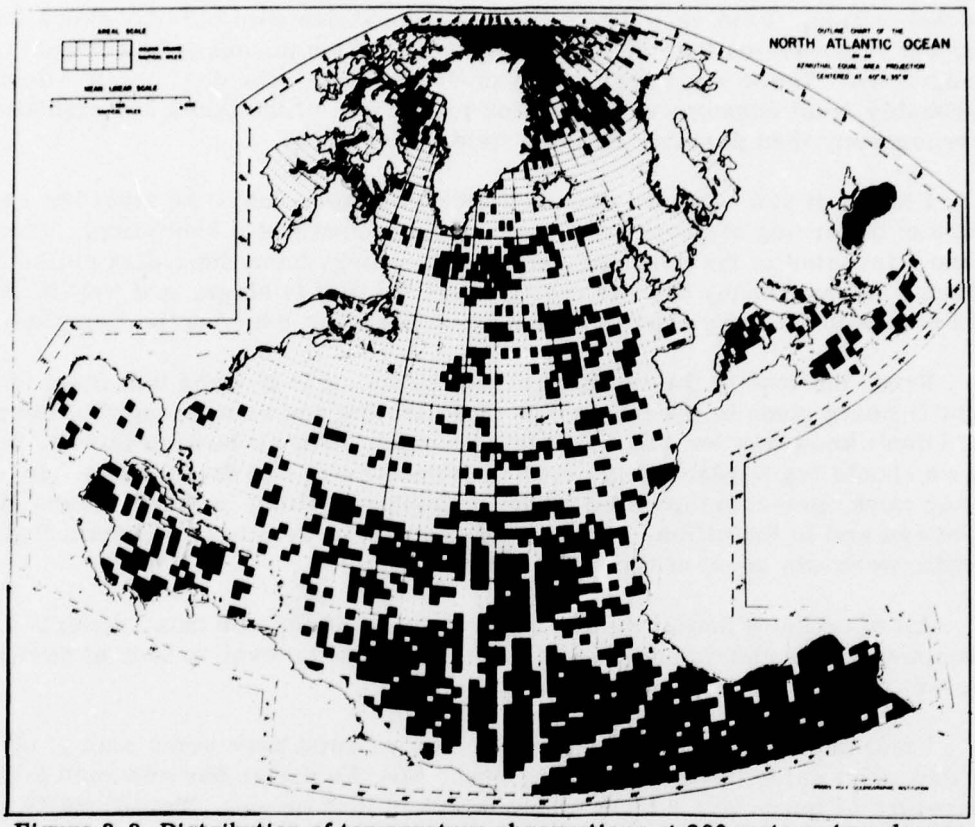


Figure 3.3 Distribution of temperature observations at 200 meters (one degree quadrangles; no observations in black)

I firmly believe that in our search for a better understanding of the ocean circulation we will find that we still have much to learn from a continued collection and study of thermal data.

DISCUSSION:

DR. WOOSTER: On the matter of publishing BT data it might be interesting to know a start has been made by the Canadians, and in the recent publication of NORPAC data by the Pacific Oceanographic Group the BT data they took at each station and those in between have been published. They have not done this by photographic means because photographs of BT slides come out so poorly that they cannot be used, but they have projected the tracing on a grid, traced it off, and this is very legible. Whether or not they are accurate, I am not sure. At least a start has been made in publishing BT data.

DR. LEIPPER: Our weather observations, taken to get the time change picture, were distributed in mimeographed form. They are all worked up, but the question is whether they should be published, or whether too much data are already published. Anyway, all this is available.

DR. FLEMING: I feel I have to put in my word here. I hoped that from this meeting, living as I do now in the far distant northwest, that I should receive

great inspiration. I am very depressed. I sympathize with both speakers very much, but it is obvious that we are preoccupied by the housekeeping details of oceanography. These are things which must take up a good deal of our effort, and probably must consume a far greater proportion of the funds and manpower in oceanography than possibly in other fields of science.

I think, if you compare oceanography to meteorology, we must have a vast network of observing stations that serve as the sources of information. Those who are interested in the development of meteorology have their data collected for them. Oceanography has not really advanced to this stage, and we are involved both with the very expensive and very laborious job of collecting them.

From the tone of the two papers just given, it is obvious that most of the thought is being given to the mechanics of collecting and processing this information. I don't know how we will solve this problem. We all have to face it, but I think we should try to place it in proper perspective in our discussions. In planning our work, provision must be made for adequate effort in these phases of the work at sea and in the office, but more particularly, we must be certain that the scientific work can go on at the same time.

All of us know that pressure is on us to assemble our data, reports of cruises, and so on, and that before we have much chance even to look at past results, we have another cruise coming up.

I feel that we cannot work blindly, that we must have some sort of objectives that must guide the over-all program. Mr. Fuglister has assumed that temperature is important. I don't know whether it is or not. Traditionally we measure temperatures at every opportunity. Sometimes I wonder why. Everybody else does it, so we feel that we have to do it too. BT's are very interesting to the people who collect them, but beyond that I wonder if they are worth publishing.

I think we should have some sort of depositories where such information is available, but I would hate to see the literature cluttered with thousands of pages per year of nothing but BT traces. I can imagine nothing more dull.

So let's look beyond some of the problems of housekeeping in oceanography, and consider what our objectives are, and the means by which we can hope to solve some of the scientific problems.

MR. ISAACS: I think this same kind of objection could be leveled against the astronomers who spend a lot of time recording temperatures and classes of stars. They certainly spent a great deal of money and a tremendous amount of effort in very dull work that went on for years and years and years, but it was just exactly this that has given the basis for some astonishing breakthroughs.

How much foresight existed in this I am not sure, but certainly the populations of stars that have been recognized by this work have made possible some of the tremendous advances in astronomy.

I think you have to look at both sides of this business a little bit. One

could have leveled this same argument against the astronomer ten years ago, I am sure.

DR. ISELIN: As Mr. Fuglister pointed out, at the moment and for the time being, temperatures are the surest means of knowing the direction and something about the velocity of the water movements. We certainly need to be able to measure water movements directly, but until somebody produces a new gadget, temperature is our main means.

Where we have good navigational control, we have evidence that the current does move along the isotherms, at least as far as the swifter currents are concerned. I think in our western North Atlantic surveys, at any rate, we have been using temperature as our principal current meter, and have come to have considerable confidence in it. There are undoubtedly cross-current components which we would like to know about. We can only guess at their magnitude at the moment. There is room for a technical development here that I feel we have to achieve as soon as possible.

MR. VINE: The larger features of deep-sea temperature distribution were known before any of us were born, whereas nearly all the deep-sea cores have been taken within the period of our own active participation in oceanography. A couple of decades ago the ocean bottoms were described from one or two deep sea cores as a uniform flat carpet of sediment. Since then every time the number of cores increased by a factor of ten, we found that we were really only collecting data, but when the number jumped by more than a factor of ten, we began to gain insight into the sedimentary process. I think this progress is still going on.

As Mr. Fuglister has said, in the parts of the ocean for which we do have quite a bit of data, we may have reached the law of diminishing returns, but if we look at the deep sediments and temperatures in these same regions, we are down to such a small number of samples it would be well within our means to increase these numbers by a factor of ten, and that would increase our insight a great deal.

DR. DIETZ: We all agree the second most important property of the ocean, after being wet, is its temperature. This property will always remain a very important parameter, probably the most important of all for the ocean, not only for its own sake, but because of its connection with the circulation.

DR. DEACON: Since it is such a tremendous task to approach the temperature distribution in a statistical way, we need more physical understanding of the processes that produced the observed temperature distribution.

What are the factors that affect the temperature of the ocean? It isn't just the sunshine and conduction of heat through the water. It is, to a large extent, the circulation. Surely a good bit of careful thinking is necessary, however inadequate the present physical basis may be to start with.

MR. LAWTON: I am not an oceanographer, but for a number of years we have had occasion to cooperate with oceanographers, notably in 1936 when LORD KELVIN took Dr. Piggot aboard to make some of his historic deep sea cores.

I wonder if it is generally known that in the course of repairing ocean cables it is routine to take bottom temperatures? There is a vast repository of such information in the cable companies' files. Of course it is restricted to the routes of the cables. However, it has been taken over a great number of years, and extends down, on our lines for instance, to a depth of 2800 fathoms.

I am also a little surprised that in this discussion of ocean temperatures nothing has been said about the very sensitive nature of fishes, and how they respond to temperatures. We know, for instance, that the cod is very sensitive, and will only live, in great numbers, within a very narrow temperature range.

We have been extremely interested in the changes in temperature off Newfoundland in recent years, which resulted in a large body of cod being found over the ground where the cables lie, and causing fishermen to raise particular Ned with the cables for the last couple of years. We have had to spend sizable sums to move cables out of the way. We are hoping the cod will move up to Greenland soon.

As far as we are concerned, I can assure you temperature is of great interest; the conductor resistance of an ocean cable varies with temperature. It isn't possible to test a cable before laying it and knowing what the effect of the immersion in the water will be. It is only after the cable is laid that you can get accurate measurements of such effects. But we do have quite a repository of temperature information that I am sure is available to anybody who wants to come and get it.

DR. ISELIN: That is very exciting news. I realized you people probably had been taking bottom temperatures, but I didn't know you had saved them. The temperature on the bottom of the ocean is probably the thing we know the least about. Mr. Worthington could give us the number of readings we have in the North Atlantic. It is less than a hundred, I guess.

MR. WORTHINGTON: About 50 in deep water.

MR. LAWTON: The first thing a cable ship does over a cable break is to stream a buoy. A good time to take temperature measurements is during the lowering of the buoy.

DR. ISELIN: In some way that I don't really quite understand, Mr. Stommel slipped from my grasp and is not here today. Since his paper seems to be a very important one, I believe we really should think about it hard. Dr. Pritchard has kindly offered to read it to us.

4. WHAT DO WE WANT TO KNOW ABOUT THE DEEP OCEAN CIRCULATION

Henry Stommel
Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

In order to illustrate the kind of question which we have about the deep-sea circulation I propose to present a line of dynamic argument which would, if true, shed light on the problem of the determination of the level of no motion, the distribution of vertical velocity, the nature of mixing across isentropic surfaces, and the reason for the existence of the main thermocline. As the tale proceeds certain questions will arise which at present cannot be definitely settled. We will state the question, and then take, at the risk of being wrong of course, what appears to be the most reasonable answer, and then follow the logical chain to the next question. We will make use of real hydrographic data; no mention is made of idealized models such as two-layer oceans or rectangular basins.

Let us consider the dynamics of the mean flow in the deep ocean, far from land, and well removed from strong narrow currents like the Gulf Stream. For the sake of being definite I choose 32°N 51°W, a point in the Sargasso Sea to the east of Bermuda. How shall we describe the mean hydrographic conditions at this point? Shall we use the METEOR Atlas, based as it is on a very limited amount of data, but sampled at different times on different cruises? If so, we have a mean of sorts and we find the area is quite free of narrow currents. On the other hand we might take the data of a single cruise through the area. This will exhibit a certain lumpiness in the topography of the isotherms and isohalines, individual deviations from a smoothed topography of about 20 to 30 meters, and amounting to as much as 50 meters. It is tempting to regard the smoothed topography as representative of the mean, and to dismiss the bumps as purely temporary features, transient eddies, or as a result of internal waves. Although this latter course is consistent with the information on internal waves accumulated on the Bermuda thermometric cable during 1955 (at 32°N 64°W), it is not certain that all irregularities of the topography of isotherms and isohalines can be regarded as temporary. Fuglister has pointed out to me the similarity of an abrupt salinity discontinuity in the surface layers somewhat to the north of 32°N which both Iselin and he observed on separate cruises many years apart. Thus, in our quest of the "real" ocean, we are faced with the perennial problem of forming a realistic mean map of the distribution of ocean properties. In the following discussion I will use smoothed data from one of Worthington's recent cruises in the area. I have also used data from the METEOR Atlas and obtained substantially the same results. From ATLANTIS stations 5203 and 5210 I compute zonal horizontal gradients of temperature, salinity, and specific volume anomaly, and meridional components of geostrophic velocity. From ATLANTIS stations 5210 and 5215 I compute meridional horizontal gradients of temperature, salinity, and specific volume anomaly, and zonal components of geostrophic velocity. The vertical distribution of the east and north components of the geostrophic velocity, U and V, is shown in Figure 4.1. The depth of zero velocity for each component is undetermined by the method of dynamic computation. The three curves labeled A, B, and C give the velocity components referred to the bottom, 2000 meters, and 1000 meters, respectively.

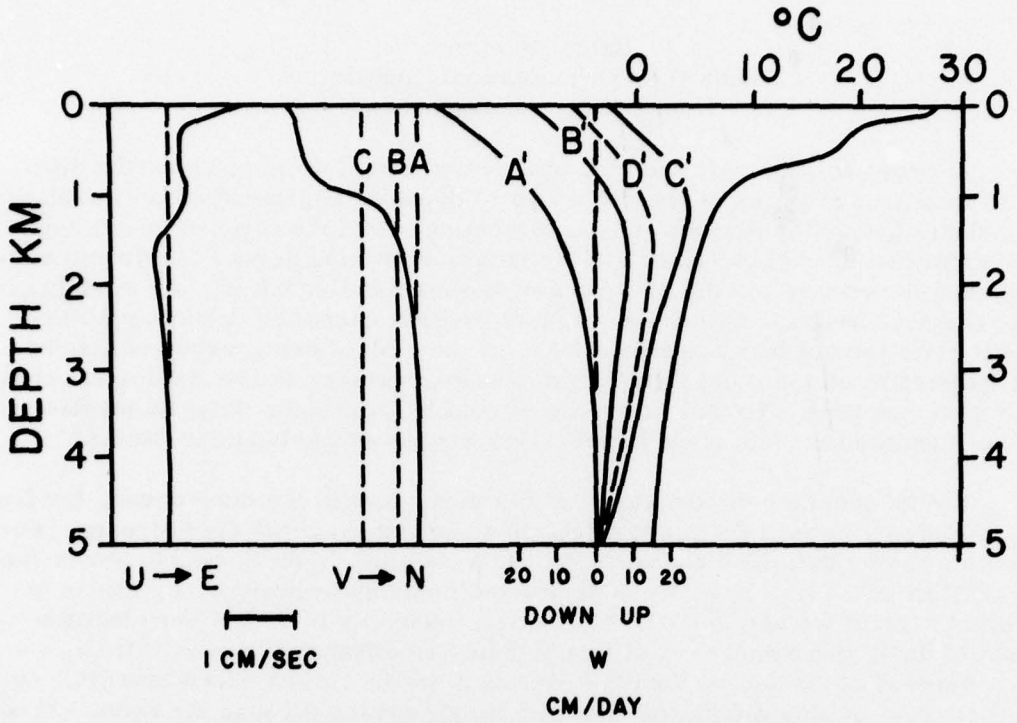


Figure 4.1 Vertical distribution of east and north components of geostrophic velocity, U and V

In view of our ignorance of the magnitude of viscous shearing stresses in the ocean (except at the surface) we are forced to make some assumption that seems, a priori, reasonable. Following Sverdrup and Munk we assert that in the deep ocean lateral shearing stresses are dynamically insignificant. We will also assert that the dynamical role of vertical shearing stresses is limited to a very thin Ekman-type surface layer — at most 200 meters deep.

The vorticity equation at any level Z (positive upwards) is

$$\beta V - f\frac{\partial w}{\partial t} + \frac{\partial}{\partial t}\left[\frac{\partial \tau_x}{\partial y} - \frac{\partial \tau_y}{\partial x}\right] = o$$

where x is positive eastward, y is positive northward, f is the Coriolis parameter, $\beta = \frac{\partial f}{\partial y}$, w is the vertical component of velocity, V is the northward component of velocity and τ_x and τ_y are the x and y components of the vertical shearing stresses. In asserting that vertical shearing stresses are concentrated in the surface Ekman layer we require that the essential dynamical balance is between the first two terms. In physical terms, layers of water moving toward the equator must shrink vertically, those moving poleward expand. The bottom of the Sargasso Sea is a fairly flat plain, thus whatever the field of the horizontal component of velocity, the vertical component must vanish at the bottom.

For any choice of level of no meridional motion at station 5210, we can therefore integrate numerically the above equation for w, starting with zero at the bottom. The three distributions of vertical velocity w corresponding to the three curves of meridional horizontal component, V in Figure 4.1 are shown as A', B', C'. How shall we choose the right curve? From the observed mean wind field one can compute the total horizontal divergence of the Ekman surface layer, in various more or less unsatisfactory ways. The order of magnitude appropriate to the area under discussion is -9 ± 3* cm/day (e.g. Montgomery, 1936): this is the vertical velocity imposed by the wind at the bottom of the Ekman layer. The dashed (D') curve between curves B' and C' is drawn to satisfy this boundary condition. Thus, we have, subject to our premises, determined the vertical velocity distribution, and the depth of no meridional motion is** 1750 ± 300 meters*. Can we now say anything about the heat (and salt) transfer processes? The equation may be written

$$U \frac{\partial \theta}{\partial x} + V \frac{\partial \theta}{\partial y} + W \frac{\partial \theta}{\partial t} = K \frac{\partial^2 \theta}{\partial t^2}$$

where θ is potential temperature and K a virtual conductivity coefficient. The zonal velocity U(t) is not known, unless we could assert that the level of no zonal motion is the same as that of no meridional motion. There is no justification to do so. It does seem from the vertical shape of the curve that the velocities must be small, which we will now assume, but this uncertainty makes the following deduction untrustworthy. Although V is not small, $\partial\theta/\partial y$ is small at station 5210, and it can be shown that dominant balance in divergence of heat transfer expressed in the above equation is between the vertical advective and diffusion terms. At the depth of small or vanishing vertical velocity (about 400 meters) we expect an inflection point in the temperature vs depth curve: this is of course what we mean by the thermocline - and it is interesting to note that this is slightly above the depth of the real thermocline. If we could allow a slightly larger coefficient of wind stress, this level would turn out to be more nearly 700 meters, the center of the thermocline. The uncertainty of the reference level for the zonal velocity component could easily account for this slight discrepancy. The chief point to notice is that the thermocline separates descending (warm) water above from ascending cold water below.***

* The limits of error are just order of magnitude guesswork.

** It is interesting to note that maximum vertical velocity occurs at the level of no meridional motion. This contradicts an assumption by Neumann (1955) that the depth of no horizontal motion (assumed identical for meridional and zonal components) is equivalent to a solid bottom.

***A similar analysis for the salinity can also be carried out. The order of magnitude of eddy conductivity is 5 $cm^2\ sec^{-1}$. If we imagine that the eddy viscosity cannot be very different from the eddy conductivity - not more than several orders of magnitude - we can show that this is consistent with the hypothesis that in Equation (1) the internal vertical shearing stresses are negligible below the Ekman surface-layer.

People who are unfamiliar with the mathematical tools of theoretical research often assume that the primary difficulties in theoretical work are mathematical. Of course there are mathematical obstacles. But in the simple example I have just outlined there is no mathematical difficulty: the questions raised are descriptive and physical ones, such as the following. Have we an accurate idea of the mean distribution of properties in the ocean? Indeed is there a mean and in what sense? A mean defined over a year or two of time is all I have in mind. Worthington's long period trends are not involved. Can we observe, directly, the depth of no meridional motion, or the mean vertical velocity? Are there independent means of measuring the coefficients of eddy viscosity and diffusivity? How big are internal shearing stresses in the ocean?

At every stage of our tale we make deductions that ought to be checked by observation - but the observations do not exist. At every stage we are forced to make assumptions about the physics of the flow because nobody has been able to figure out quantitatively what produces and controls the turbulence within the sea.

The foregoing analysis can be used over the whole Atlantic Ocean wherever east-west sections across the central regions of the ocean can be constructed. Depths of no meridional motion can be determined and the mid-ocean transports determined at each depth. By mass conservation the depths of no axial motion in the western boundary currents can be obtained. I have carried out a preliminary analysis for the entire Atlantic but I cannot enter into a discussion of the results here. The transports deduced are shown on Figure 4.2. Let me just state some of the rather remarkable deductions resulting from this analysis, and at the same time warn you that they depend upon the very same uncertainties which we have already mentioned above.

There is a narrow massive countercurrent beneath the Gulf Stream starting at 1600 meters; it flows all the way down the western coast of the Atlantic to roughly 40°S. Under the Brazil Current it flows in the same direction (south) as the surface current. This accounts for the weak dynamic topography in the Brazil Current. The total integrated transport of the Gulf Stream and Brazil Current is the same. The difference is simply one of a level of no axial motion. The discrepancy between theory and observation mentioned by Munk is resolved.

Because the thermohaline circulation is asymmetrical with respect to the equator, there is a net heat transport from the southern to the northern Atlantic Ocean, amounting to roughly 5×10^{14} gm cal sec^{-1}. Can this asymmetry in ocean heat transfer have some important meteorological consequences?

In closing let me say a few words about testing the truth of this tale. A physicist would, I think, say that the fundamental hypothesis that vertical shearing stresses are small in the interior of the ocean below the Ekman surface layer ought to be tested by direct measurement of the stresses. In the present stage of our observational technique I do not think that this can be done. We can measure deep currents directly, now, with Dr. J. C. Swallow's new neutrally buoyant floats. We are therefore in a position to check the deduced level of no meridional motion. In my judgment Swallow's device is the only means of direct deep ocean current measurement that meets all the demands. Would it be possible for the Office of Naval Research or the Atomic Energy Commission or some other gov-

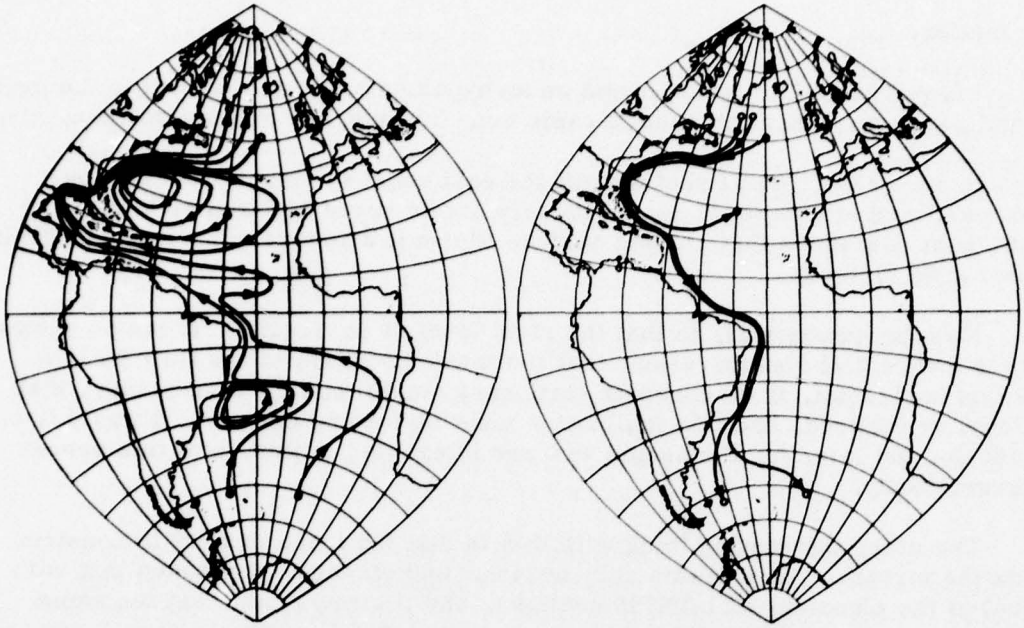


Figure 4.2 (a) Total transport in cubic meters per second ($x10^7$) for the layer above the depth of no meridional motion and (b) same as (a) for the layer below the depth of no meridional motion. (The process of vertical integration wipes out many fine details. Moreover, the transport lines are meant to be merely schematic. Thus the narrowness of the western streams is not properly scaled, etc. If the two maps are superposed and added, a simple symmetrical total transport picture, similar to Munk's is the result.)

ernment agency to underwrite the necessary ship time (say one month of ATLANTIS time) for Swallow to make, and teach us to make, deep current measurements in the Gulf Stream off Cape Romaine, and in the Sargasso Sea off Bermuda?

So far as the vertical component of velocity is concerned, there is no obvious way to check that. On the other hand, some of the deductions concerning the circulation of the Atlantic Ocean as a whole can doubtless be checked by more refined water-mass analysis, etc. Some adventuresome person might decide to try to drift submerged from Cape Hatteras to Rio de Janeiro in the deep thermohaline circulation - a kind of submarine Kon-Tiki. The trouble with such observations is that the phenomena they test are so far removed from the original hypothesis. That is why I inevitably come back to Swallow's machine. It makes possible a particular kind of measurement which is very critical in the testing of our physical ideas about the dynamics of the deep ocean circulation.

DISCUSSION:

DR. NEUMANN: I would like to say a few words about the level of no meridional motion. I think it is obvious that this depends on the definition used. And so far I think there are really no observational means to determine the level

of no motion.

I have devised a method based on an hypothesis. I applied this to the problem and some very peculiar results came out. I would like to mention them here.

In one of the typical sections off the east coast the level of no motion slopes eastward at a depth of approximately six or seven hundred meters. The Gulf Stream lies above this sloping surface where it drops very sharply to a depth of about 2000 meters.

Now the question is, is that the right level of no motion? It can be shown by some theoretical considerations that the lower boundary of the Gulf Stream cannot be horizontal, if we consider stationary conditions. It is necessary that this level be inclined, and this inclination must be toward the east. I would like to point that out later for the people who are interested in this, how this comes out theoretically.

The other interesting thing with this is that the mean density is constant across the stream. This seems very peculiar and strange. I checked this with several of the classical ATLANTIS sections, and it always came out the same. If you take this as a true result, then you will find that U, the meridional component of the total mass transport in the stream, is a constant. A consequence of this, theoretically, is that the sea surface in the direction of the Gulf Stream, must rise in the direction of the flow, even without any friction. This resulting mean sea level along the east coast checks with that of the precise levelings.

I think, in this case the level of no motion, as determined by the method of Defant, gives the right answer.

5. NOTES ON OPERATING NEUTRAL-BUOYANCY FLOATS

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Limitations

With the arrangement of listening gear at present used on board DISCOVERY II, tracking the floats is limited to weather conditions of force 4 or better (winds less than about 18-20 knots). This limit is imposed by noises generated in the ship and by waves striking its sides, as much as by genuine sea noise, and it seems likely that appreciable improvement could be made in this respect.

For similar reasons the depth at which the floats can be worked is limited. So far, 1900 meters is the greatest depth at which they have given a measurement. They can be heard when lowered on a wire to a depth of 3000 meters, but not much beyond that point - again being limited mainly by spurious "crash" noises rather than genuine background sea noise.

Increasing the transmitted power would probably involve increasing the complexity or bulkiness of the floats, and it is thought that, at present, more improvement can be gained at the receiving end. For the first measurements, in June 1955, listening was done on hydrophones consisting of 10 kc nickel scrolls, suspended on twin cable to a depth of about 20 feet below the water, and weighted with a sounding-lead to keep them approximately vertical. This arrangement was improved on, in October and November 1955, when the same hydrophones were attached to lines passed under the ship's hull and pulled up taut, the nickel rings being backed with sorbo rubber to prevent direct contact with the hull.

We are now making streamlined housings for the hydrophones in an attempt to reduce still further the interfering noises.

I doubt if we shall ever be able to use the present method of taking bearings in more than about force 6 winds, because the ship, when stopped head-to-wind, falls away so rapidly that there would be too little time to read off the time-differences of arrival of sounds at the hydrophones, as a function of bearing. Besides this, there is the difficulty of maintaining a fixed mark from which to work - using an anchored buoy as a radar target would be difficult in such conditions, both on account of poor radar ranges and the liability of the buoy to break or drag its mooring.

A Possible Way of Measuring Depth of Float More Accurately

The method at present used for checking the depth of the float is a poor one, giving a scatter of about $\pm$ 200 or 300 meters whatever the depth. An alternative method would be to lower from the ship a pair of hydrophones, with a known vertical separation, and to observe the difference in times of arrival of pings from the float, at the two hydrophones.

Assuming the wire is vertical, at a range of a kilometer from the float (horizontally) with a 100-meter separation between hydrophones, 2 milliseconds path difference is produced if the float is 30 meters above or below the mid-plane of the hydrophones. Of course, the wire will not be vertical, but a 5 degree tilt will produce, at the most, 100-meters error in depth, and only half this if the ship is 1/2 km.. away from the float. It should be quite easy to get within 1/3 km. or so of the float (if it is not moving too fast), and one could weight the lower hydrophone and buoy the upper one so as to keep that part of the wire as vertical as possible. Or one could measure the angle and azimuth (with a Carruthers wire-angle gauge) and make the appropriate correction.

For shallow depths this method could be worked with lengths of ordinary screened cable supporting the hydrophones, recovering them by hand. This would not be practical below about 200 meters, and a winch with an electric-conductor wire rope would be needed. Or one could connect the two hydrophones via an amplifier to a small battery-driven tape recorder, lowering the whole assembly on an ordinary winch. It would be necessary to stop, say at intervals of 100 meters, to let the machine record the time-differences at each of several known depths. A propeller-release thermometer-reversing frame could be fitted to indicate the true maximum depth reached, and to release a messenger to actuate a wire-angle gauge at the same time. One could play the tape back on to an oscilloscope, and since only the minimum time-difference need be observed it would not matter about the relative time scales of the recorder and oscilloscope.

Fixed Marks for Use in Current Measurements

We have been using dan buoys anchored on piano wire, and fitted with radar reflectors, as a means of fixing the ship's position, and checking the movements of the buoy by sounding over nearby features on the sea bed.

This requires a suitable deep-sea feature such as an isolated ridge or sea mount with a sharp enough discontinuity (such as a rise from a flat plain) to fix the buoy with an accuracy of the order of one-tenth of a mile. (The accuracy required in fixing the buoy naturally depends on the size of the displacement of the float that is being observed, but for velocities of the order of a few cm/sec, this kind of positional accuracy is desirable.) Such features are not very common and moreover may, if large, distort the current from what they might have been in more "typical" conditions.

Alternatively one could put a "pinger" on the bottom somewhere near the anchored dan buoy and fix the buoy relative to that. This would require some improvement in pinger performance (or listening) to allow it to be done in, say, 5000 meters depth.

It would be very much easier if the ship's position could be fixed independently of a buoy and marks on the sea bed, e.g., by Decca or LORAN, or some other navigational aid.

However, LORAN will not give one-tenth mile accuracy in most places, and Decca does not reach far enough out into deep water to be useful. I believe there is an American system for navigation to this kind of accuracy, even out to

several hundred miles off shore, but do not know any details of it, or how big an area is covered by it.

With a current of the order of 20 cm/sec, it might be sufficiently accurate to use LORAN positions; and, indeed, it would be difficult to keep track of such a quickly-moving float if one had to be constantly going back to check a fixed mark, instead of sticking to the float all the time.

Will the Floats Tend to Sit on Boundaries Between Water Masses?

It has been suggested that, since the water density in situ depends on temperature, salinity and pressure, one might tend to find that these floats stabilized themselves at the depths of boundaries between different water layers and that the velocities so observed would not represent the movements of the main bodies of water. I think this tendency is unlikely, in view of the strong dependence of the water density on pressure compared to its dependence on T and S. The tables given by Zubov and Czihirin show, for example, that approximately equal increments in density are caused by:

a. a temperature change from 8 degrees to 2 degrees C

b. a salinity change from 34 percent to 35 percent

c. a depth change from 1000 to 1200 meters

These have to be modified slightly when allowance is made for the dependence of the density of the float on pressure and temperature, but the general conclusion is still that the pressure effect is by far the most important.

DISCUSSION:

DR. ISELIN: Dr. Munk has offered to give us his thoughts on what we want to know about deep sea tides.

6. DEEP-SEA TIDES

Walter H. Munk
Scripps Institution of Oceanography
La Jolla, California

This is in lieu of my scheduled talk. I give up my claim for philosophizing at the end of the meeting.

Mr. Stommel mentioned the results of some of Swallow's observations. There are five deep-sea drifts; the results of three have been published. These are by far the best measurements of their kind, and I thought it might be well to put these five measurements on the board before we start.

Results of Dr. Swallow's Measurements

Place	Time	Depth	Speed	Direction
Lisbon	May 55	800-1500 m	6 cm/sec	250°T
	June	400	2.4*	300°T
Gibraltar	October	1200	9.1	021°T
	7-9 November	1900	2.2	036°T
	10-12 November	1900	1.7	160°T

* $\pm$ 10 cm/sec tidal ellipse

Note that in the same area, at the same depth, separated by only a few days the currents almost reverse in direction. These measurements do not support some of the hope expressed earlier that in the deep ocean relatively few measurements might suffice to establish the current pattern. These measurements are all based on averages of the order of three days, and they represent the mean drift over that period of time.

Another interesting thing is that on the second of these measurements there was superimposed an apparently semi-diurnal oscillatory motion of ten centimeters per second. The tidal ellipse agreed well with expectations. This velocity is very much larger than the mean; ten centimeters per second compared with 2.4 cm/sec.

In the next group there was no discernible tidal motion, and only in part can this be explained, according to Swallow, by the fact that the October measurement was taken during neap tide.

I want to emphasize that the tidal motion can be very much larger than the mean drift. Therefore, when we are talking about motion in deep sea, it is likely that many of the phenomena will be more concerned with the tides than with ocean

currents, and this is why I asked to speak briefly at this time. Such things as ripple marks on the bottom, and things of this kind may be related to oscillating tidal currents rather than to mean drifts.

I don't think it is clear at this time to what extent ordinary surface tides and internal tides might contribute to tidal motion near the bottom. It is quite possible that internal tides, about which we know very little indeed, might be more important than surface tides.

There are quite a few interesting problems concerning deep-sea tides. It is not generally known, I think, that there is an enormous dissipation of energy by tidal friction. Once a day or so the same amount of energy is dissipated as is present at any given moment in tidal energy. This means that any theory of deep-sea tides that ignores tidal friction is bound to be way off.

While I was in England, I saw the new measurements being made at Herstmonceaux, the new Observatory to which Greenwich has now been moved; I think there is a fair chance that measurements of basic time standards are appreciably in error due to the deflection of the vertical by the ocean tide. This might represent the largest single error in the measurements of astronomic time. Deep ocean tides should be known to correct tidal variations of gravity. Thus, aside from their intrinsic interest, ocean tides will also give other geophysical information of interest. Here is another example. The equilibrium tide is reduced due to the yield of the solid earth, and it is by no means clear whether such yield, evaluated for the Atlantic, should give exactly the same results as the Pacific. This problem was actively discussed by Kelvin many years ago as a means of studying the crust of the earth, but I do not believe any thought has been given to it in recent times.

One reason why one might profitably bring up the topic of deep-sea tides at this meeting is that I think there is a real chance of making a contribution in this field during the next ten or twenty years. The present method of tidal prediction is of course not a theoretical solution of the tidal problem, but rather an extrapolation of locally observed values. The real scientific problem is to account for the observed tides from the known motions of the sun and moon, and the shape and depth of the ocean basins.

There are two new features in this picture that might make us more successful than the last generation. First, the very large computation difficulties associated with the solution of the theoretical problems can, to some extent, be handled nowadays by modern computers - but I want to say here the problem of tidal friction makes this a more difficult thing than might appear at first. We have no clear feeling or knowledge of how this very important dissipation of energy is to be taken care of in the theory of deep-sea tides.

The other new feature is that we have, in fact, several possible methods for measuring the tides in the deep sea. Considering the great observational difficulties it might seem reasonable to attempt first order solutions using computers; then determine from such solutions critical places where measurements should be made; make measurements at those places; and then go back to the computers for better solutions. In this way we might make some progress in the

matter of deep-sea tides.

Suppose for a minute that we are in a position to measure deep-sea tides. We may present some very rough order of magnitude guesses as to what sort of effort would be needed to give some significant first order results concerning diurnal tides. It seems that if one could occupy ten carefully selected stations for two days at two intervals a fortnight apart one would gain significant information on the tides in the North Atlantic. For the world as a whole, the number might be of the order of 100 stations. So you see this represents a large effort, but an effort which is not altogether out of the question.

Now how about the methods for measuring tides in the open sea? There seem to be two different methods of attack: one, by measuring tidal currents, as Swallow has done, and the other by measuring something related to the elevation of the free surface.

How about the relative merits of these two methods? First of all, Swallow's results show that tidal currents may well constitute the major part of motion at great depth and if one can measure the currents at all, one learns something concerning tides. Tidal elevations, on the other hand, represent a change in depth of one part in 1000 and the required accuracy of observation is in the order of one part per million. So from this point of view, measuring the currents would be simpler.

What about "noise" from other geophysical events which could compete with the measurements of tidal signals? We know very little about these, but there is some indication that the noise in the required frequency range is worse in the case of measuring motion than it is for measuring elevation. From the point of view of signal to noise, it might be better to measure elevation.

Regarding the total information obtained, tidal currents give a vector, elevation a scalar. Currents provide information concerning speed and direction, twice the information obtained from measuring tidal elevation.

Finally, the difficult problem of bottom friction enters critically in the evaluation of the tidal currents.

I simply want to show here that the box score is rather evenly divided, and that it would be very much worthwhile to carry out both types of measurements.

Now regarding the methods for measuring elevation: I think there are two methods now on the horizon. Dr. Maurice Ewing and his collaborators have been able to increase the accuracy of fathometers to a point where changes in depth by something like one foot could be measured. Over regions with flat bottoms, measurement of tides can then be achieved. Mr. Frank Snodgrass, at the Scripps Institution, has been able to measure changes in bottom pressure with sufficient accuracy. It looks as if changes in elevation of a few millimeters could be measured with the instrument at oceanic depth. So far the deepest station was in 1000 meters depth, at the edge of the continental borderland and 100 miles from shore. In three days of continuous recording it was found that the tide there does not differ appreciably from that at La Jolla. I do not think this is

an expected result.

Measurements of tides in the open sea is, I think, a subject that very properly falls under the general category of topics discussed at this meeting.

DISCUSSION:

MR. ISAACS: Is this dissipation of the tidal energy on land widespread?

DR. MUNK: Jeffreys' statement that two-thirds of the energy is dissipated in the Bering Sea is, I think, open to question, if for no other reason than that new shallow areas have been discovered since Jeffreys made his calculation 25 years ago. But I don't think his calculation can be off by more than a factor of two; it is in nice general agreement with the observed change in length of day since Egyptian times.

DR. EWING: We have given considerable thought to the possibility of measuring by an echo sounder the elevation of the water above the bottom, and we believe that there are plenty of areas that are sufficiently flat to make this a worthwhile measurement. We think it is perfectly easy to make the equipment to make the measurements with useful accuracy. If you state what the useful accuracy would be, I might be a little more firm about that statement.

DR. NEUMANN: Isn't it necessary to obtain the stratification when evaluating measurements of elevation?

MR. VINE: We might know the vertical stratification over the period of time involved in tidal measurements. If we are looking for essentially a 25-hour tidal period, we would only need to be sure that our internal waves were not bothering us too much within that 25-hour period.

DR. MUNK: That is a ticklish point, though. An internal tide might cause a change in the sound velocity of just the same period as the tide to be measured, and it might be troublesome to correct for it.

7. USE OF DESCRIPTIVE DATA IN STUDYING OCEANIC CIRCULATION

Warren S. Wooster
Scripps Institution of Oceanography
La Jolla, California

I would like to say first of all that I am specifically excluding from my remarks certain topics. For example, the direct observation of currents, the use of buoys, the use of radioactivity measurements, and the Atlantic Ocean. This omission is not because I do not think these are important, but rather because either I do not know anything about them, or I do not feel that it is my place to talk about them.

Furthermore, I would like to make it clear that in talking about descriptive data I mean essentially classical oceanographic data, that is to say measurements of the "standing crop" of temperature and other non-living concentrations.

In considering the use of these kinds of data to study circulation, I think it is important to keep in mind their limitations - although the "character" of subsurface waters can be established by adequate description of the distribution of properties, the motion of subsurface waters can only be inferred, or perhaps I should say guessed at, by geostrophic or isentropic measurements. At depths much below a thousand meters, these methods leave much to be desired.

Certainly the trend in the near future is toward direct observations of both surface and subsurface circulation. It seems to me that descriptive oceanography in the Pacific has been curiously deficient in the free-wheeling, imaginative speculation that has characterized recent Atlantic oceanography. Although the Pacific has been considered as a whole, as a unit, in theoretical studies, it has not been so considered in descriptive studies ever since completion of the chapter on water masses and currents in "The Oceans" nearly 15 years ago.

One reason for this has been the poverty of oceanographic data from vast areas of the Pacific. The charts published by Vaughan in 1937 show a rather striking difference in coverage of oceanographic stations between the Atlantic and the Pacific at that time. Since the end of the second World War many of these blanks have been filled in in the Pacific, but in the meantime the descriptive oceanographers have been so busy collecting and processing data that they haven't taken time to think about them.

Except for filling certain large blanks (such as the zone between 10° south and 40° south, essentially north of the area covered by the DISCOVERY expeditions, and also the layer below 3500 meters) the day of the exploratory or survey-type hydrographic cruise in the Pacific, is about over. The present trend seems to be toward two types of operations, and you will forgive me if I use Pacific names for these.

The first we might call the NORPAC-type of operation. This is a multiple-ship, quasi-synoptic survey of a large area. On the NORPAC cruise in 1955,

some twenty ships speckled the whole Pacific north of 20° latitude, with about 1000 stations, all during a 50-day period. A similar operation is proposed for the equatorial Pacific in August 1956.

Such surveys, in addition to providing a "snapshot" of a large area at one time, also provide an invaluable reference point for subsequent investigations of time changes.

The NORPAC data, by the way, are going to be published, we hope, within a little over a year, together with a rather complete atlas showing the distribution of the properties measured in the North Pacific north of 20° north latitude.

The other type of operation which seems to be coming into fashion we might call the EASTROPIC-type. This is an intensive study of the features and phenomena which have been revealed by previous exploration. During the 1955 EASTROPIC Expedition, two ships studied certain features which analysis of previous data had shown to be intriguing, such as the Equatorial Undercurrent, north boundary of the Peru Current, a large dome (thermal anticline) off the coast of Costa Rica, and the vertical velocity structure of the Equatorial Countercurrent.

In these studies, rather than make classical type observations, direct observations of velocity (by parachute drogues and geomagnetic electrokinetograph) and quick measurements of thermal structure (by BT) were favored.

To give you an example of the kind of description which seems to me to reveal these "joints" in the ocean, as Mr. Stommel put it, which provide us with intriguing problems, I would like to discuss the following illustrations.

Figure 7.1 is just for the purpose of orientation. The section I want to talk about is the westernmost section which extends from 30° north, down to about 7° south latitude.

Figure 7.2 is a bathythermograph section made on this leg, showing the thermal structure to about 900 feet from 30° north to 7° south latitude. Joints would seem to me to be particularly evident in this picture.

First of all the pronounced depression in the isotherms at the equator is a common feature in the central and eastern Pacific. Second is the rather striking change in the distribution of temperature at about 18° north. Other than that, the picture is more or less the usual meridional picture in the Pacific; no clear counter-current or north equatorial current is evident at this longitude.

Figure 7.3 is the same section again, but in this case showing the distribution of oxygen to 500 meters, superimposed on the temperature structure. Looking again at these two joints, or places of interest, the equator is marked by a trough in the isentropes. This is true of all the properties measured. Similarly, at 18° north, there is a very striking break in properties. This would, then, seem like an obvious place to spend some time studying this feature.

And lastly, Figure 7.4 shows the distribution of oxygen from the surface

to 2500 meters, again on the same section, and here one reduces the discontinuity to somewhat better perspective. One is also struck by the fact that the discontinuity marks a change in the oxygen distribution. Very low oxygen is a characteristic of the north equatorial region. Also, the oxygen minimum is pinched off in the region of the equator.

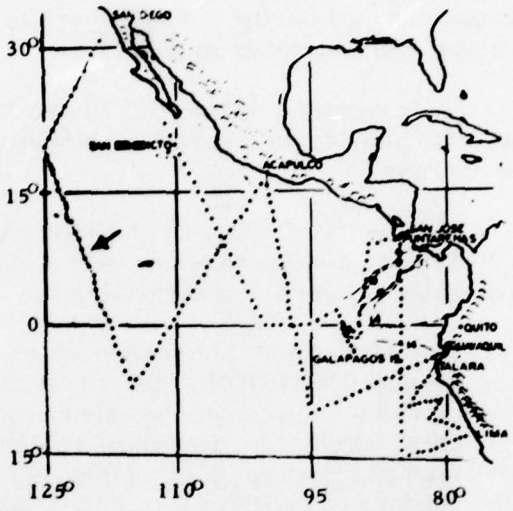


Figure 7.1 Location of section discussed

In my estimation a job of high priority in the descriptive oceanography of the Pacific is the synthesis of existing oceanographic data and the generalization about circulation and distribution of properties in the Pacific as a whole. This generalized picture is required as background information by applied oceanographers, such as fisheries scientists, and is essential in the planning of future work at sea.

Revelation of the significant features in the oceanwide distribution of properties may permit simplification of long-term studies of time changes. But what I think most important is that a successful general description of this sort should stimulate inquiry into the physical and biological causes of the observed distributions.

Every advance in instrumentation, and each intensive application of exist-

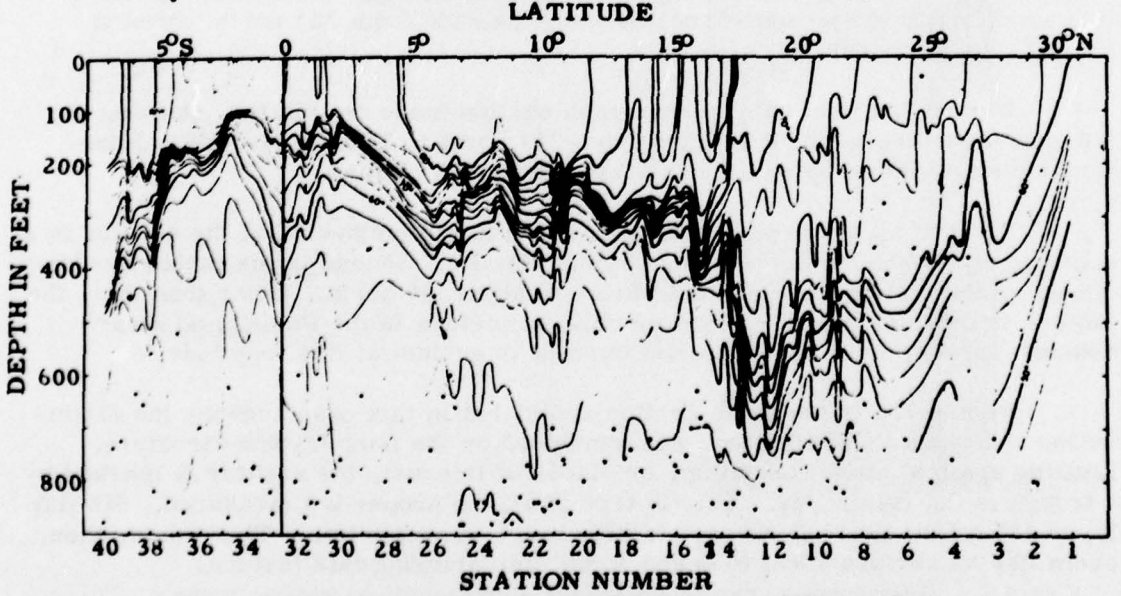


Figure 7.2 Bathythermograph record for section of Figure 7.1

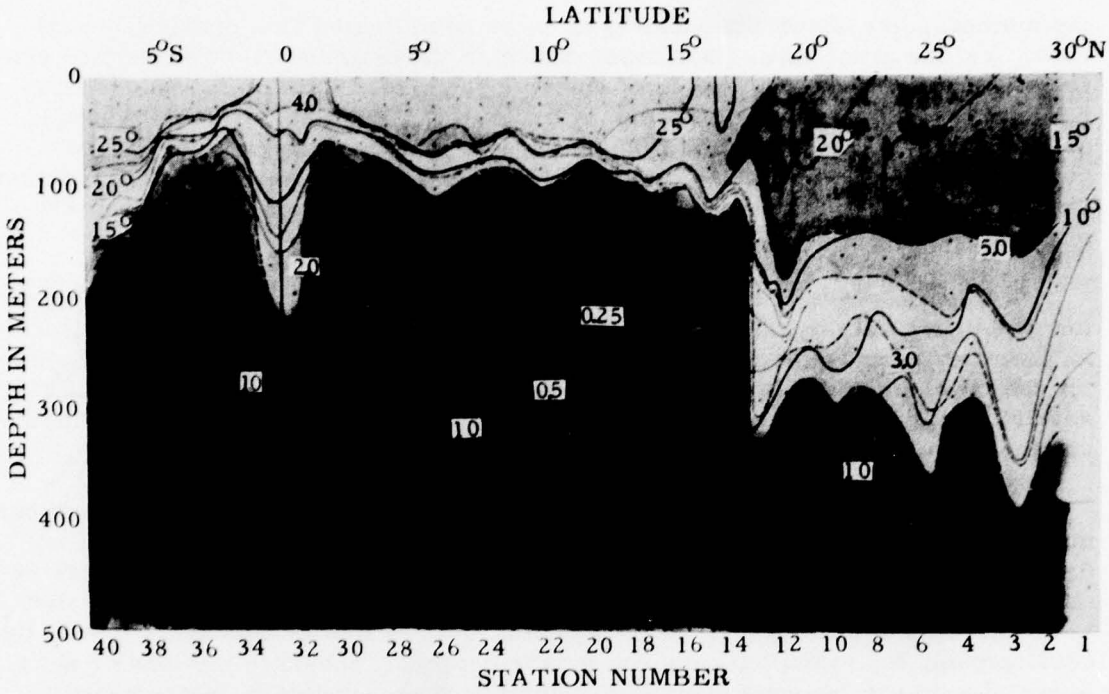


Figure 7.3 Distribution of oxygen (ml./l.) to 500 meters for section shown in Figure 7.1, superimposed on temperature structure

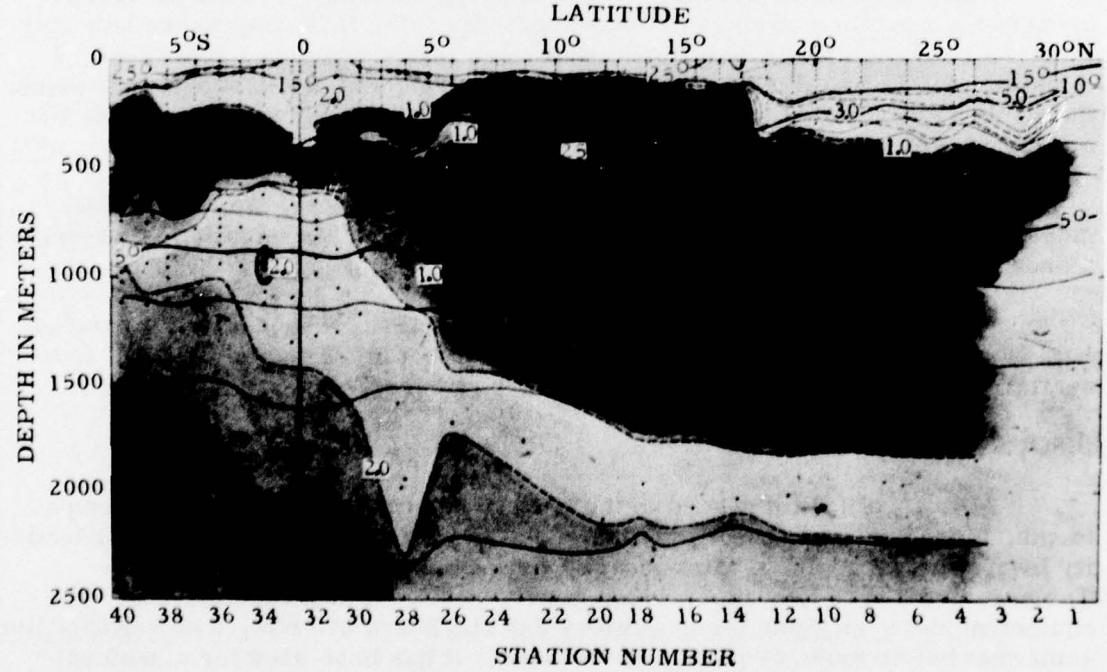


Figure 7.4 Distribution of oxygen (ml./l.) to 2500 meters for section shown in Figure 7.1 superimposed on temperature structure

ing methods, has shown the ocean to be more complicated than previously realized. Yet the great bulk of our observations of the distribution of properties continues to be at discrete points or of discrete samples. Sometimes, in desperation, we try to overwhelm the ocean by brute force, spacing our sample bottles as close together as they will fit on the wire, or locating stations ever closer, yet the only property which is commonly observed in situ as a continuous function of time or space is temperature, and that usually to depths no greater than 250 meters.

Despite years of research, there is not now generally available a device for measuring salinity, or conductivity, in situ, as a continuous function and with an accuracy comparable to the conventional method. There is now some promise for the development of in situ oxygen devices, but no perceptible progress in the development of continuous measurements in situ of any other properties which are commonly measured.

I believe major emphasis on the development of continuous recording measurements of certain properties in situ, salinity, or conductivity, seems called for now to make rapid and less expensive observations at sea, to reveal more accurately the complexity of distribution of properties, and to permit time studies to be carried out effectively. Although compromises will be necessary during the development, for example, compromises in depth and accuracy whether or not it is possible to use the equipment from a moving ship, eventually instruments should be usable to the greatest depths in the ocean, and with accuracy no less than that available with present methods.

Since the Pacific oceanographers have not been able to keep up with the present accumulation of classical oceanographic data, it is easy to see how they might be overwhelmed if they should have such instruments as I described. I think this can be avoided if, first of all, the instruments are designed to present their information in a digestable form. I believe Mr. Snodgrass is going to talk about this later on.

Secondly, it seems to me that with continuous traces, we have a little more hope in machine methods of analyzing data in that interpolation is no longer a problem.

And lastly, if the oceanographer no longer feels responsible for processing, analyzing and cataloging every observation that he collects, I think it is essential now that the oceanographer regain supremacy over his data.

DISCUSSION:

MR. VINE: With regard to the conductivity meter for special measurements, there has been a program at Woods Hole for many years to use conductivity insofar as we could. I think most of you are familiar with the Salinity-Temperature-Depth Recorder, which goes to depths of about 100 meters. The equipment has been made for laboratory and shipboard use now, with sensitivities somewhat better than .01 parts per thousand. It has been used for a year with fairly good results.

It is hoped the next step will be to make this instrument operate at the end of a cable. We tried this about two years ago and found that moment just a little too premature to get the necessary presentation. We hope perhaps in another year or two we will actually accomplish this.

With regard to the worry about too much continuous data, I still find it very difficult to believe that in this business of ours, where we have so few observations, particularly at depth, we need be worried about being swamped with our own data for the next few years. The data Mr. Fuglister was talking about occupied less space than a cord of wood. Just as anybody who is cold is glad to get a cord of wood and have the place to burn it, anybody interested in the ocean would be perfectly willing to go through a cord of data.

DR. HERSEY: There is one point about taking continuous data that should not be forgotten, and that is that it is much easier to forget about sections of poor continuous data than to forget about poor BT's.

8. THE RESPONSE OF THE ATLANTIC OCEAN TO KNOWN CLIMATIC VARIATIONS

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Woods Hole Oceanographic Institution
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In an oceanographic expedition as undertaken in the early years of this century, the physical oceanographers concerned themselves chiefly with oceanographic stations in which serial observations of temperature were made at a number of depths and water samples were collected for salinity analysis. From these basic observations they were able to describe the density distribution and from this the general oceanic circulation. In the first thirty odd years of this century most of the Atlantic Ocean was explored by various expeditions of this kind and accurate measurements had been placed throughout it so that no large areas remained entirely uncovered.

In 1936, with the publication of the METEOR Atlas of subsurface temperature salinity and density, the general feeling seems to have been that no further exploratory cruises were necessary in the Atlantic; at any rate none were made. A number of papers had been written on the circulation problem with somewhat different conclusions, but in all these papers it was implied that what was true about the ocean in 1936 was likely to remain true in the foreseeable future. If this were indeed the case the value of a great many more oceanographic stations of the conventional kind would be small.

It was not until 1954 that any serious doubts arose about the more or less steady Atlantic circulation which has been generally accepted. At that time some new observations strongly suggested that the dissolved oxygen in the deep waters of the western north Atlantic had been palpably depleted since the original surveys were made. This possibility seemed to justify a revival of exploratory oceanography and in the last two years four months ship's time have been allotted to a re-examination of part of the western north Atlantic. I would like to talk about the results of this work which seem to bear closely upon the theme and purpose of this meeting.

In the first place it seems probable now that the oxygen in the deep and bottom waters of the Atlantic is in fact being depleted. Figure 8.1 shows a comparison between the oxygen values at a depth of 2500 meters, in about 1930 according to Seiwell and in 1954-5 according to the measurements made on the recent cruises. It is difficult to judge from these scattered observations just what average loss of oxygen has taken place but it appears to be in the neighborhood of 0.3 ml./l.

This figure was corroborated by recent measurements in the Caribbean Sea. In this basin the deep water is cut off from the deep water on the outside and is nearly homogeneous. As a result of this homogeneity a comparison between old and new oxygen is more reliable than one made in the open ocean where, as you can see, there are marked geographical changes. In the Caribbean the average oxygen loss was .3 ml./l in 21 years.

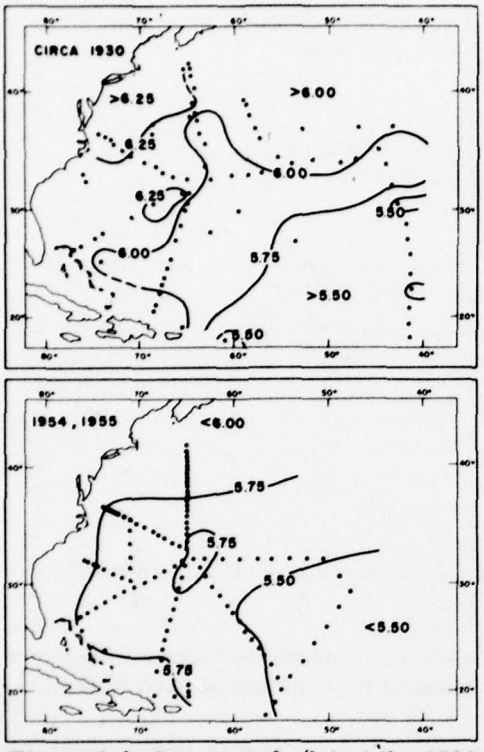


Figure 8.1 Oxygen (ml./l.) at the 2500 meter level showing the loss since 1930

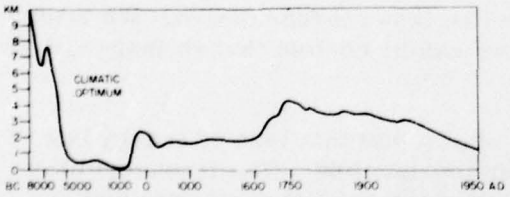


Figure 8.2 Glacier variations in Norway over the last 12,000 years (from O. Liestol in Ahlmann 1953)

An oxygen consumption rate of this magnitude places certain age limits on the water in which it occurs, because water can only contain a limited supply of oxygen when it is saturated. If we assume that this rate of consumption has remained the same since the water was last renewed by contact with the atmosphere we find that this event cannot have occurred much earlier than the late eighteenth century.

There is a great deal of evidence that a cold climate variation in the North Atlantic came to an end around that time, and one can reasonably suppose that bottom water would be formed in greater quantities during a cold variation than during a warm one. In Figure 8.2 we see the average variations in the lengths of Norwegian glaciers; the time scale is logarithmic so that the recent and better known changes can be seen in more detail. At the left we see the recession of the last remnants of the Wisconsin ice sheet. After the climatic optimum in the Stone Age, the glaciers made a short advance about 500 B.C.; there was a brief recession during late Roman times, after which the glaciers advanced very slowly until 1600 when there was a rapid advance to the middle of the eighteenth century, when, as you see, the glaciers were at their furthest point of advance since about 6000 B.C. According to Ahlmann this scheme was followed roughly by other glaciers in the North Atlantic area such as those in eastern Greenland, Iceland, and Sweden, so far as is known.

I would like to put aside for the present any speculation on the subject of bottom water conditions during the Wisconsin period, but perhaps I may be permitted to assume that the bottom water formed during that period was replaced by something of the sort which we find today at some time since the recession of the ice-sheet. During the period of increasing cold from about 500 A.D. one might suppose that the bottom waters of the Atlantic were replaced fairly regularly, especially during the abrupt change for the colder between 1600 and 1800. There is no necessity for thinking that the bottom water was formed catastrophically at the end of the eighteenth century. What is indicated by the oxygen data is that the last renewal of this water took place at that time, presumably because the winters subsequently were not sufficiently severe to produce water of equal

or greater density in large quantities. What we can conclude from the loss of oxygen in the Atlantic deep water is that with rapidly improving climate at high latitudes no appreciable amount of bottom water is formed, but as soon as the climate deteriorates in the North Atlantic, we or our successors, should begin to look for new bottom water in the deep basins.

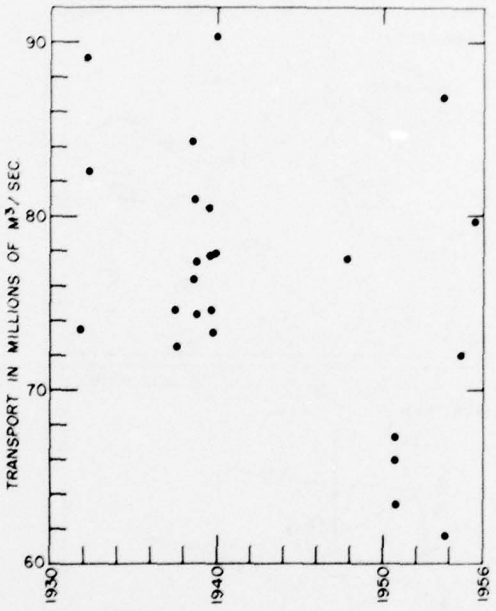


Figure 8.3 Computed volume transports relative to a surface at 2000 meters

In the main thermocline layer the effects of the climatic amelioration are not so obvious; however, there is some evidence that the intense warming at the higher latitudes in the North Atlantic during the present century has affected the volume transport of the Gulf Stream. In Figure 8.3 are plotted the computed transports of all the Gulf Stream crossings made by the ATLANTIS. They are divided into two groups, interrupted by the last war. There is a very high scatter in the values, particularly in the later ones, but it still seems likely that a real loss of transport has taken place in the last twenty years. If the groups are averaged, the decade 1930-1940 gives a value of 78.8 million cubic meters per second and the half decade 1950-1955 71.0 million, a drop of 10 percent. There are 16 crossings in the first period and only seven in the second, so the more recent data are less representative. We need to make a great many more sections before we can be certain that so large a drop in transport has taken place.

Further analysis of the computations shows that this loss of energy has taken place in the waters in or below the main thermocline. The transport between 0 and 500 meters has decreased seven percent while that between 500 meters and 2000 meters has decreased 15 percent. The seven percent drop in upper layer is entirely due to the slope of the 500 decibar surface relative to the 2000 decibar surface. In other words if we assume that the 500 decibar surface is level we actually find that the average computed transport above that surface has increased.

The loss of energy then, is confined to those waters colder than 15 percent which have their source in the higher latitudes of the North Atlantic, where the amelioration of the climate has been most marked in recent years. Figure 8.4 shows the difference in winter temperatures between the year 1930 and the twenty years thereafter. The contours have been taken off Willett's figure; they are in degrees Fahrenheit. The largest warm anomalies are in the extreme North Atlantic and in the Norwegian Sea. There are two principal ways in which this warming can affect the Gulf Stream transport. Those who believe that this current is primarily a wind-driven one may conclude that surface winds have diminished due to the lesser contrasts between the equator and the poles. Those who

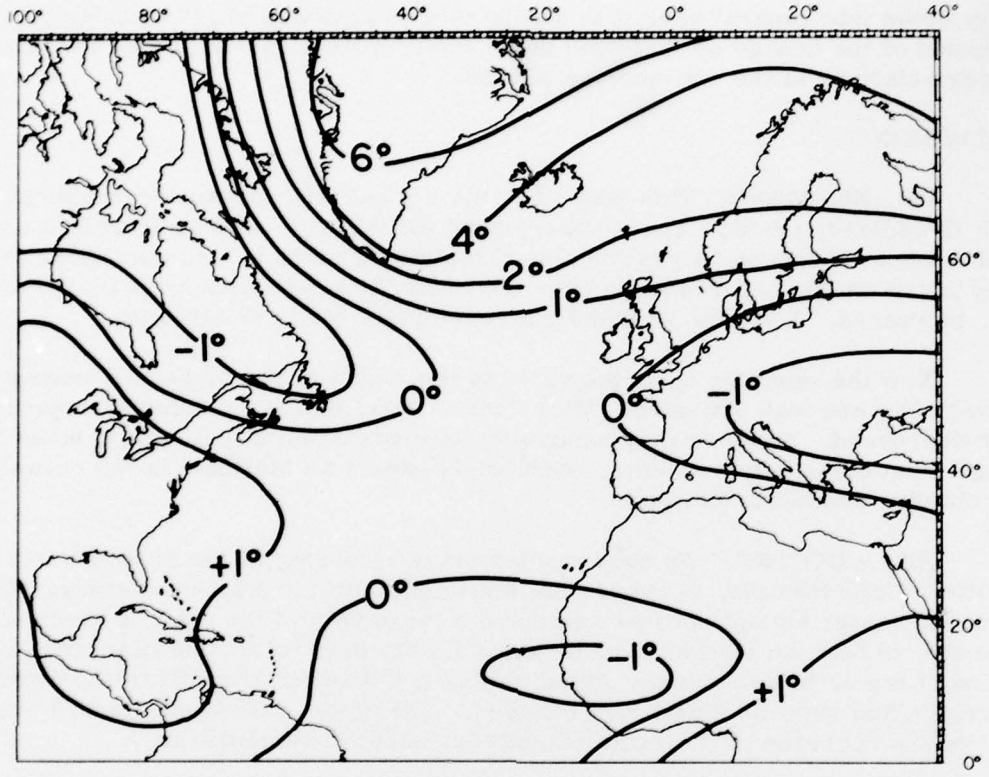


Figure 8.4 Twenty-year changes of the mean winter temperatures (°F) centered on 1930. (After H. C. Willett)

believe that the thermocline structure of the oceans depends more upon the heating and cooling processes which take place at the surface at varying latitudes may conclude that a warming of the atmosphere at high latitudes will cause decreased circulation regardless of winds.

However the mechanism works, there is evidence that the Atlantic Ocean is constantly changing as it adjusts itself to climatic changes. We cannot say how much the oceans as a whole contribute to climatic changes, most likely their effect is thermostatic, since we have had some indication that increasing atmospheric warmth at high latitudes results in loss of transport in those currents which carry warm water to the higher latitudes. Since precise oceanography is such a young science we are far behind the glaciologists and meterologists in interpreting the effects of climatic change on our medium.

If we are to contribute our due share to those studies we must, I believe, emphasize exploratory oceanography more. We will have to make more frequent, deeper and more accurate surveys than have been made to date. In particular, the high latitudes in the North Atlantic call for investigation on a year round basis, since it is here that the changing conditions seem to have the most effect.

As new techniques such as radio-chemistry and long-period recording

buoys come into general use, it is all the more important to get a really good description of the oceans so that such new measurements can be placed in their proper relations to the sub-surface climate.

DISCUSSION:

DR. NEUMANN: This last map, mainly showing the winter temperature over Greenland, perhaps can be interpreted differently, not resulting in a decrease but an increase in atmospheric circulation, since I think during the last forty years or so the circulation over the North Atlantic Ocean has, on the average, increased. I wonder what the meteorologists would say to this.

Now the warming up in the north is the result of the increased transport between low and high latitudes. Your results show that the volume transports have decreased. So there may be another interpretation if you look at those things from other points of view. One would expect an increase in the oceanic circulation, and not a decrease.

DR. WOOSTER: We have made a start in looking at the situation in the Pacific. Unfortunately, there are not sufficient data for a specific study. The Carnegie expedition did not run oxygen on a large part of the trip. I have only been able to find ten stations which reached sufficient depths and covered a time period of ten or twenty years. About half of the observations fall in the equatorial region and show no significant changes. The others fall in the North Pacific, and show a decrease of the same magnitude, about 0.3 milliliters.

DR. NEUMANN: I would like to make a correction in my statement about the speeding up of the circulation. R. Scherhag tried to account for the warming up by increase in the ocean circulation. This held very well for the decade 1910 to 1920, and 1920 to 1930, but in the decade 1930 to 1940 the circulation speed decreased despite the fact that warming still continued. So that the connection between the increase of circulation and the warming up in the high Atlantic is in doubt from that particular point of view.

Pettersson, by taking differences in winter average maps of sea level pressure the first twenty years of this present century, and back to the previous twenty years, found that in western Europe and the eastern Atlantic, there was a great increase in wind from south to north.

DR. WEXLER: We in the Weather Bureau repeated that work using not only the same area, but the whole hemisphere. We also did it for other zones and found it to be true that during the decades 1920 to 1940 there seemed to have been a phenomenal increase in the southerly component of wind in southeastern Europe and the eastern Atlantic, which would transport air more rapidly to Spitzbergen and would account for this warming up, and would also perhaps have some effect on currents, too.

DR. FLEMING: I seem to be picking on temperature observations this morning, but there is one thing I would like to point out in connection with these effects of climatic changes.

Several years ago I thought it would be interesting to classify some of the marginal seas on the basis of whether they represented areas of dilution or concentration or areas of heating or cooling. What became obvious immediately was that it was the water balance in these areas that was the critical thing. And whether there was dilution or concentration, then,really determined the heat budget in these areas.

I think this has to be considered in connection with this formation of deep water. If you compare the North Pacific and North Atlantic, you discover that no deep water is found in the North Pacific, and the reason for this is not one of temperature, but of surface salinity. It gets just as cold, I am sure, up in the Bering Sea as it does in the North Atlantic, and therefore this is not a function of temperature, but one of the salinity and the water balance, whether there is concentration by evaporation or dilution by precipitation.

I would therefore suggest that instead of the severity of the winters being interpreted in terms of temperature, the water balance, the evaporation-precipitation ratio over the ocean areas might be more important, on a climatic basis. The formation of deep water occurs, if your hypothesis is correct, under those conditions which led to the flow of relatively high salinity water into the latitudes where it gets cooled to freezing temperatures, or thereabouts.

In other words, I would like to see your climatic data interpreted more in terms of the water budget, rather than of the temperature condition.

DR. DEACON: It is a pity to find fault with such an interesting paper as Mr. Worthington has given us, but when the state of knowledge is so poor that we do not know the influence of cooling or highly saline water on the circulation, we can't compare the effects of density changes with the influence of the wind. We don't know whether these deep waters were energized by the differences of temperature or salinity, or represent the easiest way to escape the effects of wind. It adds some significance to the question to try to find out some of the differences between the Atlantic and Pacific Oceans. In these fine longitudinal sections of the Atlantic, it looks as though there are some deep water movements running more or less north and south, but in the Pacific Ocean, while we have less data, we are led to believe the circulation is more sluggish. Why is that so?

Fleming has emphasized the differences of salinity, but by and large, the wind and the temperatures are the same. The only difference is that one ocean is much wider than the other. One cannot help but feel that there are some fluid dynamics involved in this situation, and it seems rather chancy to rely on such things as differences of 0.3 in oxygen content. I have done hundreds and thousands of these oxygen analyses, and by and large it is not an accurate method. We should not place too much emphasis on these data until we have more records.

MR. WORTHINGTON: The changes in the thermocline show no measurable change of oxygen concentration at all. It is only where the water is colder than 4.2° in the Atlantic that oxygen seems to have been lost, except possibly on your side of the ocean. In the western part the oxygen depletion is cut off almost as by a knife at the 4.2° isotherm. If we are to trust the old chemical data at all, and I trust them, I am convinced this oxygen loss must have taken place. I think

the Caribbean measurements are most significant. There you do have a large number of old and new measurements, which are made in one homogeneous water type, and there the loss of oxygen in the oxygen minimum layer comes to 0.02 instead of 0.3. So despite the inaccuracy of the Winkler method, you have then a large enough body of observations where the thermocline portion has not lost oxygen, and the deep water has.

Dr. Riley suggested that possibly this water migrated to the South Atlantic; this raises a question that cannot be fully answered until the METEOR sections are repeated, but if they are found to have lost oxygen in the deep water and not in the thermocline, I do not see how you can escape concluding that there is a real oxygen loss.

DR. DEACON: It would be rather strange if this water sinking from the surface did not change from year to year. We know that the bottom currents are not at all negligible. They go a mile or two a day. It is difficult to reconcile that information on the one hand along with very slow oxygen changes on the other. I was just emphasizing some of the difficulties.

MR. WORTHINGTON: In our measurements across the bottom of the Gulf Stream, we have gone into very deep waters comparing old and new sections, and I think there is a chance that the Gulf Stream, certainly on the basis of the measurements we have made, is decreasing in volume from the bottom up. I think this is rather consistent with the picture of the oxygen loss.

If these gradients were set up during the period when the whole ocean was overturning from the equator to the poles, and such a process is no longer going on, one would expect these gradients, cross-current pressure gradients, to decrease. That is, in fact, what they have done in the few sections we have measured. As I said, we need many more measurements.

DR. MUNK: Is it fair to ask whether there is some question of significance of this diagram you showed in Gulf Stream transport? I am under the impression, from differences of volume transport observed in the Miami current, that changes by a factor of two have occurred in a week, and therefore diagrams based on 10 or 20 measurements showing factors of 20 percent are a bit small, aren't they, to be really reliable?

MR. WORTHINGTON: Not if the Florida current were a very significant part of the total volume transport, but it seems to be only about 30 million cubic meters per second, out of the total transport of 130 million relative to a level of no motion at 5000 meters.

9. ON THE PROMISE AND LIMITATIONS OF OCEAN MODEL EXPERIMENTS

William von Arx
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I would like to begin with an attempt to state a philosophy that appeals to me at the moment. It concerns our present problems in physical oceanography and offers us an added means of attack.

Dr. Warren Weaver once pointed out that the success of experimental and theoretical physics depends upon the fact that ordinary phenomena of this earth are loosely coupled, and therefore can be studied independently. He also pointed out that this is not the case in geophysics in general, and in meteorology and oceanography in particular. As we well know, the oceans and atmosphere form a closely coupled system in which probably many different feedback mechanisms play interacting roles. Thus, it is hardly possible to study the oceanic circulation in its broad aspects out of context with the atmosphere, or the general atmospheric circulation without regard for both the oceans and the continents. We also realize that in general, geophysical feedback loops must be both degenerative, and have low Q characteristics, that is to say, that resonances are rare and that small accidents do not usually lead to great upheavals. For example, the instabilities which form the meander pattern of the Gulf Stream do not grow to such a size that the general circulation pattern of the North Atlantic is greatly changed. Ordinary cyclones and even hurricanes, in their turn, do not modify the Gulf Stream circulation noticeably, while less conspicuous general climatic variations do. From this I think we can say that we have to contend with a spectrum of feedback systems that range markedly in size and period. But in general we cannot expect interactions to occur readily unless impedances match by chance or design. Thus effects on the general ocean circulation must be produced by widespread causes of sufficient duration to match approximately the natural period of response of the oceans as a whole, and for small scale high frequency effects we must look for causes of similarly small scale and short period. In order for small scale occurrences to affect the large scale circulation they must occur repeatedly and in phase over extended regions.

We know that the oceans possess two natural periods of response to atmospheric changes. There is the barotropic mode of response in which, for example, a relatively rapid change of atmospheric pressure at sea level will be communicated immediately through the full depth of the water column. There is also the baroclinic mode of response which, in time, will cancel the effects of the same atmospheric pressure change, if it be sustained, through readjustment of the fields of density and motion within the water. To the extent that the baroclinic mode of response cannot keep up with atmospheric pressure changes of even seasonal frequency, barotropic effects will necessarily be felt at all levels in the ocean in response to causes of sub-seasonal or even sub-annual duration. Similar ideas can be formulated concerning the effects of local and regional wind stress, climate change and other alterations of the normal balance of forces over the earth.

These ideas provide a framework for physical understanding of the ocean circulation, but we lack the necessary physical insight to extend our thinking very far without observations to verify our progress. Conversely a direct observational attack requires the exercise of uncommonly shrewd intuitive judgment to disengage the essentials from the bewildering tangle of non-essentials. In other words, in most instances it is beyond our present ability to simplify direct observations or to amplify our simple conceptual models enough to test one against the other with clear benefit to both. We realize, from the example of other fields such as physics, that sure and rapid progress follows whenever theory and observation are in a position to supplement each other closely.

Our observational problem is complicated by the very large size of the oceans, and the lack of any truly synoptic description of them. But were we granted this description tomorrow, it seems probable that we would find too many forces at work in too many different scales for the dynamics to be unraveled directly. If this is true, it is clearly necessary to study closed, isolated systems one at a time. This has potentialities as a laboratory problem. While such investigations cannot hope to explain the oceans by themselves, they can provide a middle ground between theory and observation which may help toward an understanding of the dynamics of the natural world in terms of the separated properties of its component systems. These systems are separable, perhaps, in terms of their characteristic size and duration.

The ocean model experiments in progress at Woods Hole are in some ways a step toward the application of this philosophy to a study of the large-scale ocean circulation. Ocean models can play two roles: one as a means to study and extend the theories of simple idealized oceans; the other to simplify and reconstruct nature in miniature. Of these two courses probably the first, the study of simple physical systems, will be most productive in the long run, but reconstruction of nature also has its value. Let me discuss each very briefly in turn.

Laboratory study of simple physical systems will be most profitable if the work in the laboratory parallels and perhaps occasionally anticipates mathematical theory. In that real fluids are used in the laboratory, the equations of motion are being solved continuously and without approximation within the experimental framework. Close study of a physical model should reveal the discrepancies of its conceptual counterpart introduced through linearization of the equations and approximations of several sorts. This should be helpful to mathematical theorists by giving assurance whenever their results bear physical confirmation or, in the event of failure, by giving constructive criticism of the most impartial sort. We need at the moment a more intuitive grasp of the behavior of fluids under the influence of rotation, wind stress, and heating from above, as raw material from which to fashion our understanding of nature. Laboratory study of fluid behavior under these influences is desirable both within and beyond the range of theoretical support.

Ocean models can be made to imitate nature to some extent. In attempting to imitate nature a small number of physical ingredients must be combined judiciously to produce a pattern of motion that resembles the oceans as we know them. This has its dangers in that the ingredients of rotation, earth curvature, wind stress, heat and viscosity can be combined in unnatural proportions to pro-

duce natural results. Fultz showed this effect clearly in his atmospheric experiments involving heating and rotation. However, through careful scaling of the intensities of known forces one can come close to the natural proportions, and except for the change in relative influence of unscaled parameters such as surface tension and mixing, it is possible to produce ocean-like regimes of motion. These regimes can be subjected to verification through comparison with observations of the oceans and minor discrepancies removed by various artifices. Once verified, the ocean model almost inevitably suggests points of critical importance for further field investigation. More than this, a carefully constructed miniature of the oceans can be observed on a truly synoptic basis and the change produced through variation of each of the constituent forces required for its operation cannot only be observed but measured.

At the present time ocean models have dealt with a barotropic mode of motion which in the steady state is necessarily restricted to a representation of the climatological mean circulation. Thus far the effects of heating have not been introduced. The barotropic mode has been excited through the influence of wind stress on homogeneous water in the presence of the equivalent of earth rotation and earth curvature. A pattern of motions resembling those of the surface layer to a depth of perhaps 200 to 500 meters has been produced for both the northern and southern hemisphere oceans with the apparatus shown in Figure 9.1. The results were shown in several color photographs which are not reproduced here.* These models are essentially physical realizations of the theoretical effects of wind stress on a viscous fluid as treated by Ekman in which the viscosity is independent of depth. Due to the presence of a layer of frictional influence that is thin compared to the total depth of water, as is the case in the real oceans, there is a powerful tendency for the surface skin in the model to be accumulated in the center of each ocean gyre while the water in the bottom friction layer is accumulated as a kind of coastal water mass. The motion between these surface and bottom friction layers is essentially two-dimensional and represents the climatological mean circulation. Although the model is barotropic, the friction layer effects conspire to segregate water masses in a manner that resembles the baroclinic oceans of nature with surprising fidelity. (A short film was shown to illustrate this.)

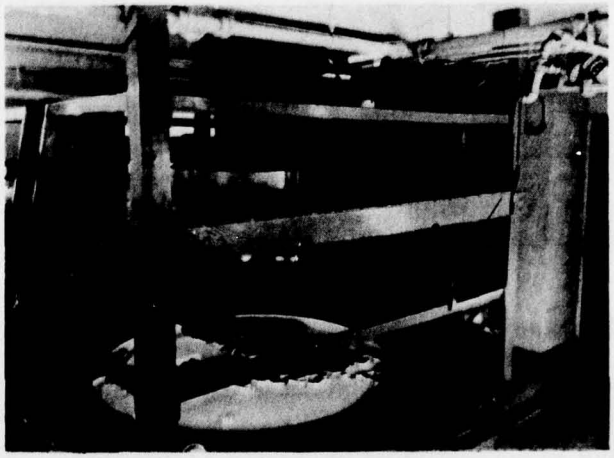


Figure 9.1 Ocean model apparatus

*See the color plates in A Laboratory Study of the Wind Driven Ocean Circulation, W. S. von Arx. Sartryck ur. Tellus. No. 4, 1952, pp 311-319.

The essential characteristic of present barotropic models is the presence of the physical equivalent of a beta plane in which horizontal divergence is substituted for the variation of the Coriolis parameter with latitude. Horizontal divergence of the proper sign and intensity as a function of change of latitude has been produced through the action of stretching and shrinking effects on water columns moving meridionally between the parabolic curve of the free surface and the flat bottom of the experimental tank. This, together with the Taylor effect, namely, the tendency for fluids in a rotating system to move uniformly from top to bottom seems to be an essential requirement for the proper simulation of the barotropic mode of motion in models of the ocean surface circulation.

Study of the deep circulation in ocean models will require the equivalent of a two-layer system in which it seems likely that the contrast in density between the top and bottom layers can be produced most easily through thermal dilation of fresh water. This simplification will eliminate the effects of salinity on vertical motions and stability. But more disturbing is the possibility that in the steady state the motion in the upper layer under the direct influence of wind stress will have two-dimensional properties to the depth of the thermocline which means that we will lose control of the beta effect produced by horizontal divergence. This may not be serious, if, as in nature, the baroclinic response of the model oceans to variations of heating and wind stress of seasonal frequency is too slow for the steady state ever to be attained. Hence one will expect communication of the two-dimensional barotropic mode of motion through the thermocline during the continuous readjustments toward baroclinic equilibrium.

Fortunately the baroclinic mode of motion is not entirely dependent upon the presence of beta effects. In general the principal scaling laws to be observed in modeling the baroclinic mode consist of a proper ratio of Richardson and Rossby numbers which are beta-dependent and the ratio of the Coriolis number to the internal Froude number which is not beta-dependent. Thus, we may lose experimental control of the beta effect under steady-state conditions but probably not under transient conditions.

A final consideration in modeling the deep circulation is the requirement of model size. We have found that the larger the model area the greater its resolving power or ability to reveal small details. This is due to two things: first, the spectrum of turbulence is more fully developed in large models (that is, the effects of molecular diffusion are more nearly as far removed from eddy diffusion in the model as in nature) and second, the effects of capillary forces are reduced. We are working at the moment on a four-meter diameter tank with the hope that this is a useful upper range of size. Models much larger than this require a great expenditure of effort to operate.

The cost of large model equipments is no longer staggering. Recently at Woods Hole we have found a means for rotating large tanks smoothly around a vertical axis, which is simple, inexpensive and not particularly limited as to size. It is conceivable that tanks 100 feet across or more could be managed if they should become necessary. However, I think that for the present it would be more profitable to encourage the construction of more tanks rather than larger ones, and to interest investigators of widely different talents and viewpoints in their use.

DISCUSSION:

DR. WOOSTER: Do you still have some doubts about the existence of the Peru current?

DR. VON ARX: In the film, you saw the origin of my doubts. I don't think zonal wind stress on a barotropic ocean can account for the Peru or Benguela current flow and in the field data I've seen there is little dynamic topography to support them baroclinically. In view of this and the model results I am forced to doubt that the Peru and Benguela currents exist to the extent that would justify the size of the print used in naming them on modern charts, or the importance accorded them in meeting the requirements of continuity in the ocean circulation.

10. PROBLEMS RELATED TO DIRECT RADIOLOGICAL MEASUREMENTS AT SEA

Theodore R. Folsom
Scripps Institution of Oceanography
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It is now evident that ocean water masses can be tagged by radioactive materials so as to permit identification for many days. Oceanographers are looking forward to following accidental and deliberate activations for studying diffusion and advection at sea.

It is now realized that waters below the mixed layer can remain stratified in very thin laminae for long periods. That is, vertical diffusion may be as small as one or two meters for as long as several weeks. This stratification has both fortunate and unfortunate consequences. On the one hand, two-dimensional spreading permits the use of safe small quantities of trace activity. On the other hand, the strata may remain so extremely thin that the detection may become very difficult.

Consequently, it now appears that in most cases location of the tracer mass will necessarily be accomplished by means of a probe on the end of a wire, even though the final measurements may be made by bringing a water sample aboard ship for analysis, or ashore where much better equipment can be employed.

Experience at Scripps indicates that stratification can be so thin that it becomes extremely difficult to trip hydrographic bottles within one of these marked layers, even though the depth has been measured by a gamma probe.

On one occasion all attempts at bringing a sample of activated water aboard ship failed until a special device was rigged at sea, which was able to close instantly on response to the activity, that is, a Geiger tube sensed the gamma activity and tripped a relay and motor, which in turn tripped off the water bottle.

A number of deep-sea probes have been put together at Scripps for somewhat specialized tasks. The thing now is to perfect specialized probes for deep hydrography and to increase sensitivity as far as possible.

Direct beta ray measurement by probes appears limited by the short range of the beta particles in water, so that only a small sample could possibly contribute to the signal. Beta measurements appear to be done best by concentrating the liquids, and this can be done either aboard ship or ashore.

Direct gamma detection is well suited for deep-sea work. A very large sample can be taken and it need not be concentrated or stored. Many nucleides have gamma rays so penetrating that activity can be sensed some distance from the probe. For example, with potassium gammas, about 85 percent of the energy comes from within a sphere one meter in diameter, a sample too large for convenient storage in bottles aboard ship.

In deep water, there are several factors which facilitate gamma measurements at sea. In deep water heavy lead shielding is not necessary, and much gamma energy readily penetrates the thick steel shells which are customarily required for high pressure deep water instruments.

The oceanographic gamma gear is not unlike that used in oil-well logging, with certain differences of course, which arise from the ship's motion and the corrosion which is found at sea. At sea, too, the temperature at great depths does not approach the boiling point of water as it does in some deep oil wells.

Ionization chambers, Geiger counters, and photomultiplier scintillometers all have been used in wells and at sea. At present Scripps has had experience mostly with late model Geiger tubes which have been designed for oil logging where photomultipliers cannot stand the high temperature. They are cheap, rugged and versatile. We like them, but recognize their limitations.

Transmission of information up to the ship by wire is practical for several thousand meters. Scripps has several cables with triple conductor armour outside of them, about 5/16 inch diameter, and about 1000 meters long. Coaxial single conductor cables are certainly more promising because of their superior strength and ability to carry information faster from end to end. But there still is no electrical cable we know of which will go to the greatest depths of the ocean.

It was encouraging to hear this morning from Mr. Lawton that a Western Union cable ship now has an instrument cable as long as 4000 fathoms. Scripps' ships use, for handling the cable, small oil-driven winches with amalgamated slip rings, for electrical continuity. These provide very good control, and good control is essential for sensing out and measuring the profile of very thin active layers.

A pressure indicator is an important accessory in this "micro hydrography," so as to give an instantaneous and accurate indication of depth. Continuous reporting depth gauges, as well as programmed-reporting gauges have been used at Scripps.

Figure 10.1 is a picture of the apparatus. High pressure underwater gear

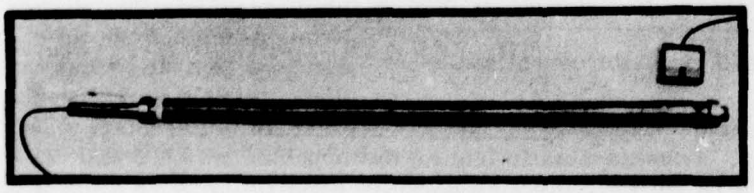


Figure 10.1
Apparatus for radiological measurements

is notoriously like gas pipe, and this is no exception. It is very similar to a six-foot length, three-inch diameter standard gas pipe. It has a one-quarter-inch wall, three-inch outside diameter, simple plugs on the end, and simple brass rings similar to fire hose connections. Only about one-tenth of the length has Geiger tubes in it, the rest is fitted with batteries. For reasons of operational convenience, we decided to use the batteries in the probe itself, and bring only the signal to the surface as an integrated current. This is not necessarily the

best procedure for all operations.

Figure 10.2 shows a detail of the method of clamping onto the armoured cable. There is a split brass slug about one foot long clamped on the outside of the cable which transmits the full strain to the armour. The pressure seal is inside below this. The small tube leading outside communicates pressure between the sea and a Bourdon-type pressure gauge: the pressure response can be tested on deck by connecting this tube to a pump and standard gauge.

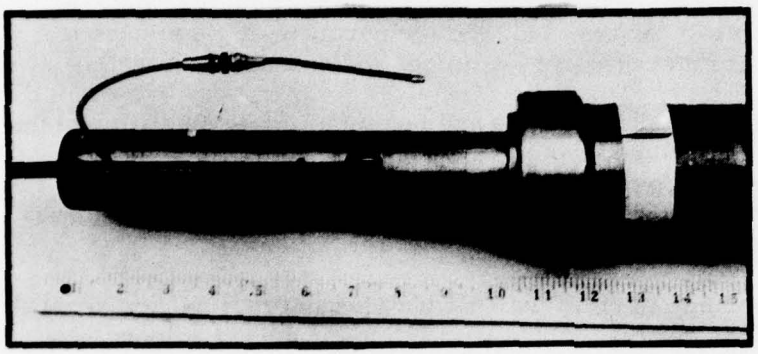


Figure 10.2 Detail of cable clamp

Figure 10.3 shows the gamma sensing device inside the cylinder on the left. This is a completely self-contained instrument which senses gamma rays, rotates a shaft, and releases a trigger, sealing the water sample. The rest at the right is a plastic water-sampling "bottle" having a piston-valve at each end. Several of these devices have been towed together on a depressed cable. The difficulty of this kind of operation is to know at what depths to tow in order to hit thin horizontal layers.



Figure 10.3 Gamma radiation sensing device

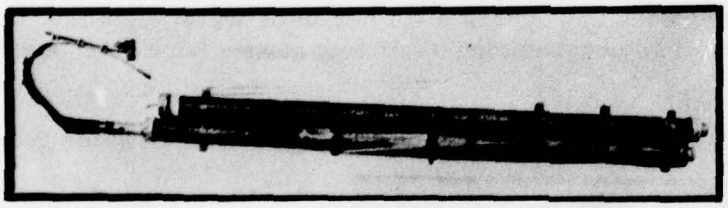


Figure 10.4 Experimental gear after use at sea

Figure 10.4 is a piece of gear that was photographed after it was brought home from an operation, showing what you do in desperation at sea. The lower cylinder is the probe which was actually connected to the ship and used for locating gamma activity. It has a plastic bag on the outside, which I will discuss later. On one end is a range switch which permits sensitivity to be selected. At the other end the cable has been cut off.

After we failed for hours to pick up samples of a thin but clearly evident active layer by several conventional means including a number of closely spaced Nansen bottles on a hydro wire, a self-triggering water sampler was improvised and attached to the gamma probe.

The upper cylinder in the figure is the self-contained, independent sensing

and triggering device made from a spare gamma probe together with parts from a device like that shown in Figure 10.3. This triggering device actuated a water sampler clamped to it but which was removed before Figure 10,4 was photographed. The device worked; a sample was taken in a layer a few meters thick.

The strip recorders are extremely useful, and in most cases X - Y function recorders can be well adapted to this work. Those contemplating the purchase of an X - Y recorder should consider the more expensive chart-table type, in preference to the more common drum-chart type. We find the chart signal is interesting both while lowering and bringing up the cable. Drum-type recorders have a tendency to obscure the record, either on the way down or on the way up.

For very low-level work pulses must actually be counted. There may be only ten or twenty photons to indicate the whole event. No standard recorder exists at present which is completely suitable, although the Clarey printer-type recorder could be adapted.

Working below natural background is possible, but it is always difficult and appears to be particularly so at sea.

Revelle, Folsom, Goldberg, and Isaacs have outlined a water tracing experiment which seems quite feasible, particularly because it makes use of existing equipment, and stays somewhat above the normal background of the sea. They propose introducing activity in amounts of roughly ten curies. They compute that measurement on an active layer, one meter thick and spread out two kilometers in diameter, would give them 19 counts in five seconds. This is the period which they feel can be allocated to measurements in a layer as thick as a meter or so. Because of the motion of the ship, one is unable to hold the probe in a layer that thin any longer.

The sea background from potassium gamma is only 0.2 counts compared to this signal of 19 counts per five seconds. The main limitation, as I say, is holding the probe for five seconds or more in a layer that thin.

They suggest use of Rubidium 86, Iodine 131 or Barium 140 because these nuclei have half-lifes of about one week, making for safety and convenience.

With these nineteen counts, statistics show there is 95 percent probability of detecting the activity, that is, measuring it within plus or minus 50 percent.

Improved equipment, including the use of a scintillometer crystal, and perhaps also a photon energy discriminator built into the head, were suggested as possible means for increasing sensitivity still further. Nevertheless, it appears that present apparatus permits detecting a specific water mass for maybe as long as a week.

Deep water has probably the lowest photon background on earth. Only deep lakes are cleaner in this respect than is the sea.

About one cosmic ray every two minutes occurs on the surface of the ocean. The Scripps probes count around 45 counts per minute on the surface.

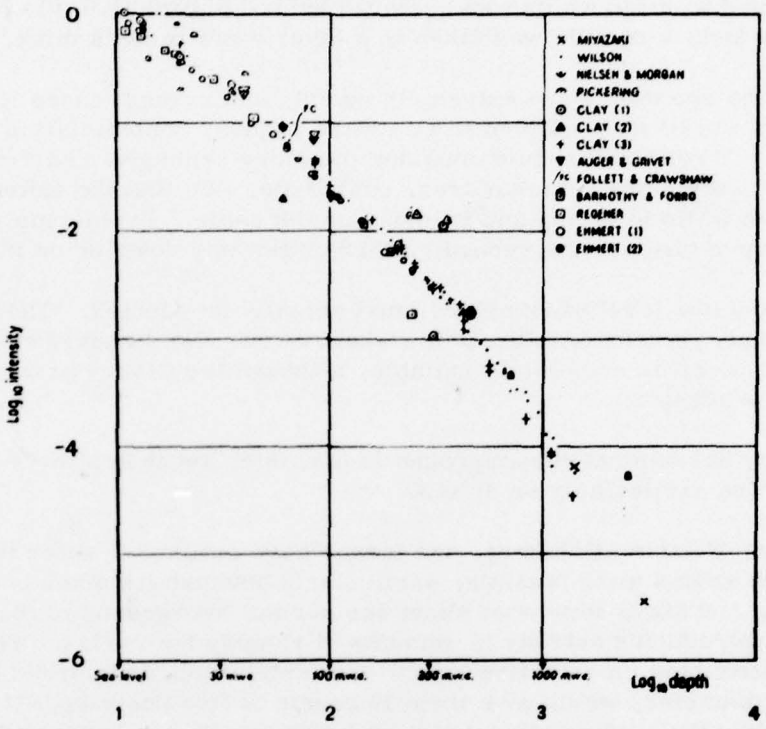


Figure 10.5 Summary of cosmic ray measurements reduced to equivalent water thicknesses

Figure 10.5 shows data compiled for all the cosmic ray measurements made either under deep water or in mines, reducing everything to the thickness in water equivalents. You will note that this was made by cosmic ray people, who always refer the thickness to the top of the atmosphere. The data obtained by Scripps falls in with this group of points down to around the 200-meter level. There is some doubt as to what the contamination of our instrument is right now, so we cannot go much lower than that. If one hundred percent is observed at the sea surface, about two percent of the cosmic rays reach 100 meters, and three-tenths of one percent reach 300 meters. Counts in the Geiger instrument occur about one-tenth as frequently as the local potassium gammas.

Evidently cosmic rays constitute no nuisance to Geiger-type probes below 300 meters. It should be pointed out, however, this is largely controlled by the preferential sensitivity which a Geiger tube has for heavy cosmic particles. Geiger tubes are 80 percent efficient for cosmic rays, while only about one percent efficient for gamma rays. However, the scintillometer may be twenty or thirty times more efficient in response to gamma, so that the cosmic background may not appear so conspicuously on a scintillometer. The practical consequence is that one can work closer to the sea surface with a crystal detector.

Geneticists and ecologists might be more interested in this particular gradient in the euphotic zone. Surface organisms would appear to be much more heavily bombarded except for the fact that the beta components of the potassium in the sea is not seen by gamma instruments, and you only have to go three or four meters under the surface before the cosmic ray signal attenuates to the level of the beta of potassium.

Submerged instruments are likely to pick up contamination on their external surfaces. This local activity is very disturbing, and must be controlled or minimized. Metals and oxides pick up much more readily than do plastic coatings. For this reason it is the practice at Scripps to put plastic bags or tapes on the probes and record how much the background signal is reduced when the plastic film is stripped off. There have been occasions when very rusty instruments have shown that about 50 percent of the total signal was due to what stuck to the rust.

At present, there is very little known as to the rate at which activity deposits upon, or washes off plastic or any other surface. We merely know that the pick-up on plastic is extremely low. We do not know how it depends upon time or upon concentration of the activities in the sea, or upon the speed of the flow of the water, in other words, how fast it is being towed by the ship. Therefore, the interpolation between these stripping tests can only be done crudely. Studies in this area should be very rewarding.

Calibration of the submerged instruments requires some special thought, since it is not as simple to calibrate an instrument under water as it is for use in the air. It is especially awkward to calibrate in a dirty area, an instrument which must be able to detect signals of a much lower level in a very clean area. This is the case where you are working in clean water and must calibrate at sea on the deck of the ship. Fortunately, time is available and statistical limitations to accuracy do not overshadow everything else.

The instrument and recorder must, of course, be able to resolve the background signal. The absolute sensitivity can be computed rather indirectly from the experimental responses to suitable standard sources in air, together with considerations of gamma scattering such as are already computed by several people and completed by Gates and Eisenhower. However, this sort of calibration is tedious.

A simple, direct calibration for "hard gammas" and direct check of the scattering theory can be carried out with the aid of a relatively small tank filled with perfectly harmless activity, which can be bought from any chemical store.

Ordinary potassium carbonate is very soluble and very cheap. It contains about 0.119 percent potassium 40, which carries a 1.5 MEV gamma ray. Some of this salt and an oil drum is all that is needed.

At Scripps we happen to use a little tin tank about 27 inches high by 27 inches in diameter, filled with water, and in this we put 8400 grams of potassium carbonate, after, of course, making a background test of clean water in the same tank.

This geometry contributes 75 percent of the signal which would come from a tank which is much larger, but corrections can be made. This concentration of carbonate solution gives a potassium activity about 25-fold that of natural sea water. The Geiger counts we use presently give about 33 net counts per minute, which is easily readable.

Specific activity of the salt is tabulated in several places, and it also can be determined experimentally in the laboratory, with the instruments in use. The Scripps' probe can be assigned with some confidence the sensitivity of 1.3 counts per minute for the potassium in sea water. And it also proves to have an over-all efficiency in gammas of moderate hardness of about 3.0 percent. Efficiency is defined here as the ratio of indicated counts to the number of photons which originate within a given time and within a volume of active water equal to the volume displaced by the detector (assumed here to be roughly one liter effectively).

One happy feature about this type of calibration is that when finished one just pours the activity in the sewer. No decontamination squad need know anything about it, because so far as I know, the Atomic Energy Commission has no interest in ordinary potassium.

In summary it appears that deep water could be traced successfully right now. The electronic gear is not particularly difficult to accumulate, and is subject to improvements in orders of magnitude. In fact, there are airborne gamma detectors which have sensitivity the order of 100 greater than what we have.

DISCUSSION:

DR. FLEMING: This was one of the new topics that John Isaacs pointed out this morning, and I am sure it is new to many of us.

MR. VINE: I had the good fortune to use some of the gear Dr. Folsom was talking about, and even though it is in its fairly early stages of development, it was real physical gear that you could lay your hands on, and could put over the side. You could also recover it. I think all of these three things show equipment is further along than most of the oceanographic equipment we talk about in meetings. For early gear, it shows great promise.

For example, this morning Dr. Iselin was talking about the use of electric cables. Here is a case in which cables have been used rather successfully and repeatedly. You may have a better idea of the total number of times you used a single cable than I do, but it must have been a couple of dozen anyhow.

DR. FOLSOM: Well, you yourself saw it used that much. I think it has now been lowered some hundreds of times.

MR. VINE: And this was without any great sign of wear and tear.

I believe you were talking about cable lengths of 1000 meters. This gives us plenty of opportunity to think about going deeper, and we certainly could put on other kinds of probes. You mentioned that you also have a temperature probe attached at the same time?

DR. FOLSOM: I didn't mention that we had it on at the same time, but we have some wires with three conductors, and at one time did have temperatures, pressure and radioactivity on the same instrument. At the present time the temperature device is not a very sensitive one, and not suitable for the great depths in which this audience is interested.

MR. ISAACS: One of the cables made about 700 lowerings, if I am not mistaken. Instruments may have been replaced which means that we had to trim off the end, but it is of that order anyhow.

MR. JAFFE: What voltages are used?

DR. FOLSOM: We use a simple Geiger tube used by the oil industry. It looks like a pencil. These are so-called low-voltage Geiger tubes, 700 volts will bring them on to their plateau. You can gang them all you want. We use batteries in the probe. A probe seven feet long and three inches in diameter, weighing 40 or 50 pounds, is not too heavy for work at sea. If they are any lighter, you have to put on extra weight to carry the cable down. There is plenty of room to have eight of these tubes and all the hearing-aid batteries you need. There is practically no current drain on the batteries. They have almost shelf-life. Battery power is quite suitable, and puts no particular strain on the wire.

We don't attempt to change the range while the instrument is over the side. We change instruments for four different ranges and never open them up at sea except when there is some complete breakdown. They are all self-contained.

MR. JAFFE: You are recording directly, or through slip rings?

DR. FOLSOM: Through slip rings. The oil companies have developed a mercury-type slip ring which doesn't have any noise in the millivolt region. We have no trouble at all with slip ring connections.

11. RADIOLOGICAL STUDIES IN THE INVESTIGATION OF OCEAN CIRCULATION*

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When the announcement of the radiocarbon method of dating was made, almost ten years ago now, we undertook to apply it to the oceanographic problems of dating strata in deep sea cores and establishing a stratigraphy for deep-sea sedimentation for as far back as possible. A break in sediment type has been established, about 10 or 11 thousand years ago, in the Atlantic Ocean and adjacent bodies of water; a change from a cold water fauna for the surface layers prior to that time, to a warm water type after that time (Ericson, et al., 1956).

The other application undertaken was the measurement of the age of the deep water. Assuming, at first, that CO_2 was in equilibrium between the surface water and the atmosphere, and that after the water sample left the surface it could be treated as a closed system, an age could be determined directly from the activity of a sample of deep water. In 1949 preliminary discussions were held with C. O'D. Iselin, A. C. Redfield, and B. H. Ketchum about these basic assumptions, with the conclusions that: (1) Approach toward equilibrium of CO_2 across the sea surface, in the high latitude sources of bottom water, would be greatly speeded by the prevalence of breaking waves. (2) Additions of CO_2 to a water mass by biological activity, after the mass sinks below the surface, are small and can be calculated for the North Atlantic from existing data on oxygen, carbon dioxide, phosphates, and nitrates. It was expected that if errors in apparent age of the water were caused by solution of carbonate from bottom sediments, these would be revealed by anomalous ages in the topmost sedimentary layers.

Later, after the discovery of natural tritium in surface water (Grosse et al., 1951), it was realized that natural and man-produced tritium might be useful for studying certain phases of ocean water mixing. Apparatus was developed at Lamont for electrolytic enrichment of tritium in ocean water and measurement of the tritium in low level counters. A program of sampling many ocean areas to a depth several times greater than the maximum thickness of surface isothermal layers was initiated.

From the rate of circulation of deep water, and from the temperature distribution in it, we hoped to estimate the flux of heat through the ocean bottom, and thereby to obtain a check on the estimates made by the direct measurements of the thermal gradient.

A project of this magnitude, extending over a number of years and using diverse techniques, requires the cooperation and effort of many people. B. C. Heezen, J. L. Worzel, J. Ewing, C. Drake, W. Beckmann and others participated in the shipboard operations, including sampler design and fabrication, taking of samples, and preliminary chemical processing.

* Lamont Geological Observatory, Contribution No. 267.

The work of processing and measuring the radioactivity of the earlier samples was done by J. L. Kulp assisted at various times by H. W. Feely, W. K. Eckelmann, L. S. Tryon, D. B. Carr, J. Gaetjen, B. Eckelmann and others. The present method employing CO_2 proportional counting was applied to this problem by W. S. Broecker, and the final satisfactory chemical system was set up by C. S. Tucek, W. S. Broecker and J. L. Kulp.

The C^{14} results discussed here were obtained by W. S. Broecker, and the tritium results by B. J. Giletti.

The project was begun in 1950, but usable results were not obtained until 1955 owing to major problems in the sample collecting, sample processing and radioactive assay. The system for collection of large samples of sea water from any depth has been entirely satisfactory except during two cruises when folding canvas bags instead of rigid metal tanks were used as samplers. No satisfactory chemical process for shipboard removal of the CO_2 from the sea water sample was evolved until 1954, when purified KOH absorbent was substituted for ascarite.

Although several refinements were introduced in the technique of natural radiocarbon assay by the carbon black method, the inherent lack of sensitivity of this system and the susceptibility to contamination of the carbon black samples by airborne fission products were limiting factors. The present system of CO_2 proportional counting reduced errors in radio-active assay to a minor role.

In 1950 three samples were taken on Cruise A-164, with a sampler consisting of two 55-gallon drums with six-inch ports top and bottom, and a chemical system containing NaOH solution as an absorbent. The samples were small and the existing counting procedure was considered inadequate for them.

Figure 11.1 illustrates some of the samplers that were used. The one on the left was used in 1950-51. There is a trap door in the bottom and in the top of

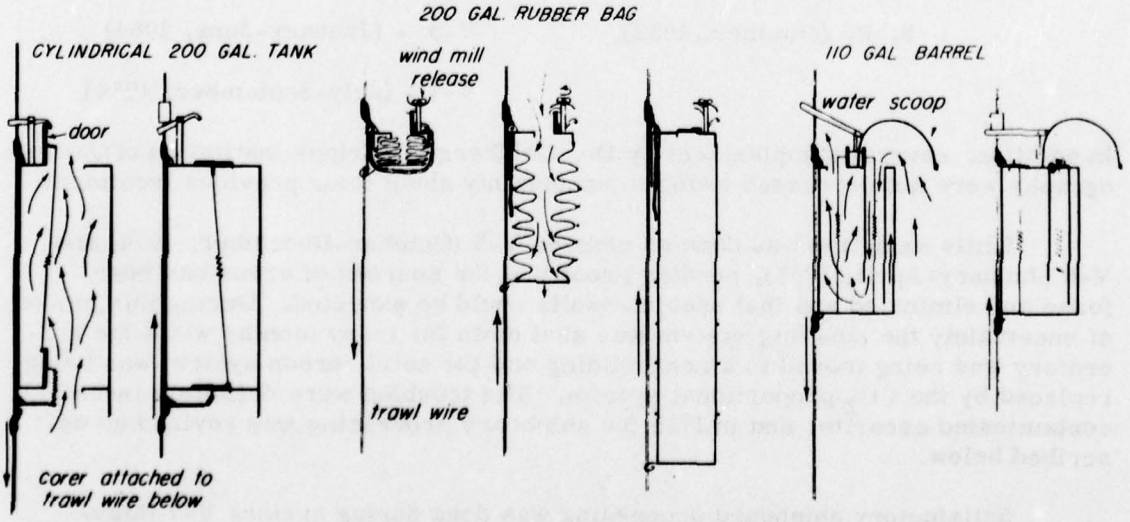


Figure 11.1 Large volume water samplers

a cylindrical vessel. In some cases this was a 55-gallon oil drum, sometimes a much larger container. The sampler was sent down on the wire of the piston coring apparatus and arranged so the ports would close when the coring apparatus touched bottom. This combination made for efficient operation and guaranteed that the water sample was taken very near the bottom. The sole disadvantage of this type of sampler was the inconvenience of having trap doors top and bottom.

The limitation of space on ATLANTIS made it desirable to have samplers which would fold up. Collapsible canvas and rubber bags were tried. These were sent down in the folded condition, and designed to open out and take a large sample at the desired depth. The canvas bag sampler shown folded, partially opened, and completely opened, in the center of Figure 11.1, was tried in 1951-52. The bags commonly came up only about one-quarter full.

In September, 1952, the first sampler of the type now used was built and adopted after successful tests on KEVIN MORAN. This sampler, now our standard type, is illustrated on the right side of Figure 11.1. It is used on the trawl wire at any desired distance above the piston core apparatus. Normally, a "messenger" sent down the wire is used to close the doors, but provision was also made for automatic closing when the piston corer touched bottom.

In 1951-54, solid ascarite was used as an absorbent in the chemical process to avoid handling caustic liquids on shipboard. Analyses showed that the CO_2 contamination on the ascarite absorbent was low, and that its C^{14} content was that of modern atmospheric CO_2. Not until 1954 was it found that the CO_2 contamination of many batches of ascarite was large, and its C^{14} concentration highly variable. This contamination invalidates all samples taken during the 1951-54 period, on the following cruises:

S. P. (summer, 1951)	V-1 - A-185 (April-June, 1953)
S. P. (January-April, 1952)	V-2 - (July-September, 1953)
S. P. (summer, 1952)	V-3 - (January-June, 1954)
	V-4 - (July-September, 1954)

In addition, several samples sent by Dr. Goldberg of Scripps Institution of Oceanography were not processed owing to uncertainty about their previous treatment.

Little sampling was done on cruises V-5 (October-December, 1954) and V-6 (January-April, 1955), pending proof that the sources of error had been found and elminated and that usable results could be expected. During this period of uncertainty the counting system was shut down for many months while the laboratory was being moved to a new building and the solid carbon system was being replaced by the CO_2 proportional system. The troubles were definitely traced to contaminated ascarite, and in 1955 the shipboard processing was revised as described below.

Satisfactory shipboard processing was done during cruises V-7 (May-October, 1955), V-8 (November, 1955 - January, 1956), V-9 (January-April, 1956),

and V-10 (May-October, 1956), which samples represent both the eastern and western basins of the Atlantic, from 40°N to 10°S, the Caribbean, and the Mediterranean.

The first usable results from this project became available about January 1956, from proportional gas counting of the samples from the cruise V-7. Radiocarbon assays for all of the samples of cruises V-7 - V-10 will be available in 1956, and appear to be highly reliable.

A few of the early measurements on deep water were published (Kulp, Tryon, Eckelmann and Snell, 1952; Kulp, 1953, etc.) suggesting ages for North Atlantic deep water in the range of 1500-2000 years. Unfortunately all of these measurements are now known to be incorrect. The new work reported herein makes it unlikely that any of the published values should exceed 700 years. It is regrettable that earlier retraction of the erroneous results was not made to prevent other authors (Cooper, 1956; Plass, 1956) from using them in their calculations.

The sampling device now in use is much like the one on the right side of Figure 11.1. Several auxiliary devices were introduced to guarantee that the sample was taken at the indicated depth and that it was brought to the surface without contamination. Standard oceanographic observations of temperature, salinity, and oxygen are made to identify the water mass being sampled. When the sample reaches the surface, it is pumped into a processing tank, and the CO_2 is removed by acidifying with concentrated H_2SO_4 and sweeping with CO_2-free nitrogen for about four hours at a flow rate of about 1200 liters per minute. This is a volume equal to about eight times that of the processing tank and removes essentially all of the CO_2. The CO_2 is carried by the nitrogen through a series of three 500 milliliter bubblers containing CO_2-free 8N KOH solution. Experiments show that at this flow rate the yield is essentially quantitative. Checks of the ratio C^{12}/C^{13} proved that the fractionation in the processing tank was negligible. At the laboratory, the CO_2 is released with 50 percent H_3PO_4, purified, and counted as described by Broecker (1956).

When satisfactory radiocarbon results for surface water were first obtained, no sample was found to be more than a few hundred years in apparent age, in accordance with the values for thermal flux through the ocean floor.

This created further problems because the radioactivity of carbon decays very little in a few hundred years. It introduced demands for larger samples, for greater purity in the reagents used, and for an accurate knowledge of the value of the radioactivity corresponding to surface water, i.e., to "zero age." Only a few measurements had been made on surface water samples, and it was necessary to get many more.

Figure 11.2 shows all the stations of 1955. The round dots are surface samples, the triangles are mid-depth, and the diamonds are deep samples. Analyses of 16 of these surface water samples from the western North Atlantic and Caribbean have been made. The average radioactivity of these samples is about four percent greater than that of modern wood, with only a tentative indication of correlation with geographical position. Extension of the measurements of surface

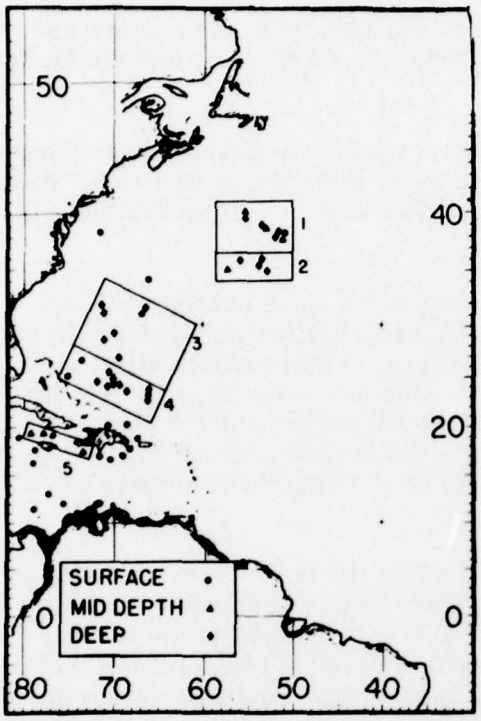


Figure 11.2 Radiocarbon Stations (1955)

water should be made to the regions of convergence where the various water masses whose motion is to be studied leave the surface. The C^{14} radioactivity of the surface water in these areas must be compared with that of the air in the same areas. It is not the present value of radioactivity of the surface in an area of convergence, but the value which existed several hundred years ago which must be used in calculating the time since a given sample of water disappeared from the surface. To estimate the variations in C^{14} concentration in the surface water at a given point during the past several centuries requires consideration of several factors. One factor is the estimate of fractionation by Craig (1954), that complete equilibrium between surface ocean water and the atmosphere would give the carbon dioxide components in the water a five percent higher concentration of C^{14} than the carbon dioxide in the atmosphere. The average C^{14} concentration for 16 surface water samples is four percent higher than that for a sample of 1938 wood from Palisades, New York, suggesting rapid equilibration by its approach to Craig's predicted value of five percent. We are at present making a further study of this equilibrium by taking samples of CO_2 from both air and surface water at a series of stations.

During the past 60 years great quantities of "dead" CO_2 have been added to the atmosphere through the burning of industrial fuels. The effect has been a "dilution" in recently formed carbon material which makes it appear older than similar material formed 60 years ago. The quantity of CO_2 added has been estimated at 6×10^{16} grams during this period. This amount is ten percent of the total atmospheric CO_2. A measure of the C^{14} activity of woods grown in 1890 and recently (Suess, 1955) suggests that a two percent change in the Northern Hemisphere and perhaps no change in the Southern Hemisphere has occurred. The absence of most of the added carbon from the atmosphere, presumably lost by interchange with the ocean, lends further support to the idea of rapid equilibration of CO_2 between surface water and atmosphere.

If the evidence for rapid equilibration is accepted, and if the apparent lack of latitude dependence of modern surface water radioactivity is taken to extend to the convergence zones, then the proper initial value, to be used in calculation of the age of present deep water samples, is the value reported for modern surface water, increased by a two percent allowance for dilution, the exact increment to be revised when better determination of the initial activity of wood during the past century has been made.

If very slow equilibrium of CO_2 between surface ocean and atmosphere were assumed, despite evidence to the contrary, then the activity of modern water should be used as the initial value for calculation of the age of a deep sample. On the latter assumption, the age of any deep sample would be about 150 years less than on the former.

The radiocarbon results obtained to date for deeper water may be summarized as follows: The C^{14} concentration for 17 reliable samples, well below the mixed layer and extending to the bottom in the western North Atlantic between 20°N and 40°N, show no significant trends with latitude. The samples are uniformly about seven percent lower in activity than North Atlantic surface water, or 450 to 600 years old.

Samples from other water masses are significantly different in activity, varying from two percent less than the surface value for one sample of water near the bottom of the mixed layer in the Western Atlantic to 11 percent less for one sample of bottom water at 10°S.

Conversion of C^{14} activities to ages requires careful considerations of the mechanisms forming deep water, distribution of surface values, and the possibilities of mixing of water masses of different C^{14} activity. Regardless of the assumptions made, however, it appears on the basis of limited sampling that the age of the deeper water in the Atlantic does not exceed 900 years.

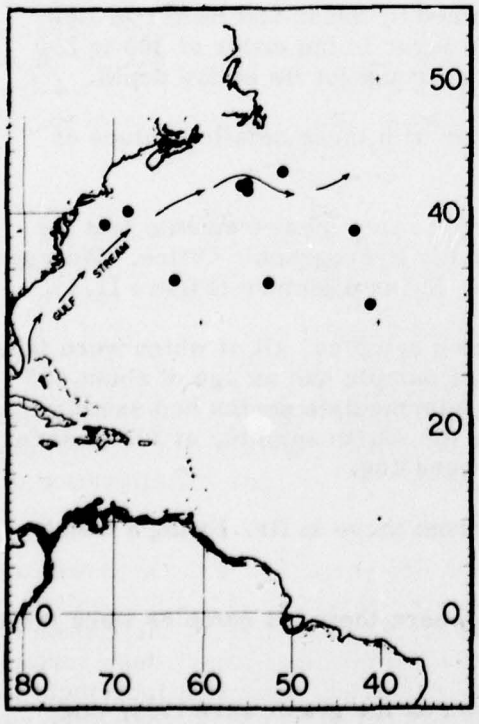


Figure 11.3 Lamont Tritium Stations

Figure 11.3 shows the locations of tritium samples. A few of the tritium measurements are completed. They are from surface samples or near-surface samples, and show no significant difference prior to 1954.

All except one are within the mixed layer. One unusually high value is discounted because there is a suspicion that it is contaminated by rain. It is now possible to say that the age in the mixed layer is less than four years.

The one value that relates to water definitely below the main thermocline is only a limit. It has less than a fifth of the tritium concentration of surface water, which averages 1 to 1.3 x 10^{-18}. The discontinuity at the bottom of the mixed layer is thus exhibited. The average of samples taken in 1953 is lower than that in 1954 by about 40 percent.

DISCUSSION:

DR. REVELLE: I was a little puzzled by the surface measurements. There is a very strange thing about all these C^{14} measurements, they all seem to be 1400 years old. Yours are less than that, by about a hundred years.

I wonder if you think it would help to determine the C^{13} partition between the wood and the water?

DR. EWING: C^{12}/C^{13} ratios have been run on these samples. They indicate that fractionation in the chemical processing is well below the error in the C^{14} measurements and hence negligible. At equilibrium the C^{12}/C^{13} ratios would predict that surface water be five percent higher in activity than currently growing wood. Our present average is four percent higher, indicating near-equilibrium conditions. However, we are endeavoring to make a better determination of this by taking many surface samples.

When early analyses suggested that the ages of the deep water were great it was not necessary to be very fussy about surface samples. We had one or two which came out approximately equal to modern wood. But new results for deep water showed the need for precise data on the surface water so on the last cruise we concentrated on surface samples. These are now analyzed, and we have a fairly firm value of surface activity. An "age" for the surface layer cannot be given until the fractionation is better understood.

DR. REVELLE: Most of the data obtained by Suess and Rafter of New Zealand give values for the water in the mixed layer in the order of 300 to 250 years. It means the ocean mixes very rapidly throughout its entire depth.

I would personally be very much happier with these smaller values of yours. That is why I am asking about them.

MR. LYMAN: I would like to comment on some measurements that the Geological Survey made for samples taken for the Hydrographic Office. We used a tank very much like the one to the left in Dr. Ewing's picture (Figure 11.1).

We have had four measurements made on samples, all of which were taken eastward of the Lesser Antilles and the surface sample had an age of about 400 years, corrected for the C^{12}/C^{13} ratio. The intermediate depths had exactly the same age within the experimental errors, and the fourth sample, at 300 meters, had an age of about 600 years. Two in there were 400.

These are located considerably apart from those in Dr. Ewing's chart, and tend to fill in the picture a little.

MR. BEGEMANN: I would like to ask where their '54 samples were taken for tritium analysis.

DR. EWING: Some of those spots shown on the graph were 1954, and some 1953.

MR. BEGEMANN: When in 1954?

DR. EWING: In 1954, the 10th and 23rd of July, the 6th of August, and the 9th of September.

MR. BEGEMANN: Then it seems quite impossible those samples have the same tritium activities as those taken in 1953, because in 1954 there was more in the samples taken from the Pacific.

Those taken in 1954 would be about ten times as active as those taken in 1953 because of the atomic explosions, and that is exactly what we find in Chicago. In samples taken from the surface down to something like 600 meters we find about a ten-fold increase over those figures taken before 1954.

DR. EWING: The tritium activities we obtained in 1953 and 1954, of Atlantic surface water samples, taken well away from the coast were not identical. The average concentration of seven surface samples taken in 1954 is about 1.7×10^{-18} whereas seven surface samples taken in 1953 is about 1.3×10^{-18}. It is quite possible that in the Pacific there were local areas of higher contamination which would produce an apparent increase of a factor of ten. Where were your samples taken from and how many have you measured?

MR. BEGEMANN: Well, some were taken in the Sargasso Sea, some taken 60-70° west and 45° north. And besides, if those high figures obtained in 1953 are correct, then all of the calculations done since must be wrong because of the radiation. If the water were mixed to a depth of 600 meters you would expect $T/H = 10^{-18}$. I think it was about ten times higher.

So all the calculations taken by tritium must be wrong as a result of radiation. But they seem to agree with what is predicted from the flux of neutrons that produce tritium, and are in pretty good agreement with what is measured. Maybe it is contamination. In one sample there was a striking disagreement, I think, between a sample measured in Chicago and the same sample measured in Lamont. Lamont got 1.8 and in Chicago it was 1.2, so those measured in Lamont were again a tenth higher.

DR. EWING: No, that is not correct. On the sample which we split and sent to you our value was 1.2×10^{-18} T/H. Furthermore this lies in the middle of our 1953 values. Since the 1953 and 1954 samples were run together with the same chemicals, glassware and counter with good reproducibility it does not appear possible that the set from 1953 could have been grossly contaminated and the 1954 group not. Your factor of ten between these two years simply does not appear to exist in surface Atlantic water.

DR. MUNK: I wonder if it is too early to make the following observation? I think no matter how you look at your data, Dr. Ewing, the drift values were at all events less than 100 kilometers a year, whereas if you look at Dr. Swallow's observations, all indicate a drift rate considerably greater than a thousand kilometers a year, after the effect of the tide has been removed. Three centimeters per second was the lowest value.

If there is anything at all to these earlier numbers, wouldn't it indicate that these (Swallow's) drifts are largely of a random type, and not a climatic mean from north to south? As measurements come in one would expect they would be rather complicated and unsystematic.

DR. EWING: I consider that a likely possibility. One reason we haven't tried to make tidal measurements by measuring the depth of the water is that I was unwilling to undertake a project like that unless measurements could be made continuously over a considerable length of time. It is equally desirable to continue drift measurements for a period of many weeks.

DR. HERSEY: I was just about to ask to what extent are we being fooled by observations on drift of a month or two duration, because of very long tidal periods?

DR. HEEZEN: I would like to point out that in your mention of Swallow's work at 17 or 18 hundred meters, and many much below that level, it would be acceptable to have a high rate or transport. In fact, the radiocarbon ages are two or three hundred years, which are very close to modern in that region. You show a radically different age much deeper.

MR. FUGLISTER: In talking about these rates of speed, there is one question I would like to ask Dr. Munk. In following Swallow's buoys for two or three days, what was the greatest distance they actually covered?

DR. MUNK: The slowest was three centimeters per second. Swallow claims the accuracy of the position is something like a tenth of a mile.

DR. FLEMING: We might ask how he does this. As Dr. Iselin pointed out this morning, navigational problems are always with us.

DR. MUNK: He has a buoy anchored on the bottom which he puts down at the start of the experiment, and goes back every couple of hours to see if it drifted according to a topographic feature in the nearby area. He used a bottom-anchored buoy which is constantly checked as the base line. And he has satisfied himself by these bottom checks that he could measure ranges to an accuracy of 0.1 mile.

12. OXYGEN ISOTOPE MEASUREMENTS OF DEEP-SEA SEDIMENTS

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The study of the deep-sea sediments adds a fourth dimension to the study of the ocean, namely time. About forty percent of the ocean bottom is coated with foraminiferal lutite. When sedimentation is continuous and not disturbed, which is rare, deep-sea sediments offer a continuous record of conditions prevailing at the bottom, because they contain benthonic organic remains; of conditions prevailing in the water column above, because they contain nekton and plankton remains; of conditions on the land nearby, because they may contain pollen and spores; and even a record of interplanetary conditions, because they seem to contain cosmic matter. Recently, Professor Pettersson, in Sweden, succeeded in extracting spherules, which he believes to be of cosmic origin, from deep-sea sediments, and he has found numbers varying between 100 and 6300 per kilogram of salt-free, water-free sediment. This is a very promising approach to see whether there have been important changes during the past in the influx of cosmic material.

If we want to study the conditions of the ocean during the recent past, the last million years or so, the best type of sediment to investigate is, at present, foraminiferal lutite. In order to have a good record we need sediments which are undisturbed and which have been deposited continuously and slowly.

The studies of Ewing, Heezen and Ericson at Lamont have given us a broad picture of sedimentation in the Atlantic Ocean, so that now these students are able to tell us where undisturbed sediments may be found and where satisfactory samples of such sediments may be obtained by coring. They have improved the piston corer so that now even the top layer of the sediments is preserved in the coring operation. The best places to do such coring are on the gentle slopes of unimportant rises on the sea bottom, in areas not below the depth at which only red clay accumulates.

There are various methods of studying such cores, and I am going to talk about the method we use in Chicago, based on oxygen isotopic analyses of shells of pelagic Foraminifera.

In normal foraminiferal lutites, pelagic Foraminifera are usually present in quantities greater than 96-97 percent by weight of the sediment size-fraction larger than 60-70 μ. About 25 different species of living pelagic Foraminifera are known, 15 of which are very common.

When calcium carbonate is deposited from a water solution under equilibrium conditions, the O^{18}/O^{16} ratio in the calcium carbonate is about 2.5 percent higher than in the water. This 2.5 percent enrichment in O^{18} is not constant, but increases with decreasing temperatures, 0.02 percent per degree centigrade. Thus, by determining the O^{18}/O^{16} ratio in calcium carbonate deposited from an aqueous solution, it is possible to determine the temperature at which the deposi-

tion of the carbonate took place. In the present case, by determining the O^{18}/O^{16} ratio in pelagic foraminiferal shells, it is possible to tell the temperature at which these Foraminifera lived, if the oxygen isotopic composition of the sea water is known or assumed.

It was found that different species of pelagic Foraminifera live at different depths, some close to the surface, some deeper, down to about 200 meters. Since they feed on diatoms, they do not go beyond the euphotic zone.

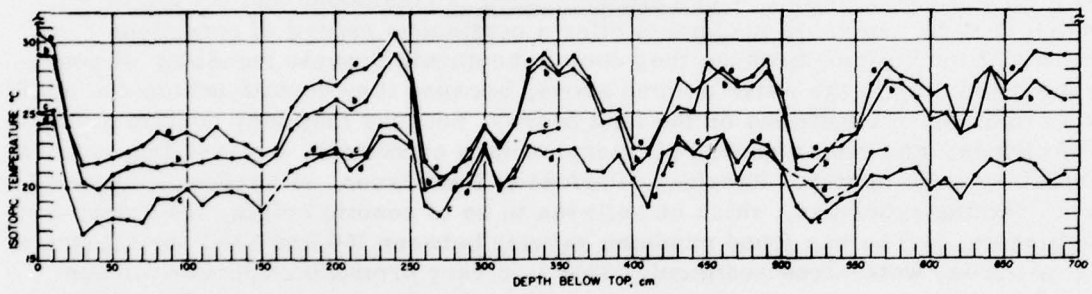


Figure 12.1 (Core A179-4): Isotopic temperatures obtained from Globigerinoides rubra (a), Globigerinoides sacculifera (b), Globigerina dubia (c), and Globorotalia menardii (d).

The curves in Figure 12.1 show the analysis of a deep-sea core which was raised by Ewing in the Caribbean and which was selected by Ericson from more than a thousand deep-sea cores as one of the most likely to give a good sedimentary record. Indeed, it does give a very good record.

On the abscissa we have the depth below the top of the core in centimeters, therefore we go back in time. On the ordinate we have the temperature in degrees centigrade. Each curve represents temperatures obtained from one species. We have a total of four species. Not all curves are continuous, because in some samples there were not enough specimens of a given species for isotopic analysis.

The two top curves give the best temperature record, because they show the temperature variations very close to the surface. The top of the core, at zero centimeters, is essentially modern, and gives a temperature of about 29 degrees centigrade. Going back in time, we see a steep decrease of temperature followed by a series of maxima and minima.

A level near the one at which the last temperature increase began, 26.5 centimeters below the top of the core, was dated by Suess and Rubin as 11,800 years old in this core. Another level, 73.5 centimeters below the top of the core, was dated as 27,600 years old. By extrapolation, and assuming constant rates of sedimentation, it appears that the preceding temperature minimum occurred about 60,000 years ago.

Figure 12.2 shows the temperature results from another core. This was also raised by Ewing in the Caribbean, but it is from a deeper zone and shows a higher rate of sedimentation. It is stratigraphically longer and brings us further

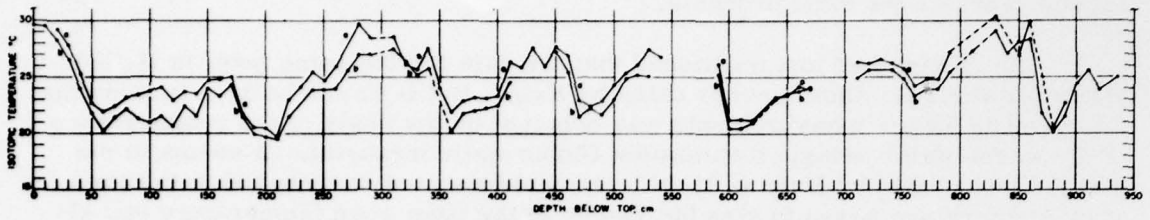


Figure 12.2 (Core A172-6): Isotopic temperatures obtained from Globigerinoides rubra (a) and Globigerinoides sacculifera (b).

back in time. It shows essentially the same story. Post-depositional solution of the calcium carbonate occurred at some levels and nothing was left for temperature analysis.

Notice that solution took place especially at times of temperature maxima. My explanation for this is based on the fact that this core was taken just above the line below which only red clay accumulates at present. Thus, a slight rise of the sea level would produce abundant solution. If, on the other hand, the sea level is lowered by a hundred meters, which occurred during glacial ages, the location of the core would be brought well above the line where only red clay presently accumulates and the calcium carbonate of the Foraminifera would be better preserved.

Age determinations on material from this core were made by Rubin and Suess using the radiocarbon method. The higher rate of sedimentation of this core permits one to pinpoint better the level at which the last temperature rise began. This level, 55 centimeters below the top of the core, was dated as 17,200 years old, while in the previous core it was 13,600 years. This same level was dated by Rubin and Suess in two more cores, one from the equatorial Atlantic and the other from the Mediterranean. The ages were respectively 13,600 and 16,000 years. (Table 12.1) On the basis of many arguments, I have selected the age of

TABLE 12-1

Depth below top and age of the beginning of the last temperature rise.

Core Number	Location		Depth below top of core (cm)	Age (years)*
A179-4	16°36'N	74°48'W	30	13,600
A172-6	14°59'N	68°51'W	55	17,200
A180-73	0°10'N	23°00'W	30	13,600
189	33°54'N	28°29'E	10**	16,000

*Calculated for the exact level on the basis of the rates of sedimentation obtained from the C-14 ages.

**Top of sediment missing.

16,500 years as the most probable.

Dr. Ewing has just mentioned that the date for the same level is 11,000 years for various Atlantic cores dated by Kulp. But it should be kept in mind that this level in Kulp's measurements was selected on the basis of the reappearance of the warm-water pelagic foraminifer Globorotalia menardii. It seems to me that the level dated by Kulp as 11,000 years old may not correspond to the time when temperature began to rise but rather to the time when temperature had already risen high enough to go beyond the threshold for the reproduction of the foraminiferal species previously mentioned. You would not expect a warm water species to reappear just at the time temperature started rising, but after the temperature had reached a certain critical value. Hence, Kulp's figures need not be in disagreement with the figures obtained by Rubin and Suess.

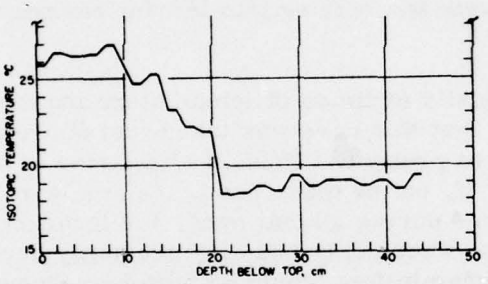


Figure 12.3 (Core 234A): Isotopic temperatures obtained from *Globigerinoides sacculifera*.

Figure 12.3 shows a short Swedish core that was analyzed by the isotopic method, and you see very well that at a certain point there is an abrupt temperature rise. This is the level which I think should be about 16,500 years old. My estimate may be too high by a thousand years or so, but it should be in that neighborhood.

Benthonic Foraminifera have been analyzed from various Swedish cores (Table 12.2). Cores 234A-234 and 246 are from the equatorial Atlantic. Core 246 was taken from west of the Mid-Atlantic Ridge, from an area somewhat

TABLE 12-2

Glacial and interglacial bottom temperature

Core Number	Location		Depth (m)	Bottom Temperature (°C)		
				Present	Glacial	Interglacial
ATLANTIC OCEAN						
234A-234	5°45'N	21°43'W	3,577	2.5		
246	0°48'N	31°28'W	3,210	0.9	0.4	4.4
280	34°57'N	44°16'W	4,256	2.5		
PACIFIC OCEAN						
58	6°44'N	129°28'W	4,440	1.7	1.6	2.4
60	1°35'N	134°57'W	4,540	1.7		

shielded from the cold South Atlantic bottom waters by the Rio Grande Ridge. This ridge is pretty deep and does not shield too well. The present bottom temperature is 0.9°C. Cores 234A-234, east of the Mid-Atlantic Ridge, are from an

area protected by the Walvis Ridge, which is considerably higher. The bottom temperature is higher, 2.5° C.

Isotopic analyses were made in correspondence of temperature maxima and minima for these cores, for core 280 from the North Atlantic, and for two cores from the eastern equatorial Pacific.

There is only a small difference between the glacial and interglacial bottom temperatures of the eastern equatorial Pacific. In the Atlantic we find glacial bottom temperatures of 0.4°C, lower than present, and interglacial ones of 4.4°C, which is considerably higher than present temperatures.

The high interglacial temperatures and in particular the temperatures of 4.8°C and 5.7°C for levels two to three centimeters and five to 15 centimeters below the top of cores 234A and 246 respectively, for which respective ages of two to three thousand and 8000 years may be estimated (Table 12.3), may be in part due to a small amount of isotopically light ice melt-water having entered the circulation of the bottom water.

TABLE 12-3

Subrecent bottom temperatures

Core Number	Present Bottom Temperature (°C)	Level below Top of Core (cm)	Approximate Age (years B. P.)	Temperature
234A	2.5	2-3	2-3,000	4.8
246	0.9	5-15	8,000	5.7

The presence in the bottom water of this ice melt-water which was presumably produced some thousand years earlier makes me think that the circulation of the bottom water is a slow process requiring some thousands of years for the water to reach the equator. This, plus Ewing's figures that bottom water is 800 years old, plus Worthington's belief that the present North Atlantic bottom water was formed less than two hundred years ago, seem hopelessly at odds. But this need not be the case. If a large amount of deep water was formed less than two hundred years ago, all we need in order to explain Ewing's figure of 800 years is that some five percent of the bottom water was a residue from the previous, older bottom water. That would give us the right age. Then we could say that the older bottom water was largely there since the time at which ice was disappearing.

The method of using oxygen isotopes for studying foraminiferal lutites from the deep sea will certainly, if pursued, throw considerable light into the recent history of the ocean, the surface and bottom temperatures, and so on. If it were possible to raise cores of the diameter of five inches instead of two, the temperature variations could be studied with greater detail. In particular, a detailed study could be made of the temperature variations during the last few hundred or few thousand years.

The temperature results which I have presented are based on certain assump-

tions as to isotopic composition of the ocean water. These assumptions may be not wholly correct, and the temperature data may be off by one or two degrees centigrade. This, however, does not impair the general conclusions.

DISCUSSION:

DR. KULP: We now have data on about six cores ranging from close to the equator up to about latitude 53° (north), which shows that the foraminiferal temperature break occurs within a rather narrow time span, around 11,000 years ago.

Of the two cores that Dr. Emiliani discussed, one of them had a temperature break, which he indicated, at around 12,000 years. Another was a Carbon-14 measurement, which led him to infer that the break may have occurred earlier, possibly 16 or 17 thousand years ago. He offered a suggestion that the foram temperature break actually represented a later event in time.

I think this is a possible theory. However, as I recall the original data, the Carbon-14 data was not precisely on the break. It was slightly above, and therefore some assumption would have to be made on the rate of deposition in order to calculate the discrepancy.

One of the six cores that we dated was taken very close to the first one shown, and can be correlated very closely. On this core we made something like 10 or 12 age determinations rather closely spaced all the way down the core. The break in the foram temperature was about 11,000 years ago, and lithologically it looked like an equivalent break in the first core that Dr. Emiliani showed, which also occurred about 12,000 years ago. Therefore, I think it possible that the foram break is coincidental with the temperature break.

We might also cite as an example of a major climatic change in a short time our dates on the Mississippi Delta area, where there is quite a thickness of silt and sand, and then a rather abrupt change to a very fine silt. The dates from this section, just at the bottom of the fine silt, are around 11,000 years, and the dating is continuous from there up to about 5000 years ago.

Along this break there are 20 or more cores, which indicate that sea level may have been higher when this very important event occurred.

We have found rather high levels of tuffa in some of the western lakes that Mr. Broecker has been dating; they are quite young so that apparently some of these lakes drained very much more rapidly than previously supposed. These levels were quite high 11,000 years ago, and very rapidly disappeared. So I think there still can be a rather good case for a fairly abrupt end.

DR. EMILIANI: As for the foraminiferal break, I was discussing this with Ericson at Lamont a few days ago, and he agreed that from a biological point of view, there is no doubt warm water species require a certain minimum temperature to appear. It could be a coincidence if this were an infinitesimal amount above the minimum. It sounds more logical to consider that the temperature increased and reached the threshold for the species.

MR. VINE: I don't know how pertinent this comment is, but it may be of some use to people making these determinations.

We were discussing sampling problems with respect to other chemical analyses of bottom sediments at Woods Hole the other night, and one of the biologists remarked that in general he expected that bottom living and burrowing animals would regularly furrow the bottom to depths to the order of seven or eight centimeters. This may have a considerable effect on samples spaced at intervals of that order. I was wondering if you had run into any evidence of such disturbances.

DR. KULP: As far as the break is concerned on these six cores, they represented widely different rates of sedimentation, so that in only one case was the break anywhere near ten centimeters from the top. But your comment is very well taken. The reason I think we have not worried more about it in the past, has been that it is difficult to know whether we have the top centimeter. One of the things not resolved in the Carbon-14 dating of these cores is that their tops seem to be a little bit too old. It is not likely this is a solution all over. Something like you are suggesting may be important.

MR. VINE: I was wondering if this might be so all through the core. There is no reason to suppose that for quite a long time the entire bottom of the ocean hasn't been turned over.

MR. FUGLISTER: I wanted to ask how you determined the percent of ice melt from cores taken at the bottom?

DR. EMILIANI: We know the oxygen isotopic composition of ice melt-water at present and assumed this was not different in the past. We also know the oxygen isotopic composition of the ocean, and we can compute what percentage of ice melt-water is required to produce the temperature effect shown by the cores.

DR. CARRITT: It seems to me in the paper discussing the isotopic temperature and in the other one discussing age for Carbon-14, there is a common factor naturally contaminating the system.

It seems to me there is a possibility that, while calcium carbonate pieces are settling in the water, continuous exchange with the carbonate in solution may take place and foram parts may become older. The water, in that sense then, becomes younger.

DR. EMILIANI: No, this does not occur, because if that happened then the oxygen would also exchange. Then the foraminifera that live in the surface, by the time they reach the bottom, should give temperatures of two degrees centigrade instead of 25. They do not.

DR. CARRITT: Could there be just a five or ten percent contamination?

DR. EMILIANI: Well, you see it must be much less than ten percent if there is any exchange.

DR. KULP: I would like to agree with Dr. Emiliani. We have taken some fine clays in shallow water near the surface, and found no Carbon-14. This would mean less than one percent of contamination.

DR. FLEMING: I think the implication of the temperatures that Dr. Emiliani mentioned to us was that these represented the warmest condition during interglacial periods, and so, by inference, you would expect this to correspond to the climatic optimum. I don't know whether this was intended or not, but I am going to start at that point. I will go back to what I talked about this morning, salinity.

If you make reasonable estimates about the removal of water from the oceans during the ice ages, you will find that this would result in an increase in salinity of about one part per thousand on the average. If that water were then cooled, and bottom water were formed with this highly saline water, by processes that are comparable to what we think are occurring today, we would have deep water with temperatures corresponding to this, but a salinity about one part per thousand higher than we have today.

Don't take these figures too seriously because obviously they differ from one ocean to another. But when the ice started to melt, presumably dilution of the surface waters would occur, and we would then have cold water produced, of a much lower salinity than we had during the glacial period. This, presumably, could not replace the deep water, which could very well stagnate on the ocean floor.

If you looked at the density tables you would find you have to heat water of, say, one part per thousand higher salinity to achieve the same density as the water existing then at the surface. It would have to be heated between about five and ten degrees, in order to become unstable, so that it is quite possible, *I think*, that following the beginning of the glacier melting, we had stagnant highly saline deep water in the oceans which may have been heated over a period of a thousand years before it was displaced or mixed with the water above it.

If I remember correctly, if you use the accepted figure for the heat flow from the center of the earth, it would take something like five to ten thousand years for this water to be warmed up. It all depends on how thick a layer you want to assume.

What I wish to point out here is that this high temperature may not appear at the mid-point in the high temperature series that you indicated, but conceivably could occur nearer the older part of that interglacial period.

DR. EMILIANI: The samples were selected in correspondence with the midpoint of the high temperature curve just for the purpose of knowing the interglacial temperature at the bottom of the ocean.

DR. REVELLE: What curve are we talking about?

DR. EMILIANI: The curves showing the surface temperatures.

DR. REVELLE: So you have the highest bottom temperatures associated with the climatic optimum.

DR. EMILIANI: I only have two bottom temperature values in correspondence of interglacial maxima. It could conceivably be that if I took older samples, but younger than the preceding temperature minimum at the surface, I would get still higher temperatures.

DR. REVELLE: I see. You selected these samples at depths which you felt corresponded to the highest surface temperatures.

DR. EMILIANI: I did.

DR. REVELLE: I am very unhappy about this 4° centigrade. Was that, too, melt-water?

DR. EMILIANI: My suggestion was that this was due to about five percent of ice melt-water that was around there at that time.

DR. REVELLE: Would you think that as long as there was any ice at all, deep water would have about the same temperature?

DR. EMILIANI: Yes. If we assume that bottom temperature two to three thousand years ago was the same as it is now, the temperature of 4.8° centigrade could be explained by the presence in that place of about five percent of ice melt-water.

DR. REVELLE: Then of course you would raise the question Dr. Fleming raised, how do you get the ice melt-water down there?

DR. EMILIANI: It must have mixed to a certain extent – it is only about five percent ice melt-water, 95 percent is normal marine water. You certainly must have some ice melt-water.

DR. REVELLE: You are saying this water mixes with all the water in the ocean, and finally gets down there?

DR. EMILIANI: I think it mixes expecially with the bottom water.

DR. REVELLE: Five percent of fresh water would decrease the salinity of the water and would raise it to the top.

DR. FLEMING: Water that sinks to the bottom is supposed to be formed during the period when the ice was forming, which produces the saline brine.

DR. NEUMANN: I was just going to say that. The ice formation is necessary in order to form the dense water which finally sinks to the bottom.

DR. EMILIANI: I was under the impression that if you want to make marine water sink, temperature is more important than salinity.

DR. ISELIN: Oh, no. Where the temperature is now -1.8°, the water has a salinity of only 32 parts per thousand.

DR. EMILIANI: It must somehow sink, otherwise how could we explain these results?

DR. FLEMING: I think this represents a temperature inversion; that the water is warmer near the bottom.

DR. EMILIANI: But two or three thousand years ago?

DR. FLEMING: Yes.

DR. EMILIANI: Well, that makes me very happy.

DR. LYMAN: I would like to ask Dr. Emiliani to clarify something he said about formation of red clay. Did you say the limiting factor is hydrostatic pressure?

DR. EMILIANI: No. Red clay accumulates below certain levels. It seems that if we decreased the sea level by 100 meters we may also lower the line of red clay formation just about a hundred meters.

DR. LYMAN: Then the limiting factor, if nothing else, is a change in depth.

DR. EMILIANI: That is right.

DR. ISAACS: It seems to me you can get a combination of saline water in which ice melts, which may then become quite dense. Is that correct? There is a difference between ice that is melting and that merely warming up – it cools it quite a bit more, of course.

DR. FLEMING: If your theories are correct, you should find the optimum isotopic dilution by using the greatest amount of ice melt-water, not in bottom water, but in some of these intermediate waters that might be near the surface, within several hundred meters of the surface.

DR. EMILIANI: Yes.

DR. FLEMING: Have you any information on that?

DR. EMILIANI: No, I haven't any, since no Foraminifera grow at intermediate depths.

MR. VINE: On a steady state solution I don't see how this could happen, but if we think that possibly the ocean had some very large gradients in the surface temperature, there may have been a possibility this could have happened.

I suggest that we might think this over fairly hard. Suppose that ice melt-water can go down if you have the proper surface temperature, it might pay us to

think about how large a change in surface temperatures and salinity would be required for the possibility of this happening.

DR. EMILIANI: This discussion has grown all out of proportion. All I claim is five percent of the bottom water during those times might have been derived from higher latitudes as ice melt-water. This seems to me a small percentage which could be made to sink under proper conditions.

DR. REVELLE: All of it comes from higher latitudes.

DR. FLEMING: What you speak of, this ice melt-water, does it have a half life that you can give us?

DR. EMILIANI: I don't understand.

DR. FLEMING: We are talking about isotopic composition. Does ice melt-water retain its isotopic composition forever?

DR. EMILIANI: Not if it gets mixed up with marine-type water, if it is diluted.

DR. FLEMING: Then you should be able to ascribe a half life to it.

DR. EMILIANI: It went from five percent to two percent in some thousand years, if the assumption I made before is taken as good. It may have a half life of some thousand years.

DR. FLEMING: We still don't know what the half life or mixing in the ocean is. Does it take a thousand years? That is really what we are talking about.

DR. BEGEMANN: You don't have to have any sinking at all; it could be just diffusion.

DR. FLEMING: Obviously, you have to trace something that maintains its identity for a significant length of time here. I think we are losing sight of this in the case of the ice melt-water. What we are really talking about is its rate of mixing in the ocean.

MR. METCALF: I may be causing more confusion but could I throw in a word I only heard once or twice in my life, and that is "cabaling?" You have this phenomenon when cold fresh water mixes with a saline water with the same sigma-t. You end up with a mixture which has the average salinity and temperature, but greater sigma-t, which would then conceivably sink, taking down some of this ice melt-water.

DR. FLEMING: If this process is a real one, it would provide a sort of intermediate water.

MR. METCALF: Yes, except that if it went on long enough it would gradually work its way down, and I don't think Dr. Emiliani cares if it is in intermediate

water or not, if he can get it down.

DR. EMILIANI: That's all I need. Marine water between a thousand and 2500 meters or so, is isotopically average marine water, but differs from the bottom water. It looks as though most of this water has a similar isotopical composition to that which sinks in the high latitudes, and reaches the bottom.

DR. DEACON: I think that is right, the salinity of water approaching from the north is about 34.7 parts per thousand and that which comes back is 34.5.

13. THE PENETRATION OF LIGHT INTO THE SEA

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Scripps Institution of Oceanography
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On a sunny day, with the sun high in the sky, every square meter of the surface of the sea is irradiated by about one horsepower of radiant power from the sun and the sky. Of this, some five percent is reflected, and the rest is available for absorption somewhere within the sea.

Three-quarters of this available power is in the infrared region of the spectrum and is absorbed very close to the surface. The deep sea, therefore, is irradiated almost exclusively by visible light most of which is in a relatively narrow spectral region centered around 480 millimicrons. But this light penetrates deep into the sea, and it is the penetration of this radiation that I want to discuss with you now.

Let me begin by identifying two types of measurements which are commonly made to describe the optical properties of ocean waters. The first of these is illustrated by Figure 13.1, which shows one of a class of instruments which are often called hydrophotometers. It consists of a source of light, an optical system

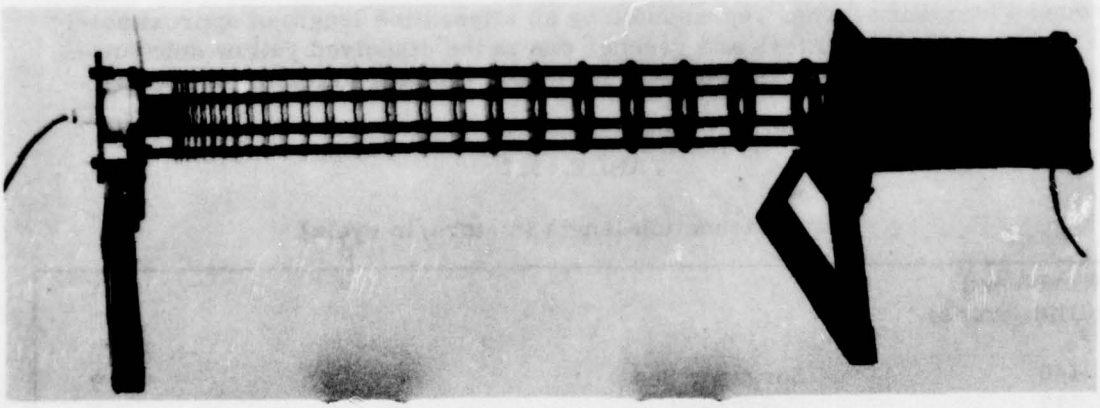


Figure 13.1 Hydrophotometer

which produces a well-collimated beam focused on the entrance pupil of a photoelectric cell. The baffles in this particular instrument are arranged carefully to eliminate multiple scattering. This instrument was designed by John Tyler of the Visibility Laboratory at Scripps and it is, I think, one of the cleaner optical designs of its class. This kind of equipment measures the transmittance of seawater for a beam of light, such as an image-forming ray or a ray of sunlight. Such a ray is exponentially attenuated with distance in any uniform hydrosol and the attenuation coefficient is, of course, a function of wavelength.

Various people have made measurements of this sort. Table 13.1 gives some measurements published by Dr. E. O. Hulburt some time ago.

TABLE 13.1

Beam attenuation length (meters/ln cycle)

Wave length mμ	Distilled Water	Coastal Water	Chesapeake Bay
400	12.6	2.5	1.6
440	21.7	3.7	2.3
480	27.8*	4.7	2.4
520	25.2	5.1*	2.9
560	19.1	4.8	3.1*
600	5.1	3.4	2.3
650	3.3	2.3	2.0
700	1.7	1.7	1.3

*Hulbert: J. Opt. Soc. Am. 35,698 (1945)

In preparing Table 13.1 I have chosen to use the reciprocal of the attenuation coefficient, rather than the attenuation coefficient itself, because a length is more readily visualizable than a reciprocal length. Thus the table gives beam attenuation length expressed in meters per natural log cycle.

You will notice that for distilled water there is a maximum transmission around 480 millimicrons, represented by an attenuation length of approximately 28 meters. Coastal waters are greener due to the dissolved yellow substances, but water in the deep sea is probably more like distilled water in its spectral characteristics.

TABLE 13.2

Beam attenuation length (meters/ln cycle)

Wavelength (millimicrons)		
440	Sargasso Sea	13.9
440	Caribbean	7.8
440	Pacific N. Eq. Current	12.2
440	Pacific Countercurrent	12.2
440	Pacific Equatorial Divergence	9.6
440	Pacific S. Eq. Current	8.7
440	Gulf of Panama	5.7
440	Galapagos Islands	3.5

From: "Reports of the Swedish Deep Sea Expedition", Vol. III, Table 27, p. 49.

Table 13.2 shows data by the Swedish Deep Sea Expedition, also given here in terms of meters per natural log cycle; please notice that none of these values comes up to 28 meters per natural log cycle that characterizes distilled water.

The statement that the water is as clear as distilled water is not borne out by experiments. On the other hand you will notice most of these natural waters are quite clear.

Let us turn now to the second type of optical measurement commonly made by oceanographers to ascertain the clarity of ocean waters. This measurement is accomplished by lowering a photoelectric cell into the sea and recording light as a function of depth. The diffused light observed by such a photocell is exponentially attenuated with depth, but much less rapidly than is a beam or ray of light.

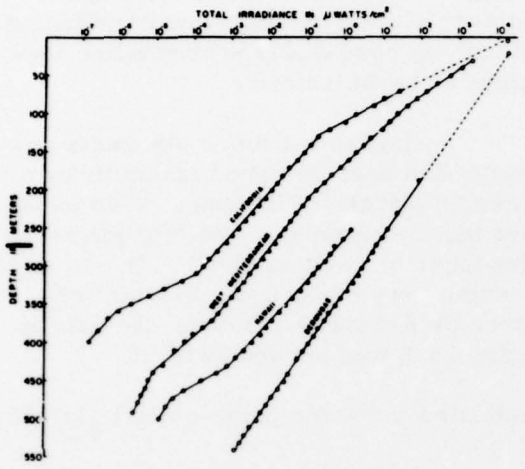


Figure 13.2 Irradiance as a function of depth at various locations

Figure 13.2 shows some data by Kampa and Boden. This is the total irradiance measured at various locations. Depth is in meters. It will be noticed that the waters differ in clarity and that there are straight line sectors which represent exponential decays with depth. The slopes are, of course, not as steep as if the ocean were pure distilled water, but nevertheless the steepest slope in the figure corresponds to an attenuation length of some 35 meters per natural log cycle, which is greater than the maximum attenuation length for a beam of light in distilled water. It is, perhaps, a startling fact that diffused light penetrates further than does a beam of light. Actually, however, this is only a matter of bookkeeping: In the case of a ray any scattering is a loss, whereas in the diffuse case light scattered in any forward direction is still counted as part of the diffused flux. Thus, diffuse light appears to experience less attenuation than does a beam.

Let us pause at this point to consider the apparatus which can be used to measure diffuse light in the sea. Simple barrier-layer photocells such as those shown in Figure 13.3 are not useful for exploring the deep sea because they are not sufficiently sensitive.

Multiplier phototubes have been used in underwater work by the Visibility Laboratory since 1949. Their sensitivity is adequate for measuring the light in the deep sea.

Figure 13.4 shows one of the simplest pieces of deep-sea multiplier phototube apparatus we have devised. The multiplier phototube is in a pressure protecting enclosure containing a single electronic tube, and batteries. This apparatus can be lowered to a depth of one thousand meters, and it has been so successful that we used a great many of them in the experiments last year off San Diego. The circuit of this equipment is shown in Figure 13.5. It was developed by R. W. Austin of the Visibility Laboratory at Scripps from a basic circuit principle due to M. H. Sweet. The multiplier phototube is advantageous for use in deep sea photometry because it can easily serve as a logarithmic element in its own circuit.



Figure 13.3 Barrier-layer photocell apparatus

The voltage which must be applied in order to maintain constant current output from a multiplier phototube varies as the log of the light, so that a vacuum tube voltmeter arrangement for measuring the potential applied to the multiplier gives a logarithmic indication of the light. Battery-operated, and connected to the surface by two demolition cables and a sea return, this instrument will give operation over six log cycles without switching and with an essentially straight-line logarithmic characteristic.

A similar but more elaborate apparatus has been designed and built by James Snodgrass of Scripps. This equipment incorporates an electrical pressure transducer to measure depth. It was used for some very basic and important research by Kampa and Boden; the data in Figure 13.2 was secured with it.

The circuit shown in Figure 13.6 embodies the same basic circuit philosophy just mentioned.

Let us now return to the data of Kampa and Boden shown in Figure 13.2. It will be noted that some of these curves contain abrupt changes in slope. These indicate layers within the sea; they show that the diffused attenuation length changes in a discontinuous fashion at the boundaries of water masses.

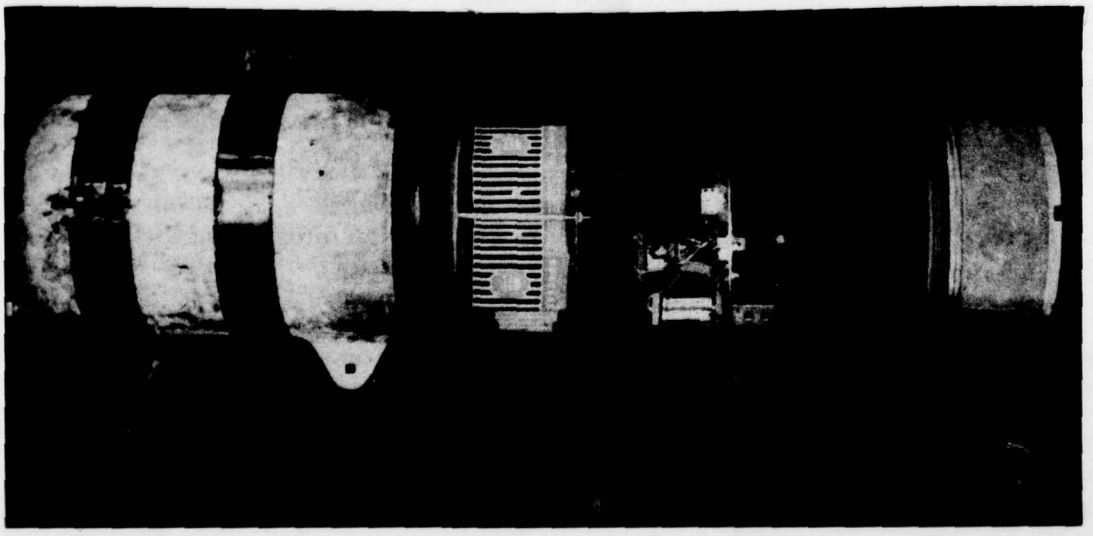


Figure 13.4 Multiplier phototube apparatus

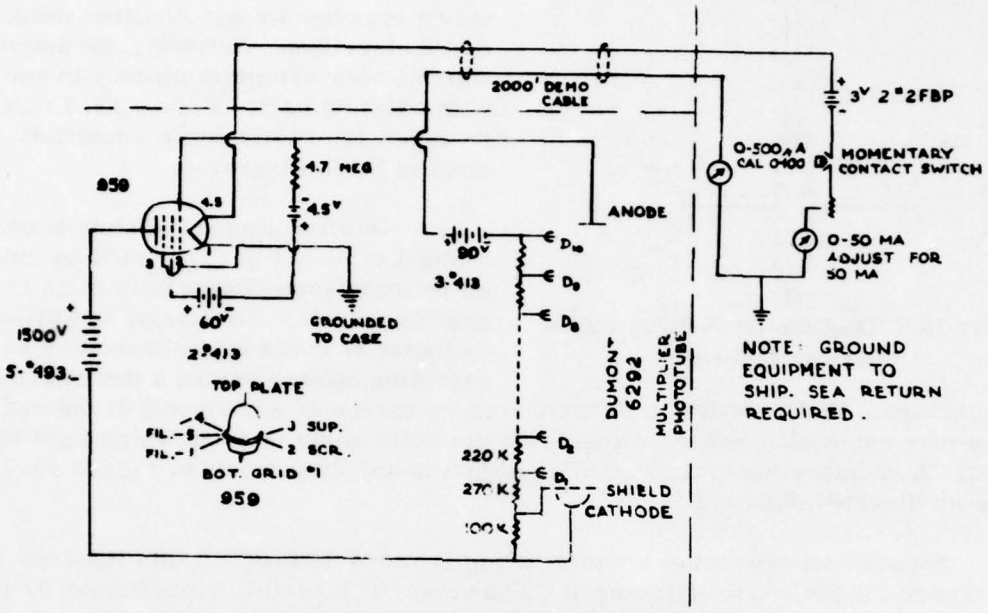


Figure 13.5 Circuit of apparatus shown in Figure 13.4

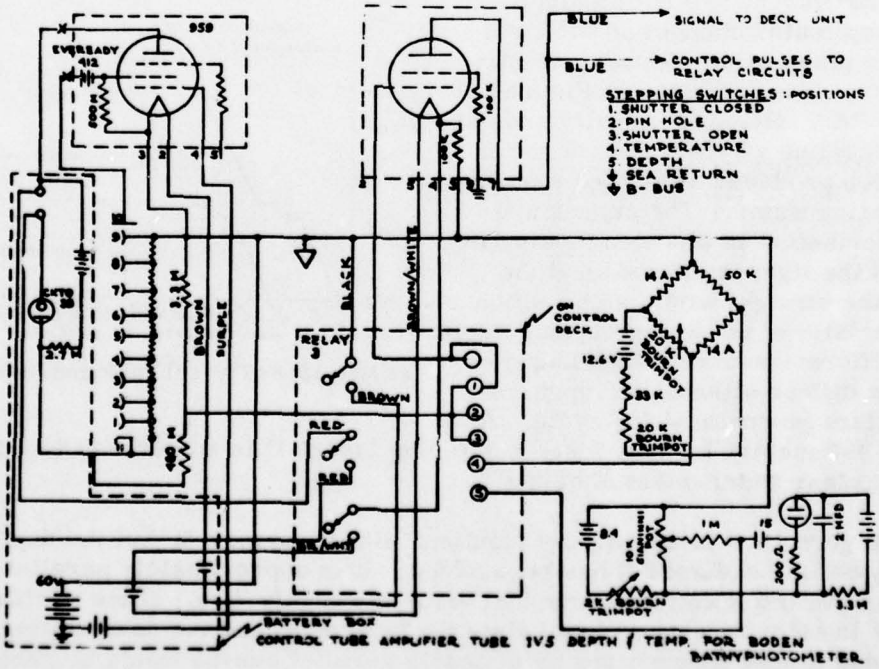


Figure 13.6 Circuit of bathyphotometer

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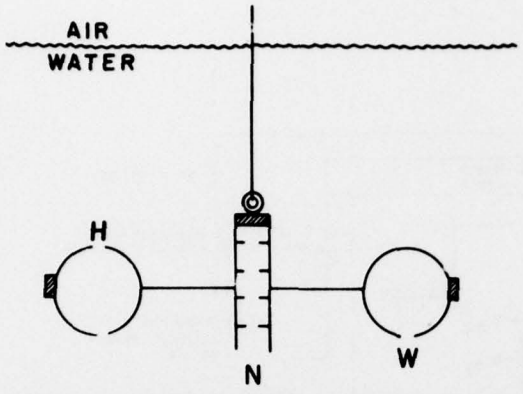


Figure 13.7 Diagram for defining radiometric quantities

The principles of light penetration into the sea can be illustrated more simply and clearly by means of diagrams which represent a hypothetical water mass structure. Actually, the diagrams which follow are quite closely in accord with existing data. Figure 13.7 will help to define the radiometric quantities to be used in these diagrams.

Downwelling irradiance H can be thought of as the quantity of light collected by an upward-facing hole in an integrating sphere. Similarly, upwelling irradiance W could be measured by an integrating sphere having a downward facing aperture. Radiance N can be measured by means of a photocell at the end of a long tube containing baffles which limit the solid angle through which light is collected. A radiance photometer can be aimed in any direction; in Figure 13.7 it is shown directed downward.

Suppose an apparatus for measuring H and W is lowered into the deep sea somewhere off the coast of Southern California. The resulting profiles of H and W with depth might appear like those in Figure 13.8.

For a few meters just below the surface the curves are not straight. Very complicated changes in the light field take place in this uppermost part of the sea. It is indicated in Figure 13.8 as layer "A". Region A usually ends at the thermocline where a concentration of plankton produces a thin layer of highly scattering water. The curves are nearly horizontal in this thin layer. In region B the light is diffused and the curves are straight with a slope which is characteristic of the water mass. Region C is a different water mass, characterized by a diffuse attenuation length of 45.6 meters per natural log cycle. At a depth of 360 meters or so a "deep scattering layer" D is sometimes found. Still deeper a clear water-mass E exists.

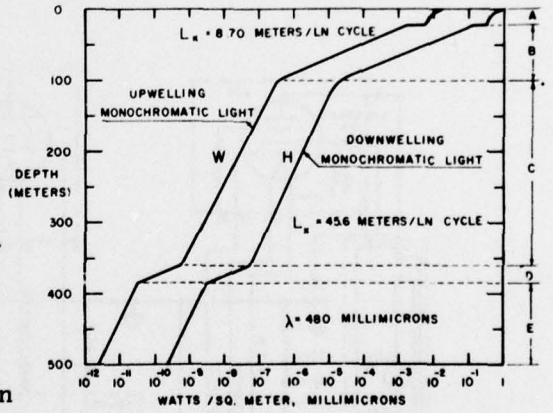


Figure 13.8 Typical radiometric curves

Figure 13.9 is essentially identical with Figure 13.8, but the depth profile of upwelling radiance N has been added. It is approximately parallel to the profiles of W and H and its numerical value is roughly W/π. If the profiles of radiance in other directions, including the horizontal and the downwelling values, were added to this figure a family of nearly parallel curves would be generated and displaced to the right of the upwelling radiance curve. The curve of downwelling radiance would be furthest to the right; its magnitude would be greater

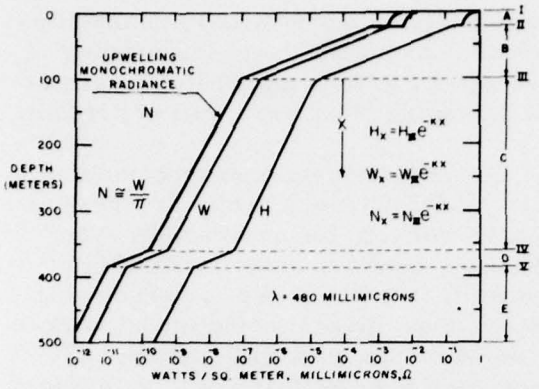


Figure 13.9 Typical radiometric curves

than H/π.

The corners of the curves have been drawn reasonably sharp, and I will discuss in a moment what happens at the junctions, because there we run into some complications. But to a first approximation and at the scale of these diagrams, the junctions of the curves are almost as sharp as the junction of the water masses.

There is a further radiometric quantity with which we should be concerned, namely the absorption of light. This is because light has to be absorbed to do any good. Let us raise the question: how much light is absorbed at any given level, and how does it relate to the attenuation coefficients which we have mentioned? The answer turns out to be extremely simple. It is illustrated by Figure 13.10, the same sort of plot as Figures 13.8 and 13.9, except that now the horizontal axis refers to the power absorbed per cubic meter per millimicron. The dotted curve is the downwelling irradiance H. First we notice the slopes are identical at every stratum; the magnitude changes abruptly because the power absorbed per unit of volume is the depth derivative of the difference between the upwelling and the downwelling irradiances.

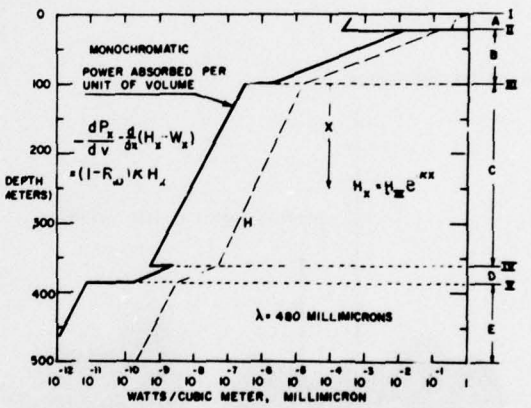


Figure 13.10 Absorption of light with depth

The power absorbed is proportional not only to the power present, but to the magnitude of the attenuation coefficient. Thus we have a basis for knowing how much light is absorbed at each depth.

I mentioned earlier that we have a further complication to be concerned with. This is really too bad, because the picture is so straightforward up to this point.

The way to unlock Pandora's box of troubles in this field is to measure the angular distribution of radiance as a function of depth. Such data can be plotted in spherical polar coordinates as in Figure 13.11. This figure was constructed from data secured by my colleague Mr. John Tyler of the Visibility Laboratory at Scripps. The almost ellipsoidal figure is a polar coordinate plot of the magnitude of the spectral radiance which surrounded the photometer. Near the surface, the "ellipsoid" is tilted in the direction of the sun, but at great depth it is vertical. The shape is affected by the optical properties of the water mass, and in descending from one stratum to another having different coefficients the "ellipsoid" changes its shape. One of our current research problems is to learn how to compute, from basic data, just what the change in shape of these

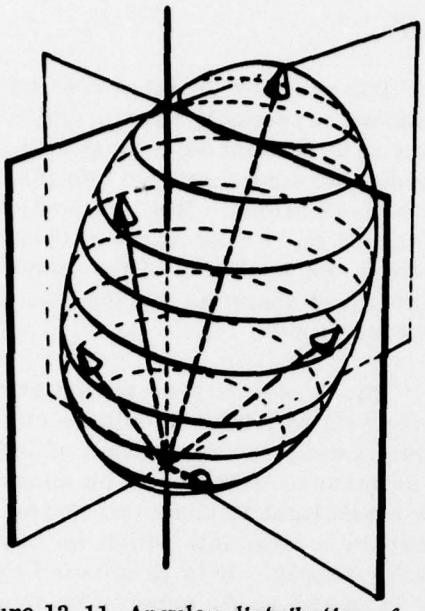


Figure 13.11 Angular distribution of radiance as a function of depth

radiance distributions will be in any situation and how to get the corresponding attenuation coefficients. This investigation is coming along very well at present.

Experimental measurements of radiance distributions are in progress. We have worked, for convenience sake, at shallow depths in rather turbid water because it is easier. We have been able to learn about principles better by working in lakes than by working at sea, although we are doing that also. At present, we are shifting over to deeper operations.

Figure 13.12 shows the radiance distribution photometer we have been using. This apparatus has remote control. There are two phototubes for making radiance measurements in opposite directions simultaneously. Filters can be changed by remote control and the mountings of this equipment enable it to change its direction in space for securing polar curves.

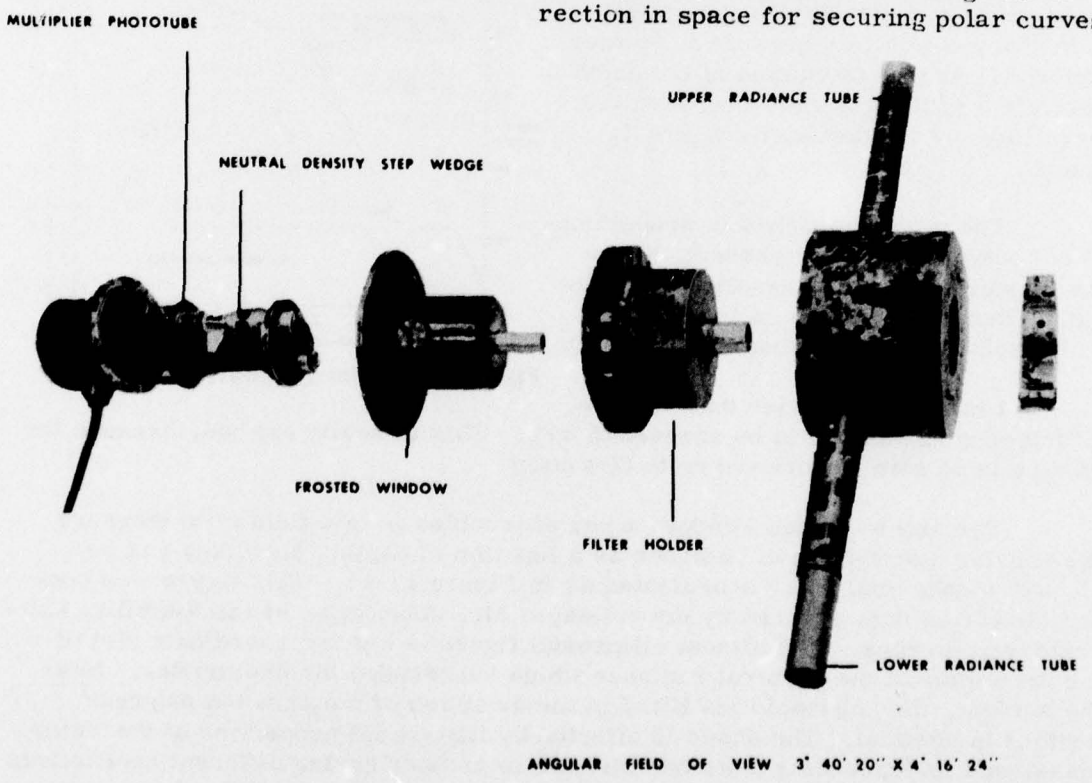


Figure 13.12 Radiance distribution photometer

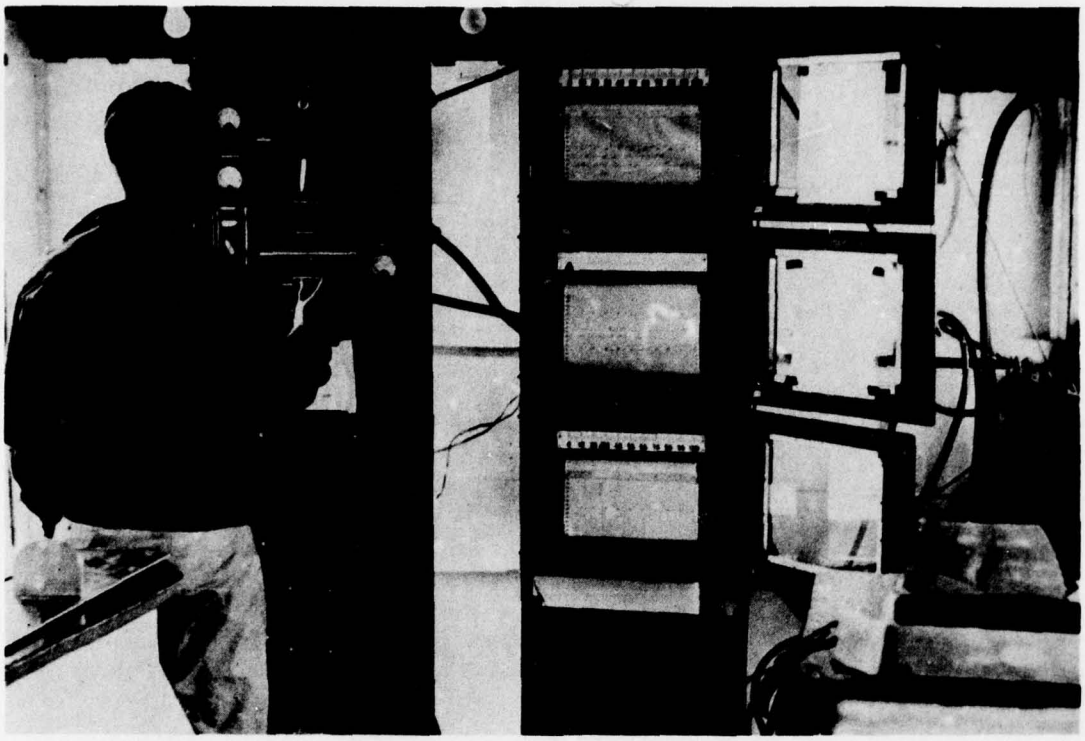


Figure 13.13 Recording equipment for apparatus shown in Figure 13.12

The recording equipment is shown in Figure 13.13. We began using underwater photometric equipment of this type in 1949 and the present apparatus has been evolved by successive rebuilding by many different individuals in our laboratory. John Tyler is in charge of this part of our program today.

In deep-water experiments our underwater radiance photometer is sometimes supported by the raft shown in Figure 13.14.

The interpretation of radiance distribution data in terms of penetration of light into the sea is too long a subject for this talk. I will say, however, that everything can be computed very nicely if the volume scattering function of the water is known.

As an illustration of this type of data, consider Figure 13.15. This is from a paper by Hulburt, showing typical data for water in the Chesapeake Bay. These data represent water brought into the laboratory. Changes in scattering always take place when water is bottled, and this curve may not be truly representative of Chesapeake Bay.

The volume scattering function is the most basic form of optical hydrologic data. From it and the beam attenuation coefficient all other optical quantities can be calculated.

Instruments used to measure the volume scattering function are often

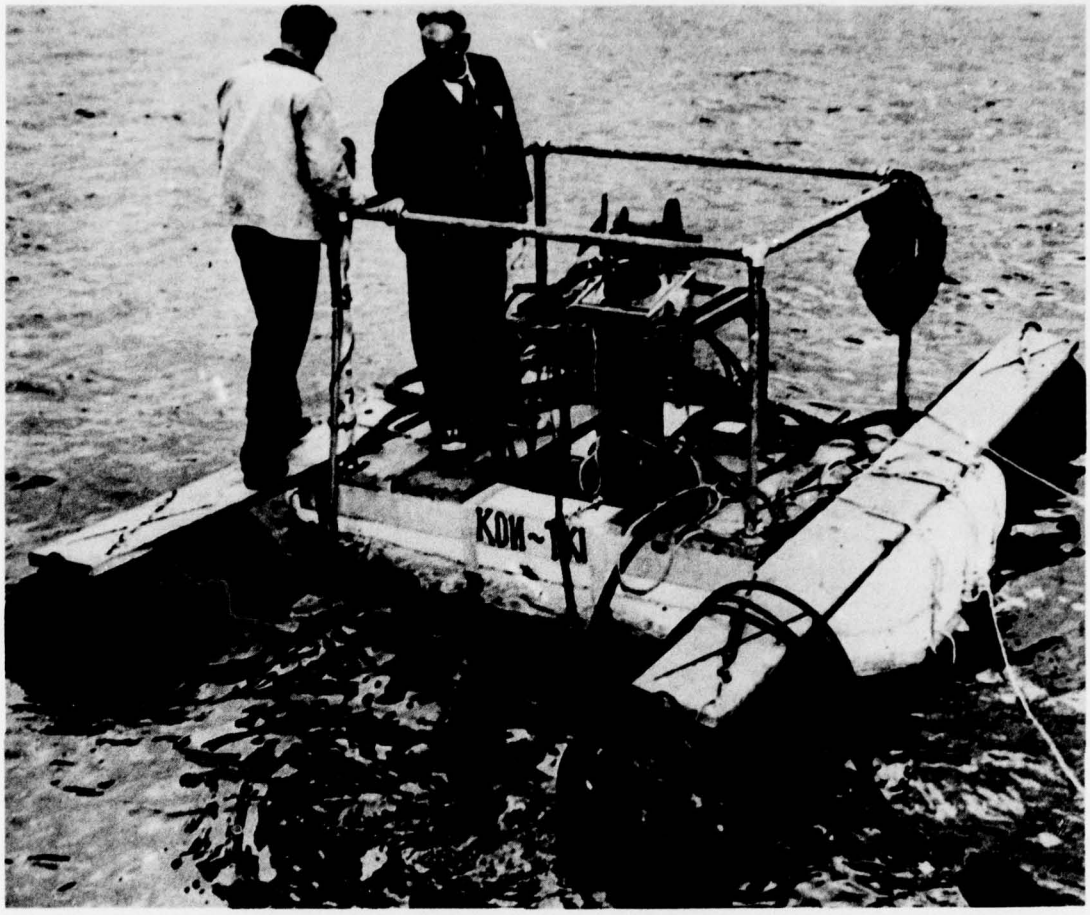


Figure 13.14 Vehicle from which deep sea radiance measurements may be conducted

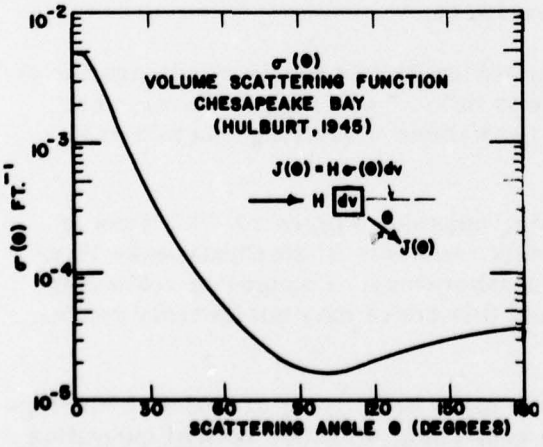


Figure 13.15 Typical volume scattering data for Chesapeake Bay

called nephelometers. Such equipment is somewhat rare, but not entirely unknown. We have had a shallow-water one running for a number of years and we have a deep-sea instrument under construction. There is a British one and I understand that Woods Hole has a very beautiful nephelometer that has been working down to a thousand fathoms or thereabouts.

Nephelometers may prove to be an excellent means for typing and identifying water masses and for identifying their boundaries in oceanographic surveys. For such purposes a deep-sea spectral nephelometer should be developed. Such an instrument is feasible

and is sure to be achieved eventually. This appears one sure direction for future research in deep-sea oceanography.

DISCUSSION:

MR. VINE: Have you done much correlating of the biological activity in the water with the back scattering?

DR. DUNTLEY: No, I am sorry, I haven't. This is clearly indicated for the future.

MR. VINE: The English and Germans are doing this in connection with their fishery problem.

DR. DUNTLEY: It is a fertile field.

DR. BURT: On a recent cruise I used a spectrometer to measure transmission and scattering on a number of samples of deep ocean water. In every case below a thousand meters, the water was more clear than any distilled water I was able to get hold of, which was discouraging. We wanted to get some standards to use.

DR. DUNTLEY: You have raised a good question here. I don't believe the optical qualities of distilled water are known.

DR. BURT: I don't think we have ever had pure distilled water. Any dust in the distilled water will make more scattering than the molecular scattering of the water itself.

I have heard that work is being done on this in Germany right now, and it should be a great contribution were someone to tell us what distilled water is like.

DR. HERSEY: This is just a note on the use that has been made of the Woods Hole instruments by Dr. Clarke and Dr. Backus. They went down to depths, I think, of the order of a thousand meters, in any case, deep enough so that the background level of bioluminescence was well above that of sunlight at that level, so that they were seeing very large signals above the background.

DR. STEINBERG: How does the axis of the "ellipse" vary from the vertical?

DR. DUNTLEY: In general, just at the surface, the "ellipse" points at the refracted direction of the sun ray, but only a slight distance down it turns toward the zenith. As it goes down, the "ellipse" shrinks in size, flattens, and becomes vertical. This takes place in roughly twenty beam attenuation lengths.

14. APPLICATIONS OF ACOUSTICAL TOOLS TO PROBLEMS OF DEEP SEA RESEARCH

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Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

What I have to say about the use of acoustical tools will ramble considerably. This is not an apology, it is just a prediction. Also, a few of the techniques I shall describe are not particularly new. I have been in this field only a very short time compared with several of the people at this symposium, and I understand that when they, who are at least ten years older than I am, were young men, these techniques were well known and had already been used effectively by scientists. I will discuss them only briefly:

Measurement of Acoustic Travel Time

The measurement of travel time of acoustical waves provides a very simple direct means of measuring distances precisely in the ocean. It matters not whether they are distances along the vertical line or along the horizontal.

Perhaps one of the best known and most extensive examples of measurement of horizontal distances is the surveying activities of the Coast and Geodetic Survey in the 1930's. All of you, I believe, are familiar with echo sounders for measuring vertical distances. While echo sounders are widely employed, I am somewhat bothered by the fact that little use is now being made of horizontal travel times for positioning at sea. It remains a technique capable of considerably greater accuracy than any now available for work more than a few tens of miles from land.

A second use of travel time measurements is as an indicator of variations of the physical properties of sea water. SOFAR indicates very clearly to us that it is possible to study these variations in the structure of the ocean waters by the measurement of travel time differences between different acoustical paths in the ocean. Very little has ever been done with this, but many of us who have made these travel time difference measurements are well aware that this is a possibility. (Post symposium note by author: precise measurement of travel time differences with less precise measurements of total travel time obviates the need for fixed transmitter and receiver. Where fixed installations can be employed, total travel times can be useful for this purpose.)

Acoustical Telemetering

Recently there has been quite a lot of interest in our group at Woods Hole in using acoustics for telemetering data from other instruments in the water to a ship or shore station. This is done basically just as telemetering with radio is done. It appears to be just as simple, just about as easy to do, if you are used to working with acoustical radiators and acoustical receivers.

Continuous or pulsed information can be transmitted. The only applica-

tion we have reduced to programming is a very simple one which has been described in the literature by Dow (1954)*, so it seems scarcely worthwhile dwelling on it here. It is an instrument used to broadcast the depth of a net being towed in the water in order to enable the biologist operating the tow to know at the time the depth of the net.

Now this particular application can probably be done in other ways, such as the electrical schemes that are being pursued at Scripps. However, I have felt that it was very valuable for us to become accustomed to making this sort of observation against the day when we may want to transmit fairly complicated information from remote buoys which cannot be cable-connected because we want them to be free-floating or to remain anchored for long periods impractically far from land for experimental cable connection.

Sound Scattering Studies

Sound scattering studies are useful to deep water problems that have been touched on very little at this symposium, the most obvious of which is in marine biology.

Now, returning to travel time measurements, it is patent that echo sounding constitutes their most wide-spread use amongst oceanographe[illegible] a result of the paper by Luskin et al.(1954)** precisely measured sound[illegible] been much discussed recently. There is division of opinion about the value [illegible] precision here. The people truly interested in precise measurements are m[illegible]up on this stage, and I don't know how you people stand out there. I think we need not go into that very fully except to point out that two slightly different sets of instruments have now been thoroughly tested at sea and can be counted on to provide echo sounding travel times with precision generally limited only by resolution on the record. Recently a writing rate of approximately 288 inches per second was employed in sounding for depths of about 21,000 feet; on such a scale a foot of water is represented by slightly over a tenth of an inch on the record, which can be shown to have a timing uncertainty better than one part in 50,000.

Such high resolutions are not useful except over relatively smooth topography. Recently both the Lamont and Woods Hole groups have been using them to look at the small details of bottom roughness in deep water. We have had little opportunity to compare our findings in detail, but their general, character should come as no surprise. Over a flat, smooth bottom at resolutions commonly employed, successive echoes are found to agree very closely in travel time and general appearance, often showing a strong recording the same length as the outgoing ping and followed by a much weaker reverberation tail, as in Figure 14.1.

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* Dow, Willard, 1954: Underwater Telemetering, A Telemetering Depth Meter. WHOI Ref. No. 54-39, May 1954.

** Luskin, B., B. C. Heezen, M. Ewing and M. Landisman, 1954. Precision Measurement of Ocean Depth, Deep-Sea Research, Vol. 1, No. 3, April 1954.

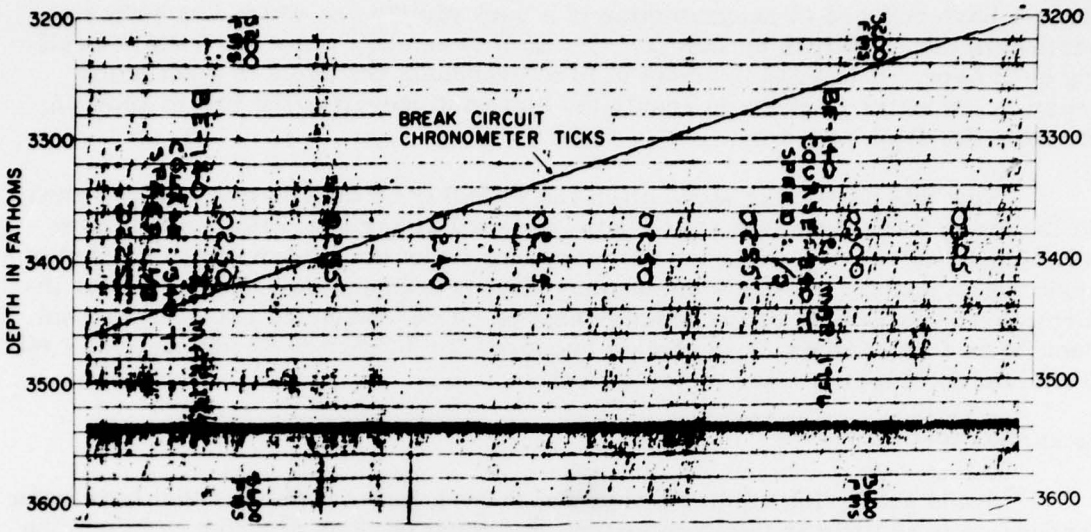


Figure 14.1 High-resolution echo-sounder trace

Now, if you blow this picture up, say ten to a hundred times and use much shorter pulses which, incidentally, present no difficulty whatever, you will see a considerably more complicated record (Figure 14.2). On this record a very striking feature is the variation in sounding due to the roll of the ship. Thus, for this work we need a more stable platform. Further, the echo sequence is no longer continuous, but consists of interrupted sequences of echoes whose travel times show at least small variations. Other examples show sequences having a crescent form, as in Figure 14.3, which corresponds to what one would expect from the ship passing by a small reflector; the minimum travel time corresponds to closest approach and the two horns to the approach and departure. Figure 14.3 is an especially striking example of a complicated echo sequence over a rough

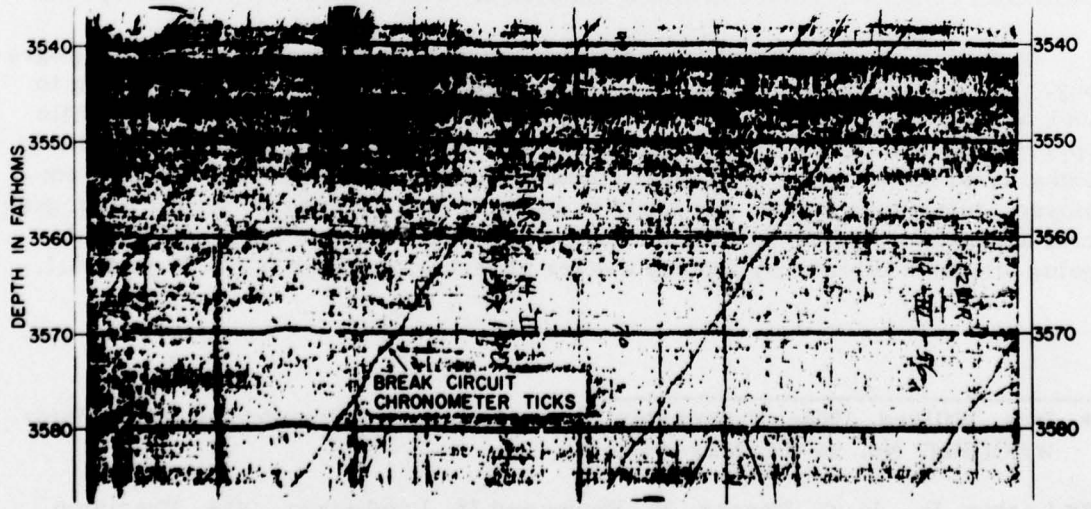


Figure 14.2 Expanded echo-sounder trace



Figure 14.3 High-resolution echo-sounder trace for a rough bottom

bottom. Interpretation of the form of echo sounder traces has been attempted only to a limited extent so far as I am aware, and we often need better information than is obtainable from a ship-mounted instrument anywhere from one-half to over four miles from the feature under study. But we can learn considerably more with high resolution than we now know; these studies are underway and can be expected to tell us more about small-scale features of the bottom.

I think it would be very profitable for us, and would free our minds a great deal, if we thought about echo sounding not as what was originally intended, namely to take the place of the lead line, but if we regard it as the whole art of measuring position in the vertical, of any object such as the ocean bottom, fishes, or an instrument in midwater, or the ship on top. Then the subject logically divides into measuring the depth of water, looking at these bottom roughness features that I have just talked about, observing natural sound scatterers in the water and also measuring the depth of any instrument.

The exciting example today is Swallow's floating buoy. I understand from Dr. Deacon this has not been actually finished yet, but as I suspected, a big part of Swallow's hope for the future is to be able to make precise measurements of the depth of this buoy by having a sound source on it, or by echo sounding on it.

Now the sound source on the buoy has particular attractions because you do not have to be directly above it in order to make a precise measurement of depth. You can be off to the side somewhere, as long as you know precisely your horizontal distance, and if you have a sound source loud enough to measure a number of surface and bottom-reflected sound arrivals and you have a sufficiently accurate measure of the shape of the bottom (the terrain should not be rugged). This is just a problem of measuring depth, which, under those circumstances becomes a trivial one. So the problem of depth measurement in that case goes back to having a sufficiently loud sound source to make these paths easily observable.

Now there was a lot of talk yesterday about how we have no means of know-

ing where we are geographically in the ocean. And this was brought to a head a little bit when the question arose as to how we knew about the drift of Swallow's buoy as accurately as we did over such a short time, and I think — and I do not want to be misinterpreted here — it is correct to say an oceanographer can have very precise locations whenever he wants them, even tomorrow, if he wants them badly enough.

I think we tend to think too often in terms of the big, highly integrated systems like LORAN nets, which do not really cover much of the oceans, or SOFAR, which obviously has great potential. There are very simple systems which could be used over very long ranges that just do not tax anybody's ability to handle electronics or the various mechanical contrivances involved.

Swallow has already produced what amounts to part of this system, in using a single buoy, and if you want to use three buoys, a positioning system can be devised that is unambiguous to interpret and simple to operate. I will just sketch out a system which could be used and has been used in part, several times in the course of our own acoustical work. We have not had all of the elements of this sort of a system in use at once, but I see absolutely no difficulty in running them, on, say a 40-foot power boat like the ASTERIAS.

It is so simple it hardly seems worth taking the time to describe. Consider the system to consist of three anchored sonobuoys, let us say of the sort that Gaskell and Swallow used in their seismic observations on the CHALLENGER Expedition recently, the same family of instruments developed by the Cambridge geophysics group. These sonobuoys each transmit on a different frequency to a ship that wants to know where it is. I will say, just to make this problem really easy, that we will restrict it to knowing where the ship is within a patch 50 or 100 miles across, so that this distance is approximately the separation of the buoys. A very simple sound source that will transmit readily over those distances at fairly high frequencies is achievable by anybody who can operate the sonobuoys. As far as I can see, only the horizontal motions of the buoys limit the accuracy with which you could know the ship's position. If you feel that the buoys will move around too much on the surface for the depth of the water you are in, I suggest that you make a submerged buoy. An oceanographer would have to tell me where it would best be put, for tolerable motion, and run a light cable from the sonobuoys up to the radio link. That is one possible scheme. The second possible scheme is an acoustical transponder, of which several have been designed, mounted on the buoy, and dispensed with the radio.

The Lamont or Woods Hole precision recorders, or, in fact, any echo sounder recorder, can record position data for such a system on a continuous basis, say with a duty cycle of every few seconds if such detail is required or useful.

Now suppose you want to know where a free-floating instrument is in this area. If you can afford a surface drogue, the ship, located with respect to the anchored buoys, can follow the drogue visually. Or you could make the drogue itself a sonobuoy or transponder. If the observation requires such refinement it would probably be better to employ a transponder on the free-floating instrument and dispense with the drogue. Another possibility which intrigues me very much

is that of using a precision timing device for actuating sound sources on the anchored buoys. I see no practical obstacle to this except the necessity of putting enough power in the buoy to that it may operate as long as necessary. But I see no difficulty right now in doing this for a few days.

In short, it is possible for any very small group of individuals to have precise position-keeping relative to an arbitrary datum, if they want it.

I think it is not worthwhile to discuss telemetering in detail. It seems to me so obvious a possibility for future mid-water work that it is going to have to be developed by individuals who want to have the information that it can give.

Our group at Woods Hole is interested in that. We are not interested in it at all as a technique. We are interested in it as a tool for doing other jobs. We wanted to know the depth of a net, therefore we built a depth meter. We have other problems that require other instrumentation, and we have built those. I think the development of telemetering with acoustics will come along when, as, and only if, that kind of interest is shown in it.

I thought some of you might be interested, since the work has not been published, in hearing a little about what we have been doing in scattering layer research, and what we think this may mean in such research in the near future.

You may have seen the note in Science, by Kanwisher and Volkmann (1955)* pointing out a fact which I think probably was known to many of you before this, but it is an important one in an orderly discussion of volume scattering. When an observation is made with an echo sounder transducer near one of the scattering layers, the chances are very good that it will be found to be composed of rather widely dispersed individuals. This was observed by Anderson, in his paper of some years ago (Anderson, 1953)** and almost anyone who stops his ship and continues to observe with the echo sounder will find this to be the case upon occasion even at shallow depth. One sees a fantastic collection of the crescent type echoes of the sort I described, about a rough bottom (see Figure 14.4). When we noted that this was commonly observed in the deep scattering layers, we realized that we had a perfectly obvious combination acoustical and photographic technique for identifying the scatterers by mounting an underwater camera with the echo sounder. We have used this combination over the past year and a half. Some of you have heard the long, sad story, which I will not repeat here about how such a simple experiment can take so long and be so complicated. But nevertheless, we are now making successful photographs at rather shallow depths of these crescent-shaped echo sequences which we see generally above the scattering layer, or

* Kanwisher, J. and G. Volkmann, 1955: A Scattering Layer Observation. Science, 121 (3134):108-109. WHOI Contri. No. 744. 21 January 1955.

**Anderson, V. C., 1953: Wide Band Sound Scattering in the Deep Scattering Layer. SIO Ref. No. 53-36. University of California, MPL, Scripps Institution of Oceanography, 1953.

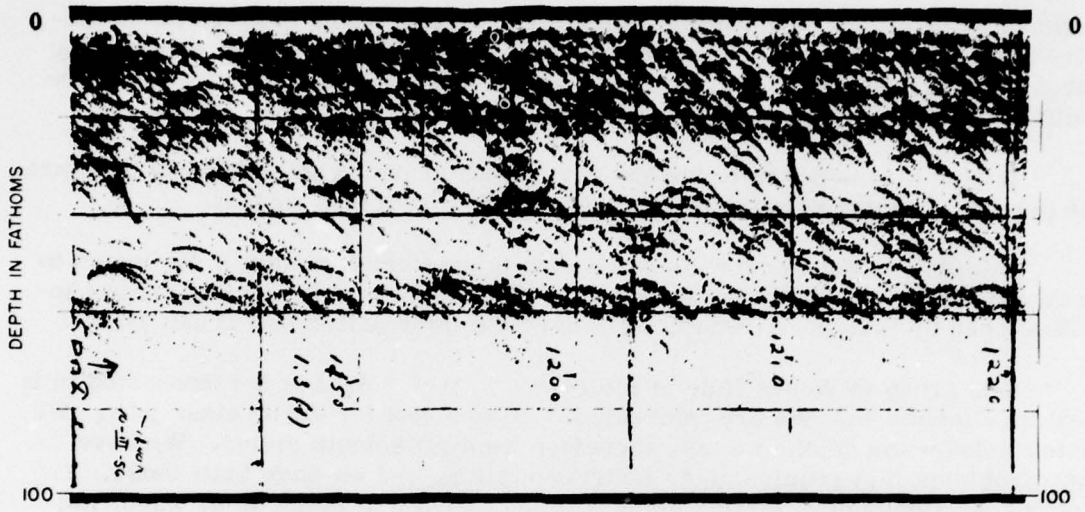


Figure 14.4 Echo-sounder trace showing dispersion of individuals in a scattering layer

in the evening, when all the layers intermingle near the surface. We have not made successful observations during the middle of the day at greater depth, when you can be sure what layer you are dealing with, but at shallow depths. We always photograph fish when we find anything at all in the field of the camera. (A report of our work to date is in press.)

Now, if you look at scattering in the ocean by lowering an echo sounder in steps, as has been done on one occasion at night by Backus and Johnson, a section can be made of the distribution of scatters as a function of depth. The reverberation near the surface, which I am sure is not all surface reverberation but is indeed mostly volume reverberation, appears like unresolved background noise with possibly some crescent-shaped sequences superimposed. At depths, say, greater than fifty fathoms, you no longer see this general background, but crescent sequences stand out, more or less densely distributed. A layer will show up as a series of crescents at nearly the same depth. Some layers appear to be confined within relatively narrow depth limits.

No extended, systematic observations have yet been made with either of these techniques but the limited observations available lead us to hope that the depth of the layers can be determined and the resolved individuals identified at any one place. Furthermore, we have been studying the scattering spectra of the layers. The spectrum of a layer changes during its vertical migration from which we hope to study its behavior during migration. Further, it may be possible to identify a population group from its spectrum, thus allowing us to study its geographic distribution. It is not demonstrated, but appears likely that the groups which may thus be studied are also the resolved scatterers, which may well all be fishes. But these are comparatively large animals, far from the base of the food chain. Consequently their study does not contribute to such problems as the basic productivity of the sea. And I have the notion, and it is only a notion, that we may be able to learn a lot more about productivity problems with acoustics by studying the near-surface scattering deliberately, probably going to somewhat

higher frequencies, and by taking a harder look at what amounts to the fainter scattering which apparently comes from a much denser population in the ocean than the deeper scattering layer.

DISCUSSION:

DR. DEACON: I am sure our navigation problems can be solved with buoys, but I have had some experience with three buoys floating in the surface, and they do move about quite a lot.

MR. ISAACS: How do you locate the buoys in the first place?

DR. HERSEY: You can locate the buoys relative to themselves.

MR. ISAACS: Fifty miles apart?

DR. DEACON: Ours were only three miles apart.

DR. HERSEY: Steam around them. You cannot know their absolute position.

15. VEHICLES AS INSTRUMENTS FOR OCEANOGRAPHERS

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Woods Hole Oceanographic Institution
Woods Hole, Massachusetts

In discussing vehicles as instruments, we should look over a span of several years, both in the past and in the future, and try to show that actually there is a very wide number of classes and individual types of vehicles used for learning about the ocean. The reason for enumerating the several systems is to show that in our customary zeal for using a particular one, we do not forget that there are many.

I am sure if the airplane had been the only craft invented to date and we did not know about ships, we would still be doing oceanography, but we would have a different point of view about the ocean and we would be learning different things about it. The ability to cover large areas quickly would be much more under control than it is now, merely because we would be travelling at 300 knots rather than 10. If we had had only a deep-diving submarine and not the surface craft, I presume that we would still have had this meeting and that we would still be doing oceanography, but I feel sure we would have an entirely different concept and different perspective of the ocean. As a result, we would be discussing other interesting aspects of oceanography.

Another point is that vehicles have come along which are called new even though most of them have been resurrected out of the past because new problems or new techniques have made them more useful.

Actually, I think there are about four things required: the problem, the vehicle, the man, and the time.

(Comment from the Floor: "And money.") No, I am not really worried so much about the money. Most of these things are copied from work done in the past when people had far less money than we have now. I am not worried about money as much as about the zeal of people to identify the problem and the particular vehicle that will help them solve it.

For example, the buoy work which is now going on was started when there wasn't very much money. The techniques were not perfected then but neither are they perfected yet. The cable companies did their work before the turn of the century, and I don't know if that has been changed very much since then, but what I want to get across is that things usually get started on a shoe string.

Furthermore, if somebody from another profession and financial source does furnish a new vehicle, which often happens, we want to capitalize on using that vehicle as much as possible. We may be able to go along as free passengers, or if we talk to the people who make the vehicle they may be delighted to make it so that it will help solve some of our problems.

I now call your attention to Table 15.1 to show the broad spectrum of

measurement situations that exist. The first one is the beach. The oceanographic library is the most obvious, but it is not self-sufficient because it needs frequent recharging. The study of waves and swell and the study of ocean weather have probably been studied more from the beach than they have from the sea. Our radio stations are going to be one of the most important parts of the rapidly growing buoy program about which we will hear more from other speakers. The seismograph stations have been and are going to be the warning indicator for unusual activity when it occurs at sea.

TABLE 15.1

Platforms for oceanographic research

Beach	Library Radio* Waves Weather Seismograph Submarine Cables
Ships	New Research Ships* Converted Research Ships Barge Buoy
Aircraft	PBY Patrol* Seaplane Helicopter
Submarine	Conventional Small Deep* Bathyscaphe Robot

Submarine cables have big oceanographic effects, and now that a few oceanographers have got together with the cable companies, we have learned a great deal from them with negligible expense and profit to all.

On ships, perhaps the first thing really is that they provide opportunities which have been used a great deal but most oceanographic work has been done from conversions. Probably more oceanographic work has been done from converted ships than physics has been done from converted cyclotrons. That is why I have a star* on research vessels, and here I mean vessels designed for research. I think Don Pritchard was right when he said to leave research ships out of this conference or we would never get through. But I do want to leave the star.

Next is the barge. Al Crary went to sea on the ice island, T-3, which was a huge "barge" in the Arctic Ocean, and he got not only one of the best depth profiles made anywhere, but he put three hydrophones out and also measured slopes. Al Crary, being the man he is, made unique use of the "barge" he was on.

When it comes to buoys, I want to emphasize that there are more different

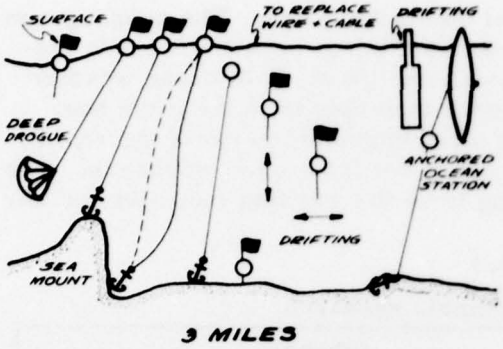


Figure 15.1 Types of buoys potentially useful in oceanographic research

kinds of buoys and more uses for buoys than there are speakers at this symposium. Figure 15.1 shows that buoys are of many kinds and can serve many functions. Starting at the left, we have a plain simple surface float that drifts with the wind and tide. This is the marker buoy or drift bottle which has been used so often with or without radio links. An oceanographic ship hove to for measurements is acting like a sophisticated surface buoy.

Next in line comes a surface buoy with a deep drogue attached to it so that it will tend to move with the deeper water. The third surface buoy is anchored to a seamount as is often done to make a reference marker when surveying a seamount. The fourth buoy is anchored in deep water. It has a solid anchor line drawn for a heavy metallic anchor line and a dotted one drawn for a lighter-than-water polyethylene line.

The fifth buoy is submerged and as a result has some special characteristics. It keeps its anchor line taut and hence holds depth fairly well and does not yaw around very much when the current changes. Also it will not be run down by surface ships.

The sixth and seventh type are unique because they go down and up with ballast and buoyancy. As a result they dispense with wires and cables and make it possible for a ship without a winch to have several separated instruments on the bottom at the same time.

The eight buoy is a splendid newcomer to the field and was developed by Swallow of England to drift with the current and depth. This is accomplished by trailing a buoy which is less compressible than water. Swallow followed his buoy acoustically and many of us will probably follow Swallow enthusiastically.

The ninth buoy is a necked-down version of the common wave pole designed to be very stable in a sea-way so as to approximate a fixed reference in the waves.

The tenth buoy is a favorite of mine because it offers the possibility of a stable platform at sea and a deep-water research station.

When oceanic islands are drawn to scale they don't look like mid-ocean research stations. The base of Bermuda is 100 miles across and Hawaii is part of a long, broad chain of mountains, mostly submerged. A deep-water anchored vehicle out in the center of an ocean basin could contribute a great deal to our understanding of the ocean basins. Several other speakers will talk about specific kinds of buoys and buoy problems.

When you come to aircraft, Woods Hole has been using a PBY, while Gifford Ewing at Scripps has been using a small Grumman. Ewing is gaining a new

perspective of the ocean from his airplane, and when you talk to Giff you get a little of that perspective yourself. The work at Woods Hole, by Bunker, Malkus and Richardson has shown they can get vertical air motion from acceleration measurements and also make temperature measurements of the sea surface from overhead. The PBY is not the right kind of airplane to cover an ocean in a day but I think, before this decade is over, that with a better plane we may be in a position where the Gulf Stream can be traced from the Straits of Florida to Iceland or England in one day, and get a pretty good temperature profile of the surface to within a degree or a few tenths of a degree. Planes and equipment, or the prototypes of equipment, exist and I think there is every reason to think that, within a decade, we will do experiments on this. And when there are planes that are able to land on the sea, there will be or should be oceanographers aboard.

The day of the helicopter is here. Because of its expense, it is largely the military who use it but it is hard to think that ten years or maybe five years hence, oceanographers will not use some small helicopters to operate within a few miles of a parent ship.

Then there is the question of the submarine, first the conventional submarine — which I am ashamed to say we have done very little with in oceanographic research. At the time of the last war they took out the glass ports for the perfectly good reason of not wanting water to come in after a depth charge, but we oceanographers have not put any of those glass ports back in so that we can look at the scattering layer in the evening when sea creatures are close to the surface. An experiment of this type would involve far less money and accumulated time than this meeting has.

Then there is a small deep submarine, which can go as deep as the principal scattering layer goes during the daytime. I believe that Piccard calls this a mesoscaphe. Techniques for building a submersible of this type are fairly straightforward and have been used on oceanographic instruments.

The bathyscaphe is a vehicle which is with us today and, in fact, has been for some time. People who have far less money than we do have built it, used it, and gone down in it. I am sure we will hear more about that, and we certainly saw some fine movies about it last night from Dr. Piccard.

There is also a robot submarine to consider. You may call it a submarine, a vehicle, a drogue or something else, but Bob Waldon and Bill Richardson at Woods Hole have built a little robot dory. I am sure someone will put diving planes on it before too long.

Figure 15.1 merely indicates that there is a very wide range of buoys. We have surface buoys, which we have all used, from laboratories on both the east and west coast. The west coast people have used the deep drogue. Seamounts have been used by every institution in the business. But we have done little but talk about anchoring close to the surface and do some work in shallow water.

Before the war, Ewing and I made up floats which went up and down, to replace wires and cables. Swallow has made a buoy float also, to move horizontally.

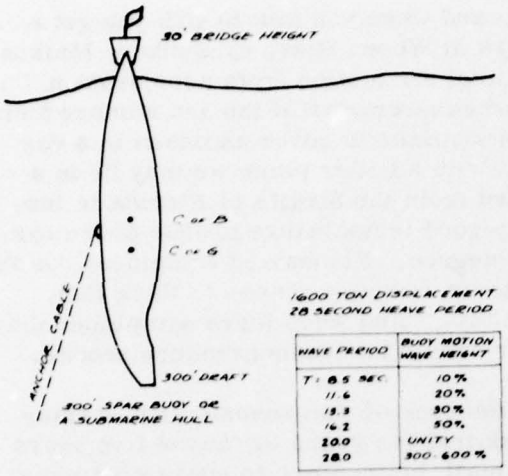


Figure 15.2 The ocean research station

We have used various types of wave poles and large floats. The two shown are meant to indicate buoys which are large and stable enough to give us a new concept of a platform at sea.

Now, we all know that a wave buoy, or a spar buoy, is a more stable device than a surface craft of comparable displacement. If we want stability, draft is a great help. In these two cases we essentially change our primary goal from transit to occupation. We keep talking about the open ocean and the deep sea. We say how nice it would be to make a time series, that the anchor cable broke after 36 hours of anchoring, but we are still trying to do all of this with a vehicle which was primarily designed to get out there and get back, to leave a shallow harbor and get back to a shallow harbor. It may pay us to look at Figure 15.2 showing a vehicle which was primarily designed to go out and stay.

I do not want to give the impression that this idea is particularly new. It has been kicked around by various groups at Woods Hole and Scripps for several years. I would like to call it an ocean research station. Whether or not it is a large buoy, as indicated by Devereaux or Van Dorn and others is of no consequence. I have indicated it as an up-ended submarine simply because every time I go by New London and see all those submarines in moth balls, I wonder if it would not be better for the country and for us if one were used as an ocean research station.

There are several interesting things about a buoy of this type. One, it is big enough for people. I believe firmly that a good instrument can measure almost anything better than a person can if you know what you want to measure. I think most instruments are so good that they tend to end problems. But people are so versatile they can sense things to be done and can instigate problems. I find it difficult to imagine what kind of person should have been put on the BEAGLE instead of Charles Darwin.

One of the characteristics of a buoy of this type is that it could be made with a 28-second period. This means even with a wave period as long as 20 seconds the vertical motion of the buoy will be no longer than that of the wave. For shorter wave periods such as eight-twelfths seconds, the vertical motion of the buoy will only be ten percent of the wave height. This means it would probably be as or more stable than any ship afloat irrespective of size. However, an iceberg would be even more stable.

Where you would put such a buoy and what you would do on it are interrelated. Looking at Figure 15.3 I would, myself, tend to think that in the beginning, off Cape Cod (point one), in 40 fathoms, is a likely spot to ride out the first few

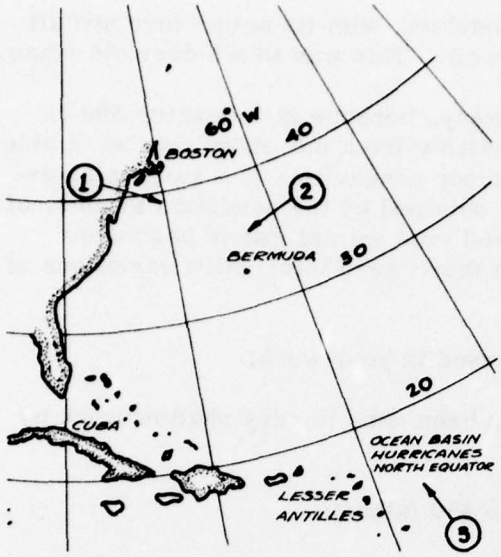


Figure 15.3 Areas where the "ocean research station" might find use

storms but if this test period were successful it would seem best to get out where the bottom is flat for hundreds of miles in every direction. Point two in the Sargasso Sea should be one very good position. Point three is in an ocean basin, in the north equatorial current, and in the general area where we think the hurricanes start.

Because the motion of such a buoy has a longer period than the ocean waves, it would help establish the true surface perhaps better than any other platform available to us. As such, it would serve as an excellent platform for an echo sounder to study open ocean tides over flat bottom. Its vertical motion would be so much less than that of a ship that it should be much easier to anchor.

In conclusion, I want to emphasize that with better vehicles we can do things better, but I think the main advantage is that we gain a better perspective of the ocean and our problems.

DISCUSSION:

DR. HERSEY: We found that buoys planted in 20 fathoms of water have been able to ride out all of the storms of, I believe, approximately two winters, in Hawaii, on the western side of the island. Their motion was observed in the heaviest storms we could find, with a sextant held horizontally, and knowing their distance from shore, the horizontal motion of a target above the surface. This amounted to seven feet in the most severe storm, approximately ten to fifteen-foot waves.

So I think it is quite optimistic to have very good performance in, say 40 fathoms.

MR. LAWTON: It seems there were two potential vehicles that haven't been mentioned. One is the Texas Tower, the other is that used by the cable company a number of years ago in a problem I was connected with, entrenching cables in the ocean bottom. It has potentialities for oceanographic work, I think.

We wanted to establish the contour of the bottom of great accuracy over routes where we were plowing the cables in, as we called it, and we were particularly concerned with avoiding small concavities on the bottom and would lay the cable in the trench, because of the greater strain when it came out of the hole on the other side of the concavity.

A sled was built which was towed by the ship. The towing cable contained

an acoustic probe and we had a recording pendulum, with its points fore and aft on the sled, and damped by being enclosed in oil. This was towed over the route.

We obtained, I think, very good accuracy, because in reversing one of those and going back as close as we could navigate from our buoys, we were able to reproduce the exact curvatures of one of these concavities to a surprising degree. The integration of those slope curves obtained by the pendulum graphs, of course, gave us the contour of the bottom, and what we did was to pass over those parts which were uninteresting. When there were interesting variations of slope we took a good look at it.

Possibly a vehicle like that could be used in your work.

MR. VINE: I believe a bit of that has been done in very shallow work by oceanographers.

MR. LAWTON: We used this down to 450 fathoms.

16. DEEP-SEA MOORED BUOY INSTRUMENT STATIONS

Willard Bascom
National Research Council
Washington, D. C.

At first, a subject so simple as buoys, against the background of the complex subject of instruments, may seem to be almost be almost beneath contempt, but I submit if we inspect the subject a little more closely, we may find that there are some very valuable techniques available to us which have been very little utilized. I missed the first part of Dr. Hersey's paper, but I am sure that he talked about some of these matters, and I would like to go into some slightly different ones.

There are very good scientific and economic reasons why oceanographers should demand that considerably greater attention be given to the development of deep-moored buoys which can assist them in investigating the ocean basins. Comparatively simple buoy-systems can be harnessed to a surprising number of tasks to bring the ocean research within technical reach of certain important and long-needed data which would be difficult, if not impossible to obtain otherwise.

Special buoys, and their moorings, can be used to hold instruments in a fixed position so that continuing measurements of ocean dynamics can be made — or rare physical phenomena monitored — far from land. In so doing they relieve the investigator of a considerable financial burden by replacing the expensive* ship.

Expendable, easily moored marker-buoys should prove to be valuable in the on-station navigation of research ships which seldom know their position with respect to the bottom or to deep water. By improving the likilihood of taking a successful core or making a satisfactory net or dredge haul in a single try, the efficiency of the entire ship operation can be improved.

In the day when a deep vehicle is available so that oceanographers can go to the depths in person, the buoy mooring wire may prove to be a valuable aid to vertical navigation, corresponding to the descending line used by the human diver. As experience accumulates, no doubt many additional uses and applications will be thought of.

It is nearly four years since the first deep mooring of this type was hopefully and successfully tested off San Diego. Since then there have been a number of improvements made in materials, in the surface station or float, and in handling techniques. Most important, enough of such moorings have now been made that the feasibility of installing and utilizing them in fairly rough sea conditions

*Oceanographic ship time, for our present small ships, is valued at about $40 per hour — plus scientists' time.

is well established.

The components of this deep mooring system were previously described* in detail but their principal features can be quickly recounted.

The high-tensile-strength, low-drag music-wire mooring line is the mainstay of the system. It is anchored by means of a heavy and compact clump and is kept taut, and nearly vertical, by an underwater stretcher-buoy floating some 50 meters below the surface. To this underwater buoy a surface float in the form of a Fiberglas skiff is tethered by a slack line. Many variations of this general layout are possible; naturally, the specific equipment and techniques to be used will depend on the job to be done.

The purpose of this paper is to examine a number of possible uses for deep-moored buoys in the hope of attracting partisans who will exploit their advantages.

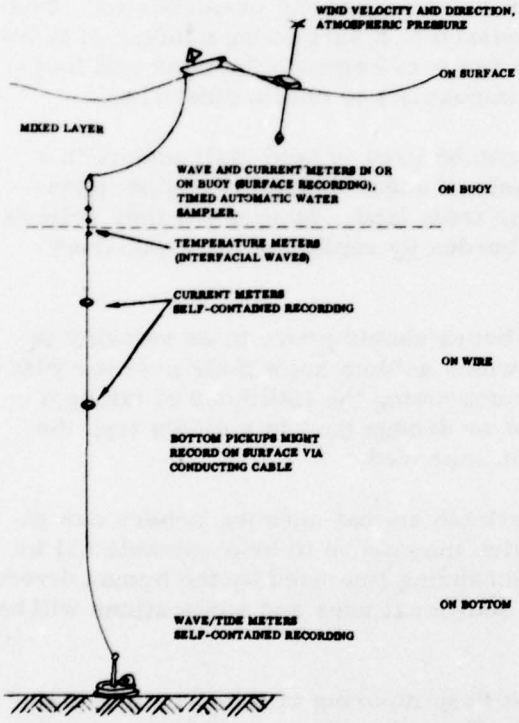


Figure 16.1 Possible Instrument Station buoy

Instrument Station Buoys

Figure 16.1 is a grand composite diagram of a single mooring burdened with many instruments — nearly all of which are quite imaginary — but technically feasible. Some day they will be designed and built and installed in a manner similar to this.

Instruments can be attached to the anchor, the wire, the buoy, or the surface float (and its extensions) in a variety of ways.

Bottom wave/tide meters can probably be designed which are either self-recording or can transmit signals, via a conductor in the mooring line, that are recorded on the surface. A series of temperature pickups (surface recording) could be attached to the wire near the thermocline in the hope of measuring amplitudes and velocities of interfacial waves. An array of three deep-moored stations so instrumented might

*Bascom, W. N., A Deep-Sea Instrument Station, Scripps Institution of Oceanography, Reference 53-38, June 22, 1953:
Or, in revised form, Bascom, W. N., A Deep-Water Instrument Station, National Research Council, Washington, D. C., November, 1955.

make it possible to determine the direction of the waves.

Pressure pickups for wave-meters which record in the skiff at the surface can be attached to the underwater buoy; in fact, this is the purpose for which the original stations were built. Measurements are subject to some inaccuracies with respect to long-period waves but would give a fairly accurate account of swell. Current velocities above the thermocline are likely to be substantial and simple current meters capable of making direct measurements might be attached to the underwater buoy which would give satisfactory data. A series of samplers might be pre-set to take samples of water (possibly containing plankton or fall-out particles) at timed intervals.

At the surface, meterological data can be collected and recorded — or perhaps telemetered to distant receivers. Combined wind and wave information received from moored stations during a passing storm should contribute measurably to the knowledge of wave formation.

It is fun to dream for a moment about a deep-water current meter. The requirements of orientation, of response and delicacy of measurement and recording, are less demanding than those of a guided missile, for example — and the environment is more gentle.

If it is desirable to measure very small currents (below one cm/sec) in all directions at all depths, an instrument to do the job can probably be devised. (See Figure 16.2) Calibrated paddle-shaped discs, both horizontal and vertical, equipped with strain gauges could be interposed in the current field and their signal could be amplified and recorded on wire. Orientation could be controlled by a gyromechanism and programmed measurement could be made at compass points; recent technological advances in miniaturized amplifiers, direction control devices, in compact power supplies, and in packaging of delicate parts, would of course, need to be fully utilized. Housed in a proper container, such a current meter could be hung on the buoy wire and left for several hours or days to obtain data. Several might be used simultaneously at different depths or a single one might be taught to lower or raise itself at a known rate to obtain a picture of current distribution with depth. Over sea-

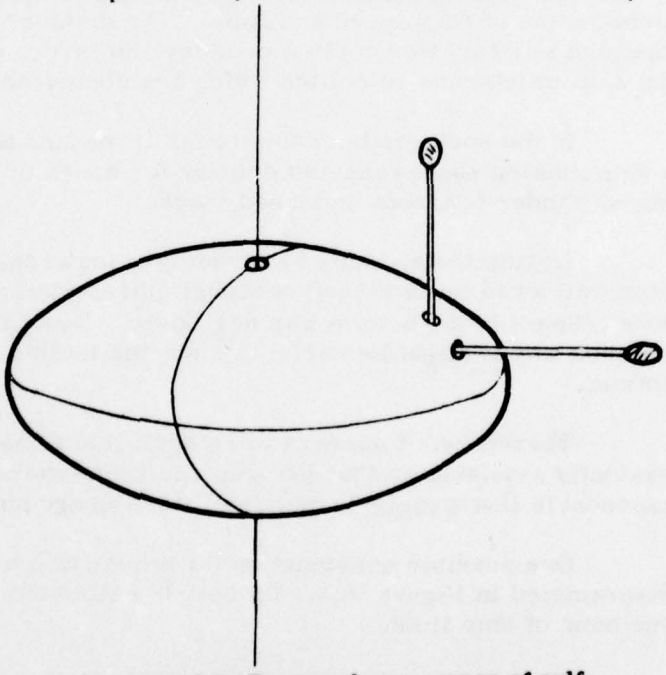


Figure 16.2 Proposed appearance of self-contained meter for measuring low-velocity currents at all depths. (Supported on taut-wire below buoy)

mounts, near surface convergences, at points where upwelling is suspected, or at the approaches to the Arctic sea, such an instrument might return otherwise unobtainable information.

The construction and installation might be difficult, expensive and possibly frustrating — but it seems to be possible. The problem is: How badly do you want to know?

Other instruments, or variations on these, might be designed which would wait passively on the mooring for extended periods of time to be triggered into operation by a change in environment. Winds above 50 knots; waves above some specified height, or longer than some period (tsunamis); or radioactivity or twice background might be the criteria under which monitoring instruments would be automatically started.

So much for moored-buoys as stations which make it technically and economically feasible to instrument the deep sea.

Marker Buoys

A chronic problem of the research vessel is that of station-keeping and precise navigation with respect to the bottom and the deep water while gear is overside — for a ship is only hove-to with respect to the surface water. In a dredging operation, for example, it might be desirable to move the dredge some specified distance along the bottom — which would apparently result from some combination of ship speed and time. The distance which the dredge actually sampled is likely to remain a mystery, however, since currents may be acting on the ship which have velocities which are comparable to dredging speeds.

In the southern hemisphere far from land and LORAN in cloudy weather, a ship making short runs and drifting for hours or days on a series of stations might wander far from a planned track.

Coring tools, badly battered by being dragged along rough bottom have been retrieved by surprised oceanographers who have supposed that ship velocity with respect to the bottom was negligible. Doubtless one can think of many other reasons why it might be useful to know the motion of the ship with respect to the bottom.

Therefore, it appears to be desirable to have moored marker buoys conveniently available so that the ship can keep track of its position. They should be expendable (inexpensive), require little storage space, and be easy to place.

One possible construction for a buoy which meets these requirements is diagrammed in Figure 16.3. Its cost is estimated at $40 — or the equivalent of one hour of ship time.

Light-gauge music wire (.02 inch to .05 inch) is inexpensive when bought in coils — without the spools. Coils of wire, in the approximate length range, and marked as to length, could be kept on shipboard as a stock item. Rubber buoys, shaped like a cable spool, could be inflated inside the coil of wire, slipped

over a short spar, (two-inch by six-inch by twelve-feet) and held in place by dowel rods. A concrete clump shaped for rapid fall would make a satisfactory anchor — cheap and easy to stow.

These parts could be assembled as the ship approached its station. On station, the self-mooring buoy would be jettisoned — and by the time the scientific gear was on its way down, there would be a reference buoy to guide ship operations.

A bright new future seems to be in store for the deep-sea instrument man. With comparatively simple and inexpensive moorings he has nearly achieved the starting point of the shallow-water instrument man. The new item among his available tools is a fixed and known position, philosophically similar to a single piling in shallow water. I believe that in perhaps five years, deep moored buoys will be in such common use as instrument stations for taking continuing data that we will be hard put to say how we got along without them in 1955.

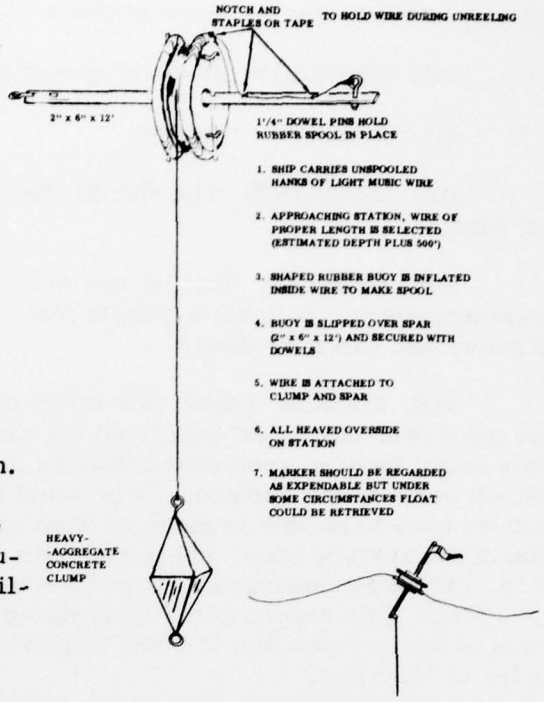


Figure 16.3 Self-mooring marker buoy

I have not the slightest doubt but that they will contribute substantially to man s investigation of the oceans.

DISCUSSION:

DR. NEUMANN: I would like to ask if these current meters are attached to a wire, and then this wire attached to a buoy at the surface and anchored at the bottom. Is that right?

MR. BASCOM: These current meters are purely imaginary, I should say. I was indicating one line of attack which might end up with a satisfactory meter, and I anticipate your question. The buoy at the surface is moving, and the wire is moving, and there are other difficulties.

DR. NEUMANN: I was thinking of that. In order to overcome these difficulties, it might be possible to use the mooring, let's say, with three anchors, as I remember was done almost fifty years ago by Pettersson. He used it in relatively shallow water, but it should also be possible to use it in the deep sea.

MR. BASCOM: I considered for a moment sketching an object using three anchors, but the difficulty is that there is such a tremendous vertical extent to the wires when you portray these as they would be in the deep sea, you find the upper parts of the wire are substantially vertical for a long way before flaring out. We

may be fooled by having three anchors.

DR. NEUMANN: It would give it more stability, maybe?

MR. BASCOM: It might.

DR. NEUMANN: You should also register the movements of the buoy at the surface.

MR. BASCOM: That is correct. This would probably be possible with accelerometers. It is not a simple test. The question is how much do we want to know, and can it be done?

MR. ISAACS: I think this might be done in a better way. If three wires are put down, one would expect all the variable portions to be at the surface. This might be done somewhat better by having a buoy for instruments which also did not come to the surface. This would not vary very much. The best calibration we have been able to make on what we can expect in the way of horizontal motion of the surface buoy, where you have a skiff or something similar at the surface, and also submerged float in the surface layer, is something on the order of 1200 feet. This supposes that everything is working right, and even so, the variation of surface position is going to give the motion of the surface buoy on the order of 1200 feet.

I think the only way to do this, rather than try to put out three lines to the surface buoy, is to put out an individual independent buoy which has no surface components whatsoever.

This is more difficult, of course, because there is the question of how to make your recovery.

MR. BASCOM: In all cases except the last one, my suggestion was that the tension would be taken by an underwater buoy. Of course there are many technical changes which can be made in the present systems which would probably decrease its drag, and increase what might be called the available tension on the wire.

MR. ISAACS: But you still have a lot of drag. It is surprising how much this actually is. If you take the submerged buoy, the one that gives you the tension, and get it away from the surface layer, you still have a lot of drag, regardless of how you do it. It turns out under the conditions we have, that there is 60 or 70 pounds of horizontal drag up to the surface layer, and about 120 pounds more of variable drag in the surface layer. It is tough.

MR. BASCOM: It is tough, but it can be done, I think.

DR. NEUMANN: I meant the three-anchor mooring, not for the surface buoy, but for the lower buoy.

MR. BASCOM: I think a three-wire mooring is not so difficult, and I am pretty sure it can be done. It probably involves a lot of wires left over when you

get through.

For example, you might put down one attached to a surface buoy temporarily and continue another of the wires on through and hook an anchor onto it on shipboard, then let that wire run on until its anchor is dropped to the bottom, and then draw it in. You attach a third wire to the buoys, and another anchor, and another wire is attached to the ship. It is certainly feasible, and not too difficult.

17. THE OCEANOGRAPHER MUST NOW GO DOWN HIMSELF

Jacques Piccard
Committee for Oceanographic Research
by the Bathyscaphe TRIESTE

All kinds of exploration of the sky present a danger which is sometimes very difficult to avoid; it is the moment of returning to the earth surface. The problem is how to break the descent and land safely.

In the field of sea exploration, the question is how to return to sea-level because the most apparent physical laws in the sea tend to attract a body to the bottom and to keep it down.

In my talk I shall first describe two methods of deep-sea exploration in which there are absolutely no possibilities of remaining a prisoner of the ocean, and after that I shall explain some circumstances in which it is very useful for men to go down personally, or even absolutely necessary to do so when automatic instruments cannot take the place of human observer. Finally, I shall discuss a new kind of very fast submarine.

The Bathyscaphe System

One vehicle for deep-sea exploration, called the bathyscaphe, is shown in Figure 17.1. In order to carry passengers to 10 to 20 thousand feet below the

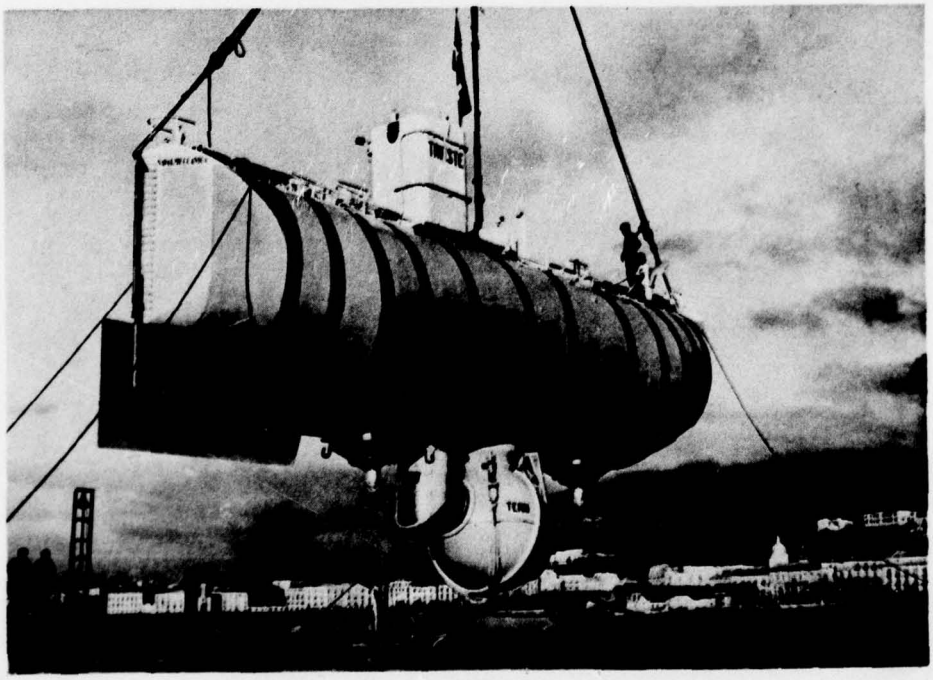


Figure 17.1 The bathyscaphe TRIESTE

sea surface, a steel cabin has to have a thickness of about 3.5 inches, for a diameter of 2.2 yards. Its weight will be about 10 metric tons in the air and five metric tons in the water. This cabin can be given neutral buoyancy at sea level with 15 cubic meters of white gasoline (3900 U. S. gallons). However, we must also allow for the weight of the gasoline tanks. Taking into account the weight of the gasoline tank and all accessories, we required for the FNRS-2, 30 cubic meters of gasoline, and for the TRIESTE about 100 cubic meters of gasoline. In order to begin the submersion, a small quantity of ballast is added. When the bathyscaphe arrives on the bed of the sea or when the pilot desires to surface, the ballast is dropped, giving the submarine a specific weight less than the specific weight of sea water.

Time does not permit me to describe all the details of the construction of a bathyscaphe, but I will discuss some of the important questions.

Gasoline is relatively more compressible than water; therefore it is necessary to maintain free communication between outside pressure and the inside of the gasoline tank; the consequence is that the volume of the gasoline will be reduced by water pressure, and this means that the specific weight of the bathyscaphe will increase during the descent. If the bathyscaphe is at a neutral buoyancy at sea level and if we add a small weight to commence the descent, the buoyancy will be lost when we arrive at the bottom of the sea; dropping the small amount of ballast will not compensate for the loss of buoyancy due to compression of the gasoline. The loss of buoyancy of the bathyscaphe would be about one ton for every 1000 meters of depth. For example, if the sea level water is at 15° C, we shall have with white gasoline such a compression that at 4000 meters of depth the loss of buoyancy of the bathyscaphe will be 41 kilograms for each initial cubic meter of gasoline. For the TRIESTE this will be 8000 pounds. If the sea level water temperature is 30° C and if we go to 6000 meters, the compression of the gasoline will be still more important and we arrive at 61 kilograms for each initial cubic meter. That means we will have to abandon 12,000 pounds of ballast. (For this calculation I have considered a temperature of 0° C at the bottom of the sea.) During the descent, on account of the compression, the temperature of gasoline can increase about 2.5°C for each kilometer of depth, therefore at 6000 meters, the temperature of gasoline could be 30 + 15 = 45°C, more than water. After a certain time, the gasoline will drop to the temperature of the ambient water (about 2°C) and if we return to the sea surface rapidly, the temperature of gasoline will drop (due to expansion) to minus 15°C and the bathyscaphe could arrive in 30°C water (at sea level) covered with ice. Thus, it is necessary to use tubes and lines which cannot be blocked by ice. For the same reason, it is necessary to adopt a system of steel construction able to dilate or to shrink without dangerous internal stresses.

Of what does our ballast consist: And how are we to get rid of it at the right moment? In a balloon, the ballast generally consists of sandbags and it is very easy for the pilot to open the bags and get rid of the sand. In our case, it was clear that the sandbag solution would not apply; the ballast must be exterior to the cabin and we must be able at the right moment to release it from inside. An electrical system, one's first thought, especially in salt water under pressure, is continually liable to a breakdown such as a short-circuit, and we do not wish to face the possibility of remaining forever at the bottom of the ocean. In order

to avoid this danger, my father found the following solution. To retain the ballast, we employ continuous electric current so that if it fails, the ballast would automatically be released and the bathyscaphe would rise to the surface. On the TRIESTE, we have two large containers filled with solid iron pellets, similar to bird shot, but made of iron instead of lead. On the bottom of these containers, there are tubes for the outflow of the ballast. Around these tubes there is a solenoid. When the electric current is on, a magnetically-operated clamp prevents the iron pellets from rolling out of the tubes, but the moment the current ceases to act, this ballast is automatically and infallibly released. Thus, the deep-sea free balloon doesn't need the dangerous cable for suspension which, in the case of the old bathysphere, is continuously subject to the danger of breaking.

Laboratory tests have shown that ordinary glass is not suitable; my father built the windows for the bathyscaphe FNRS-2 and FNRS-3 now used in France and the TRIESTE with Plexiglas; the form is that of a truncated cone with the smaller end toward the interior of the cabin. The solidity is determined in our case (aperture 90 degrees) by the ratio little diameter to thickness. If the ratio is two, the Plexiglas window could resist more than 1000 atm. (30,000 feet); our windows have a ratio of two-thirds which gives security until between 2000 and 3000 atm. (up to 40,000 psi). In order to avoid the optical effects from eventual minor defects of the inside surface of the Plexiglas, we placed an optical glass in front of the window, between this glass and the Plexiglas there is a small quantity of glycerine which has the same refractive index as the Plexiglas.

Our new searchlights are of fused quartz and filled with mercury vapor, put directly in sea water for cooling. We use 500 volts and obtain about 30,000 candle power from each lamp. The direction of the light is vertical, from up to down, that is to say, nearly perpendicular to the direction of the observer's view. The ground is strongly illuminated and each little grain of plankton floating in the water before the window appears clearly before the dark of the bottom water (like in the ultra-microscope system).

With the TRIESTE, we can go down and up with the ballast system. During a submersion, after having left the bottom of the sea, we can easily return to it again by just valving a certain quantity of gasoline for which we have a special valve. With our two propellers, we can move forward or back and we can turn left or right exactly as we wish. Our total power is four horsepower, which is sufficient to cruise slowly in order to observe the bed of the sea. If there is a submarine current, to extend our range we have (as in a free balloon) a guide-rope which is used to keep the bathyscaphe at a constant distance above the bottom; we keep it always in the right direction, with the principal observation window forward. We can also "anchor" ourselves on the bottom.

This bathyscaphe was built for the purpose of going easily to 6000 meters, or about 20,000 feet. At 20,000 feet the factor of safety is still three; at a depth of 30,000 feet, the factor of safety would be two and at the greatest depth actually known (35,000 feet) this factor would be 1.6 which is less than what is generally considered to be good practice. It would be possible to build for the TRIESTE a new cabin which would have a factor of safety equal to three in the deepest part of Pacific; the weight of that cabin in the air would be 14 metric tons and in the water nine tons (inside diameter 180 centimeters; outside diameter 210 centi-

meters; thickness 15 centimeters.) The actual gasoline capacity of the TRIESTE would be just sufficient; for each dive at 35,000 feet, it would be necessary to drop about nine metric tons of ballast.

The Mesoscaphe System

Another system could be used. It is the self-floating cabin. If it should have exactly the same weight as the displaced water, such a steel cabin would be destroyed at a ten-kilometer depth and could be normally used at 2.5 kilometers. In order to have a useful positive buoyancy of 3.44 tons, its thickness should be reduced to one centimeter; this cabin would be destroyed at 2000 meters and could be used with a safety factor of four to a depth of 500 meters. A cabin of Plexiglas having about the same buoyancy (3.9 tons) would be twice as advantageous from the point of view of pressure resistance; it could be used to 1000 meters (3000 feet) and would have to be 20 centimeters thick; it would be destroyed by implosion between 3000 and 6000 meters (depending on the duration of the test). Such a cabin would offer marvelous possibilities to oceanographers; the panoramic view in all directions could not be offered by any other system (except by a glass cabin, as proposed by Mr. Vine, from Woods Hole). The great buoyancy (3.9 metric tons) would permit the use of a large number of accessories, particularly two electric engines with vertical axis propellers, which would give the mesoscaphe the necessary power to descend. If the motors fail or stop, this submarine helicopter will regain sea level by itself through its positive buoyancy. We envisage building above the Plexiglas cabin a little craft (in aluminum and Plexiglas) with a normal propeller and gasoline engine which will make it a completely autonomous free submarine.

Some problems will need particular attention; for instance, the effect of the duration of the pressure on the Plexiglas. Tests made in our laboratory in Italy show that the Plexiglas has a coefficient of resistance between 7.5 kg/mm^2 and 15 kg/mm^2 with a duration from one second to five days. It should be noted that when a Plexiglas sphere is broken at high pressure, it falls into thousands of little pieces. However, when it is destroyed at low pressure (after a long time) the entire cabin remains spherical and the failure may be just a little hole with only local deformation. But for the passengers, the question is not "how the cabin will be destroyed" but "how can the mesoscaphe be built to avoid failure."

Some Cases in Which it is Necessary for Oceanographers to Go Down Themselves

One of the important questions in oceanography is "what is the character of the deep sea currents" (direction, speed, volume of flow)? With the bathyscaphe, it is possible to lay down (in the direction of the current) a rope with a visible mark, for instance at each yard. Afterwards it will be easy to measure the movement of the bathyscaphe along the bottom as it is moved by the current along the rope.

Specialists studying the "deep scattering layer" believe that the only way to determine the cause of this mysterious phenomenon is to go down, to remain at the depth of the layer and then to observe it with the eyes and ultra-sonics. The mesoscaphe would be especially useful here.

It is highly interesting to see the bottom of the sea and compare it with its particular reflection on the ultra-sounding screen.

The density of deep-sea life could be estimated only by very long observations and with a large visual field; the bathyscaphe is well suited for such observation.

It would be very interesting to take cored samples at a place which could be chosen with great precision, for instance, in order to carefully collect the little sand burrows and mole hills noted either on submarine pictures by Dr. Edgerton, or visually during submersion with the bathyscaphe (TRIESTE - 1954).

The sensitivity of television is not sufficient to record the little phosphorescent planktons and if artificial light is used, the phosphorescent points will disappear from the screen. The only possibility of seeing them is to be there in the obscurity close to them.

I remember that during one of our dives with the TRIESTE, we arrived on a golden sand bottom, with numerous small holes. At the beginning, we weren't able to distinguish anything in them. Quite near to us, we could see small crabs, peacefully walking along the bed of the sea. We distinguished some star-fish and in the water some jelly-fish were floating, but ordinary fish were not seen. Suddenly, the head of a small fish emerged from one of these numerous sand-holes. It seemed to be looking at the bathyscaphe and apparently did not judge it to be too dangerous since it came out of its hole. It was similar in size to a normal sardine, probably it was a kind of gobbius, and it began to swim. After one minute other small fish came out of their holes. We put our propellers into action and the bathyscaphe began to move along the bed of the sea. Immediately, all the fish disappeared back into their sand-holes.

I gave you that little description to show how important it is to descend personally whenever it is possible to do so.

Our hope since we built the bathyscaphe is that, as the stratosphere balloon with its first air-tight cabin opened up the way to modern air navigation, so the bathyscaphe will open up the way to a new era of oceanic discovery.

It is well known that when a body is moving through the sea it has to fight against the resistance of the water. The necessary force generally increases according to the square of the speed. All the energy developed by this body is lost in whirlpools created by the body.

As an experiment, we will create a model, in paraffin-wax, which will have the exact form of a dolphin, which is one of the best swimmers, and draw it through the water. If we think that this body will go through water as easily as a dolphin, we shall be mistaken. On the contrary, the resistance of the water is very great and behind this artificial dolphin there will be found a trace of its passage, as when a ship sails on the water. But these whirlpools are never created behind a real dolphin. The energy given to the cord, by the holder, is changed into whirlpools.

Now, let us see, with greater precision, what happens when a body is moving through the water. Nothing obliges whirlpools to begin at any specified moment or point but even the smallest whirlpool might develop at any moment. That is to say, the water near the ship, or near the artificial dolphin, is always in a state of instability.

In the case of the real dolphin, its instinct enables it to act in accordance to this existing instability: a whirlpool is on the point of forming but the dolphin interferes and, with a very slight movement of its skin, is able to neutralize the forming whirlpool. The dolphin must act immediately, otherwise it will be too late, as the whirlpool would have had time to develop and increase its force and then the dolphin would not be able to destroy it.

This is explained by the fact that the dolphin has, underneath its skin, certain very sensitive nerves which work as manometers and which signal the forming of a whirlpool. Thus the dolphin can neutralize it, and the water, instead of forming into a whirlpool, slides along the body of the fish.

Fish do this instinctively.

Now, would it be possible for a naval constructor to apply this theory to shipbuilding? My father thinks that it would be possible.

The big trans-Atlantic ships use many tens of thousands of horsepower. We can see where this energy is being wasted: behind the stern of a ship the water remains agitated for many miles. Is it really impossible to avoid these whirlpools? Could not this water be made to re-form behind the ship in the same way as it is separated in front of it?

Let us try to imitate the dolphin. Imagine, for instance, a ship with her hull covered with a rubber skin. Under this skin could be put a great number of manometers which would give the signal of the first formation of whirlpools; this signal could then be transformed into an electrical current which would contact the electronic brain of the ship. In its turn, this electric "central" would send back electric impulsions to small magnets fixed between the manometers; in front of each magnet would be a small piece of iron glued to the rubber, which would give the necessary movement to the rubber skin.

By this system, it is possible to imagine the hull taking on, at every moment, the best and most suitable form to advance through the water. In practice, should a first "dolphin-ship," similar to a sea-level ship be constructed, or one in the form of a submarine? Certainly, in the form of a submarine, as we know that even the dolphin has certain difficulties in swimming on the surface, as it is partly in the water and partly in the air.

Thus, it is possible to imagine a big submarine, capable of carrying several hundred persons and which would go at a speed of, for instance, 60 knots. Naturally, it would be necessary to make a serious and long study of this question, but with modern techniques it may be said that almost everything is possible.

DISCUSSION:

DR. VON ARX: I'd just like to mention that W. E. Schevill also thought of high-speed vehicles after watching dolphins swim. But it appears from his photographs that the amplitude of the motions of the skin would have to be large unless the hull were at least the dolphin shape, that is, of suitable width in proportion to the length of the vehicle. The motions involved in the real porpoise skin are quite large. Have you looked into the amplitudes?

DR. PICCARD: There would be little movement of the skin. But it is necessary that it begin to move immediately, at the first of the whirlpools.

DR. VON ARX: Yes, but in addition to turbulence there is the bow wave that is large in amplitude. Just moving a stick vertically through the water shows this. A simple cylinder creates a wake that has disturbances which are large compared with its own diameter. Thus a ship would have to be longer than the bow wave length to prevent the generation of a wake. As you get into higher speeds, this process of bow wave development seems to expand to more than the dimensions of the object. Obviously, the porpoise does this within its own dimensions, or limits its speed accordingly, but I was wondering whether we are clever enough to do the same.

DR. PICCARD: As I understand it, the dolphin's movements of the skin is one or two centimeters; maybe less. If the ship or boat is 100 meters in length, the movement of the skin would not have to be fifty times more because always it is necessary to make a little movement of the skin at the backing of the whirlpool and this would be the same whether it was a dolphin or a big boat. Of course, it's just an idea.

DR. VON ARX: With regard to Mr. Schevill's photographs, part of the problem seems to be that the porpoise has a flexible spine and can bend his whole body as necessary to a large turbulent motion in addition to wrinkling his skin.

DR. PICCARD: Yes, probably it would be impossible to have the same power that the dolphin has, but it is surely possible to increase by a great proportion the result we have now with an ordinary submarine.

DR. DIETZ: The whale is quite different.

MR. ISAACS: There's something here I don't quite understand if you're talking about small fluctuations. It is engineering practice in aircraft to attempt to bleed off the boundary layer because you indeed have turbulence. Now there is a contradiction that I don't quite follow. In the one case engineering procedure is to attempt to destroy the boundary layer, yet the proposal here is to attempt to retain it.

DR. PICCARD: But the movement of the whirlpools are sometimes different between the air and the water. What happens in the air doesn't happen in the same way in the water. Speed could be different. I'm not sure that I understand you exactly.

MR. ISAACS: There are possibilities of doing it with a ship. I would guess the fish is doing the thing right. But this is a completely different approach. The usual idea is that it is desirable to destroy the boundary layer.

MR. LAWTON: In the ship, you're dealing usually with resistance to two factors; one is skin friction, the other is turbulence. I think that the balance between those two is entirely different in aircraft to what it is in ships. For normal slow tramps, skin fraction accounts for one-half the total power. In faster ships, the turbulence is far more important.

MR. ISAACS: These are two different things.

MR. LAWTON: The aircraft, in attempting to get away from laminar flow, is attempting to get away from the drag of the surface. Here reference is being made to the wave.

MR. ISAACS: You're talking of the ship on the surface. There is not this kind of a drag on a completely submerged object.

MR. LAWTON: I admit that is out of my province.

MR. LILL: My question concerns quite a different matter. How fast would the bathyscaphe come up if you drop all ballast?

DR. PICCARD: About five feet per second.

MR. LILL: This is not fast enough to cause damage to people inside?

DR. PICCARD: No, but one danger is that of touching a ship when you are at sea level. A collision between the bathyscaphe and a ship would not be bad but propellers hitting one of the compartments of the gasoline would sink it. Until now we have taken every precaution by posting Navy ships to keep other ships out of our zone. It would be much easier to have an ultra-sounding apparatus for communication with our own ship on the sea level so that we could say: "Look, we are beginning to come up." And at that moment this ship could leave.

MR. VINE: It's nice to hear about the large factors of safety of two and three at the bottom of the ocean. Some of the deep-water instrument cases, in which we have only instruments, we have usually designed so closely we figure if they work-harden on the way down they may hold when they get there. A large factor of safety is more than 30 percent.

DR. PICCARD: Obviously the figure of safety has to be greater when humans are inside.

DR. FOLSOM: What is your primary source of energy?

DR. PICCARD: We had until now two batteries. One is a normal lead-acid battery, and my father had the idea of putting this immediately in the water in order to avoid using a very heavy hull for protection against hydrostatic pressure. So we put the battery in an iron box, open to free communication with the

sea pressure. In the water and acid we put a little gasoline. The second battery we have is a silver cell battery — silver zinc, so we can put it directly in the cabin.

MR. BASCOM: I'd like to ask how far along the bottom you traveled when you got down there.

DR. PICCARD: With our props, up to now we have not moved far. But with the movement of the current, we moved once probably about one mile.

MR. BASCOM: I'd also like to ask about your release valve. There was no other mechanism and it was just the shot itself; is that correct?

DR. PICCARD: We have to interrupt the current to leave the ballast.

MR. BASCOM: But no valve?

DR. PICCARD: No; you have the ballast and a solenoid. It is just a hole in which the current holds back the shot, so by cutting the current the ballast falls without any mechanical system.

MR. BASCOM: What's the relationship in size between the shot and the opening?

DR. PICCARD: This is six centimeters in diameter so we can let out 600 kilograms of ballast per minute from each tank. We can let out a total of 1200 kilograms per minute.

DR. DEACON: Dr. Piccard has given us new information on the way animals live on the bottom. But there are a lot of things you will not learn on the sea bottom by looking. I hope they will concentrate on some of the things we can't do like getting profiles and currents and that sort of thing.

Nevertheless, the real way to make any sound measurement is to go and have a look first. We can only congratulate them and acknowledge work they have done which very few of us would have attempted, and they have made a success of it.

18. THE BATHYSCAPHE AND DEEP-SEA RESEARCH

Robert Dietz
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Our honored Swiss guest, M. Jacques Piccard, has just described a fascinating new deep-sea craft, the bathyscaphe, developed by his father and himself, and further developed by the French Navy. In connection with scientific liaison work in the earth sciences in Europe, over the past eighteen months I have been fortunate in having had the opportunity of examining in some detail both the French bathyscaphe, FNRS-3 at Toulon (ex-FNRS-2 of Professor A. Piccard), and the TRIESTE of the Piccards' at Castellammare di Stabia in Italy. The TRIESTE is indeed a remarkable addition to the "Swiss Navy." As an oceanographer, I have been greatly impressed with the potential of this new craft for deep-sea research; hence, I would like to take this opportunity to try to convey to you some of the reasons for my enthusiasm.

When first invited to take part in this symposium, I thought that I would discuss, in a general manner, recent developments in Europe in deep-sea research, but, in view of M. Piccard's participation here, I decided that it might be more appropriate to add further details regarding this most significant development and to present the point of view of an oceanographer. However, in order not to ignore other European developments completely, I have mimeographed some recent notes, obtained from Dr. J. C. Swallow of the National Institute of Oceanography of Great Britain. These give his latest thoughts concerning his deep drifting neutral buoyancy float, which, as pointed out in a recent issue of Deep-Sea Research, already has made a significant contribution to our knowledge of deep currents. I have also duplicated some copies of a translated paper by Academician Zenkevitch on his thoughts on deep-sea research in the USSR. It is especially noteworthy that, according to Zenkevitch, the Soviets already have obtained deep-sea cores of 34 meters in length from the VITYAZ and that they soon hope to be obtaining cores of up to 100 meters in length. Further, in order to document what I am about to say, I have mimeographed some copies of an ORL Technical Report by myself on the bathyscaphe TRIESTE, which gives a summary of technical details. These handouts are available at this meeting for those who wish them.

Inasmuch as the general principle of the bathyscaphe is well known to all of us here and has been further elucidated by M. Piccard, no discussion is needed. It is sufficient to say that although the bathyscaphe has been called the underwater equivalent of a balloon, the analogy to a non-rigid airship, a blimp, is probably more apt because the bathyscaphe is streamlined and does have propellers for horizontal maneuvering. The gasoline-filled hull is strong enough to withstand the rigors of towing and buffeting by surface waves, but it, of course, is non-rigid in that it does not resist the hydrostatic pressures of the oceanic depths.

The plan to build a bathyscaphe was initiated by A. Piccard in the late '30's but was interrupted by World War II. However, the financial assistance of the Belgian Fonds Nationals de la Recherche Scientifique was obtained in 1945 and in

1948 the FNRS-2 was completed and an unmanned test dive was made off Dakar to 1400 meters. The bathyscaphe was then damaged by surface handling and the tests terminated. This somewhat abortive effort was a journalistic flop because of the lack of a deep manned dive and a consequent lack of heroics, but it was, nevertheless, a red-letter day in bathyscaphe history inasmuch as the ability of the craft to dive to great depth was demonstrated. Since that time, both the French Navy and the Piccards have continued bathyscaphe development. Also, the Polish Institute of Fisheries has recently announced that they have constructed a small two-man bathyscaphe for diving in the Baltic to 200 meters. (It is not clear, however, how this device can be of much use in the murky waters of that sea.) Since 1948, nearly fifty dives have been safely made, including one made to 3150 meters by the TRIESTE, and another to over 4000 meters by the FNRS-3. Thus, it is by now a rather well-tested device.

Perhaps one can say that there have been two important break-throughs in the development of submersible craft since World War II, namely the nuclear-powered submarine and the bathyscaphe. The application of atomic power has been a remarkable development, for it has freed the submarine from dependence on the air and permitted the development of a "true submarine" - that is, a craft which can cruise submerged indefinitely at high performance in contrast to the submarine of World War II, which was essentially a surface craft with the ability to dive. The atomic submarine involved no great compromise of hull design of the conventional submarine. Hence, its military potential was readily apparent so that it was rapidly developed.

In contrast, the bathyscaphe does not amount to much. Yet, we cannot ignore this revolutionary new craft which has increased the depth of diving by a factor of 20 over the conventional submarine. In fact, the entire volume of the ocean is the realm of the bathyscaphe. No craft, based on any other principle, has been able to match this performance. Consider how far advanced the bathyscaphe might be today if a small fraction of the effort which has gone into the NAUTILUS had gone into the bathyscaphe's development. Compare on one hand the great technical facilities, men, and know-how which have backed up the atomic submarine, and then consider on the other M. Jacques Piccard, assisted only by one full-time shipwright, carrying out the further development of the TRIESTE, and, at the same time, seeking the necessary subventions.

There are other reasons for the slow development of the bathyscaphe as well. Firstly, if the bathyscaphe were a "flying machine," there is no doubt but that all of its potentialities would have been thoroughly explored in the U. S. and that it would be in an advanced state of development. In an article on industrial research in the January 1956 Fortune magazine, Bureau of Labor Statistics figures for 1951 are quoted showing that the aircraft industry spends 12.7 percent of its gross from sales on research and development, which is 85 percent government supported. The comparable statistic for research and development by shipbuilders is apparently too small to be considered in Fortune's article so that it must be a small fraction of one percent. Nearly every day we hear of new developments in non-conventional aircraft such as ones which fly straight up from a standing start, or perform equally remarkable capers, but it appears that funds to develop non-conventional ships are comparatively difficult to obtain except in certain special instances. Those of you who have labored with the problem of lay-

ing the keel for the first specifically oceanographic ship are better acquainted with the reasons for this than am I.

The bathyscaphe has been the victim of a second misfortune also. This misfortune can be likened to that of the man who was almost a millionaire. It seems that he was born on the same day as Henry Ford II, but alas, he was born in the wrong place. It is indeed unfortunate that the bathyscaphe was not invented in the U. S. A.

However, the question which concerns this audience is the relation of the bathyscaphe to deep-sea research. Let us consider the following questions:

a. Is there a bathyscaphe program in the U. S. ?

b. Is it feasible to operate a bathyscaphe in our country?

c. Who should do it and what part, if any, should the oceanographers play?

Of course, the most critical question is what can the bathyscaphe do which cannot be done by wires, by cameras, or by geophysical techniques such as those utilizing sound waves. The advantages of the bathyscaphe are essentially those of being able to see and to sample the water volume and the ocean bottom at close range. Fortunately, we can anticipate that the deep water is everywhere clear so that with suitable lighting a visibility up to 100 feet or more could be obtained. Of course, the deep-sea camera has already given us a very limited vision; but, the human eye is an incomparably better device. The eye and the brain, for example, can integrate what they see, both in space and time. We say that seeing is believing and it has been estimated that 80 percent of our knowledge is acquired through our sense of vision. It is, in fact difficult to establish the mode of operation of any process which cannot be seen. A case in point is the rough road over which the concept of turbidity currents operating on the ocean floor has progressed. We have accepted them only recently and we still do not agree upon their importance.

An inspection of the bottom from the bathyscaphe would give the marine geologist, for the first time, some concept of the "spectrum" of topography that lies between the small mounds and ripples displayed on bottom photographs and the gross topographic forms revealed in shadowy outline by echo-sounders. It would be exciting to see the detailed form of some of the recently discovered deep-sea channels. One could quickly determine if these channels are, in fact, erosional features and if they are freshly cut, indicating the operation of an active process. It would be exciting also to be able to view the deposits of manganese oxide which cover many sea mounts, or to see the phosphorite deposits covering the banks off Southern California.

Secondly, the bathyscaphe would give the oceanographer an ability to see what he was doing when he obtained his samples. Thus, his sampling techniques would become much more sophisticated. Only a few years ago the rate of sedimentation determined in one core was considered to be a valid figure for the entire ocean. We now know that the ocean floor is an exceedingly complex sedimen-

tary environment and that it is not safe to assume that cores even a few meters apart are the same. The sampling techniques of the oceanographer have been frequently and aptly compared to those of a zoologist attempting to assay the earth's fauna simply by lowering a bucket down from an airplane flying over a cloud deck.

But, without doubt, the main result from the use of a bathyscaphe for deep-sea research by oceanographers would be the discovery of processes operating in the ocean of which we know nothing at present. Look at what sonars have done for oceanography! The discovery of the deep scattering layer is an example. The French, in their use of the FNRS-3, have made the observation that the sea is remarkably full of scattering particles which have been poetically called "sea-dust," or "sea-snow." But, when they come to within about 10 meters of the bottom, they invariably find a zone of crystal-clear water. What is the reason for this? Is there some electrostatic attraction between scattering particles and the bottom? Is the bottom water rapidly filtered by the benthos? This exceedingly interesting observation now requires confirmation and explanation. Our understanding of the deep sea has become a lot more sophisticated than in the days of Maury, when he considered that the deep sea was silent, still, and lifeless. However, we now know that we can no longer assume that, because we do not know what is going on, nothing is going on. All of our sampling of the deep sea is done in terms of a preconceived model and we will all freely admit that the 1956 model is still a simple one. We have, of course, made great progress since the early days when oceanographers located their stations on a neat square lattice grid and spaced sampling bottles in uniform stages with depth.

Only a few bathyscaphe dives have been made to date, and these have been with almost no scientific instrumentation, except for Professor Edgerton's camera. For the most part, they have been simply test and record establishing dives, but some important research results already have been made. Some of these have been briefly summarized in a recent note in Deep-Sea Research by Commandant Houot. Other French marine biologists have also written concerning their observations from the bathyscaphe - notably, M. le Prof. Monod, M. Peres, M. Piccard, and M. Furnestein. These persons have made some fascinating observations concerning bathypelagic life and the bottom fauna, but, to me, one of the most interesting observations has been that appreciable currents have been almost invariably observed on the dives made in the Atlantic, whereas in the almost tideless Mediterranean Sea the deep water appears to be stagnant. .

Possibly the potential value of the bathyscaphe to deep-sea research can be explained by two analogies. First, one can say that the bathyscaphe could do for the marine observer what the airplane has done for the meteorologist. However, the bathyscaphe is undoubtedly even more crucial to oceanography inasmuch as the meteorologist can see his realm from the ground and examine it optically or by electromagnetic radiation, but the oceanographer cannot do this for the deep sea. It is fair to say that the meteorologist played no part in the development of the airplane, but that he, nevertheless, found it to be enormously useful once it was developed and made available to him. I am sure that there are many atmospheric phenomena which were first discovered on the basis of airplane observations. But, whereas it was unnecessary for the meteorologists to play any part in the development of the aircraft because of its many uses, it is likely that the

bathyscaphe will be further developed only if oceanographers boost the project.

A second analogy can be drawn between the bathyscaphe and the development of SCUBA (self-contained underwater breathing apparatus) diving - also a European development. I believe that you will agree that SCUBA diving techniques are revolutionizing our understanding of the marine biology, marine geology, and physical oceanography of the shallow fringes of the ocean. This has resulted simply by permitting the oceanographer to get a more intimate and close-up view of this environment and to grapple with it in a very personal way. He has thus been able to recognize subtleties of this realm to which he was formerly blind.

Now, let us consider the question of the feasibility of operating a bathyscaphe in the U. S. A. I believe that the requirements are essentially the following:

a. deep water near the shore so that towing distances will be short,

b. a harbor sufficient to accommodate the 20-foot draft of a bathyscaphe and an isolated berthing because of the fire hazard,

c. shipyard facilities for maintenance and handling with a crane adequately stout to lift a bathyscaphe out of the water,

d. an oceanographic laboratory near at hand so that oceanographers can collaborate and provide the necessary scientific instrumentation,

e. calm seas and preferably clear water, and

f. the necessary funds.

We can meet these requirements at several places in the U. S., for example, Honolulu, San Pedro, San Diego, and Key West. The more northern parts of the U. S. are perhaps unfavorable, both because of the stormy seas and the broad continental shelves. The latter point also applies to the Gulf Coast, but, of course, the craft could be launched from a mother ship. In nearly every respect, the U. S. is more favorably located than Europe. However, we cannot match the clear and quiet surface waters of the Mediterranean.

Assuming, for the moment, that the further development of the bathyscaphe is a worthwhile project, let us next consider how this can best be done and what part oceanographers might play. It seems to me that the bathyscaphe will undergo a fitful development for several years with private funds, even if oceanographers stand on the sidelines and there is no governmental support. We can anticipate that the romance of deep-sea diving will make this possible until all the important records for deep dives under assorted conditions have been established. Public interest will then wane and private subventions will dry up. We can further anticipate that the scientific results from these dives, unfortunately, will be almost nil. On the other hand, if the oceanographers see fit to encourage bathyscaphe development and are willing to collaborate closely to provide scientific instrumentation, some governmental agency may decide to establish a bathyscaphe

program in the U. S. It would seem that this would inevitably fall within the province of the U. S. Navy, and, in fact, if the Navy did not take an interest, no other agency would. It may be that the Navy might see sufficient justification for the bathyscaphe, strictly from its military potential, even though this seems to be unlikely to me. Of course, the Navy will stand to gain, in any event, a great deal of useful knowledge about problems of deep submergence. Nevertheless, it seems that the principal justification for a bathyscaphe program must be based upon its usefulness as a tool for deep-sea research. Its future, then, depends upon the approval, or disapproval, of oceanographers such as those of you who are gathered here. The time has arrived when oceanographers should cease having no opinion at all.

It should be emphasized that the future development of the bathyscaphe need not be along a single track. The Piccards already envision a lighter-than-water sphere, the mesoscaphe, which is the underwater equivalent of the helicopter. Such a craft could be built, for example, of twelve pentagons of Plexiglas cast into curved segments such that they would fit together to form a perfect sphere. Even when manned, such a sphere could be so designed to have positive buoyancy such that it would float back to the surface if the propellers, needed to keep it submerged, and to maneuver it horizontally, were to fail. With the Plexiglas there would be no need for portholes inasmuch as vision in all directions would be a simple matter. It has been estimated that such a craft could be operated down to a depth of 1000 meters with a safety factor of six.

With the recent successful assault on Mt. Everest, attention has naturally turned to conquering the world's greatest depth, the nearly 11,000 meter Challenger Deep off Guam. This will, no doubt, be accomplished within the next few years, because, from an engineering point of view, it is a comparatively straightforward project. All that one needs is a one-man bathyscaphe designed to minimum dimensions because a small sphere will be extremely strong. It would also be cheap inasmuch as the cost would probably be a function of both the cube law for volumes and the square law for surfaces; hence, a one-meter sphere could be built for about one-sixth the cost of the two-meter TRIESTE and FNRS-3. Of course, we must recognize such an effort for what it is, a prestige project, and not cloak it with the magic adjectives of "scientific" or "military;" but "prestige" is now, to borrow from Stephen Potter, "an O.K. word," at least, in some countries - notably in the USSR.

It seems to me that there is ample justification for oceanographers to support vigorously the further development of the bathyscaphe. No one else has yet been able to design a vehicle which offers any similar prospect of opening to detailed explanation the deep volume of the ocean and the sea floor - the earth's Third Surface. An enormous effort is going into the exploration of outer space, whereas the exploration of the ocean floor represents a much more important and readily obtainable objective.

Unless the oceanographers of the U.S. support a bathyscaphe program and are willing to collaborate, such a bathyscaphe development is not likely to get very far, here, or anywhere else in the world. The main raison d'etre for the bathyscaphe appears to be as a tool for deep-sea research, as the airplane was for the meteorologist.

BATHYSCAPHE TRIESTE*

General

Two bathyscaphes have been built to date - the TRIESTE and the FNRS-2, later modified and called the FNRS-3. The general principles of bathyscaphe development have been widely publicized so that no detailed description is needed here. The reader is referred to the following semi-technical descriptions: Houot and Wilm (1955) 2000 Fathoms Down, Hamish Hamilton and Rupert Hart-Davis, London - English edition of a book describing the results obtained with the FNRS-3 up to 1954; A. Piccard (1954) Au Fond des Mers en Bathyscaphe, Arthaud, Paris - a book describing the TRIESTE. Several descriptions of the French bathyscaphe are also included in recent issues of the National Geographic Magazine (e. g., April 1955) and of Life.

In principle the bathyscaphe is closely analogous to a lighter-than-air ship such as a balloon, or, more accurately, to a blimp inasmuch as the latter has horizontal maneuverability. It consists of a heavier-than-water manned sphere capable of resisting the great hydrostatic pressures found in the deep ocean and a hull filled with gasoline to provide buoyancy. The hull is sufficiently rigid to withstand the buffeting of surface waves but not to resist the hydrostatic pressure of the sea. Inasmuch as gasoline is lighter than water and immiscible with it, the sea can be admitted to the gasoline compartments so that the internal pressure in the hull is maintained the same as the ambient pressure of the sea. A decrease in buoyancy comes about by jettisoning some of the gasoline, filling an air chamber with sea water, cooling the gasoline, or compressing it. Only the first two of these operations can be controlled by the bathyscaphe operator. An increase in buoyancy can be accomplished by jettisoning iron shot ballast. It has been amply demonstrated that the buoyancy can be controlled to within several kilograms so that the craft can rest on a guide chain on the bottom without itself touching the bottom. Here, battery-powered screws permit some horizontal maneuverability.

Brief History

The plan to build a bathyscaphe was started by A. Piccard in the late '30's but was interrupted by the war. After World War II, the plan was commenced anew with the financial assistance of the Belgian Fonds National de la Recherche Scientifique. A stipulation of this collaboration was that the FNRS retain title to the craft and that a Belgian citizen (Max Cosyns) take an equal part with Piccard in the development. The first bathyscaphe (FNRS-2)** was completed in 1948 and tested off Dakar with the assistance of the French Navy. An unmanned dive was made to 1400 meters and a manned dive was made to 25 meters. The FNRS-2 was unable to stand up to buffeting by heavy seas and towing by the ELIE MONNIER; she was damaged and the test terminated. Because of the shallowness of the manned dive and consequent lack of heroics, this abortive effort was

*This is the report referred to in the preceding paper.
**The FNRS-1 was Piccard's stratosphere balloon.

a journalistic flop but it was nevertheless a great scientific success inasmuch as the ability of the craft to dive to great depth was amply demonstrated.

Through an agreement signed in 1950, the FNRS-2 was turned over to the French Navy with the stipulation that three dives were to be made to great depths as a joint French-Belgian venture. It is believed the deceased French oceanographer, Claude Francis-Boeuf, and Captain J. Cousteau were instrumental in convincing the French they should take over this project. Since the French Navy apparently had insufficient funds even to accept the FNRS-2 as a pure gift, the Belgian research foundation made an additional gift of one million Belgian francs ($20,000) to help out the program. A. Piccard was appointed as a technical adviser to the French, but because of differences of opinion with regard to technical details, this collaboration was not successful and eventually was broken off.

The modification by the French Navy of the FNRS-2 consisted of constructing a new flotation hull which could withstand the rigors of rough weather and long tows. This craft has proved very successful and has made about 30 dives to date, including the record descent to 4050 meters in 1954.

In May 1952, A. Piccard and his son, Jacques, found it possible to build a new bathyscaphe (the TRIESTE) with Italian and Swiss assistance. This was accomplished during the following 15 months, and the first dive was made on 11 August 1953. Subsequent to that time, 15 dives were made in 1953-54, among them one to 3150 meters and another to 1080 meters, off Naples. The TRIESTE is now based at the Navalmeccanica (*a private shipyard*) in Castellammare near Naples, and is currently (July, 1955) being refitted for a series of dives to take place late in the summer and early fall off the Naples area to depths between 2000 and 3000 meters.

Technical Description

Although simple in principle, the realization of the TRIESTE required the solution of numerous technical problems. Many of these had been previously solved in the construction of the FNRS-2; nevertheless, the completion of the TRIESTE in the short time of 15 months was a remarkable achievement. The following is a brief account of some of the technical details relating to the craft.

The ten-ton sphere or cabin is, of course, the most critical part of the TRIESTE, and its dimensions and weight determine the over-all size of the craft. For the TRIESTE the Piccards used the same size sphere as for the FNRS-2, i.e., two meters inside diameter and with a wall of nine centimeters thickened to 15 centimenters in the vicinity of the ports, lead-through, and entrance hatch. Although designed as a two-man craft, the sphere is roomy and can easily accomodate four persons without undue crowding.*

*The bathysphere of Barton and Beebe has an inside diameter of 1.37 meters and proved it to be quite feasible to build a considerably miniaturized bathyscaphe with a small sphere. It seems likely that the first attempts to descend into the greatest deeps of the oceans such as the 11,000 meter Challenger Deep may well be done with a miniaturized one-man bathyscaphe inasmuch as a small sphere can be designed so that it is extremely strong.

The TRIESTE sphere was forged out of high quality "fatigueless" steel by the Societa per l'Industria e l'Electricita. In contrast, the FNRS-2 sphere was cast and, for this reason, has a smaller crushing strength. The TRIESTE sphere is calculated to be able to withstand the pressure at 15-16 kilometers before collapsing; hence it is considered perfectly safe for 4000 meter dives and reasonably safe for dives to 6000 meters.

The cabin of the TRIESTE is pierced by 12 holes, two of which are occupied by snorkels so that it is possible to breathe surface air prior to diving. Two other holes are plugged and reserved for scientific equipment. At present, eight cables, each containing seven conductors, pass through the sphere. The pressure-proof seals are rather intricate and are of two types - one for high tension leads and the other for high current leads. Inasmuch as shorts have rarely developed and no water has leaked into the cabin, the Piccards seem to have solved the problem of high-pressure lead-throughs. However, these are at present being redesigned to increase the safety factor.

It is not possible to jettison the batteries aboard the TRIESTE as it was in the case of the FNRS-3, but the general procedure for jettisoning equipment which has electric leads is to have an outboard junction box which is free-flooding. The junction is made within the box beneath an immiscible dielectric liquid such as trichlorethylene which has a density of 1.44. If the equipment is jettisoned, this connection is easily pulled apart.

Free-flooding outboard junctions which are not jettisonable are also made inside a pot filled with Araldite which can be poured when hot but hardens upon cooling to a clear rockline substance. A layer of a soft and plastic material called Bathycire, manufactured by Pirelli, is placed above the Araldite and is forced into any cracks which develop, thus preventing water from reaching the leads and causing a short.

The TRIESTE has two portholes - one looking forward and slightly down and the other looking aft through the access hatch and slightly upward. The sea floor can be seen only through the front porthole. After considerable experimentation it was found that Plexiglas was superior to glass or fused quartz (as used by Barton and Beebe) for the windows or ports. It is claimed that the ports will withstand a pressure equivalent to 30 kilometers of water before yielding - hence there is a safety factor of five at the greatest depth to which it is planned to descend. The ports were fabricated by Ventrocoke of Venice and have the form of a truncated cone with a thickness of 15 centimeters, 10 centimeters across on the inside, and 40 centimeters across at the outside. The apical angle is 90 degrees, providing a 90-degree field of view. The Plexiglas cones are seated into conical portholes out through the sphere. A ring of rubber provides the low pressure seal and a ring of metal holds the ports securely in place. The high pressure seal is provided simply by the pressure of the sea which seats the Plexiglas firmly against the metal. This method has worked perfectly and is used on the FNRS-3 as well as on the TRIESTE. It is said that there has never been so much as a drop of water of leakage into the cabin.

The ballast for the TRIESTE is composed iron shot 2.6 millimeters in diameter; 4.5 tons of ballast are contained in each of two bins, one fore and one aft.

This ballast can be jettisoned through two magnetic valves at a rate of 50 kilograms per minute for each valve. The valves are coils of polythene-coated copper wire of about 500 turns and exposed to the sea. As long as a current of approximately four watts is flowing through the coil, a magnetically "frozen" plug of ballast is present in the valve; but if the current is shut off (or if there is a power failure or short) the ballast will fall. A current of 15 watts is required to stop the ballast after it has started to fall. Once the ballast has become wet it must be stored under water to prevent it from rusting together and caking.

As an additional safety precaution, the entire bin of ballast can be jettisoned if for any reason the ballast outlet becomes plugged. The bins are suspended by chains which in turn are attached to a lever arm held at the far end by an electromagnet. The magnet exerts an attractive force of two tons at a power consumption of 40 watts. Combined with the seven to one ratio of the lever arm, this force gives a total supporting strength of 14 tons. The bins when full of ballast weigh 5.5 tons; thus there is a safety factor of about two and a half. In the event of power failure the bins are jettisoned automatically and the craft returns to the surface.

The gasoline required to furnish buoyancy for the TRIESTE is expensive but it is largely conserved. Therefore, it has been loaned to the Piccards by the Esso Corporation for each series of dives. Sixty tons (104 cubic meters or 104,000 liters) of white gasoline are required. In 1953, gasoline with a density of 0.66 was used but this light gasoline was rather highly compressible with increase of pressure and with decrease of temperature; gasoline with a density of 0.70 was therefore used in 1954. The buoyancy provided by the gasoline amounts to 25 to 30 tons.

The gasoline is contained in 12 separated compartments, one of which can be vented from inside the sphere so that the total buoyancy can be decreased at the will of the operator. There is one additional compartment in the hull which is air-filled to give the craft more freeboard when on the surface and which is flooded to begin the descent. The bulkheads between the compartments are made of corrugated steel so that they will yield to compression or expansion without rupture and thus accommodate temperature and pressure changes. Even though an entire compartment should be ruptured and the gasoline escape, there is still sufficient over-all buoyancy to permit the craft to return to the surface.

In theory it is only necessary to have small holes in the bottom of the compartments in order to permit free access of the sea, thus maintaining the inside pressure the same as the ambient hydrostatic pressure; however, safety requirements and practical operation of the craft require rather elaborate plumbing connections between the various compartments, and sea water is permitted to flood the central compartments only. Inasmuch as the hull must be able to "breathe" sea water with each small change of pressure and temperature, it was necessary to design a compensation valve that was certain to operate. The French did not consider this possible and hence avoided the need for one in their plan. However, A. Piccard designed a foolproof valve which appears to be certain of working whenever the pressure outside the hull is one-third atmosphere greater than that inside. In the event that the valve should fail to operate properly, a column of mercury in a U-tube would be blown out and the valve by-passed; however, to date

this safety by-pass has not been required.

The TRIESTE is propelled by a group of 10 batteries in series, each containing 25 lead cells, thus generating 500 volts and five amperes. The batteries are exposed to the pressure of the sea but the electrolyte is sealed from mixing with sea water by a bath of gasoline. The electric motors which drive the two screws are also exposed to sea pressure but are immersed in a low viscosity liquid dielectric (trichlorethylene) which is immiscible with sea water and has a density of 1.4. Hence, no propeller shaft seal is necessary. A gear reduction of 18 to one takes place between the two-horsepower motor and the screws. In order to conserve the batteries, the horizontal maneuverability of the TRIESTE has never been tested, but it is optimistically estimated that she can be driven for about five kilometers at about one knot on the available electric power. The motor weighs six pounds and is enclosed in a steel cylindrical box 25 centimeters in diameter. (The FRNS-3 is said to have a 28-volt, 50 amperes, and one-horsepower motor.)

It may be of interest to list some of the requirements for bathyscaphe operation:

a. A shipyard where the craft can be adequately fitted, serviced, and maintained. A crane with a capacity of 35 tons is needed to lift the craft (empty) out of the water for drydocking.

b. A harbor capable of taking the six-meter draft of the bathyscaphe with an isolated dock, inasmuch as there is a considerable fire hazard in the 104 cubic meters of gasoline used for flotation.

c. A short towing distance to deep water and good weather conditions with calm seas. The experimental nature of the entire operation requires that ideal sea conditions prevail.

d. An oceanographic or Navy laboratory near at hand to provide scientific instrumentation and a program for the best scientific utilization of the craft.

To the writer's knowledge, these conditions can be found in the U. S. at least at San Diego, San Pedro and at Pearl Harbor. However, we can nowhere match the calm sea conditions found in parts of the Mediterranean.

Present Status of the TRIESTE

The TRIESTE is now operated by Jacques Piccard with only occasional consultation with his 71-year-old father who is now living at Villa Aigueveve, Chexbres, Switzerland. The Navalmeccanica, where the TRIESTE is now based, is a private shipyard working mainly for the Italian Navy. The shipyard has taken an interest in the TRIESTE and has made its facilities available to the Piccards at a nominal cost. The Italian Navy assists to the extent of providing a patrol boat to tow the bathyscaphe into position for her dives and standing by during them. However, it has no official connection in the TRIESTE and makes no financial contribution to her development. In this present era of elaborate planning and organi-

zation, it is a refreshing experience to see the entire TRIESTE project, involving the development of an elaborate and experimental submersible craft, being carried on by J. Piccard with only a single, full-time assistant (a shipwright).

Although there is a small outstanding indebtedness of about $15,000, the TRIESTE is privately owned by the Piccards. The craft cost about $50,000 to build, money which was presumably supplied by the Piccards themselves and by gifts from various Swiss and Italian sponsors. Most of the components of the TRIESTE were furnished free of charge or at a nominal cost by various industrial concerns. The great Fiat Corporation seems to have been one of the principal contributors. The cost of replacement for the entire craft is estimated at about $175,000 - $75,000 for the sphere, $50,000 for the hull, and $50,000 for accessories.

Jacques Piccard computes the cost of each dive to 1000 meters to be about $750, with an additional cost of $150 for ballast for each additional 1000 meters. The main expenses involved are for ballast, amortization of the batteries which have a short life, wear and tear on the bathyscaphe, and such incidentals as oxygen, soda lime carbon dioxide absorbent, etc. In contrast, the French compute their cost to be about $2000 per dive for the FNRS-3.

Now that the TRIESTE is a reality and deep record-breaking dives are already accomplished, public interest in the project has waned so that it is difficult to obtain funds for her continued operation. As a result, a good deal of effort must go into the soliciting of sponsorship for the dives contemplated for August and September of this year.

Although Italian journalists frequently spin fantastic yarns about the bathyscaphe - generally along the conventional story line of fights with octopi, etc. - Jacques Piccard makes no extravagant claims regarding any observations or scientific results with the TRIESTE. "Sunday supplement" journalism is considered by the Piccards as their "worst enemy" inasmuch as it has done much to discredit their effort among those who might really be able to help them. In fact, the lack of funds for scientific equipment and the need to concentrate on operational requirements have allowed only the simplest of visual observations to be made during the dives to date. In this respect, the Piccards are far behind the French who are rapidly building a research program around the dives of the FNRS-3. Some of these results have already been published; see, for example, Deep-Sea Research, Vol. 2, No. 3, 1955, 247-9 and the proceedings of the Academy of Sciences, Paris, Vol. 238, 1954.

It is, of course, evident that the Piccards cannot compete with the French Navy and the scientific resources of France. Thus, little scientifically useful information will come out of the TRIESTE effort until some active collaboration is achieved. The Swiss and Italian scientists show little evidence of taking any active interest.

On his dives, J. Piccard has seen only sparse bottom life and has seen none of the deep-sea sharks which have been reported by the French. This is not surprising since the Mediterranean is a comparatively sterile sea. Only two deep-sea fish have been seen - one at 1800 meters and another at 2000 meters. These

were about eight inches long and appeared to be lit up with weak blue bioluminescence. Currents have not been observed to extend below 150 meters; this agrees with the French observations of strong deep currents in the Atlantic but none in the Mediterranean, presumably because of the absence of tides in the latter. When the lights are turned off, small flashes of light are commonly observed at all depths and, with the lights turned on, small scattering particles are seen. However, layers of scatterers have only been noted in the surface waters and nothing has been seen which might be correlated with the deep scattering layer phenomenon revealed by echo-sounders. On one descent (August 13, 1953) the TRIESTE had insufficient ballast to pierce the thermocline where an abrupt change in temperature from 27°C to 12°C was present, and was forced to return to the surface for additional ballast (in lieu of jettisoning gasoline or waiting for it to cool down to the ambient sea temperature).

There is a common impression that bathyscaphe dives are highly adventurous and are made at great risk of life. However, safety has been given first consideration in the TRIESTE design. For almost every possibility of failure there is an alternate plan so that there is no dependence upon perfect functioning of any particular component. In general, proper functioning is required to make the craft descend and, in the event of malfunctioning, it will automatically return to the surface - thus the craft is inherently safe. The fact that the French have now made more than 30 successful dives and the Piccards 15 dives provides good evidence that bathyscaphe operation has become fairly safe and routine. The writer has spoken to three persons who have made descents in bathyscaphes. These are Harold Edgerton of Massachusetts Institute of Technology (who made a dive in the summer of 1954 in the FNRS-3), Engr. Traetta of the Navalmeccanica, and Dr. Tardent of the Zoological Station of Naples, the latter two having made dives with Piccard. All three denied any feeling of apprehension and expressed great confidence that bathyscaphes are quite safe.

The Mesoscaphe

Jacques and A. Piccard are now actively working on the design of a mesoscaphe which is a sort of submarine helicopter. This is presently envisioned as an independent craft which will be able to leave and return to a harbor as a surface vessel but also to make descents down to 1000 meters.

The mesoscaphe is built around a Plexiglas spherical cabin which provides visibility in all directions without the need of special ports but, even more important, this sphere can be made to withstand the pressure at 1000 meters (with a safety factor of six) and still be positively buoyant. Thus, the sphere can be forced down into the sea by two counter-rotating screws, in a manner analogous to a helicopter, but will drift back up to the surface if the power is reduced or in the event of any power failure.

According to Jacques Piccard, plastics engineers who were first consulted thought that such a high pressure sphere was an impossibility but now agree that it can be achieved. The sphere will be neither a single casting nor a combination of two hemispheres; rather it will be made up of 12 pentagons which will fit together to form a spherical dodecahedron.

The strength of small Plexiglas spheres has been forcibly demonstrated to the writer who assisted in making a few tests in a small pressure chamber at the Navalmeccanica on spheres about the size of ping pong balls. A thin-walled buoyant sphere (two millimeter wall thickness and 24 millimeters in diameter) collapsed at a pressure equivalent to 5000 meters of water. (This sphere simulates a 2.4-meter sphere with a 20-centimeter wall thickness.) A similar small sphere with four-millimeter wall thickness collapsed at 9500 meters, and a thick-walled sphere (non-buoyant) with a six-millimeter wall thickness withstood the maximum pressure of the pressure chamber equivalent to 13,000 meters of water.

The effect of fatigue, of course, was not considered in this experiment. These spheres consisted of two hemispheres sealed and held together only with a rubber band - the rubber provided the low pressure seal and the spheres are self-sealing at high pressure. Regardless of the feasibility of the mesoscaphe, similar plastic spheres would seem to be very useful chambers for housing oceanographic instruments for submergence to great depths.

Conclusion

The success of the bathyscaphe marks an important post-war "break through" in the development of submersible craft. In this respect it is perhaps comparable to the recent successful application of nuclear energy to submarines, providing for prolonged submergence at high speeds. Unlike the bathyscaphe, the nuclear-powered submarine does not seriously compromise the hull design of the conventional submarine; hence its military usefulness has been immediately evident.

The military potential of the present bathyscaphe is, of course, nil and even future application of the principle to a militarily useful submarine seems remote. Nevertheless, the usefulness of the bathyscaphe as an auxiliary naval craft for oceanographic research or for deep salvage work is apparent. And, in any event, a great amount of technical information on the problems of deep submergence may be learned through the further development and use of bathyscaphes. Now that atomic power has freed the submarine from dependence upon the air and has for the first time permitted the development of a "true submarine" in contrast to a surface vessel that is able to dive, it seems that future emphasis will be placed upon deeper and deeper submergence.

DISCUSSION:

DR. FLEMING: I'm not sure about this, but haven't the Japanese built a bathyscaphe?

DR. DIETZ: Yes, I've been down in it to 120 meters but it is designed to 200 meters and there are variations of the bathysphere with better instrumentation and designed for continuous exploring of the continental shelf rather than the deep sea. It's a good device.

DR. FLEMING: One other thing I ought to mention, and that is again a subject on which I know practically nothing, other than it is being thought about,

and that is the use of the bathyscaphe for the exploitation of the oil resources. Some of the oil companies are considering the feasibility of permanent structures on the sea floor rather than the drilling platform technique. How far this has gone, I don't know. They were talking of working to depths of 100 meters. This was in their thinking at least, along with buoys and everything mentioned this morning; I think we shouldn't lose sight of the possibility of having laboratories on the sea floor rather than the mobile type you've been talking about.

DR. DIETZ: There are two main levels, the main continental shelf, and the deep sea, 2000 fathoms. You either design to one or the other and not to the deep trenches which is one-half of one percent of the ocean floor, or for the slope, which is one percent. They're quite different problems, of course.

LCDR CHIMIAK: LaCoste-Romberg produced a gravity meter and I had the good fortune to look at some of his records. He has on his horizontal and vertical movements a recording that indicates that he can measure horizontal and vertical displacements of a magnitude of four centimeters. It appears that with this bathyscaphe and with this meter there would be a possibility of determining either currents in deep water or of studying the internal wave in the area, say about 200, 300 or 400 meters.

He was down 200 fathoms and recording displacements. The instrument gives a continuous record in a log form. I think that it has possibilities. With the bathyscaphe in neutral buoyancy trim, and knowing those displacements can be calculated, a good platform in which to study internal waves or currents in the ocean can be obtained.

DR. DIETZ: I'd like to say that I think current is one thing we can't do well by. It would be better measured by instruments. One reason for the bathyscaphe is to do things we can do by seeing and thus getting a more sophisticated model of the ocean. Then we will, in the future, do our sampling on the basis of a new model of the ocean.

MR. BASCOM: I think oceanographers, and I mean the people in this hall today, should declare themselves for some sort of deep-sea vehicle or bathyscaphe for the United States. What can a person do better with instruments? If you have headphones, you hear, if you have cameras or TV, you see, but it seems to me Al Vine's remark this morning that the best possible instrument to send around on the BEAGLE was Charles Darwin is pertinent. This thought might readily apply to the bathyscaphe because by doing so we know what we should be looking for when we send instruments down. I think it's time for us to stand up and be counted.

CHAIRMAN EWING: Do I understand that that suggestion was in the form of a motion for consideration here?

MR. BASCOM: I would like to make it in the form of a motion, in the broadest sense; that oceanographers form themselves behind an organization which will be in favor of producing a deep vehicle as soon as possible.

DR. DEACON: I would like to say Dr. Dietz has given such a good account that

it doesn't leave room for comment except to emphasize this point: that the bathyscaphe is losing the news value that might help it be developed in the right way and it is time for scientists to make use of it. We shall come to it sooner or later.

DR. DIETZ: Perhaps a show of hands for the people generally for or opposed to the idea Mr. Bascom has proposed might help.

ADMIRAL SMITH: I think it ought to be put to a vote by the body at large. That might be perhaps a more effective means for oceanographers to express themselves than a showing of hands. I think oceanographers should make up their minds about this new device as a scientific tool, good or bad. There are, of course, going to be strong political pressures and factors involved.

CHAIRMAN EWING: Thank you. Would it be satisfactory that we form a committee?

MR. BASCOM: Yes.

CHAIRMAN EWING: I'd like to appoint a committee consisting of yourself (Mr. Bascom), Admiral Smith, Mr. Vine and Mr. Isaacs, to frame a resolution to be submitted later in this meeting for a vote.

19. THE USE OF RECORDING AND TELEMETERING BUOYS IN DEEP SEA RESEARCH

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Several papers presented in this symposium emphasize a need for oceanographic data the acquisition of which is well within our technical capabilities and which, as far as actual sensing elements are concerned, can be obtained by the use of techniques which are already developed in oceanography itself, as well as in other fields. This statement is, of course, subject to considerable qualification; for some areas we already have a fund of data of a specific kind that even now is taxing our ability to process and digest.

This bears on two problems beyond the scope of my remarks here: the choice of what measurements must be made to give the best picture of the real ocean, with minimum expenditure of effort, and, where large quantities of data are necessary, how most efficiently to bridge the gap between the raw record itself (smoked slide, plastic tape, or an observer's log sheet) and the desk, and in fact the mind, of the oceanographer. The point here is that while, of course, there are vast areas from which any information would be valuable, the synoptic picture of the best studied regions can be most effectively clarified, not by a saturation attack by classical techniques, but by supplementing these techniques (and I emphasize "supplement") with those which make similar measurements over long periods of time.

Another important qualification to my opening statement is that while indeed in most cases of interest in field work in physical oceanography the actual measurement techniques are well developed, the adaptation of these techniques to long-term, unattended recording or telemetering devices, packaging the product in a case, mounting it in a suitable vehicle, and finally consigning it to the sea, is a process fraught with peril. There is no need to elaborate this statement except to point out that once a buoy is clear of the ship, the opportunity for postmortem is so rare that there is instilled in the person charged with the success of the experiment a conservatism verging on the cowardly as regards the rigorous testing of components.

It is the subject of the vehicle itself that concerns us at this time. When we speak of in situ time studies in the deep ocean, as Dr. Wooster did yesterday, we are almost automatically speaking of buoys. To be sure, a ship will occasionally function as a buoy, either anchored or drifting, and while it makes a much more versatile one than any I have contemplated, it is certainly a very expensive one, and one which sorely tries the patience of many of those concerned. The definition of a buoy, for our immediate purposes, must of necessity be broad; I find it best to base it on function, not even including the requirement that it ever have positive buoyancy, or even lift. This may disturb the semanticist, but the need for a new word is not pressing; in this paper, "buoy" refers simply to an unmanned but man-made object, unattached to ship or shore by any stress-carrying member, which, when placed in the ocean, will provide certain information to an

observer, or assist in providing such information. This includes navigational buoys, drift bottles, telemetering devices which may be heavier than water and are expendable, or devices recovered by dragging. It does not include towers, manned units of any kind, towed apparatus, icebergs, or dead whales, although a whale containing a dart which transmits a radio signal approaches the limit of the definition.

In a general consideration of buoys for instrumentation purposes, it is tempting to make a broad classification according to some scheme whereby we can present a concise picture of all possible designs. One can do this, but my attempts do not lend themselves particularly well to visual presentation, and time is too limited to touch on all the buoys suggested by such a classification.

Hence, I will limit my remarks to such as have actually been used with success in oceanography, which are in the process of development, or which show enough promise to warrant considerable effort in the future.

Let us first consider surface vs sub-surface buoys.

Among other functions, surface buoys act as navigational markers, as markers for the recovery of submerged objects and as actual instrument stations of various degrees of complexity. Sub-surface buoys are at present more limited, but would include the neutrally buoyant floats of Dr. Swallow, the Woods Hole Instrument Recovery Buoy, and certain recorders that have been placed on the bottom and recovered by grappling.

Now let us look more closely at these two broad types.

The anchored surface buoy is probably of most general application. It can be a platform suitable for the measurement of phenomena both on or immediately above the surface; suitably designed dynamically, it can measure waves, it can function as a local navigational or maneuvering aid to a ship, and, since it is connected to the bottom by a cable, it can make measurements right down to the bottom. It can telemeter information or record it and, at least in the larger sizes, it can generate its own electric power. It is also the one that presents the most difficult engineering problems, at least when we are speaking of a buoy capable of staying on station and functioning for long periods of time in all sea states, and when a larger safety factor, or what I prefer to call a factor of ignorance is required. The problem becomes less impressive but no less difficult if we attempt to reduce the size of the unit, so that it can be used by small oceanographic vessels while keeping costs at a reasonable level. Of course, small units of limited sea-keeping ability are very valuable and a proven adjunct to the research vessel, as Mr. Bascom has pointed out. There is no doubt that all sizes of anchored surface buoys have their place and their successful application is to be expected in the foreseeable future.

If the function of an anchored buoy is to collect oceanographic data of no pressing urgency, data which are as valuable next year as they are today, long-range telemetering, aside from the natural desire to know how things are going, is probably not worth the price we have to pay in power supply, added displacement, and stronger mooring.

For non-meterological data, the small recording buoy looks attractive economically; the most difficult problems are those of mooring and of final location for recovery.

If the data measured have immediate value, telemetering is necessary, and in the case of surface buoys far from shore, radio telemetering is at present the most highly developed. Although some remarkable ranges have been obtained using low-powered transmitters, my feeling is that for reliable results over ranges greater than 500 miles, 100 to 200-watt transmitters are the minimum that should be considered.

For units capable of performing for from six months to a year, this means a large buoy (a ton or more) and it is in these sizes that generation of power by wave action (for which we have a preliminary design) becomes worth the added complexity. Since such a large part of the payload is devoted to the apparatus for transmitting the data, and since sensing elements are in general small, any such buoy might as well measure everything measurable; we are paying very little in added displacement for additional sensors. It will have a sizeable mast for the transmitter anyway, it might as well make meteorological observations as well as oceanographic ones; it will, in fact, be the unmanned deep-sea station. Since it must not submerge, it must have proportionately greater reserve buoyancy than the small recording station; it needs a minimum drag hull to ease the mooring requirement, and it will probably resemble a boat or catamaran, possibly with a mast mounted on gimbals.

The mooring problem is really the most challenging. Since we pay little penalty for an anchor of very conservative size, dragging should be unlikely; failure of single-part mooring systems is most likely to occur through the submergence and collapse of the buoy under high drag conditions (insufficient buoyancy), through chafe or wear of some part of the mooring system (probably near the region of greatest motion), or through the kinking and subsequent failure of the cable (most likely to occur near the bottom by release of load on a cable in torsion under conditions of low buoy drag). This latter difficulty does not occur with single strand wire, but larger buoys undoubtedly require stranded cable. Multiple moorings I will not discuss here; except for very special problems they are applicable only to the largest structures, which are probably manned. On the larger buoys it is probably practical, although possibly not the most efficient scheme, to use conservative, conventional ground-tackle supplying sufficient reserve buoyancy to keep the buoy on the surface under all conditions, using heavy chain for the upper part of the anchor rode where chafe is the greatest problem, and wire, broken by swivels which actually work in salt water, to reduce the probability of kinking when the wire is slack, particularly near the bottom. However, we can reduce the surface buoy displacement, and hence the drag, by supporting the cable with a sub-surface float which is presumably in a region of lower current velocity. The problem boils down to choosing components and so arranging them as to give a maximum over-all lift/drag ratio for the worse conditions.

The downward component of forces acting on the buoy, aside from its payload, is a function of cable weight and of the drags of all components of the system. The lift in a conventional design is a buoyant force only, but in regions of strong, deep currents it may be profitable to consider a sub-surface float whose

lift is in part dynamic. If a float with hydrofoils could be designed with sufficient stability under all conditions, it might enable us to achieve an optimum lift/drag for the system as a whole while using wire.

For relatively low velocities, buoyant cable looks very attractive. Many of us have thought about this problem for some time, and, at least on paper, have tried various designs, none of which have looked too promising because of the large diameters required.

The essence of the problem is that until recently no buoyant material had sufficient strength to carry the load by itself, or was elastically compatible with any likely reinforcing material. Using polyethylene as an example, if reinforced with steel or Fiberglas (this is a material with problems of its own), the reinforcing material can be loaded to the breaking point before the polyethylene takes any appreciable share of the load. If reinforced with nylon, the polyethylene parts before the nylon is carrying its share. In either case, some of the material is just going along for the ride, and analysis of a typical mooring problem showed no marked improvement over conventional methods. Nylon by itself seemed the best competitor to wire.

If I dare make a statement about an untried material, I would say that I think we are about to crack this particular problem. A high-strength polyethylene is now available for rope manufacture which is only about 15 percent weaker than nylon, with a density of about 0.94.

Even though we pay a considerable drag penalty when compared with the same strength in wire, this material in a rope may solve several of the mooring problems. It allows us to use generous scope without fear of the material chafing on the bottom under low drag conditions. The load is substantially uniform along the cable so we avoid the problem of throwing in kinks. It is easier to handle than wire and easier to recover, if experience shows that recovery is worthwhile.

There is also a form of a jacketed copper conductor available whose elastic properties are compatible with polyethylene. While I do not now know the effectiveness of the insulating material in sea water, it is beginning to look as though we will be able to anchor small buoys efficiently in deep water, and to use them to make measurements at depth.

One problem with the synthetics to which we do not know the answer is the attractiveness of these materials to fish with teeth. Present experience is varied on this score, but I do not think we should be frightened by the possibility until we try the materials.

The other problem, that of final location for recovery with present-day navigation, has probably been solved. We have had considerable success in homing on small buoys using low-powered transmitters and a conventional radio direction finder. More recently there has been developed at Woods Hole, by Walden and Ketchum, a completely transitorized receiver which responds to a tone-modulated signal to turn on the buoy's own transmitter; the battery drain is low enough to keep the receiver operating many months on a few pounds of batteries,

and reliability is excellent to date. Such a unit, with a low-powered transmitter, should solve the location problem for most surface buoys.

Where it is not necessary to measure surface phenomena, our problem may be vastly simplified by using a system no part of which is subject to wave action. This whole class of buoys is in its infancy and standards of reliability are still to be determined. However, it has been brought out in this symposium that both drifting and anchored sub-surface buoys, recording or telemetering, recoverable or expendable, can provide answers to some questions that cannot be obtained in any other way, or which could be obtained only with the use of much more expensive anchored surface buoys. An existing example of such a buoy is Dr. Swallow's neutrally buoyant float or submerged drift bottle. While in its present state of development this device requires the presence of a ship, it is well within our capabilities to produce a longer-lived telemetering buoy of this nature, possibly as has been suggested many times, dropping SOFAR charges periodically for location. The larger, longer-lived units would probably require a depth-sensing element and servo, but the power requirements should be small. It might even be worthwhile to have it surface periodically for radio location, although the departure from depth might make the current data subject to question.

The Woods Hole Instrument Recovery Buoy is another device in this class, designed primarily as a vehicle for recording instruments. It is an anchored buoy, with a life as yet undetermined but in present models believed to be of the order of six months. It can be moored anywhere between a depth of about 50 meters and the bottom, the maximum working depth being about 2500 fathoms. It releases its anchor on receipt of an acoustic signal -- at present the explosion of a small charge within a radius of a few miles -- and rises to the surface where it transmits a radio signal for final location. This device is undoubtedly not in its final form but we have used it successfully, without instruments, several times. We now have a number of these units and our experience with them should increase very rapidly.

The present Instrument Recovery Buoy weighs only about 35 pounds exclusive of ground-tackle and instruments, so it is obviously very easily handled on small vessels. It is limited in lifting power, the instruments it carries must be at least neutrally buoyant in themselves. If the present model proves itself, however, we can consider a larger unit, displacing about 100 pounds, which will be capable of carrying at least certain instruments within itself and of descending to better than 4000 fathoms.

All buoys which rise from great depth by virtue of their own buoyancy require considerable emphasis in instrument design on light weight. Many existing instruments can be lightened appreciably by a general substitution of magnesium and plastics for brass on interior parts, and by the operation of as many components as possible outside the pressure case. I think that Scripps is ahead of us at Woods Hole, and is capitalizing on this latter technique.

Radio location makes the use of buoys which record while going down and coming up quite practical, although to be competitive with the hydrographic wire their rate of rise must be comparable. This is difficult for maximum depth designs. Acoustic telemetering by expendable buoys which merely sink to the bot-

tom may be economical in certain cases. Certainly, deep bathythermograph (BT) information could be transmitted by such units to a rapidly moving vessel in less time than the conventional BT cast could be made. The depth telemetering system designed by Dow at Woods Hole for use on a wire could be adapted to a freely falling buoy, and while Doppler corrections would have to be made, this should present no insurmountable problems.

To recapitulate, surface buoys are necessary for the measurement of surface phenomena, but when anchored in the deep ocean, they are apt to be expensive. If a program can possibly be conducted with drifting telemetering buoys, even if they must be considered expendable, it may well be more economical than a number of anchored buoy stations. However, there is certainly the need for reliable, all-weather anchored stations. If the information obtained is of immediate importance, long-range telemetering is required, and the whole installation will be large enough and expensive enough to justify making all the measurements that can conceivably be of use.

If there is no urgency in gathering the data, smaller, cheaper units can be developed, with only a low-powered radio transponder for final recovery. Buoys now in existence which represent progress toward the above include Stommel's telemetering drift buoys for relative current measurements, the Scripps Instrument Station, and the telemetering drift buoy developed at Woods Hole which can be interrogated.

If it is not necessary to measure surface phenomena, the sub-surface devices, at least when they achieve sufficient reliability, will in most cases be more economical, primarily by virtue of smaller size and smaller moorings. Telemetering must be acoustic, and for long-range work will probably be able to convey position only, or at least the information transmitted will be very limited.

As adjuncts to a ship, however, acoustic telemetering by expendable buoys looks promising for certain purposes, but recoverable buoys must compete with the hydrographic wire. For measurements over long periods of time in one place, the Woods Hole Instrument Recovery Buoy, or a similar device that can be called to the surface, promises to be more economical, in ship time at least, than grappling up a length of cable, although the latter is a technique we have tended to overlook. Swallow's submarine drift bottle has achieved successful results and can undoubtedly be further developed.

In closing, let me suggest one possibility which, as far as I know, has not been explored. In designing for the deepest places in the ocean, it is very difficult to provide buoyant lift to give a reasonable rate of rise to a recoverable buoy. It would be advantageous to be able to recover a heavier-than-water buoy; a solid-fuel rocket motor might possibly be adapted to such use. Surface buoyancy could be provided by air-filled chambers. Usual combustion chamber pressures are less than sea pressures at great depth, but some propellants might work, and the ability to get the instrument up in a hurry might make the free-falling recorder a real competitor to the hydrographic wire, at least for thermometric data.

DISCUSSION:

MR. LAWTON: I was asked to come down here by Dr. Stommel, to discuss this paper.

It occurs to me that perhaps a little discussion might be in order on the mechanics of mooring in deep water, where it is necessary to use any sort of stress member which has a spiral lay, such as wire sheathing. We have run into this, of course, in cable work for many years, because cable ordinarily contains such a spiral member in the form of a single sheath of wires laid helically around the insulated conductor.

The subject has been exhaustively dealt with by two Britishers, Besly and Higgitt, back in the 1930's. What actually happens when you get a member such as that suspended vertically, is that there is a tendency to open up the lay of the wires, or at least to elongate the lay in the upper part nearer the surface of the water, and to tighten the lay in the lower part of the suspended length.

If the ship's head drops quickly on a swell, there is an inevitable tendency to throw a round turn in the bottom, which is pulled into a kink when the tension is restored, and that has been the biggest handicap in repairing cables in deep water that we have had to face, because as cables deteriorate, their sheathing does not deteriorate uniformly. There are weak places which are rusted more than others, and a kink always seeks out the weakest point.

In mooring buoys where you have to insert one or two conductors in the line for various purposes, at first blush the technique of using two opposed lays of sheathing, one over the other, looks attractive, and I am not one to say that it can't be done, because we have used that method successfully. But I would like to point out that such cable, in order to be designed for torsional equilibrium under tension, is very difficult to manufacture. You want to keep the helical angles with the longitudinal axis small in order to minimize torsional forces, but you run into difficulty in manufacturing the rope unless you keep the angles large.

It is not only difficult to manufacture such cable; it is very difficult to use it. For instance, we spent a lot of money to design and build such a cable, and had fairly good results on the tests. On board ship, on the first try, the whole thing was nearly wrecked because the people using it tried to handle it like ordinary telegraph cable.

It has been the practice in deep water on our ships and other cable ships, for years, where the bow sheaves are equipped with vee-shaped grooves to pick up cable which has a left-hand lay on the starboard side of the groove to cause the cable to rise onto the cheek of the sheave, and in so doing, to roll down into the root of the groove, tightening the lay. Thus, as it passes to the picking-up gear, the normal lay has been partially restored, and there is less tendency to kink when the tension is relieved and it is coiled down.

On this occasion someone was absent-minded, picked up the double-sheathed cable with a starboard lead, succeeded in twisting it, thereby tightening and putting all the load on the inner sheath and bird-caging the outer one, to the

extent that when coming off the picking-up drum and being relieved of tension, it threw itself into a series of twisted pig-tail bights and "cat's paws."

There was nothing wrong with the cable itself, but I am trying to point out how tough it is to handle a long cable like that. Once it has a twist in it, you are finished, because you can't reverse the process.

One thing should be guarded against. If swivels in the line are too free-turning, the tendency is to elongate the lay near the surface. The more you pick up, the more turns come out.

Of course, you people are not interested in recovering mooring lines to the same extent we are. We go on using the same lines many times. But it occurs to me that these points are all of a practical nature. Perhaps they would be regarded by Dr. Fleming as "housekeeping," but they are handy to know about.

I am very interested in this all-polyethylene rope which Mr. Frantz has mentioned, and it seems to have great possibilities.

We have an index in cable work which we use, which you may have heard of, called the Modulus of a Cable, or Modulus of a Rope, which is defined as the length in nautical miles of the rope or cable in vertical suspension in sea water which can be supported by its own strength. Ordinary deep-sea cable has a modulus of around 8, that amount of safety factor being desirable because of the gradual deterioration of cable over the years, and the necessity to raise it subsequently for repair.

In grapnel ropes, buoy ropes, and things of that kind, the modulus isn't far from that figure, but we have gone as high as 13 in modulus. If I interpret the data given me correctly, this polyethylene rope we are speaking of, being weightless in water, has a modulus of infinity, which would seem to be about the ultimate.

MR. FRANTZ: To date our anchoring attempts have been with short, temporary scopes 20 percent greater than the depth of the water. However, for rough weather, buoy design problems become very acute. If we use a scope of two and one-half to one, though, in good weather we are going to have slack cable on the bottom, and there this matter of kinking will become important.

MR. SNODGRASS: As far as the matter of attack by fish goes, we have been using a black polyethylene for better than two years, and we haven't had a single such case.

MR. FRANTZ: Does the use of black indicate you had had experience with other colors?

MR. SNODGRASS: No.

MR. LAWTON: The carbon black, I believe, is the anti-oxidant.

20. DRIFTING-BUOY-TYPE AUTOMATIC WEATHER STATIONS

W. A. von Wald, Jr.
U. S. Naval Research Laboratory
Washington, D. C.

About a year ago NRL, together with the National Bureau of Standards, undertook to manufacture six drifting buoys for use during the fall of this past year, for hurricane reconnaissance. These buoys had been previously developed during the war, but nothing much had been done with them. Right after the war there seemed to be a kind of lull in interest in these things, which recently has been revived.

The motion picture we are going to show gives a few of the highlights of this manufacturing and checkout, and the actual operation of putting the buoys at sea.

(Dr. von Wald then ran off the motion picture. As his remarks referred to it, they are not being included in this report.)

DISCUSSION:

MR. MAXWELL: I would like to make a couple of comments on this interesting film. One is that oceanographers in general have always complained about the lack of an adequate size oceanographic vessel, and adequate people to handle the equipment. I think this is a good example where they had more than they needed, in both cases.

Also, it looked as though the Navy was adverse to stopping the ship before putting the buoy in the water.

DR. VON WALD: They stopped the ship. There is a lot of footage showing the ship running along, but on the second trip they actually slowed down to about two knots. The water was a little rough, and the Captain felt the waves might bash the buoy into the ship if he didn't keep a little way on.

On the ROANOKE the weather was calm, and they actually came to a dead stop. In the case of the first buoy, they actually had to put the boat over, and spent an hour on that station.

MR. MAXWELL: The second comment I would like to make is this: I feel that this is an excellent example of work going on in the ocean that isn't necessarily being done by oceanographers. There is probably a great deal more of this going on of which the oceanographers are unaware, and they should make a great effort to try to find out about it. I believe we all can gain from it.

DR. BATES: Speaking on Mr. Maxwell's point, one of the next projects we have for the buoys, I believe, is to put two of them in the Arctic Pack beyond the Alaskan coast this summer, if everything works out.

DR. VON WALD: That is correct. We are preparing to put a couple of them on the Arctic Pack ice.

MR. SHAEFFER: Was there anything interesting about the buoy that was recovered?

DR. VON WALD: The buoy didn't look any different to some of those left at the laboratory. There was virtually no corrosion; just a little on the outside and none at all on the inside. It was operable in every respect. Apparently a set screw came loose on the program timer selector switch so that the transmissions weren't timed right, but other than that single failure, the buoy was completely normal in every respect. In fact, one of those we are fixing up for the Arctic was one of the first ones tested at sea.

DR. VON ARX: Did you have occasion to check the results of the indicated wind direction and speed against an anemometer in a small craft alongside, to see what the heeling error might have been?

DR. VON WALD: This we did from the anemometers aboard ship. The agreement was very good while the ship was in the area. The barometer was reading within a millibar of what the ship's barometer read. The direction was within 10°, which was our resolution in this case, and the wind speed was within about three knots, I believe.

DR. VON ARX: Was this accounted for by the difference in anemometer heights?

DR. VON WALD: This could well have been.

DR. RICHARDSON: Were you able to pick up the second buoy by the direction-finding of the FCC, or was this happenstance?

DR. VON WALD: I am not sure what went on in the recovery of that buoy. I haven't had a chance to talk to the ship's Captain -- messages indicated there may have been some Italian Air Training Command aircraft in the area, which helped send the SAN PABLO in on the recovery. The people on the SAN PABLO indicated they could locate them from the direction finder.

MR. WORTHINGTON: Do they transmit about every two hours?

DR. VON WALD: That is right. If you wait too long and are a couple minutes late, that is tough. We staggered buoy transmissions by half-hour and hour intervals. In the case of the first one we had time for a short beer and a smoke before the next one came on.

DR. LEIPPER: In meterology, there is a better opportunity to obtain synoptic data, but in 1943, when meteorologists began developing single-station techniques, they were of of considerable value. In oceanography, the single-station technique might be more important and more work should go into this type of thinking.

21. OBSERVATIONS OF BIOLUMINESCENT FLASHES IN AND NEAR THE DEEP SCATTERING LAYER

James M. Snodgrass
Scripps Institution of Oceanography
La Jolla, California

I have been given permission to take the time to present some observations I had meant to show earlier. They are germane to the talks by Dr. Duntley and Dr. Hersey.

Figure 21.1 is data taken by Boden and Kampa, of Scripps, off the California Coast, just this last month, and I think, were taken late in the evening if I remember correctly. This shows the light intensity and irradiance against the depth in meters.

The light intensity function shows the spikes which you heard about before, which are certainly luminescent organisms.

Since the recording equipment has a relatively long time constant, these are almost

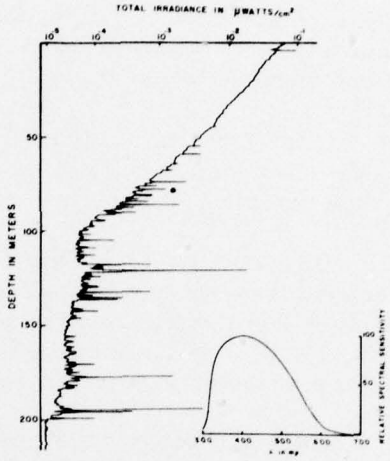
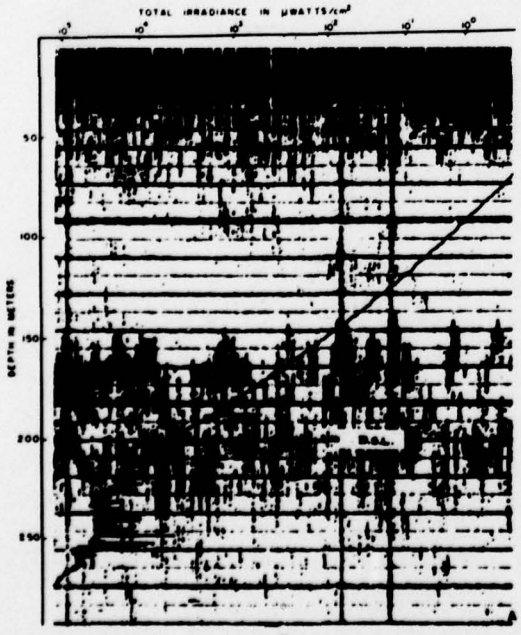
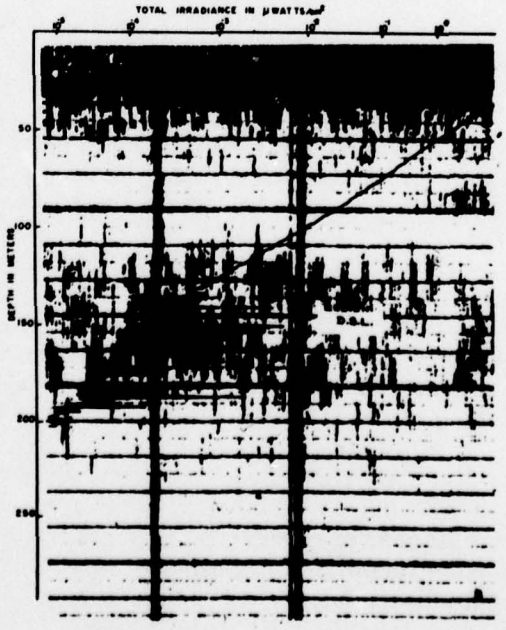


Figure 21.1 Variation of light intensity and irradiance with depth

Figure 21.2 a and b (below) Superposition of Figure 21.1 on bathymeter traces



a



b

substantially below their true amplitude.

Figure 21.2 is composite and shows the same curve superimposed on the scattering layer itself. Everything is to the proper scale and you can see the flashes just below and in the scattering layer.

I would like to see what Dr. Hersey might get if he left his camera shutter open occasionally, without using his flashlight. I think that might be very well to be done.

22. CONSIDERATION OF FUTURE OCEANOGRAPHIC INSTRUMENTATION

James M. Snodgrass
Scripps Institution of Oceanography
La Jolla, California

It is common knowledge that oceanographers are beginning to find themselves embarrassed by a wealth of data. Perhaps this is too mild a statement since many oceanographers may be said to be suffering from a form of indigestion due to their inability to assimilate an excessive amount of roughage. This roughage is the raw data obtained from field work. Shots of vitamins and nostrums will be of little avail to our patient unless more digestible fare is forthcoming. This is a fairly serious situation and our patient may actually expire altogether unless a new regimen is available in the near future.

To continue for a moment in this vein, it would appear that our patient may be beyond the skills of the general practitioner and that a consultation of specialists should be considered.

I do not wish to give the impression that I have all the answers to our various problems, but I do wish to emphasize the fact that I believe the necessary tools are now available to help us find them.

Some small beginnings are now being made but it is high time for a comcerted effort. First, I feel that we should examine our recording instruments. It is now possible, in fact quite practical, in many respects to design our instruments so that their records may be processed and analyzed essentially automatically, permitting the data to be worked up with a minimum number of man-hours. This, of course, requires that we take advantage of any and all developments in the field of recording, such as data handling and processing and the use of high speed computers, both analog and digital.

It should be emphasized that these requirements do not of necessity place impossible restrictions upon the design and use of each instrument. It is only necessary that each record be capable of automatic processing. This, of course, leaves a rather wide latitude for individual preference. At the same time, however, it would then be possible automatically to convert one type of recording into another. None of our requirements should preclude the possibility of making the record available for immediate inspection. As is often the case, field plans are modified by the interpretations placed on the recordings being obtained. At present there are literally so many methods of recording data now available that it is impractical to take time to cover them in detail.

Each medium and method has its own advantages and limitations, and it is imperative that these factors be well understood so that the best use may be made of the available techniques. Magnetic tape, for example, is invaluable as a recording medium particularly in the field of guided missile telemetering. It is also regarded as indispensable in the aircraft industry in the recording of tests on aircraft. In spite of the relatively great field simplicity of magnetic tape recording, it currently suffers a serious limitation to certain applications since it does

not permit the ready sorting of data. However, automatic equipment is available to convert the data on magnetic tape into a form which may then be readily sorted, as, for example, punch cards.

It is highly probable that in the future each laboratory will find it necessary to have available its own individual computer and data processing facilities. However, this is no longer quite the problem it was a few years ago. Computers which used to require several rooms full of equipment are now being replaced by desk-mounted units consuming little power which do everything and more than the older more bulky units accomplished using many kilowatts of power.

Furthermore, the cost has been very substantially reduced. The reliability of computers has also tremendously improved, particularly with the advent of the transistor and various magnetic components. It would, of course, be desirable for each laboratory to equip itself with the type of computer most specifically adapted to its problems. However, with all of this diversity we should maintain the ability to convert data from one form to another. If this is done, it will become practical for each institution to use another's primary data without the necessity of cumbersome manual copying techniques. The international exchange of oceanographic data would be readily facilitated if our records were in such form that automatic processing could be used.

We should also be able to handle field data not obtained by ideal recording instruments. This means that in order to use our automatic devices we must have the facilities available for encoding field observations into a form which will permit automatic processing. Unfortunately, one does not just dump quantities of data into the automatic equipment and sit back calmly awaiting the results. Many of the automatic machines must be programmed. By this we may assume that certain orders or directives must be fed into the machine in order to tell it what to do with the data with which it is being supplied. This whole process of programming is rapidly becoming a rather highly specialized art and is often the real key to the successful use of the automatic equipment. However, this is not to be considered entirely unfortunate limitation since much of the automatic equipment is able to remember what it was told to do and to repeat the desired processing on command. Thus a whole library of programming orders may be made available for any given piece of automatic equipment.

For example, in the machine tool industry which has found itself compelled willy nilly to accept automation, we find milling machines cutting intricate metal shapes entirely automatically, controlled by high speed computers in response to programming orders contained in a simple roll of punched tape. In order to change the shape of the metal being machined it is only necessary to insert the proper roll of punched tape. Thus instead of carrying a large parts inventory, it is only necessary to file this roll, since at any time in the future the necessary part may be quickly and automatically fabricated on demand.

I should make haste to point out that in order to make an effective beginning in the handling of rather large amounts of awkward data, one does not actually require expensive and elaborate facilities. Dean Bumpus, of the Woods Hole Oceanographic Institution, has done a beautiful job of applying inexpensive punched cards to the working up of drift bottle data. This is an immensely effective though very

simple technique.

The U. S. Navy Hydrographic Office has perhaps used certain types of automatic equipment more than any other institution with respect to oceanography. The IBM punch cards which they have been using have certain definite limitations and they have recently embarked upon a radically different technique requiring digital computers for the processing of information which is not well adapted to IBM punch card techniques. I hope that we will have some opportunity to hear from them about their new equipment.

As desirable as obtaining a large number of new oceanographers may be, I do not believe that this is a fundamentally sound solution to the problem of handling the immense mountains of accumulated data. It may be that in their early days they will be effective in aiding the handling of data but as soon as they become more proficient oceanographers they will become interested in problems of their own which will again produce the accumulation of still other mountains of data which will again require still more oceanographers, ad infinitum. We must, I think, turn to other possibilities for the solution of our problems and I feel that we should very seriously begin to study and evaluate the various available automatic tools which can truly free our oceanographic slaves.

I should like to take the liberty of touching briefly on some of our recent work at the Scripps Institution of Oceanography which is a preliminary experiment as to the feasibility of building instruments to do some of the things which we have outlined, namely, to supply a record which is available for immediate inspection, and at the same time furnish a record which is capable of automatic processing. This instrumentation was carried out under a contract with the Bureau of Ships.

To date we have worked in relatively simple terms, but the results have been very gratifying. The instrument to be described is essentially a bathythermograph in modern dress.

The pressure, or depth sensing, element which we use is known as a Vibrotron. This is a transducer which converts pressure directly into a frequency of electric current which is very precisely related to the pressure. In our own case the Vibrotron received a small amount of electric power from a transistor amplifier which was entirely self-contained, with batteries permitting continuous operation for periods in excess of one week.

Parenthetically I might add that Vibrotrons are available on the commercial market with transistor amplifiers and batteries occupying a total volume of approximately six cubic inches; the entire system is capable of operating without the replacement of batteries for a continuous period of approximately three months. Depending upon how the Vibrotron is used, it is capable of very high orders of accuracy. In certain applications made by Dr. Walter Munk and Frank Snodgrass, of the Scripps Institution, sensitivities of better than one part in 250,000 have been obtained; in our own case our accuracy is at best one part in 3500.

The temperature-sensing element consists of a bridge oscillator whose frequency is determined by the dc resistance of two arms of a bridge. These two

frequency-determining arms are a matched pair of 14B thermistors. The power for the bridge is obtained from a small sub-miniature vacuum tube with batteries capable of supplying it for continuous operation of approximately one week. The constants of our resistance bridge are such that we obtain a frequency change of approximately 44 cycles per degree centigrade. We do not yet know what the long-time stability of our particular thermistor system is, however, it is very easy to calibrate and make any necessary long-time corrections. The present accuracy is of the order of 1/20th of a degree centigrade.

Our present system uses a single electric conductor as both the supporting and the telemetering cable. On shipboard, two records are made, one on magnetic tape and the other is presented in analog form on a Leeds and Northrup X-Y coordinate recorder.

Figure 22.1 illustrates the type of record obtained on the X-Y coordinate recorder. It was prepared in haste and the coordinates are unfortunately lacking. For the sake of convenience, the depth is approximately 100 feet per large division and the temperature is approximately two and one half degrees per large division. On the record each large division equals one inch. The records shown were obtained at approximately 15-minute intervals and show interesting details. Some of the by-products of this type of recording are very attractive, for instance, it is quite possible, with a supplemental recorder, to plot an immensely expanded portion of the indicated curve so that details may be read more easily. Gradients may also be directly plotted. After the field trip, the magnetic tape may be played back in the laboratory and analyzed in any way desired. With the proper facilities, punch cards could be prepared from the magnetic tape.

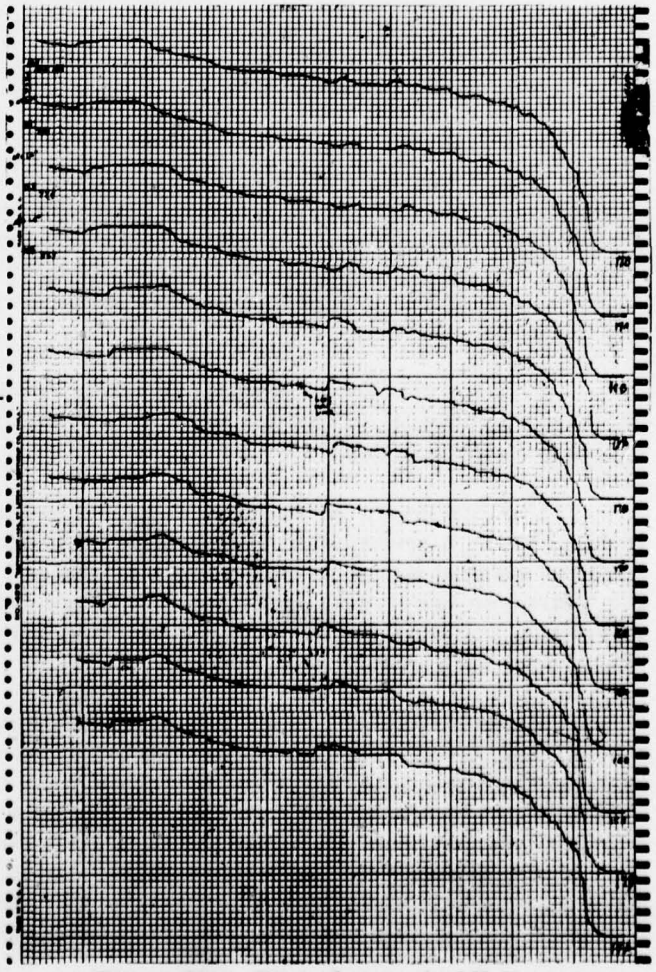


Figure 22.1 Typical record of X-Y coordinate recorder

Our own work to date which will permit automatic processing should be considered only experimental. A truly sophisticated approach to the problem would require that considerably more additional information be encoded on the magnetic tape, such as perhaps latitude and

longitude, Greenwich Civil Time (GCT), symbols corresponding to sea state, etc. This supplemental information is exactly the kind of thing to which we need to give considerable study and thought.

To conclude, I feel that there is very real hope for the ultimate well-being of our oceanographic patient. The prognosis is good, but it will require a careful concerted effort to get our patient back to robust health.

DISCUSSION:

CDR ROBINSON: I can say I have been heartily refreshed by Mr. Snodgrass' remarks. I am no oceanographer, as all of well know, but I have been amazed at the amount of data that have been accumulated in our dusty rooms. The Oceanography Section (U. S. N. Hydrographic Office) is always asking for more space, and I have a hunch it is to file more data. So we have been triggered by a report put out by Woods Hole, where they use what we call the knitting needle approach. We figure all this can go on IBM cards now that there are systems, thanks to guided missile progress, that convert IBM cards to digital analog analyzers on tape. The storage requirements are small. It is perfect.

But one thing which Mr. Snodgrass pointed out that I can't over-emphasize, is that all of our information, to go along with Dr. Iselin's statement, must be readily available for inter-communication. That means the information that we hold must be suitable for your individual techniques, and that much thought must be given to the form in which this information is collected and recorded.

What we must do is reach the stage in the recording of information where, if anyone wants to analyze temperature, he may do so, and the whole of recorded data should be available in some one place for his use.

Now I cannot speak of what we have. I can only speak of what we plan. Our progress will necessarily be slow because it must be lasting. If we can organize a bank of information available to all, we will have made a great step forward in the housekeeping business of oceanography.

So I urge, since this is a pressing problem apparently with all of you, that serious thought be given to the problem of how to decide on a system of recording so that everybody's needs are satisfied.

DR. MUNK: I wonder if I may be the devil's advocate? I think I am right in saying there are very few parts of oceanography where we are sure that we know what we are looking for. There have been exceptions, as in wave work, but I know of few other examples where we know enough to make effective use of the automatic kind of analysis.

I would think Mr. Fuglister, who has hundreds of thousands of BT's, has learned more by poring over his data and pushing his pencil around in different ways than by being sophisticated about it.

Is that a shortcoming of oceanographers? Has Dr. Wexler, who has so much data in South Carolina been more successful?

I am merely raising this as a question.

DR. BATES: I think this is one of the problems facing oceanographers and the Navy.

Dr. Fleming worried about the question of whether all data is being pooled so that it will be generally available five or ten years from now. It is almost a "now or never" thing within the next year or so to submit to enough regimentation to make all our data available to our successors.

I am not sure whole books of numbers is what we are looking for. There are many types of measurements that are not suitable for publication in this manner.

MR. MAXWELL: Have oceanographers made any attempt to collate data collected by engineers and other organizations? Many data they collect may have direct application to the oceans and I wonder how much of that is lying around the country.

DR. LYMAN: I think that the Hydrographic Office, in recent years, has made a concerted effort to locate as much of these data as it is possible to find. It is not always easy. We have not tried to do it ourselves. We have hired people to do it for us under contract.

MR. ISAACS: It seems to me we have to tread a little carefully here. I have heard quite a few people say recently that only by poring over the data themselves could they achieve satisfactory answers. Only in this way do they notice any oddity, or strangeness, or hints of where to look next.

I don't know how much adaptability these systems have, but I feel that where we do know what questions we asked and what answers we got, we have something definite. I am not sure we can accommodate cases where we are not sure of either.

MR. SNODGRASS: This is just exactly the point I wanted to make clear. None of us today knows the questions we are going to ask ten years from now. The thing is to get our raw data in such form it isn't distorted, and then it can be used in any way we wish to answer the questions we will ask ten years from now. We don't want to destroy our raw data. What we do with it is another question.

MR. ISAACS: Some quite unusual findings often get thrown out.

LCDR CHIMIAK: I go along with what Mr. Snodgrass said, and I say there is a common denominator in oceanographic data, and think that, if nothing more, it should at least be catalogued. It acts as a rough hand-drawn road map showing where you are going. The individual who has a specific problem can have this as something of a guide, and further, he can build on it. One big item that has been stressed here is that we have very few deep oceanographic stations. This is a pity because we spend much ship and manpower time in the ocean. It would have taken so little more time to go deeper. Being a Commanding Officer of a ship, I know it would have taken me little more time to lower a little farther to get informa-

tion which is now apparently of value, and the same thing could have been done by others.

We should have the classical type of data recorded and readily available in some form which is usable by, say, better than 50 percent, of the researchers, and catalogued in such a way as to give him a lead on where he is going. It helps the young oceanographer, not the people sitting in this room. But you are overlooking the young boy coming up.

DR. ISELIN: We have thought about this problem a little at Woods Hole. It boils down to roughly this in my own mind. If you are primarily employing the ship to do research, that is, to solve an immediate problem, the data handling part of the job is not serious because the people on the ship are intimately interested in what they are observing at that time. Usually the answer to the problem comes out of a few of the observations that you were lucky enough to make at the right time and at the right place. You solve the problem. But if you are doing a survey job, just trying to map the ocean, why then it is a more serious problem.

As far as I know, we don't try to take observations if somebody isn't right there at the instrument watching, and wants the data to study.

For instance, there is this very nice sea and swell instrument on the ATLANTIS. It has been on now five months. But since the first week no one on the ship was interested in wave data. Each group has a slightly different problem. It depends on what you do, what the objective of the cruise is.

Furthermore, I think there is the point we are all well aware of — you have to be sure you don't direct data. This means it all has to be looked at and studied before it goes into the record or the mistakes will defeat you in time.

DR. FLEMING: I have had a hand in two of these decisions. The whole program of BT processing was started by Ray Montgomery and myself. I don't know whether it is a good thing to go into parenthood in this case, but at least we did make a decision on how we might preserve bathythermograph records in readily usable form.

When I was in the Hydrographic Office, after much debate and argument, we finally made the decision as to what might go on the IBM tabulation, and this is continued. Certainly there are limitations to all of these more or less mechanical ways of compiling data. You have to weigh the advantages against the disadvantages, and try to optimize the system looking ahead as far as you can to what may be the needs of the future.

In order, however, to give a slightly new slant to this problem, John Isaacs and others at this meeting have stressed the importance of being able to communicate oceanographic information. I think this is very closely related to the storage of this information, in a way.

This is what the Weather Bureau does. It communicates the weather observations, then they are stored. I think oceanography has to look to this, and only have to look a few years ahead to the International Geophysical Year where

the multiple-ship operations are going to be truly effective. These ships must be able to communicate with each other synoptically the essential features of their observations. I just offer this as another aspect of the problem that will have to be solved very shortly.

We experimented with communication of data during Operation CABOT in 1950, and there is going to be exactly the same problem to face during the International Geophysical Year; that of being able to transmit essential information on the spot, to other vessels.

What is this information going to be? If it is carefully thought out, this is the kind of information which I think you want to keep on hand in your data files, in your laboratories.

23. MAPPING OCEAN WATER

Ray B. Montgomery
Chesapeake Bay Institute, Johns Hopkins University
Annapolis, Maryland

My comments concern certain aspects of the present and future development of descriptive oceanography, which is the branch that has as its task the description of the distributions of variables in the ocean. The variables of first concern are temperature, salinity, and specific volume, but the comments apply as well to other variables, for example, to the concentrations of oxygen and other constitutents and to velocity.

I want first to point out certain shortages or deficiencies. Any scientist or engineer concerned with the ocean at times needs information about the distribution of temperature, salinity, or specific volume. For instance, any adequate study of circulation or currents must take into account the known distributions of variables; these distributions alone do not fully reveal the currents, but they impose conditions that must be satisfied by any realistic current pattern, in such ways as those discussed by Dr. Wooster and by Mr. Stommel. Where does the scientist or engineer find the descriptive information he wants? He may have his own observations, and in specialized or routine work he may have a special source of information. Otherwise he goes to general sources, of which I shall mention three.

The greatest single source is the voluminous report of the METEOR Expedition published during the 1930's. This report is limited to the Atlantic Ocean (and there is nothing like it for the other oceans). Furthermore, in spite of the variety of charts and sections, the representation one wants may be lacking.

The second source is the very useful pair of books by Gerhard Schott, the geographies of the Indian and Pacific Oceans (1935) and of the Atlantic Ocean (1944). Although these books contain some comprehensive charts of conditions at the ocean surface, the information about subsurface water is rather scant.

Third, there is the well-known Sverdrup-Johnson-Fleming book "The Oceans." (1942)

These sources are far from adequate to meet present needs. There is no modern, comprehensive work that describes the waters of the ocean.

At the same time that we need comprehensive studies, there is great need to get on faster with the analysis of the postwar observational material. In the central Pacific, where there had been few hydrographic stations before the last war, a large number now exist. Of course all the existing general sources are too old to include any of this new material. The new material has been only partly analyzed because there are not enough oceanographers working on analysis. To the large amount of data already waiting to be analyzed have now been added the results of two big operations in 1955, NORPAC and EASTROPIC. Furthermore, present plans call for EQUAPAC, then International Geophysical Year.

If we are to keep up with this vast observational program, we must have more oceanographers. They need to be trained physical oceanographers who are interested and competent in the field of descriptive oceanography.

Analysis and publication have been neglected in the planning of oceanographic programs. When several oceanographers go on a field trip for two months, the material gathered can easily keep them busy for ten times as long in the analysis. The cost of analysis and publication becomes comparable with the cost of the field work. It seems to me that when plans are made for field work, with allocation of funds and personnel, consideration should at the same time be given to the availability of funds and personnel for the subsequent longer, and perhaps more difficult, task of analysis and publication. The field work alone is of little value; the results become useful only when they are analyzed and published.

Now I want to call attention to the division of descriptive oceanography into analysis and synthesis. This division has already been used in separating the deficiency in analysis of observations from the deficiency in comprehensive sources. The study of some given observational material, drawing out relationships and effectively depicting the observed distributions, is analysis. On the other hand, the task of constructing the distribution throughout a whole domain, using all means available and interpolating and extrapolating from the existing observations (which have preferably first been analyzed), is synthesis. Of course the two processes are partly identical, but the purposes are different. The purpose of analysis is to draw all information from given material. The purpose of synthesis is to construct the best answer to a given question.

In analysis, some improvements have been achieved in the last few years:

First, there had previously been a tendency to study the different variables (temperature, salinity, oxygen, and so on) independently. Now we have methods for preserving the interrelationships between variables. Each station is analyzed as a unit, all the curves for the one station being drawn on a single diagram. Then, for a line of stations, the distributions of variables on the vertical section can be drawn from the station curves in a consistent manner.

Second, it was formerly customary to use temperature and salinity to calculate sigma-t, a form of density, and then from sigma-t to calculate specific-volume anomaly for use in hydrostatic computation. It is direct and simple to avoid density entirely and to adhere to forms of specific volume. This direct procedure has been adopted for some of the analysis in the Pacific.

Third, a further step that has been discussed and appears entirely feasible and desirable is to publish, along with the tabulated observations for each station, a small graph showing the complete set of analyzed station curves. This step will greatly facilitate further use of the data.

Further changes in analysis will surely come, but we have adequate methods at hand now to get on with the big task of analysis if descriptive oceanographers were available in adequate numbers.

In the subject of synthesis, on the other hand, the way is far from clear.

Mr. Stommel's paper mentions "the perennial problem of forming a realistic mean map of the distribution of ocean properties." To return to the Pacific, suppose we could stop further observing for a time and first get all the existing stations and sections nicely analyzed. Then we would be in a good position to undertake the synthesis of the distributions of all the variables throughout all the Pacific Ocean. How, in some economical set of cross sections, charts, and other diagrams, could we best represent these distributions? What is a good way to map ocean water? I do not think enough attention has been given to this question for anyone to give a good answer.

The problem of synthesis is shared by oceanography in common with meteorology and climatology. The problem is to depict the relation of several variables to space and time coordinates and to one another, in short, the representation of interrelated variables in four-dimensional space. The possible representations are infinite, while for effectiveness the number must be kept very small. It is easy, indeed, to prepare a large number of charts and still to omit the interesting and important information. There are some existing collections that in my opinion suffer from this defect.

Synthesis is an important subject that has not been widely recognized and that has received little or no systematic study. Conscious study is needed, using imagination and experience.

These comments I have made are a small selection from many that could be brought forth to offer evidence that descriptive oceanography merits greater emphasis and support.

DISCUSSION:

DR. ISELIN: About a year ago I was worrying about very much the same thing, and I had what I thought was a good idea, which I will pass along so you can bear it in mind.

In this mapping problem, which is certainly in a measure the responsibility of oceanography, there are some people we could perhaps call in to help us, who would be glad to help us, namely the geographers. Why physical geography turned its back on the ocean, I never quite understood. These people are trained, to some degree, in this synthesis problem, and they have run out of things to map. About a year ago I went to Dr. Hitchcock, who is the Director of the American Geographical Society, and asked him if he was interested in a big project. Strangely enough, he said, "Sure. We planned to map Africa, but we find the British have photographed it so there is no sense in doing that."

Geographers apparently like things that take them ten years to do.

We held a number of discussions and meetings, and Hitchcock is still keen to do this job. He is thinking not so much of strictly a physical atlas, as a complete geography of the oceans, which would include mapping all the ecological factors, all the factors of human geography, the number of ships per square mile and all -- geographers can think of a million things to map.

We would get it out as a sort of loose-leaf book, each map being complete in itself, with the descriptive work on the back. The question is, which areas do you start on? Do you want to use one base map for the whole world, or break it up into pieces?

He is thinking about this, and wants some support to get started. If he does get started, he will be coming around to these figures we are talking about, and will ask us for data!

I think if anybody besides myself feels that this is another source of manpower, that these are proper people perhaps to handle some of our problems, if they would encourage him, he might get going on this sooner than otherwise.

DR. LEIPPER: It seems to me in general, oceanography is ten or fifteen years behind meteorology, but in this case maybe one or two years behind. There is a school in meteorology in weather analysis that has the same philosophy but worked out a little further than we have in oceanography. It seems to me to be a very worthwhile approach.

MR. WOOSTER: I think, if I understood Dr. Montgomery correctly, the point here is that describing the ocean is a vital part of oceanographic research. It has suffered in emphasis in recent years, and perhaps needs to be restored to its proper place in the over-all scheme in oceanographic research.

DR. DEACON: It all depends on the details. My last respectable report, published in 1937 and finished in 1935 was based on about five years work. We didn't finish the survey then. We went on until 1939, and even after the war, getting more sections over the southern ocean. We worked on through the war. You may improve it a little, but you can't add much to what you have already described and distributed.

The next step is to get something about distinguishing the water layers, rates of movement, and so forth. But this was not possible. I came to the conclusion it wasn't worthwhile going on with these sections across the southern ocean until we could make the sort of measurements we really wanted to make; the kind of measurements stressed at this meeting.

We should know more about the mixing of the water, we must have a decent theory about the thermodynamic and wind effects. The descriptive oceanographer is still very much with us; you have papers at scientific meetings now that might have been read a hundred years ago.

A couple of years ago I listened to a paper by a descriptive oceanographer trying to account for the existence of a certain water mass. He pushed his arguments to the utmost extreme, but took no account of anything like wind, because he wasn't able to. This being the case, he chose to ignore it. Eventually there was some argument between this oceanographer and a physicist in the audience. The physicist couldn't understand the descriptive oceanographer, couldn't believe what he was saying, and the oceanographer couldn't understand the physicist. It was the most remarkable situation. They both sat down shrugging their shoulders. I doubt that Professor Schott could add anything now to his study of the Atlantic

and Pacific Oceans.

MR. FUGLISTER: I rather doubt that Professor Schott would want to change his description of the Pacific Ocean. In fact, I think even if he had all the data available today, or all the data that are going to be available in the next five or six years, and didn't pay attention to extraneous details, he probably would confirm the picture he had.

I feel that the descriptions that have been made in the various parts of the ocean are correct, that the very early ones were correct, and that they continue to be correct. But it is a question of bringing in what the earlier people would have considered extraneous details. The first person who noticed that the water was cold at the North Pole and warm at the Equator, and changed as one went from south to north was correct. The big advance was made when we found that in some places the water actually warms when going from south to north.

I believe if Schott had had a lot more data, that he would reproduce essentially the charts that he had already drawn, but I don't think that somebody else would necessarily draw a chart like Schott, especially if he hadn't seen Schott's.

DR. FLEMING: I think it is of interest to look a little at the history of oceanographic data in this connection, because we are apparently at a stage in our history where we are rather confused about this whole problem.

Early oceanographers not so long ago, -- such as the CHALLENGER and the workers of the late 19th century -- measured temperatures at various depths, and then drew a smooth curve through them. They didn't place too much credence on what Mr. Fuglister has just referred to as extraneous detail. Also, they were aware of the fact that their instruments weren't too accurate. Up to about the turn of the century there was a great tendency to average data and present only averages. With the development of more precise instruments about the turn of the century, particularly the reversing thermometer, and of salinity determination, it became possible to get more precise figures.

Hansen was a great advocate of the tradition which has been handed down to us through the Scandinavian school of oceanography, that every individual observation has a unique significance. Most of us follow this tradition today in the way we analyze our vertical series of observations. We try to make the curves pass through every point. We don't throw out a point on any curve unless we can find a justifiable argument for doing so. We think our instruments apparently exercise better judgment than we do. We are at a stage now where we are being confronted with a large number of presumably accurate observations.

We are apparently completing a sort of cycle or wave, or periodic variation where we have gone from averages to individual observations and we are getting back to the stage where we must think in terms of some sorts of averages again.

MR. METCALF: It seems to me that if we could assume that the oceans were no longer changing, it would be easy to foresee a time when there is no place in oceanography for descriptive oceanography. But if we can't show that

we can never foresee a time when there will be no place for it. Worthington has indicated that changes are going on, and as long as changes go on, we are going to have to continue to describe the ocean.

DR. DEACON: Is detail really a useful thing to describe when you describe the ocean?

We have divided the southern ocean into different regions, where we know there are animals typical of the sub-tropical zone. I have no doubt if we had more observations, we could put minor wiggles into this division but not change its shape significantly. We have yet to find how the waters move. We have a good picture of the water masses. If you get more density observations, you might achieve something, but I don't think very much.

Dr. VON ARX: The thing that appeals to me in consideration of the plight of descriptive oceanography given here, is that if we limit our attention to salinity, specific volume, and temperature differences in the ocean we are forced to fall back on steady state to interpret them.

It seems Mr. Worthington has shown there is a secular variation in the ocean, and Dr. Swallow has shown there are short period variations, even in the region of the sea that we tend to consider to be very quiet and which, under geostrophic principles, would seem to be entirely motionless.

In our own work we have shown that the Gulf Stream is indeed a pulsating system, with a period of pulsation of less than a day. If we are to utilize our instruments and ideas to the greatest advantage, we must think that the oceans are responding continuously to various influences with long periods, intermediate periods and very short periods of action. Therefore, it seems a little too discouraging at this point to argue in terms of a complete description of the oceans according to any one single view. Rather, I think we must take account of the fact that some of the motions in the sea are only partially accounted for by the geostropic processes, that there are other things going on which are non-geostropic, and that each of the various tools that are becoming part of our ordinary equipment can be used selectively to examine each of these separate motions that must ultimately go into a total description of the physics of the ocean. I believe this is the picture we are trying to get.

Therefore, I submit that any program that tends to encourage preoccupation with average conditions and a description of the mean ocean, and to ignore, say, the secular variation, or exclude from consideration the short-term variables, is necessarily putting blinders on us at a time when, to progress, we must have the broadest point of view possible.

24. DEEP-SEA RESEARCH AS A COOPERATIVE ENTERPRISE

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We oceanographers commonly talk about the special difficulties of our science, the expensive and uncomfortable ships in which we must do our work, the nearly impenetrable curtain of the sea surface and the poor visibility beneath it that prevent us from seeing our oceanic realm, the problems of maintaining a network of observation points at sea or even of accurately locating a single station. Indeed these difficulties are formidable.

The Japanese and American oceanographers of the Pacific are proud of their last summer's achievement, the series of expeditions collectively called NORPAC, in which a more or less synoptic survey of the Pacific north of 20 degrees was made. This is an area larger than the continents of North and South America; such a snapshot of a large piece of the ocean has never been attempted before. We can regain our proper humility when we realize that our scientific cousins, the meteorologists, obtain an almost equally good picture of the entire northern hemisphere every six hours.

Our inadequacy compared to meterology is perhaps due less to incompetence than to the special difficulties of oceanography. But the chief difficulty of our science is shared with other branches of geophysics, and indeed with several other sciences, including anthropology and astronomy.

This difficulty is that oceanography deals with the real world. The classical sciences of mathematics, physics and chemistry, to a considerable extent make their own world in the laboratory; we, like the astronomers and anthropologists, must deal with the world as it is. There are thousands of possible oceans, but there is only one real ocean, and we oceanographers are stuck with it.

The real-world character of oceanography has several consequences. In the first place, the real ocean is so big, so complex and changeable, and our ignorance of it is so great, that we tend to place our principal emphasis on exploration and surveys, on the accumulation of masses of descriptive data, rather than on gaining understanding.

We tend to look at the ocean rather than ask questions of it. Because of our ignorance, descriptive exploration is necessary, but I believe we ought to use it primarily as a means of formulating problems -- as an aid in thinking of questions to ask.

In the second place, the size and intractability of the ocean make it hard to answer such questions as we can ask because it is difficult to carry out controlled experiments, those peerless tools of the classical sciences.

Our principal problems in oceanography are to overcome these two charac-

teristics of a real-world science; we must learn to ask questions and we must find substitutes for the method of controlled experiment. In both these problems, scientists and engineers in other fields can help us.

First, they can help us to ask questions. The engineers can give us several very practical reasons for attempting to learn more about the deep waters and thus can be used in formulating our questions. One practical reason is that the deep currents may be of importance in long-range climatic forecasting; that is, the rate of transport of deep water between high and low latitudes and the rate of exchange with surface water in high latitude may determine the possibilities of storage or loss of heat on the earth over decades or centuries. C. G. Rossby has pointed out in a personal communication that a very small change in the temperature of the deep water would correspond to a considerable departure from a radiation balance between the heat received from the sun and that radiated back to space by the earth.

Second, because the fertility of the ocean depends at least in part upon the nutrient exchange between the deeps and the surface, the amounts of fish and other foods we can harvest from the ocean are limited by the rate at which the ocean overturns and thereby fertilizes itself. In order to get a better estimate of the potential food supply from the sea, and to learn what we can do about it, we have to obtain some quite specific answers about the deep currents. Third, the development of peaceful uses of atomic energy will result in great quantities of radioactive substances, and somehow these must be safely disposed of. One possibility is to dump them in the deep sea. At present we cannot say whether this would be safe or not, because we do not know the rate of diffusion in the deep waters, or how fast these waters move from low latitudes to the Arctic and Antarctic, where the radioactive substances contained in them might enter the surface layers that are the zone of life.

Finally, the complex of military problems involved in navigation, detection, and defense against deep submarines will necessarily require detailed and accurate estimates of the velocities and variability of chemical and physical properties in the deep water.

Scientists from other fields can furnish us intellectual and scientific reasons for studying the physics of the deep sea. The biologists, for example, find different but closely related species of zooplankton organisms in different water masses. These water masses are not entirely separate entities; we define them by their physical and chemical properties but the same water particles are involved at one time or another in all of them. As the waters move around the ocean carrying their contained plankton organisms, the chemical and physical properties change. In a sense, new water masses are continually formed from the same water particles as they complete their cycle around the ocean. The biological properties of an animal species change at a vastly slower rate than the physical properties of the water in which it lives, yet the boundaries between related species of plankton organisms at the edge of water masses are often quite sharp, of the order of 20 miles. How do these organisms manage to live and reproduce in a particular water mass, in spite of currents and mixing? How did the different species of plankton organisms in different water masses evolve and how do they maintain themselves? These are questions for the physical oceanographer, as

well as for the biologist. As Professor Isaacs pointed out yesterday, the physical oceanographer's model of the ocean is inadequate unless he takes the plankton into account.

Recent studies of the animals of the deep sea indicate that for the most part the abyssal and hadal fauna is not geologically old (Brunn, 1956). It arose during the later Tertiary and the Quaternary, less than a few tens of millions of years ago. What profound changes in the deep circulation could have changed the fauna of this vast area?

Other sciences can also help us in several ways to achieve substitutes for controlled experiments. For example, new kinds of measurements can often be used in oceanography to test hypotheses in the same way that controlled experiments are used in the classical sciences. The underlying ideas and instruments for new techniques of measurement stem largely from the basic sciences and from broad advances in technology. It is worth remembering that throughout its history, oceanography has progressed through new equipment and techniques largely developed in other branches of science or engineering. This was true in the decades after the CHALLENGER Expedition, when long wire ropes first became available, and it is true today when the techniques of mass spectometry and electronics are yielding such spectacular results.

Another kind of substitute for controlled experiment in a "real world" science depends on studying the same or similar processes under a variety of conditions. Scientists from other fields can help us here by extending our time scale, by enabling us to find out something about the past when one or more conditions in the ocean were different to what they are today. An outstanding example is the work of Emiliani and his associates (Emiliani and Edwards, 1953), who showed by studies of the Oxygen 16/18 ratio in benthonic foraminifera that the temperature of the deep water during the Tertiary was not 1 or 2 degrees, but more than 10 degrees.

Another example comes from the geological study of sediment cores taken in the Atlantic and Pacific. Arrhenius (1952) showed that in cores collected in the Eastern Tropical Pacific, both the number of diatoms and the amounts of calcareous skeletons of foraminifera varied quite markedly with depth below the surface of the core. Higher calcium carbonate and diatom contents occurred in those segments of the cores laid down during glacial periods, indicating a higher organic content, higher standing crop, and higher productivity than during interglacial or the present, possibly postglacial, times. On the other hand, in the Atlantic, Schott (1942) found more calcium carbonate in the surface layers of deep-sea sediments than in the layers laid down during the last glacial period. This strongly suggests that the deep and bottom water circulation during the ice age was more intense than at present, allowing more calcium carbonate to be dissolved by the cold, carbon dioxide-rich waters of the great depths.

Finally, results from other sciences can be used by the physical oceanographer to build scientific fences or boundaries within which his hypotheses about the deep waters must be confined. I shall cite four examples of such fences in which I have had a personal interest, but many others could be given.

The Finnish chemist, Kurt Buch, found that the carbon dioxide content of the air over the sea north of Spitzbergen during the summer months was .015 percent by volume, less than half the usual amount in the atmosphere of temperate latitudes. This means that at the time he made his measurements, carbon dioxide was proceeding from the air into the sea. It would be tempting to assume that this observation supports the classical picture of the cycle of carbon dioxide between the ocean and the air. In this picture, carbon dioxide is supposed to pass from the warm waters of low latitudes to the atmosphere, by which it is transported north and south to high latitudes, where it passes back again into the cold Arctic and Antarctic waters. But we know from elementary considerations that this cannot be true at all times, because the deep waters returning to the surface in high latitudes must have an excess of carbon dioxide resulting from the oxidation of organic matter; this excess must escape to the atmosphere. Two explanations of Buch's observation are possible: either overturning of deep and surface water in high latitudes does not occur continuously but in pulses, or else the high rate of photosynthesis in the surface waters during the summer months depletes the carbon dixoide in those waters and in the overlying air. The latter explanation is supported by Deacon's observation that the carbon dioxide content near the surface in the Antarctic is relatively high in winter but low in summer. It seems likely, however, that both explanations may be valid and that further observations could distinguish between the two effects.

The second example comes from recent work in geodesy and glaciology. During the past century, mean sea level has risen by about 10 centimeters (Gutenberg, 1941). From recent glaciological work, this rise can be accounted for almost quantitatively by the melting of ice caps and Alpine glaciers. Thus, except for the waters added by the melting, the volume of water in the ocean has not changed appreciably for 100 years. An increase in the average temperature of the ocean by 1°C would raise sea level by 60 centimeters (Munk and Revelle, 1952). We may conclude that the temperature of the deep water has probably not changed by more than a few hundredths of a degree during the century, though, of course, it is possible that a fortuitous increase in the volume of the ocean has almost exactly compensated for any change in temperature.

As a third example, consider the problem of the average rate of exchange of water between the deeps and the surface. An upper limit to the time constant of this exchange can be obtained from recent measurements of heat flow through the sea floor. The heat coming from beneath the ocean bed would continually warm the deep and bottom waters if it were not carried away by circulation and mixing. The waters, cooled to near the freezing point at the surface in high latitudes, which sink and form the deep and bottom waters, must nevertheless be warmed somewhat as they travel throughout the deep ocean basins on their slow round-trip voyage from the Antarctic and Arctic. The fact that the temperature of the deep water is fairly, though not completely, constant throughout the ocean basins indicates that the time required for the round trip is relatively short. If we assume steady-state conditions and consider only the north-south motion, neglecting zonal transport and zonal mixing, we may write (Revelle and Maxwell, 1957):

$$\bar{v} \geq \frac{C_y + C_x + \frac{\Sigma Q}{d}}{\left\langle \frac{d\bar{T}_w}{dy} \right\rangle - \frac{2}{h} \left\langle T_{wd} - \bar{T}_w \right\rangle}$$

where $\bar{v}$ equals the average horizontal velocity in the meridional (north-south direction); C_y and C_x are respectively the heating due to horizontal and vertical mixing; ΣQ is the heat added per unit area from the interior of the earth; h is the north-south distance traveled by the bottom waters; d is the thickness of the bottom water mass. T_{wd} is the temperature of the water at the height d above the bottom; $\bar{T}_w$ is the average temperature of the vertical column of water from the bottom to the height d, and y is the north-south coordinate. The symbol $\langle\ \rangle$ indicates average values over the distance h.

Measurements of heat flow indicate that ΣQ is about 1.3×10^{-6} cal cm^{-2} sec^{-1}. From oceanographic measurements, we know that $T_{wd} - \bar{T}_w$ is about 0.1°C, the average horizontal temperature gradient in the bottom 1000 meters is of the order of 10^{-9}° C cm^{-1} and h is about 10^9 cm. To estimate the order of magnitude of C_y and C_x following Sverdrup, Johnson and Fleming (1942) we may use the concept of "eddy" conductivity:

$$C_y = A_y\left(\frac{d^2 T_w}{dy^2}\right); \quad C_x = \frac{A_x}{d}\left(\frac{dT_w}{dx}\right)d$$

where A_y and A_x are respectively the horizontal and vertical coefficients of "eddy" conductivity. A_y is probably less than 10^7 and A_x is probably not much more than 1. The vertical temperature gradient 1000 meters above the bottom is of the order of 10^{-6}°C per centimeter and $\frac{d^2 T_w}{dy^2}$ is about 10^{-18}. Hence C_y and C_x are both approximately 10^{-11} and $\bar{v}$ more than 3×10^{-2} cm sec^{-1}. The bottom water must travel approximately 7500 km from the Antarctic to the equator; with the computed value of $\bar{v}$ this journey will take less than 1000 years.

A somewhat tighter fence around the "age" of the deep water comes from recent results in geochemistry.

From a comparison of measured C^{14}/C^{12} and C^{13}/C^{12} ratios in wood grown in the 19th century, and in bicarbonates, shells and organic matter from the mixed layer of the sea, Craig (1954) concluded that the radiocarbon activity of these various materials is about five percent lower, when corrections are made for isotopic fractionation effects, than that in the 19th century wood corrected for radiocarbon decay. This difference could be attributed to three effects: (a) slow mixing of carbon dioxide across the air sea interface, resulting in a "holding" of carbon 14 in the atmosphere; (b) reduction of the former C^{12}/C^{14} ratio in the surface layer by dilution with carbon 14-free carbon dioxide from fossil fuel combustion during the past century (Suess, 1953); (c) mixing across the thermocline which might reduce the carbon 14 in the mixed layer carbon dioxide by dilution with carbon 14-poor carbon dioxide from the deeper water (this would be signifi-

cant only if the deep water were quite "old" compared to that in the mixed layer.)

Revelle and Suess (in press) and Craig (in press) have shown that the residence time of carbon dioxide in the atmosphere is about 7 ± 3 years. Craig concludes that even with the smallest possible atmospheric residence time not more than about half the low radiocarbon content in the mixed layer can be attributed to dilution with carbon 14-free carbon dioxide from fossil fuel combustion. Since the total amount of this carbon dioxide produced during the past century is about 10 percent of the CO_2 in the mixed layer at most, with an atmospheric residence of seven years, this must have been transferred to the sea. Craig concludes that the residence time for carbon dioxide in the mixed layer before it is exchanged with that of the deeper waters is most probably not more than ten years, giving a most probable residence time or "age" for the deep water of not more than 500 years. From somewhat uncertain considerations of the amount of tritium (the radioactive isotope of hydrogen) in the surface layer we find a deep sea residence time of about 300 years.

None of the above examples, of course, required a detailed description of the oceans. They are simply fences, to which all oceanographic observations must conform. But if we can build enough of these fences, we can take such measurements as Swallow's and interpret them in terms of what they mean for the transport, as well as for the detailed motions of the sea water.

DISCUSSION:

MR. MAXWELL: I would like to make a comment on Dr. Revelle's paper. With respect to the fences he mentioned that might be drawn in various portions of the ocean, fences from other disciplines, I think a good example of this has been shown in some of the earlier papers in this meeting, namely, from some of Dr. Ewing's and Dr. Emiliani's work.

I think that it also should be pointed out that we can't always place too much reliability on some of these fences because there are various ways of interpreting the measurements. I think certainly there was some question on the temperatures of about a thousand years ago that Dr. Emiliani expressed. In explaining this discrepancy he said it may be a mixing of melt-water. If this were true, in addition, it would probably have some bearing on Dr. Ewing's results.

So here is an example where just in the interpretation of some of these measurements you might want to mend all your fences. I don't think you can get around some of these questions unless you make the direct observations.

DR. REVELLE: How do you make an observation a thousand years ago? You have to make it by some indirect method.

MR. WORTHINGTON: In connection with your opening remarks about measuring temperature to one hundredth of a degree, and salinity to the tenth of a part per thousand, I wonder if I might be permitted to discuss a figure which shows some of the results of the new machine we have at Woods Hole.

Here, for example, for the first time we were really able to trace the dif-

ference between the deep slope water and the water in the Bermuda area, and in addition, we were able to show that there was a very small scatter. Whether this scatter is in the instruments or the ocean, it is impossible to say. The envelope of error for titration is plus or minus .02 parts per thousand which is a fairly optimistic estimate. Such mavericks as the point to the right of the envelope are invariably on the saline side; probably the result of the faulty sampling techniques.

We have found no water below the three degree isotherm that departs from this curve, by more than plus or minus .01 salinity, that is, on two cruises separated by six months where this instrument has been used.

I think it is an invaluable instrument in studying the very deep water because we definitely find it is by no means homogeneous. We do have now a method for tracing water masses which was not available under the old system.

DR. LYMAN: I confess myself baffled by Professor Revelle's remarks concerning water titrations. A better method was worked out in Dr. Revelle's own office.

DR. REVELLE: I didn't mean this wasn't possible to do. I mean in general you don't trust them better than a tenth. These of Worthington's are obviously better, but there are a lot of others about which not much is known.

DR. LYMAN: If you want to trust them, use Worthington's method.

DR. CARRITT: I want to say in the Chesapeake Bay Institute we have, during the past year or so, been working with a rather new technique for measuring salinity, involving an induction measurement with no electrodes. Recently we have completed a rather long series of calibrations with this device. I have to admit the calibration was made in the laboratory. The bench wasn't moving, and all the contrasts that you get on the ship were not present. However, the standard deviation of a large number of observations made over a salinity range of 0.5 to 18 is something on the order of plus or minus .01. This was actually the design placed on the instrument on the linearity of the electric components.

I am not sure whether or not this device is generally useful in open ocean research, because the present model is rather cumbersome and has cable connections. However, it does do away with one trouble since it has no electrode that can be fouled.

FROM THE FLOOR: I must admit I agree with what John Isaacs said a few minutes ago. It seems to me there is a missing link in the data storage question; how it should be stored and later used. I think one of the primary objectives of all science is to achieve a state of prediction. When we reach the point where we know everything about the universe, we will be able to predict all of its properties. Until we can get to that point, I think we find ourselves generally divided between two general points of view.

In the first we have a model or a structure about which we can ask specific questions. The problem here is to ask questions that are important. When this

model and structure exists and we can ask specific questions about it, we can go out and collect data, store it, process it, or do anything we want with it in a rational way.

On the other hand there are what appear to be anomalous deviations around our observations about which we can make no statements and can only ask very vague questions. I think because of this there is no ideal storage and sorting machine or technique for processing data.

MR. ISAACS: It seems quite possible that data storage in some ways might enhance or make more conspicuous this sort of anomalous results. When I made the point a while ago, I wasn't taking violent exception to handling well-known types of data in some fairly well designed and thought out way, but I want to emphasize that we do not want to lose our flexibility. If this is done right, we might emphasize the unusual better than is possible to do by going through a great many salinity cards.

DR. REVELLE: Mr. Chairman, I think we should abandon the term "data." It seems to me to be misleading. It gives a false sense of accomplishment to say we have a whole room full of "data." Perhaps to make measurements would be more significant. This is what the physicists and chemists do. They try to do something meaningful, not just go out and collect some more data.

The characteristic thing about geophysics, because of our inability to make controlled experiments, is that we make a lot of measurements, most of which are thrown away. There are only a few measurements where the situation is clear-cut enough to permit one to answer some question. For this reason, of course, it is good to make a lot of measurements, more than the physicists make in the laboratory.

The thing I object to about the use of the term data is that it becomes an end in itself, or they become an end in themselves. We just have these things on file. Whereas, as Dr. Montgomery suggested, we should put them all together in three kinds of sections. I don't think it makes any difference how you present them unless you can reach some conclusions.

MR. ISAACS: I am always impressed by what the astronomer did, and what he did was to start counting stars. He didn't assemble data on a non-realistic plan. As long as he put stars in constellations that looked like dragons, he didn't get anywhere. But as soon as he asked some questions like how many stars were there and what type were they he laid the foundations of science. For long years he counted stars and typed them, and then somebody looked at the results. This mass of data plus a close look at small areas was a very valuable thing.

DR. REVELLE: I agree. I am taking quite an extreme view just to put across a point here. Certainly most astronomy is very dull, but the accomplishments based on this dull work are tremendous. They are thrilling.

MR. ISAACS: What we really need are the analogs or homologs: the Hale telescope and the Schmidt telescope. The Hale telescope has great limitations on what space it can cover. The Schmidt takes a broad look and allows you to cover

many things at once. I talked to Carl Eckart the other day and he takes a more extreme position than I do.

Kepler made 300 laws out of 300 empirical relationships. Out of these Newton picked just three to build the structure of classical physics. The point is not that Kepler got 300 laws, but the right three were included even though he didn't know which they would be. It was essential to get those three, however, or we still wouldn't have classical physics.

MR. FUGLISTER: You mentioned the important thing was to reach a conclusion, not just present the data.

DR. REVELLE: I said the important thing is to try to answer questions -- or at least that is what I should have said.

MR. FUGLISTER: What I feel is that very often in presenting data, people have reached conclusions and, in their own minds, answered some questions in their presentation of the data.

If somebody wants to tell about the temperatures, or something else, in a place like the Pacific Ocean, he has some numbers, certainly, but he has decided on answers to some questions before he presents these. He has to answer some questions just in order to present them, I think.

DR. LYMAN: I would like to call on Mr. Wayne Magnitzky for a brief description of an investigation conducted by the Hydrographic Office.

MR. MAGNITZKY: Although the Symposium has particularly stressed the usefulness of anchored deep-sea buoys for obtaining oceanographic data, it must be remembered that data of this type has been and can still appropriately be taken by oceanographic vessels anchored for a considerable length of time. In fact, in the early stages of such studies, oceanographic anchored vessels are able to provide much more comprehensive data coverage than an unmanned buoy.

As an example of a recent method of obtaining such data, it is worth remembering that the Hydrographic Office oceanographic vessels, USS REHOBETH and USS SAN PABLO continuously anchored for a total of 27 days in 850 fathoms of water some 135 miles northeast of Bermuda in June-July, 1953. In the middle of this period there was a five-day overlap with both ships on site supplemented by the BLUE DOLPHIN of the Woods Hole Oceanographic Institution, so that directional control could be provided to any wave measurements.

25. SUMMARY

G. E. R. Deacon
National Institute of Oceanography
Wormley, Surrey, England

I think this making of a summary an almost impossible task, but since I am so very grateful for being invited here, I really can do no less than do my best.

Before I begin, I must echo the general appreciation of the value of the meeting. It has been very useful and very stimulating. Perhaps one of the most outstanding things in marine science today is the conflict between the people who have too many data and draw too few conclusions, and those who draw too many conclusions from too few data. I think we have heard representatives of both schools.

I think the first few papers rather emphasized the inadequacy of the physical models on which we work, but that was put right to some extent by the paper by Mr. Stommel, who agreed that our physical models were inadequate, but showed theoretical workers were doing their best. If they could not do better it was because oceanographers are rather slow in making the right kind of observations. I think there is no doubt, if you look at all the oceanographic data, that there is a mass of useful material which is being dealt with in very general terms, and yet there are a few clear-thinking physicists who really can't find just what they need among all this. Such delays are not necessary, because the very experiments that they want to make, and the sort of measurements they need, are the sort of things we ought to be able to do.

Until twenty years ago, oceanography was an easy science. Anybody could do it. We just needed a lot of enthusiasm and moderate facilities and we could produce a good general description of an oceanic region. But then, having found something one year very different from what it was another year, we had to try to explain it. We knew something about convection but tended to use the domestic hot-water apparatus as a model and if this did not give quite the right answer one could always blame a submarine ridge or some other rather doubtful phenomenon in an unknown part of the ocean. But all the time there were more significant things, like the drag of the wind. Because we didn't know much about them we felt quite entitled to ignore them altogether.

The time has come when ocean physics is growing alongside ocean geography. The physical models may still be inadequate, but the most promising ideas and results seem to come from the fairly precise treatment of elements of the basic picture as we already know it.

Physics is the science of motion and we must know whether the wind drag depends on the square or cube of the wind velocity. When the energy gets into the sea, we must know how much goes into waves and drift currents and how much into the deep-sea circulation. These seem to be the most important problems today, and the papers read during the past two days show that the theoretical and

practical approaches to them are at last strong and ingenious enough to promise satisfactory progress to their solution.

I have been a bit disparaging about descriptive oceanography, but the meeting has proved that we still have need of it. There are areas like the Indian Ocean where we haven't much idea of the qualitative distribution of the water masses.

It is interesting to compare our progress with that of meterologists. For many years they have insisted on more and more observations, but now they seem to be placing more emphasis on exact knowledge of the physical processes. I think we might learn from their experience and take the same line rather earlier in our development.

One of the most encouraging aspects of the symposium has been the evidence that experts in other sciences are becoming more interested in applying their knowledge to the sea. I was relieved to see the estimate of 2000 years for the age of the bottom water in the northwest Atlantic basin come down to something less than 400, and hope that it may be reduced still further. It is very useful to have these new techniques applied to the problems of water movement, but there is really no excuse for not making direct measurements, as of course the papers have shown we are trying to do. Quite a few papers yesterday, and today, were concerned with new techniques which look very promising. We have, however, been reminded that following a float for a few days will not solve all our problems: we shall have to track some water movements for months or years.

One of the highlights of the symposium has been the description of the bathyscaphe. Many of us may have been skeptical about the bathyscaphe a few years ago, but sooner or later one gets converted to new ideas. Two or three years ago not many of us believed in turbidity currents, but I believe most people are convinced by now. Today there are very few of us who aren't convinced that the bathyscaphe can be made a very useful tool for scientific measurement as well as exploration.

DISCUSSION:

DR. LYMAN: At this time I will turn the meeting back to Dr. Iselin.

DR. ISELIN: I think the people who planned this meeting had perhaps more or less the idea that oceanographers came out of a mold, that we were all interested in about the same thing, all doing about the same kind of work. If they have made that mistake, I believe we have corrected this error in that it is perfectly clear there aren't even two of us in this room that are interested in just the same thing, that we are going at the job in different ways.

This is healthy, I think. I believe this is the way research should be done. If we were all poured from a mold, we would only attack a problem in one way.

Dr. von Arx has agreed to do a little editorial work on our papers and our discussion, and I think that probably with reflection it will be possible to come to some general conclusions as to where we stand and where we are likely to go next.

The main interest on the part of the members of the Undersea Warfare Committee is to assure themselves that we are making as good progress as can be expected, and that we aren't too seriously limited by the facilities available to us.

Before leaving, we have a resolution that we should consider.

MR. BASCOM: I am the Recorder of that committee, Mr. Chairman, and will read the resolution agreed upon.

We considered the resolution, and proposed the following: (reported here in preliminary form, Ed.)

"The careful design and repeated testing of the bathyscaphe has clearly demonstrated the technical feasibility of operating manned vehicles safely at great depths in the ocean.

"The scientific implications of this capability have enormous potential.

"We, as individuals interested in the scientific exploration of the deep sea, wish to go on record as favoring the immediate initiation of a national program aimed at obtaining for the United States undersea vehicles capable of transporting men and their instruments to the great depths of the ocean."

E. H. Smith
A. C. Vine
J. D. Isaacs
R. B. Montgomery
R. H. Fleming
W. N. Bascom

We propose that the group be asked to vote on this, and we hope that the resolution will be accepted unanimously. If not, we propose that those against it stand and identify themselves. If no one does so, we would take it that the symposium as a whole had concurred.

DR. VON WALD: I have a question. I am concerned by the words "enormous potential" (in the original version, Ed.). That is quite a big statement. What is meant by enormous in this particular application?

MR. BASCOM: The last several days have been spent discussing the possibilities still open to us in oceanography. One promising way to determine which way to go would be to go down. That is what we mean by enormous, because it is a very big place to look around.

DR. VON WALD: You mean geographically?

DR. ISELIN: Can anyone think of a better word than "enormous?" I think the point was well taken.

DR. REVELLE: Tremendous.

DR. ISELIN: I think "very great." Let's put it that way. Enormous just seems a little far-fetched, and immediately raises the question that was asked.

FROM THE FLOOR: Far-reaching.

DR. ISELIN: I believe that would be a good one. Is it agreed that we change this to far-reaching?

FROM THE FLOOR: Is it intended that this be a number of vehicles? Was the word vehicles plural?

DR. ISELIN: They have it plural here.

MR. VINE: It was meant to be a plural, because it was meant to indicate a trend. That is, there might be some question as to whether even one should be made, but I am sure not everybody would agree the kind to build. If we are to be successful at all, we should have more than one.

MR. FUGLISTER: If we vote for this, does this commit us in any way to go down in it?

DR. ISELIN: I take it the chain of action in this case is that the symposium submits this recommendation to the Committee on Undersea Warfare or the National Research Council, and that they will consider how it is to be transmitted to the Navy officially.

MR. BASCOM: May I add it was our feeling that you, as Chairman of this symposium, would see to it that it at least reached Dr. Bronk, the President of the Academy, and the Chief of Naval Research.

DR. HERSEY: Would it be fair to regard this resolution as indicating one direction, at least, in which our facilities were inadequate?

DR. ISELIN: That, I think, is the point.

DR. DEACON: The paper Dr. Dietz read us this morning was sufficient to remove any doubt.

DR. ISELIN: Well, if there is no further discussion, I will ask everyone in favor to say "Aye."

.....There was general response .....

DR. ISELIN: Contrary?

.....There was no response .....

DR. ISELIN: The motion is carried.

I thank you all for coming, and we will adjourn.

REPORT OF THE RESOLUTIONS COMMITTEE

The resolution agreed upon unanimously by the symposium and subsequently transmitted to the National Academy of Sciences and the Office of Naval Research follows:

> "The careful design and repeated testing of the bathyscaphe has clearly demonstrated the technical feasibility of operating manned vehicles safely at great depths in the ocean.
>
> "The scientific implications of this capability are far-reaching.
>
> "We, as individuals interested in the scientific exploration of the deep sea, wish to go on record as favoring the immediate initiation of a national program, aimed at obtaining for the United States undersea vehicles capable of transporting men and their instruments to the great depths of the oceans."

EDITOR'S SUMMARY OF SYMPOSIUM ON ASPECTS OF DEEP SEA RESEARCH

Washington, D. C.
29 February - 1 March 1956

One of the most important properties of the deep sea is its role in the general circulation of the oceans. To date this has only been inferred. Direct observations of the currents at great depth have been made at isolated points but these are so few in number that no general pattern can be discerned. Those attending this symposium felt that special efforts should be made to increase knowledge of this problem area, first, in regard to circulation and then with particular emphasis on the distribution of properties and the physical processes that control such distributions.

In opening up this region to scientific study, several important factors are already obvious:

a. Suitable vehicles are required for the exploration and measurement of ocean properties and processes in general. Distances at sea are large and, particularly in high latitudes, storm seasons restrict work to more favorable areas. Designs for air, surface, and subsurface vehicles meeting contemporary requirements for research and year-round study of all parts of the oceans are a basic requirement to progress.

b. Vehicles for air require great endurance, generally slow speed and large load-carrying capacity. Surface vehicles should have extraordinary sea-keeping qualities, be capable of running at high speed for short periods and at moderate speed for very long periods, and be capable of effective silent operation. Subsurface exploration to a depth of 6000 meters appears possible with the bathyscaphe which the members of this symposium regard as an important tool to the scientific exploration of the great depths of the oceans.

c. The value of data obtained at sea is largely determined by the accuracy with which it can be placed in time and space. A world-wide precision navigation system is urgently needed as are means for precise navigation in deep water relative to an arbitrary reference grid.

d. Synoptic study of ocean areas requires simultaneous observations at many points. It is to be hoped that some organization may undertake a continuing program, in cooperation with oceanographers, that would lead to the production of unmanned buoys to aid in the collection of data from all depths in a regular network over the oceans.

e. More time and thought needs to be given to the analysis and interpretation of existing data as a means toward increased understanding of ocean processes. Much of the scientific and practical value of oceanography hinges upon our ability to predict conditions in space and

time. At present our abilities are extremely limited in this direction and it is only from better understanding of the relationships between oceanographic events that we can ever hope to devise accurate prediction techniques.

f. Until recently it has seemed impossible to apply the experimental method at sea. Recently tools have become available, namely, radioactive tracers, available in such large quantities that through either deliberate introduction to the ocean, or as waste products from nuclear power plants or experiments, water parcels can be traced. Proper use of these materials is not self-evident but investigations by such means may well constitute an important field of research in the years to come.

The field of deep-sea research must be entered and rapidly broadened to meet contemporary needs and to complete man's exploration of the hydrosphere. This field requires the utmost ingenuity and resourcefulness in devising methods of observation and measurement, and offers an intellectual challenge in bringing together the implications and applicable findings of many fields of knowledge to make a sound beginning. The circulation of the deep ocean seems to be the problem of most fundamental interest.

W. S. von Arx,
Woods Hole Oceanographic Institution
Editor

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