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DISCUSSIONS ON THE RELATIONSHIPS BETWEEN METEOROLOGY AND OCEANOGRAPHY

By

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G. E. R. DEACON:

There must be some of you who, like me, did most of their seagoing 20 to 30 years ago and find it difficult to keep pace with the present urgency of introducing more precise reasoning into oceanography. This afternoon's session dealing mainly with such precise arguments has not been easy to follow, and it is clear that there is much serious hard work ahead. Nevertheless the meeting seems to be ending on an optimistic and cheerful note. The signs of light relief remind me of the Promenade concerts held in the United Kingdom. When all of the serious work has been done, the last concert of the season deteriorates into a merry party in which the audience more or less takes charge and shows its enthusiasm by light-hearted co-operation with the orchestra; "Britannia rules the waves" is always the favourite song. I dare not hint at such a possibility either for sea waves or microseisms.

I should like to do what I can to emphasize the essential unity of the aims of oceanography with those of meteorology. In both there is growing emphasis on the study of general circulation and on the exchange of energy between the oceans and atmosphere, and there seems to be every reason why we should follow each other's work closely. This afternoon's papers have shown that when a meteorologist talks about circulation of air over the ocean he has figures for all the variables such as distribution of wind strength and angular
momentum, but, so far as I can judge, he does not have a very complete picture of the circulation of the air. He knows how fast everything goes but has no certain information of where the air comes from or where it is going. He still talks in terms of the pressure distribution which results from movements of air masses, but most of us, not carrying a specimen weather map in our heads, would rather be told in plain words that a current of cold air is approaching than be required to work it out for ourselves from what we are told about variations in pressure distribution.

The oceanographer has experienced the opposite approach. It is so easy for him to plot the horizontal and vertical distribution of temperature and salinity that he has a good idea of the origin and fate of different water masses. But, although he can show the complex interplay of subsurface currents in magnificent coloured diagrams, he has no idea how fast they go, and he is just as uncertain about the driving forces. For 100 years or more most oceanographers have attributed surface currents to the effect of wind, but, since it is not easy to see how the wind can energize deep-water movements, there has been a tendency to attribute deep currents to differences between the density of surface waters in different climatic areas. Twenty years ago I used to argue with Professor Sverdrup my conviction that such convective processes were all-important in deep layers of the ocean. I should be much more careful now, and he may have modified his ideas too. We have to recognize the possibility that the wind is the main driving force, even of deep currents; the fact that such currents appear at appropriate density levels in the ocean may indicate only that these are the paths of least resistance. Stommel has emphasized the weakness of our approach in his recent pamphlet "Why have our ideas on oceanic circulation such a dream-like character?" It is simply that we have not made sufficiently determined attempts to measure actual velocities of subsurface and deep currents.

Oceanography already owes much to meteorology, since most of our scientists who can deal with the theoretical complexities of the study of water movements on the rotating earth have been trained in schools of dynamic meteorology. We also seem to be learning from the measuring techniques used in meteorology. I have just taken part in attempts, which had a fair measure of success, to follow the equivalent of a radiosonde which sank slowly to the sea bed instead of ascending into the sky. W. V. R. Malkus has just made measurements by towing a current meter from a ship; this method seems more promising than that of making measurements from a ship anchored in deep water and is rather like using an aeroplane to find relative
wind speeds. Measurements are also being made by suspending large drogues on thin wires from small surface markers. The meteorologists are not likely to gain much from oceanographic methods and techniques, but some of their theoretical conclusions may be more easily tested in the relatively sluggish ocean, and they need to know the part played by the ocean in heat and energy exchange and in the transport of heat from one region to another. There seems to be every reason for close co-operation.

H. U. SVERDRUP:

When the chairman approached me, I replied that I might say a few words about the transport of heat from lower to higher latitudes by ocean currents.

For the earth as a whole it is well known that in low latitudes the annual incoming radiation from the sun exceeds the outgoing terrestrial radiation, whereas in high latitudes the loss of heat by terrestrial radiation exceeds the gain by solar radiation. The total amounts received and lost by areas between every tenth parallel of latitude in the northern hemisphere are shown by curves a and b in Fig. 1. In order to maintain a constant average temperature in the different latitudes, the excess energy received south of about 36° N must be transported into higher latitudes to compensate for the loss in these areas. The necessary transport of heat across parallels of latitude is shown by curve c in Fig. 1.

It is frequently assumed that this transport is accomplished by atmospheric circulation, but it is obvious that in the northern hemisphere the ocean currents must also play a part. Warm water is transported to the north in the western parts of the oceans and cold water flows south in the eastern parts, meaning that there is a net flow of heat to the north. From a knowledge of the water temperature and of the current direction and velocity at all depths it should be possible to compute this heat transport, but so far only a single estimate seems to have been made. According to this the transport of heat by ocean currents across 55° N in the Atlantic Ocean amounts to about 0.3 × 10⁻⁶ g cal min⁻¹, which is about 7% of the total transport across that parallel.¹ Although the transport by current in lower latitudes has not been examined by direct calculation, it is possible to obtain some idea of the effect of ocean currents by using another method of approach.

Figure 1. Curve a: Average annual amount of energy received by solar radiation between parallels of latitude that are 10° apart. Curve b: Average annual amount of energy lost by terrestrial radiation between parallels of latitude that are 10° apart. Curves a and b apply to the northern hemisphere (F. Baur and H. Philips, 1933, Beitr. Geophys., 45: 82). Curve c: Average annual transport of energy to the north across parallels of latitude in the northern hemisphere. Curve d: Average annual transport of energy to the north across parallels of latitude by the ocean currents of the northern hemisphere.

We know that in all latitudes the oceans receive in one year more energy by solar radiation than they lose by terrestrial radiation and that the surplus is used for evaporation or is given off to the atmosphere as sensible heat. If there were no currents, the surplus of energy received by radiation, $Q_r$, would have to be used locally for evaporation ($Q_e$) or be given off as heat ($Q_h$):

$$ Q_r = Q_e + Q_h $$

(1)

This simple equation must hold for the oceans as a whole, but, when dealing with a limited area, say an area between parallels of latitude, it must be borne in mind that heat may be transported in or out of that area by ocean currents. Calling this amount $Q_v$, we must have

$$ Q_r = Q_e^1 + Q_h^1 - Q_v $$

(2)
where $Q_e^1$ and $Q_h^1$ now represent the amounts of energy that are used within the area for evaporation or are given off to the atmosphere as heat. Therefore,

$$Q_v = (Q_e^1 + Q_h^1) - Q_r .$$

A negative value of $Q_v$ indicates that heat is carried out of the area by currents.

Values of $Q_e^1$ and $Q_h^1$ have been derived by Jacobs from meteorological observations, and values of $Q_r$ have been computed by W. Schmidt and H. Mosby. Jacobs' computed values of $(Q_e^1 + Q_h^1)$ have been adjusted so that his averages for the oceans of the northern hemisphere equal $(Q_e + Q_h)$ of (eq. 1). Even if the values of $Q$ need revision, this means that the difference $(Q_v)$ between revised values of $Q_r$ and correspondingly revised values of $(Q_e^1 + Q_h^1)$ need not be greatly influenced.

The numerical values have been published elsewhere, but here only the results are represented by curve $d$ in Fig. 1, which shows the transport of heat across the northern hemisphere. Note that a transport across the equator has been added because there warm surface water flows north along the coast of South America whereas cold deep water flows south.

Evidently the transport by ocean currents is not negligible, particularly to the south of $30^\circ$ N where it accounts for more than 30% of the total transport. The values apply to the combined effect of currents in the North Pacific and North Atlantic Oceans.

Corresponding values for the southern oceans are not available, but in the southern hemisphere a smaller effect of the ocean currents must be expected. This expectation is substantiated by observations of evaporation from pans aboard ships, which have been discussed by Wüst. In the northern hemisphere the observed values of $Q_e^1$ agree fairly well with values computed by Jacobs and deviate insignificantly from $Q_e$, but in the southern hemisphere no such systematic deviations occur, indicating that there the term $Q_v$ is small.

If these results are correct, it must be concluded that the atmosphere transports significantly less heat from lower to higher latitudes in the northern hemisphere than it transports in the southern hemisphere; therefore, the atmospheric circulation within the two hemispheres must show certain characteristic differences.

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HENRY STOMMEL:

I am asked to discuss the papers just delivered by Drs. Palmén and Charney and to consider in general the relation between oceanography and meteorology. I believe I can do this most effectively by a comparison concerning the present status of meteorological and oceanographical science.

It is not an exaggeration to state that efforts in oceanography for the past three-quarters of a century have revealed essentially a climatological mean distribution of temperature and salinity, and possibly a distribution of surface current velocity but certainly not of the velocities involved in the deep circulation of oceans.

A meteorologist might be appalled to realize that we still have no mean current charts other than those for the very surface. Due to technical difficulties, the distribution of currents in deep water has not been directly observable. We don’t even know how to make such observations, and therefore we must exercise our technical ingenuity to measure deep current velocities. Yesterday Dr. Deacon described to me briefly a sonic technique which his laboratory is now using for measurements of weak currents to a depth of 1000 fathoms. I hope he will take the opportunity of this discussion to describe the nature of this new technique.

The climatological mean distribution of temperature and salinity now seems to be fairly complete for the North Atlantic Ocean, especially in the surface layers. Fuglister’s recent article in Tellus is most up-to-date. And a chart of the climatological mean distribution for the North Pacific is just on the verge of being finished. With the resources that we have had at hand it has been no mean task to get even this far. An analogous task for meteorologists would be the development of the climatological mean distribution of temperature and humidity over the northern hemisphere by using half a dozen automobiles and kites to which air sounding instruments were attached and by doing all of their work on dark moonless nights when they couldn’t see what was happening in their medium.

It is inevitable now that the survey type of expedition is reaching the end of its usefulness to physical oceanography. What do oceanographers do when they have completed the climatological mean picture of oceans of the northern hemisphere! It is in this connection that it is instructive to attempt to draw from meteorological science some ideas about what we can hope to investigate and how we might go about it.

To begin with, we cannot propose an ocean-wide network of observation stations similar to the radiosonde stations available to meteorologists. We are not justified in doing so because hardly anybody
wants detailed forecasts of oceanographic conditions badly enough. Thus, it seems, oceanographers are immediately excluded from the types of analysis which are the very backbone of many aspects of meteorological science, such as the detailed aerological studies of the motion and development of large-scale disturbances that play so large a rôle in synoptic meteorology, the statistical studies of mean transport of properties across latitude circles, and so on.

Going back to our analogy again, let us assume that the meteorologists have completed their climatological mean distribution with their half dozen perambulating automobiles and kites; if they were then denied the use of a wide network of upper air stations, they would undoubtedly settle for as many permanent upper air stations as they could get. They would set up these stations in the best locations they could think of and would do everything possible to make observations of winds as well as of temperature and humidity distribution. From that time on they would probably use their automobiles primarily as a means of transportation to and from their few precious weather stations.

It seems to me that oceanographers should now give attention to the problem of setting up permanent oceanographic observatories, using moored buoys and midocean islands. The only available continuous observations for oceanographic purposes are (a) surface observations obtained by tide gauges and lightships along the shallow margins of the sea and (b) more recently a series of temperature measurements taken by bathythermograph to 900 feet at weather ships. Serial observations of the deep structure of the ocean have been made for a few days at a time at perhaps two dozen anchor stations. Anybody who has ever tried to analyze these anchor station data for anything such as internal tides will know how hopelessly inadequate these short series are. Would any meteorologists care to generalize on the diurnal surface pressure fluctuation by using absolutely nothing but two or three days’ barograms from Charleston?

Last night I asked Dr. Palmén if he would suggest an intelligent question that I might ask him at this time. He replied, that, inasmuch as he had specifically avoided discussing deep-sea data, he thought I could not ask an intelligent question, and I think he was right. However, we did discuss the problem of making precise measurements of wind stress in the ocean by trying to measure the mass transport of water to the right of the wind by means of a buoy network or otherwise. It was Dr. Palmén’s belief that there are too many complicating factors in the response of the ocean to a suddenly applied wind stress to permit valid measurements of wind stress in the deep ocean. In fact he believes that large shallow basins, such
as the Baltic Sea, where he has already obtained such definite results, are preferable. But these very complications need our attention. We have no direct knowledge about what a storm does to the velocity field at all depths. There are many complications involved in the response of a stratified ocean to the passage of a storm, many internal modes of motion; there is the possibility that the surface pressure field can induce water motions even at the bottom of the ocean. If it is hopeless for oceanographers to provide useful data on stress at the sea surface, then perhaps they might at least investigate the complicating factors.

Similarly, so far as the temperature field is concerned, successive bathythermograms at a fixed deep ocean station (weather ship data) show fluctuations in structure which are attributed to (a) advection, (b) diurnal heating, (c) wind mixing, (d) free internal waves, (e) forced changes in depth of density surfaces accompanying horizontal convergences due to atmospheric disturbances, (f) internal tides, (g) macroturbulence, etc. It would be a wonderful thing if a network of telemetering buoys could be used some day in an attempt to sort out some of these wonders of the deep.

So far as the development of large-scale meanders and eddies in ocean currents is concerned we do have some synoptic charts of such a development. In fact we have two successive charts. Would any meteorologists care to make generalizations about the development of cyclones and anticyclones over the United States from two successive surface temperature charts, and nothing else?

I do hope you will not think I am trying to paint a black picture. What I am trying to do is to convince meteorologists that we oceanographers have nothing that compares to a world-wide network of radiosonde stations and that what we can hope to achieve in the immediate future in our understanding of the ocean will be determined to some extent by what one can hope to learn from a very limited number of permanent observation points. And I am trying to convince oceanographers that their need for permanent observation points has passed the stage of idle discussion and has become an essential need.

Now, it is important for us to realize that all of the large-scale problems that meteorologists are working on do not depend upon their worldwide network of radiosonde stations. For example, Bunker’s studies of the turbulent exchange of heat and water vapor which occurs as an air mass moves out over the first few hundred miles of sea depends primarily upon soundings made from a single airplane. Studies by aircraft of tropical storms which often occur in remote oceanic areas outside the network are another example. Certainly investigations of the high atmosphere by means of rockets
are of a kind not dissimilar to the type of observational program which is available to oceanographers. We can certainly learn much by tracking clusters of drifting buoys (by wireless-direction-finding or by SOFAR) just as meteorologists are beginning to use constant level balloons to map air trajectories. In a recent issue of the *Journal of Meteorology* there was an account of tracking groups of balloons for thousands of miles and recovering them on the same patch of ground—direct evidence of the small amount of lateral mixing present. We need to make similar studies in ocean areas where we imagine large-scale lateral mixing occurs, just to see if it is really there after all. Were it possible to design cheap neutrally buoyant floats which could signal their position by means of a SOFAR network, then we could start immediately to study the deep water circulation of the sea.

In regard to Dr. Charney's interesting remarks on quasigeostrophic flow, it is worthwhile to repeat the unhappy fact that we have little observational information that can be of direct use in applying such ideas. We have no data for studying time sequences of events over intervals of many pendulum days. Rossby's early papers on the mutual adjustment of pressure and velocity fields in an ocean current, in some respects similar to Dr. Charney's example, were also developed as a sequence of processes which slowly vary with time and which follow an initially concentrated unbalanced current; but he applied them toward an explanation of the spatial sequence of downstream features of the Gulf Stream which appear after it has left the Florida Straits. According to our recent experience, however, there is little variation in the Gulf Stream velocity or density structure for many hundreds of miles after it passes Cape Hatteras, and its final dissolution is catastrophic or abrupt—a manner quite the opposite of a gradual, slowly varying process. I recognize the fact that Dr. Charney's ideas are a useful tool for large-scale planetary phenomena. I think it would be helpful to oceanographers in visualizing the meaning of such ideas oceanographically if data on some actual states of affairs could be obtained; that is, if some real ocean phenomena to which these ideas might be applicable could be made available. This might lead to types of observational programs which would supply the data necessary for their application.

Comparison of meander-waves in the Gulf Stream, as well as of waves in atmospheric currents, is hampered by lack of information on the structure of meander-waves. The basic current, the Gulf Stream itself, is a steady phenomenon—perhaps more so than most atmospheric currents, and for that reason it may actually lend itself more easily to dynamical explanation.
As an example of the possible dynamical simplicity of the Gulf Stream, I should like to exhibit the results of a vorticity analysis of Worthington's 1950 hydrographic traverse perpendicular to the Stream, at about 68° 20' W, across what appears to be a very straight portion of the Stream.

The potential vorticity of the water between any two isotherms is defined as

\[ \frac{f + \zeta}{h} \]

where \( f \) is the Coriolis parameter, \( \zeta \) the relative vorticity, and \( h \) the vertical distance separating the two isotherms. In the "free stream region" of the Gulf Stream, only the axial component of velocity \( v \) is important, and for this reason we are justified in replacing the relative vorticity by the term \( \partial \zeta / \partial x \). For convenience we direct the \( y \)-axis along the Stream and the \( x \)-axis toward the Sargasso Sea. Since there is so little evidence of any mixing or frictional processes in the region of the free stream, it is reasonable to suppose that the potential vorticity of the water between any two isotherms is uniform horizontally even though the thickness between the two isotherms changes as a function of position across the Stream. Moreover, in the Sargasso Sea itself we note that the relative vorticity nearly vanishes so that we are justified in expressing the conservation of potential vorticity between any two isothermal surfaces in the following form:

\[ \frac{f + \partial v / \partial x}{h} = \frac{f}{h_0} \]

where \( h_0 \) is the vertical distance between isothermal surfaces in the resting water of the Sargasso Sea. Thus the axial velocity of the water between any two isothermal surfaces should be determined by the following relation:

\[ v = \int_{-\infty}^{z} f \left( \frac{h}{h_0} - 1 \right) dx \]

If velocity determined in this way is similar to the geostrophic velocity as determined from dynamic computations, then we may feel that the hypothesis concerning the conservation of potential vorticity is supported by facts. Fig. 2 shows the results of such a comparison; the upper part shows the vertical thickness of the water layers between the 17° and 19° C isotherms, across the Gulf Stream; the lower part shows the comparison between the velocity as given by Eq. (3)
(solid line) and the geostrophic velocity. The two curves agree surprisingly well. Similar constructions for other pairs of isotherms down to the 8° isotherm give similar results. We take this as evidence that potential vorticity is actually conserved in the Gulf Stream.

The conservation of potential vorticity in the Stream is important because it determines its width and velocity. If we were to make the Stream wider or narrower in an arbitrary fashion, then the two velocity curves in Fig. 1 would not coincide.

The geostrophic law defines the total transport of the Stream, not its velocity profile. The first theoretical explanation of the width of the Stream was that given by Munk in his linearized theory of wind-driven ocean circulation. He found that he could control the width of the Stream by varying the value of the coefficient of lateral eddy viscosity. This is, of course, a rather unsatisfactory feature of the theory because almost nothing is known about the value of
this coefficient in the vicinity of the Gulf Stream. In fact, most recent evidence suggests that the coefficient must be much smaller than the value Munk used. By diminishing the coefficient, we would be led by Munk's theory to a Gulf Stream that is much narrower than that actually observed, were it not for the essentially nonlinear effect of conservation of potential vorticity which now appears to dominate the dynamics of the Stream and to give a unique velocity profile.

This analysis is possible only because of the close spacing of Worthington's traverse. One cannot help wondering whether meteorologists will discover the same thing about the atmospheric jet if they make a closely spaced cross-section of it sometime.

C. W. THORNTHWAITE:

At this stage of the proceedings, very little is expected of me. Accordingly, I will limit my remarks to four or five minutes. In that brief time I can only indicate by means of an example or two how climatology has been able to contribute to the solution of some important oceanographic problems.

Among other things, in climatology we are interested in the water exchange between the earth's surface and the atmosphere. We cannot tell whether a climate is moist or dry merely by measuring the rainfall. It is necessary to determine the relation between what falls as rain and what is lost through evaporation, or through evapotranspiration. If one determines the water needs of an area and relates those needs to the water supplied through rainfall, a bookkeeping system can be set up to determine whether the rainfall is greater or less than the need and whether there is in consequence a water surplus or a water deficiency.

These relations can be expressed graphically. In Fig. 3 the march of average precipitation and of potential evapotranspiration through the year are shown for Seabrook, New Jersey; Berkeley, California; Montgomery, Alabama; and Albuquerque, New Mexico. At Seabrook the potential evapotranspiration is negligibly small in winter, but in early spring it begins a rapid rise which reaches the high point of the year in July with more than 15 cm. It falls rapidly during the autumn months. The corresponding precipitation is far more uniformly distributed through the year, being close to 9 cm in nine of the twelve months. The rainiest months are July and August, when each receives slightly more than 11 cm; November, the driest month, has only 7 cm.
In this example rainfall and water need do not coincide. There is too much rain in winter and too little in summer. Thus, at the time of maximum rainfall in July and August, there is a water deficiency, whereas in November, when rainfall drops to the lowest value of the year, there is a water surplus. In early autumn, water need falls below precipitation. For a while the surplus rainfall replaces soil moisture that had been used up previously. From then on the surplus water raises ground-water levels and produces surface and subsurface runoff. In spring both transpiration and evaporation increase rapidly, so that water need soon surpasses precipitation. Thereafter, through early summer, the excess demands for water are satisfied from the soil moisture reserves. When these reserves are exhausted, evapotranspiration is limited to the current rainfall. This shows that if the vegetation is to have all the water it needs and if the crops are not to suffer from drought, additional water must be provided through irrigation.

From study of this graph we see that there are two important climatic elements that can be derived from comparison of precipitation with potential evapotranspiration. One is the water surplus, which occurs in winter in Seabrook and which amounts to about 40 cm, and the other is the water deficit, which occurs in summer and which amounts to about 10 cm. Through the course of the year there is a net water surplus amounting to 30 cm. Through this system of water bookkeeping it is possible also to determine the water that must be accounted for as soil moisture storage.

The three other illustrations in Fig. 3 show the relation between water supply and water need in other climates. Berkeley receives nearly all of its rainfall in winter and almost none in summer. Accordingly, the winter water surplus and the summer water deficit are both comparatively large; the surplus is 23 cm, the deficit 31 cm. Montgomery is similar to Seabrook. However, the water need is greater in every month and the average precipitation is less uniform through the year, ranging from 16 cm in March down to 6 cm in October. Montgomery, like Seabrook and Berkeley, has a water surplus in winter and a water deficit in summer. Albuquerque is an arid station where precipitation satisfies the water need only in the three winter months but produces no water surplus and where the water deficit otherwise is very large.

The method outlined above, whereby water need and water supply are compared, has a number of practical applications. Water surplus comprises accretion to ground water and surface runoff which is equivalent to stream flow. Thus the method enables one to use climatological records for the determination of values of stream flow
Figure 3. Water balance of selected stations in the United States.
Figure 3. Water balance of selected stations in the United States.
in areas where no stream gage records exist. The method also keeps account of soil moisture storage and ground water accretion. It is thus possible to estimate from climatological data the amount of water temporarily stored on the land in any time of year.

The total water temporarily in storage at the end of each month at the four stations summarized in Fig 3 is given in Table I.

<table>
<thead>
<tr>
<th>Station</th>
<th>March</th>
<th>April</th>
<th>May</th>
<th>June</th>
<th>July</th>
<th>August</th>
<th>September</th>
<th>October</th>
<th>November</th>
</tr>
</thead>
<tbody>
<tr>
<td>Seabrook</td>
<td>18.1</td>
<td>17.6</td>
<td>18.1</td>
<td>16.2</td>
<td>13.1</td>
<td>7.5</td>
<td>2.6</td>
<td>0.4</td>
<td>0.2</td>
</tr>
<tr>
<td>Berkeley</td>
<td>15.1</td>
<td>16.5</td>
<td>15.7</td>
<td>11.0</td>
<td>4.8</td>
<td>0.7</td>
<td>0.4</td>
<td>0.2</td>
<td>0.1</td>
</tr>
<tr>
<td>Montgomery</td>
<td>17.4</td>
<td>19.7</td>
<td>20.8</td>
<td>17.9</td>
<td>12.1</td>
<td>4.4</td>
<td>1.0</td>
<td>0.5</td>
<td>0.2</td>
</tr>
<tr>
<td>Albuquerque</td>
<td>1.5</td>
<td>1.4</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
</table>

In Seabrook, for example, at the end of March 18.1 cm of water are temporarily trapped in the soil, in the ground water reservoir, and in the streams. At the end of September the water in temporary storage has fallen to 0.2 cm; the range between maximum and minimum values is 17.9 cm. In Berkeley, the difference between the February maximum and the October minimum is 16.5 cm. In Montgomery, the range between March and October is 20.7 cm. In Albuquerque, on the other hand, the precipitation is never great enough in any month to produce a water surplus and consequently no water except a meager amount of soil moisture is held temporarily in storage on the land.

These examples introduce an important oceanographic problem that has been a special concern of Walter Munk for some time. By studying the records of tide gages from all over the world he has found that the oceans contain less water in March than in October. He seems to have lost $5.10^{19}$ grams of water. There are two possible places to look for this missing water: the atmosphere and the land. By extending the bookkeeping methods used for individual stations (as illustrated in Fig. 3) over the whole land area of the earth it becomes possible to determine how much water is in temporary storage on the land and in transit over it.

In the northern hemisphere the greatest supply of water is locked up in the land in spring while the minimum level occurs in autumn. This is confirmed by the graphs of Seabrook, Berkeley, and Montgomery. Although there are local exceptions, it is generally true that a maximum of water is locked up in the land in the form of snow, soil moisture, ground water and stream detention storage in March and that a minimum is present in October, just as Dr. Munk would like to have it.
But in the southern hemisphere the seasons are reversed and so are the months of maximum and minimum water storage in the land. However, the land area of the southern hemisphere is so much smaller than that of the northern that conditions in the northern hemisphere prevail. It would seem that the missing water has been found, but completion of the inventory, country by country, remains to be done to see if we get the answer that Dr. Munk requires.

Another problem is one that was presented by Dr. Alfred Redfield. In a letter which he wrote to me in August 1953, he said, "Oceanographers have recently developed an active interest in the circulation of estuaries, and in this connection data on the accessions of fresh water from river flow and seepage are essential. In many cases river flow is not adequately gauged and seepage may also become an important factor. For this reason, we would like to estimate fresh water accession from rainfall corrected for evapotranspiration."

Then he went on to give a case in point; a large coastal lake is linked to the ocean from which it receives salt water sometimes and to which it loses fresh water at other times. It is in a part of the world where no stream-gaging stations exist and where there is no possibility of getting them in time to aid in the solution of the problem.

By making use of data from the climatological stations in the catchment area tributary to this lake and by determining the potential evapotranspiration as well as the water surplus and other elements of the water cycle month by month, it has been possible to work out the water economy of the lake to show that in certain months it has an excess of water which flows into the ocean and in others a deficit which is made up by inflow from the ocean. This study is not yet complete; but the work done so far demonstrates that the methods of climatology are indeed of real assistance in the solution of oceanographic problems.