

GENERAL DISCUSSION

Revelle: This panel * has been conspiring at intervals in the past two days as to what it might do, and we have reached a unanimous agreement on one point, that is, that the panel try to do a minimum amount of talking and to have a maximum amount of talking from other people. We could spend about as much time trying to recapitulate our various ideas as we have spent in this entire conference.

I think we might try to cover the following general subjects this afternoon:

1. What might have happened? (The broad, general hypotheses)
2. What did happen? (The evidence)
3. What was the effect on organisms?
4. Does the ocean effect persistence in the atmosphere?
5. Do we have any possibilities of prediction?
6. What more research needs doing?

What did happen in 1957-1958? What happened in the atmosphere? What were the average conditions and what was the sequence of events? The same questions pertain to the ocean. What were the average conditions of 1957 and 1958 and what was the sequence of events, not only along the California Coast, but throughout the Pacific and perhaps in the Atlantic, if we can bring Atlantic information to bear. In trying to decide what happened in the oceans, we ought to attempt a brief evaluation of the evidence. We have evidence for California, for Japan, for the Gulf of Alaska, for some of the South American area, for the Central Pacific, and we may have some evidence for the Atlantic.

Of what kind of observations does this evidence consist? I have listed the following kinds of evidence: (1) temperature and salinity of the water; (2) various kinds of direct measurements of the water motion: drift bottles, drogues, and G.E.K., (3) evidence from tide gauges of changes of sea level, (4) properties of the water other than temperature and salinity, such as the oxygen content. Possibly in this category of direct measurement there falls also some biological information, and we can add a fifth sort of evidence, (5) the distribution of the phytoplankton and the non-swimming zooplankton. And perhaps in this discussion of what happened in the ocean, we ought to talk about (6) the area and time of fish spawning—how it was affected by temperature; (7) survival of fish larvae as they were perhaps affected by the greater on-shore movement of the water; (8) the distribution of the adult fishes, both sardines and related species, and the larger fishes. In addition we might discuss:

* A panel consisting of Revelle, Isaacs, and Munk, had met on the two preceding evenings to formulate models for presentation at the general discussion. (Eds.)

has there been any effect of the environmental conditions upon the availability of the fish apart from their actual distribution? Any effect on schooling behavior? On the depth at which they are normally present or other aspects of their behavior that make them easier or harder to catch? What has been the effect of the environment conditions on distribution and growth of phytoplankton and the distribution and numbers of zooplankton?

I think that this morning, we had a thoroughly adequate discussion of what we might call the local model, the possible relationships between the oceanographic conditions and the distribution of plankton and sardines and other fishes in the Southern California region.

Finally, we might talk a little bit about some more speculative subjects: (1) what is the relationship between the changing atmospheric conditions and the ocean? Is the ocean in a way a conservative mechanism that tends to prolong a marked change in atmospheric conditions? Or to cause these conditions to oscillate in a certain possibly definable fashion?

Another quite speculative subject, of course, is the possibility of prediction of the oceanic events. It may be possible to make oceanic predictions over several months because the oceanic changes are much less rapid than those in the atmosphere. The essence of prediction is to be able to recognize one event that is followed by others, to recognize that event as the beginning of a sequence of "results" of a particular "cause."

One way to look at this question of prediction is to consider this "abnormal year," (or is it more normal than those of the previous decade? I think you might say, in general, that it does appear to be an abnormal year—a very unusual or exceptional year). We are wasting our time thinking about these long-term trends, unless we can use this abnormal year as a kind of experimental year, wherein we can see how conditions were varied by nature, and, hence, conduct a sort of controlled experiment.

Finally, we might talk about the various kinds of future inquiry and observations that would help us to obtain a better understanding of this year in relation to other years. Walter Munk and Dick Fleming suggested that we might talk about what might be done from the physical point of view. First, is it possible or desirable to attempt to compute the motions in the ocean—current motions—from wind stress, leading perhaps to a more realistic way of computing the anomalies of the oceanic motions from the anomalies of wind stress? Second, what can we do about curl analysis? Third, are means available to make direct current measurements in time series in a meaningful way?

So I think that we might then start on events in the atmosphere and ocean, divided into the two categories: first, what might have happened, and, second, what did happen. Under what did happen, let us consider physical, chemical and biological evidence. The third question is: what was the effect on organisms? The fourth question: do changes in the ocean effect changes in the location of the pressure systems in the atmosphere? Fifth question: are there any possibilities of prediction? The sixth question: what research should be done on such matters as (1) wind stress currents, (2) sea level, and (3) direct measurements? I think that the sun spot effects represent competition, so we shall leave them out.

We might very well start with three different hypotheses, represented by three straw men of what happened. One possibility of what happened in the Pacific Ocean is this. This is a map of the Pacific Ocean. Ordinarily we have a globule of hot water, like an oil globule, in the central part of the Pacific. During 1957-1958 this globule may simply have thinned and spread out. (Fig. 169).

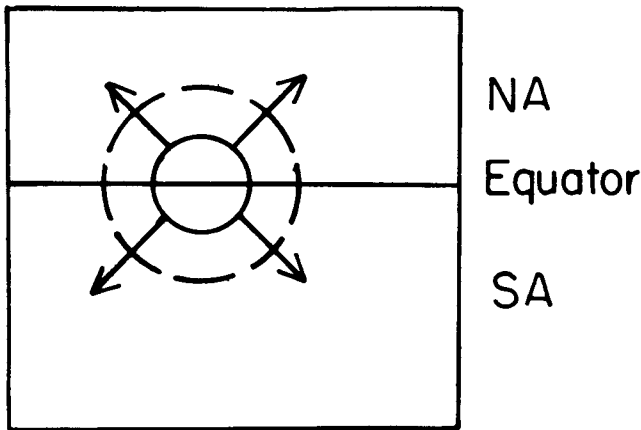


FIGURE 169. Straw man I (Revelle)

The temperature changes have only been recorded in the top 100 meters, but the circulation extends down at least to 200 meters. So then we suppose, when we have no information to the contrary, that the 200-meter temperature charts would be similarly changed. The 300-meter chart might be unchanged.

This "oil globule" on the surface is spread out, but not to the deeper waters where there is no change.

Isaacs: Straw man II is a little more complex and less refined than straw man I. (Fig. 170).

The upper part of this arc is now the Aleutian Archipelago of the Pacific Basin. The North Pacific Gyre has expanded. Whereas originally the West Wind drift was at say, 46°N, the axis of drift was moved south, not the water *per se*, but the current axis has shifted to the south under the influence of the anomalously large and deep North Pacific low.

The results of this would be that the water that now turns north into the North Pacific Gyre, is the water that was previously in the higher latitude portion of the Central Pacific Gyre. Thus, there is a

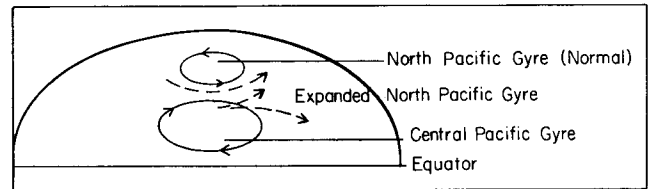


FIGURE 170. Straw man II (Isaacs)

warming in the Gulf of Alaska and also in the Pacific Coast of the United States. By the same token, there is a cooling on the Japanese side. By this model, the sequence of events is as follows: first, the boundary between the gyres shifts south and the water that originated from farther south than previously, now moves north into the Gulf of Alaska. Water that originated further south than normal has also moved south along the California Coast. These warm-water masses also may be spreading out. All effects thus weaken the California Current, but I do not know anything about the effect of this on the countercurrents. But as the main current weakens, it may tend to become unstable and greatly influence the circulation off Point Conception.

The movement of warm water into the Gulf of Alaska intensifies and expands the atmospheric low, consequently still more warm water enters the Northern Gyre.

At the same time, colder Oyashio water comes in from the west and in addition, thinning of the central warm water may result in cooling by vertical mixing. So, eventually colder water moves into the central latitudes of the Pacific. That seems inevitable and occurs very soon on the Japanese side. How long a period is required for the cold water to reach the California side from the Western Pacific by advection, I surely do not know—a year or so, I suppose.

Thus, from one or both of these effects, there is a later flow of cold water into the Gulf of Alaska and along the Pacific Coast, and possibly a consequent weakening of the meteorological system, bringing the entire fluctuation to an end. I think it is not absolutely necessary that we now explain the strong countercurrent development along the California Coast.

A summary of events implied by this model is: first, warmer water moves into the Gulf of Alaska intensifying the Aleutian Low, and off California, and colder water appears off Japan immediately. Sometime later colder water moves into the Central Pacific areas, and

later cold water moves into the Northern Gyre. This may react on the meteorology to terminate the entire sequence.

Revelle: I believe it would require about three years for something to move across the North Pacific in the westerly drift. Its speed is about five nautical miles per day, I imagine.

Munk: I am trying to construct a third picture of what might have happened based on Sverdrup's curl of the wind-stress method of calculating vertically integrated transports (Fig. 171). Suppose we have an

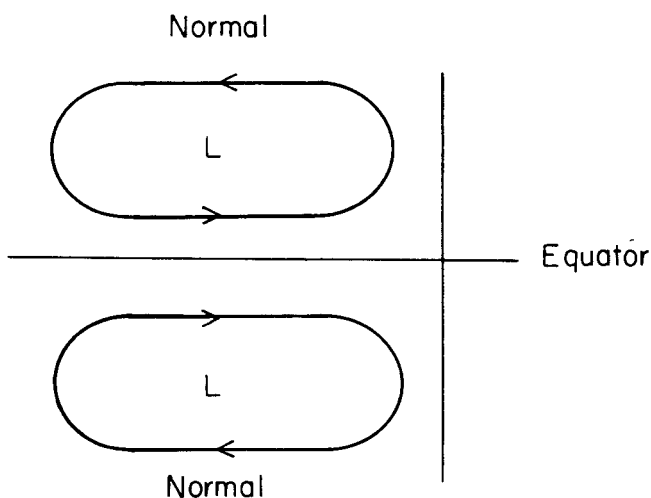


FIGURE 171. Straw man III (Munk).

anomalous low pressure (over and above the normal pressure distribution) over the Pacific in low and middle latitudes. The arrows show the resulting anomalous ocean circulation, again superposed on the normal pattern. At the eastern boundary in both hemispheres the water is anomalously light (warm) and the sea level high, both because of the steric effect and the direct pressure effect.

Saur: I would like to propose a sort of combination of Straw Men II and III. In order to heighten the contrast between possible atmospheric causes, we examined the daily and monthly atmospheric pressure charts from warm winters and cold winters selected on the basis of the shore temperatures along the west coast of the United States. For example: the winters 1925-26, 1940-41, and 1957-58 were extreme years on the warm side, as has been mentioned informally by several others during this conference. The monthly mean pressure charts bear out for the Northern Hemisphere what has been assumed by Dr. Munk in his Straw Man III. The pressure pattern is dominated by a large low pressure system over most of the North Pacific Ocean with a center of low pressure anomaly in the Northeast Pacific. However, in relation to Straw Man II, I would like to extend the possibility of a little different reaction of the ocean to these atmospheric conditions. Instead of shifting the axis of the west wind drift current to give warmer water in the Alaskan area, allow the west wind drift current to remain fairly constant in position but move the split in the current that occurs near North America farther

to the south, so that more of the transport goes into the circulation of the Alaska Current and the Oyashio Current. There would be a coincident decrease in the strength of the California Current and the large gyre of the North Pacific Ocean and also the countercurrent could develop along the California Coast.

Revelle: Why do you have to have countercurrents at all?

Saur: I will leave that up to the theoreticians to say. These models do not include countercurrents. Now in the cold winters, if you examine the daily pressure maps, you find that about one-half the time there is a very strong high-pressure cell that extends over the Gulf of Alaska, which tends to inhibit the Gulf of Alaska Current gyre. This suggests a possibility that during cold years the amount of circulation within the Gulf of Alaska actually shrinks. More of the water coming across the Pacific turns away from the Gulf of Alaska and runs down the California Coast, and, hence, there are the lower temperatures along that coast. This idea will agree with the drift bottle measurements obtained from weather station "Papa." I think it would be helpful if there also were temperature records to show if the axis shifted or if there is only a shift in the position along the Western Coast where this current splits.

Isaacs: In effect we are saying the same thing. The cold years are presumably the other extreme of hot year conditions.

Of our present evidence of a change in circulation, the dependability of the drift bottle information is critical, is it not?

Saur: The drift bottles show a part of the circulation going around the east part of the gyres.

Revelle: Let us go on to the evidence. I would like to point out one piece of *a priori* evidence that I believe you mentioned on Monday. It is one that we want to keep in mind and which seriously affects the atmosphere. The idea of the water coming from the south,—we are not really saying it does come from the south, we are saying that there is a weakening of the motion from the north. It seems to me that this is not in itself an adequate explanation for the following reason. This kind of temperature increase we have had, that is to the order of 2° to 3°C down to 200 meters, represents a total energy of 40,000 calories per square cm. We cannot get this much additional input of energy into the system through the surface.

Munk: My model, number III, does not imply the currents come from the north. You can have the west-east reorientation of the water masses as a result of this. If you believe in geostrophic compensation, number III would be in agreement.

Revelle: I think everyone can easily see there is advection and not local change.

Stommel: Unfortunately, we labor under some difficulties in trying to relate the behavior of any of our models—or straw men—to what presumably may actually be happening in the ocean. One difficulty is that there are no theories that can deal properly with Straw men I and II. The other is that it is difficult

to think of any way of obtaining the kind of observational material that can properly be compared to the theoretical deductions of Munk's Straw man III. We do not have a theoretical model that enables us to describe the mean vertical thermal structure of the ocean, and that permits us to decide what perturbations of the mean state would be like. On the other hand, the evidence that we have at hand is not the kind that permits computation of vertically integrated geostrophic transport over the vast central regions of the Pacific, yet this is what we would like to compare to any predictions of the Straw man III.

A change in temperature of the surface is scarcely evidence of a shift in the dynamic topography of a large oceanic area. The temperature change may be confined to a thin surface layer, and I am afraid most of the mid-ocean data is surface data only. If the major change of local heat content over the whole Pacific is produced mostly by advection and not by heat flux through the surface, the amount of heat for the entire Pacific has not changed but simply been redistributed. By Munk's model, then a cooling in the center of a gyre would be balanced by heating at the rim.

Munk: Or increased depth of the thermocline around the edges.

Revelle: This is what Straw man I says. If the deepening occurs only in a narrow region near the coast, say in only 1/20th of the rest of the ocean, the related shallowing of the layer depth, in the central region of the ocean might be small. The change in heat content off California is about 40,000 calories per cm². This may be an extreme area so we might consider 20,000 calories as average. It would take about 1,000

calories in the central regions to make up the -20,000 gained on the periphery or a shallowing of the mixed layer in the central ocean of only 5 meters. This might be too small to be observable and would have a very small effect on the dynamic topography in the center.

Now that we have talked a little about what might have happened, let us turn to an assessment of the evidence (Anomaly Table).

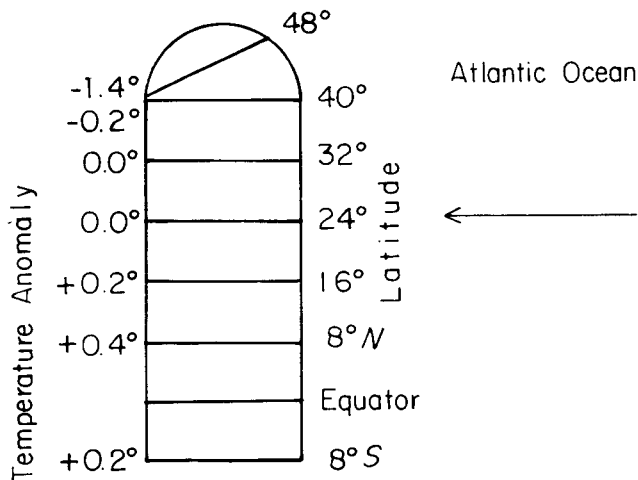


FIGURE 172. Temperature anomalies at 200 meters, Atlantic Ocean.

Fuglister: Here are some remarks about 200-meter temperatures in the Atlantic, which I shall quickly sketch (Fig. 172).

I was asked if anything had happened in the Atlantic and I looked at some 200-meter temperature

ANOMALY TABLE

TEMPERATURES ARE IN CENTIGRADE

	Winter 1956-1957	Spring 1957	Summer 1957	Fall 1957	Winter 1957-1958	Spring 1958
Longitude 140°-165° W	(3° to 4°) ¹ Warm	2° Warm	0 ¹	0 ¹	Warm	Cold
Latitude 20°-50° N						
Hawaii* (Sal) (Temp)	0.7° Cold	+ Sal 0.1° Cold	+ Sal 2° Warm	++ Sal 1.1° Warm	++ Sal 2° Warm	++ Sal 1.8° Warm
Gulf of Alaska (Salinity)	----	----	+ Sal	+ Sal	++ Sal	++ Sal
Entire Coast ⁵ (West Coast)	-.14 ft.	+0.06 ft.	+0.018 ft.	+0.11 ft.	+0.41 ft.	+0.22 ft.
CALIFORNIA N. of Pt. Conception S. of Pt. Conception	0 0	0 1° Warm	1° Warm 1° to 2° Warm	1° Warm 1° Warm	1° to 2° Warm 1° to 2° Warm	2° Warm 2° Warm
Peru ⁴	----	----	----	----	----	4° Warm
Japan North South	Cold 0	Cold Cold	Cold Warm	Cold Warm	0 Warm	Warm Warm

¹ Considerable cooling in September in NW part of area, warming in the SE part, net effect was about zero (0).

² In August, there seems to be a net warming.

³ These are comparisons with the 30-year mean for the Hawaiian Area.

⁴ Warming in Spring 1958 is all that is known.

⁵ Stewart: Anomalous sea level heights from tide gauges.

⁶ Added by Editors.

(EDITORS) The above table was assembled on the blackboard from the evidence presented in the contribution of the various participants.

data just before coming here. I had not expected that our data were worth mentioning, but maybe they are. In 1954 I got out an average temperature chart for each degree square using all the data we could obtain up to that period. Last year we made trans-Atlantic sections beginning in January, roughly 48°N and others at 40°, 32°, 24°, 16°, and 8°N, and one at 8°S. We did not have South Atlantic temperature charts. In 1957 the average temperature difference for the whole thing was minus 0.1 degree. So that is my first reason for not having said anything about it before. Around 48°N, we had a -1.4 degrees anomaly, the greatest one shown on the chart. The rest are very small changes. If we could possibly say that here is a shift, then there was colder water up north and warmer water in the south as far as the main water mass is concerned. It did reach south for there is also a plus 0.2 degrees at 8°S. There was no indication of a systematic variation when examining changes in one degree squares (our data usually contained about four or five observations at each degree square), but the anomaly is only apparent from the examination of larger areas.

Isaacs: Was this a vertical motion of the thermocline?

Fuglister: This is very small I am sure. At 28°N and southward, the 200-meter level is in the thermocline.

Revelle: Obvious question. Do you get this type of fluctuation every year?

Fuglister: Yes, except for the data at the 38° parallel, which show a greater change than I would expect. The fact that the change is negative in the north, zero around 28°N and 32°N, and positive in the southern part, is not compatible with a seasonal variation. It is something very interesting.

Namias: I would like to have seen the winter situation here. I would be extremely surprised if there were not very large anomalies in the winter on the basis of the abnormality of the wind stress in December, January, and February 1958.

During this past winter the greatest anomaly was neither off California nor off Florida, but in the Davis Straits area (Fig. 8). With a greater frequency of southeast winds than normal over the Northwestern Atlantic, I would suspect sizeable water temperature anomalies there.

Fleming: Extending how far south on the Atlantic Coast?

Namias: About Cape Hatteras.

Charney: We ought to examine the supposition that a change in atmospheric circulation is followed in the same seasonal period by a corresponding change in dynamic topography. I would like to ask both Munk and Stommel if they really think that a baroclinic adjustment would take place in that time.

Stommel: On the whole I would think it would not have sufficient time for a seasonal adjustment. I think that the topography would not have a chance to fully readjust. That, in a way, makes it a little awkward to see the picture. The heat content was associated with the topography and if overall adjustment of the top-

ography did take place, it would be warmer. On the other hand, you can construct an entirely different picture from the anomalous low if you say that the temperature is in an upper thin layer, the Ekman Layer, and then the low blows this surface water away and makes it colder in the center of the low, then the topography adjusts. If you do not have some kind of a model of what is doing the heating, you can make the temperature do the opposite there off the coast, according to the Straw man given us.

Pattullo: Stewart has the only piece of evidence of which I know showing the surface temperature anomaly and an associated difference in height of sea level. He reported an increase of 2 degrees for a 200-meter change, requiring 40,000 calories. I obtained the same thing by going in the other direction, using just surface temperature data and assuming that the change was confined to the upper layers.

Namias: It is really quite striking to see that the anomalies of water temperature agree with the anomalies of the temperatures in the lower 300 millibars of air. As we check these off, all except the last one fits. Perhaps the atmospheric temperature anomalies can be used as a measure of the surface water anomalies.

Revelle: Namias' observation essentially says that the temperature must have increased down to a depth of the order of 100 meters to account for equilibrium between air and water. This accounts for the steric increase.

Fleming: I think from what has been described, we are talking in terms of the short-term circulation. I am quite sure that the countercurrent, at least during the past winter, has been an important feature in both coastal and offshore temperatures. I can not say about sea level, but I do know that the warming extends to a depth of a couple of hundred meters or so off our coast. It seems to me that this warm water is a band less than 100 miles wide.

Murphy: I think the trends shown by the nearshore stations are compatible with the offshore conditions as far as California is concerned. If you look at the temperature charts of this area, the greatest warming in fact was on the outer part, not the inner boundary.

Namias: It looks like the dimension of the warming extends well to the Gulf of Alaska, over a rather large area. From the looks of the air temperatures, this aberration is not a local matter.

Fleming: To make my point, there are three different processes that can affect the coastal conditions: (1) spreading shoreward of oceanic waters (Roger Revelle's model); (2) modifications in the amount of upwelling, which can certainly affect the temperatures and sea-level conditions along the shore; and (3) the structure of a countercurrent.

Munk: I believe these are all three the same thing. I am certain (1) and (2) must be.

Stommel: They are probably linked together by some kind of mechanism. If we could plainly state the mechanism we would have a theory of the phenomenon.

Revelle: What I would be inclined to say is as Dr. Munk said, I think this is very primitive. Speaking of the slack of the north winds, they reduce the diver-

gence along the coast to allow offshore water to come in, reduce the upwelling, and increase in temperature. The upwelling affects the countercurrent in some way. In some way these have a sequential occurrence. They do not all occur at the same time, but they may be all the same kind of phenomena, but not all due to the same fundamental cause. I lean to the same kind of change in the wind system. I will make a somewhat different remark than Charney made. You actually do not have to have very much movement in the area a couple of hundred miles offshore to show a temperature anomaly of 2 degrees everywhere.

Stommel: Charney's model supports this: near the coast there is a rapid response of the thermocline to variations in the wind system but at some distance (greater than 50 km) the response of the thermocline is negligible.

Revelle: If we can make a simple model, what the model exhibits depends upon the kind of periods you consider for the wind system. You might get either warming or cooling, or even no relationship between the amplitudes of the temperature fluctuations and the wind. From the different kinds of physical properties on which you have data and which you are talking about, you have stressed the Ekman Layer, and it might be the dominant feature. Alternately the geostrophic flow might be the dominating thing. All we are saying is that the various water movements are tied together with a scale of a frequency of the wind system. And even in a purely formal model, it is evident that a model is a far cry from a true description of the physical processes. You get a time constant and responses that will be different for the barotropic and the baroclinic modes.

We have discussed temperatures, salinities and water levels. Is there anything we can learn about the direction of the currents from drogues, drift bottles and G.E.K.'s?

Munk: How about the Alaska drift bottles?

Revelle: What do they show us, Dr. Fofonoff?

Fofonoff: The problem is, does the distribution of currents suggest that the presumed shift in the surface waters occurred? In Straw man II the northward component of the current would shift close to the coast and become stronger. All the data were taken after September 1956. The northward component weakened in January 1957, strengthened in March 1957, and appeared to remain stronger through early July 1957. It was strong through March, through July, and that is as far as the returns go. Thus these data apparently agree with Straw man II.

Fleming: You say "weaker" "stronger"; to what are you comparing it?

Fofonoff: These terms are based entirely on how far north the drift bottles went. During 1956 they went straight east to the Canadian shore. In the summer of 1957 they went far up to the northern end of the Gulf of Alaska.

Fleming: Then you are referring "strength" to the currents in the summer of 1956.

Revelle: According to Isaacs' model, the divergence lines would shift to the south of Station "Papa" in 1957.

Robinson: The boundary between the Aleutian Current and the West Wind Drift Current is in this vicinity. From the pattern of the isotherms on the monthly charts, areas of divergence can be seen to shift north and south, but the shifts do not appear to be seasonal.

Sette: Perhaps you are thinking that this is a normal seasonal shift rather than a difference between years.

Robinson: Up to now, the statistical evidence of a seasonal north-south shift in the area of divergence has been inconclusive. There are undoubtedly random short-period shifts such as those occurring along current or water mass boundaries. These may be superimposed or systematic seasonal shifts, but there must also be non-periodic shifts of considerable magnitude. The question is: which one of these was responsible for the differences in drift-bottle recoveries between August 1956, and August 1957?

Revelle: I still think the drift bottles show a major shift, although the release point is in a location that may give as significant results as it would if it were farther away from a boundary.

Wooster: But you can see such changes do not happen every year. The difference between 1956 and 1957 is quite real.

Stommel: Do they drop a certain number of these drift bottles every day?

Fofonoff: No. They drop a thousand bottles immediately after arrival on station, which is about every six weeks.

Stommel: Since they are dropping them all at once, there may be some danger in the interpretation of their drift as representative of a six-week mean flow pattern.

Revelle: What do we have about the California area drift bottles?

Reid: The drift bottles off Southern California show a strong countercurrent in January of 1958, particularly north of Point Conception. Some of the January drift bottles moved north of Point Conception and were carried very strongly toward the north, but unfortunately we have very few comparable data for previous years.

Isaacs: In March 1958 drogue survey (Fig. 173) there was no apparent countercurrent off Monterey. Drift bottle results also bear this out, but we have reason to believe that the surface countercurrent ceases about March. There were two sets of drogues released. The first set was from several miles offshore to thirty miles from the coast. These drogues were followed for fifteen hours before a storm terminated the survey. These drogues drifted southeastward parallel with the coast, with some oscillation due to tidal effect.

The second set of drogues were released from thirty miles offshore to 100 miles offshore. They were fol-

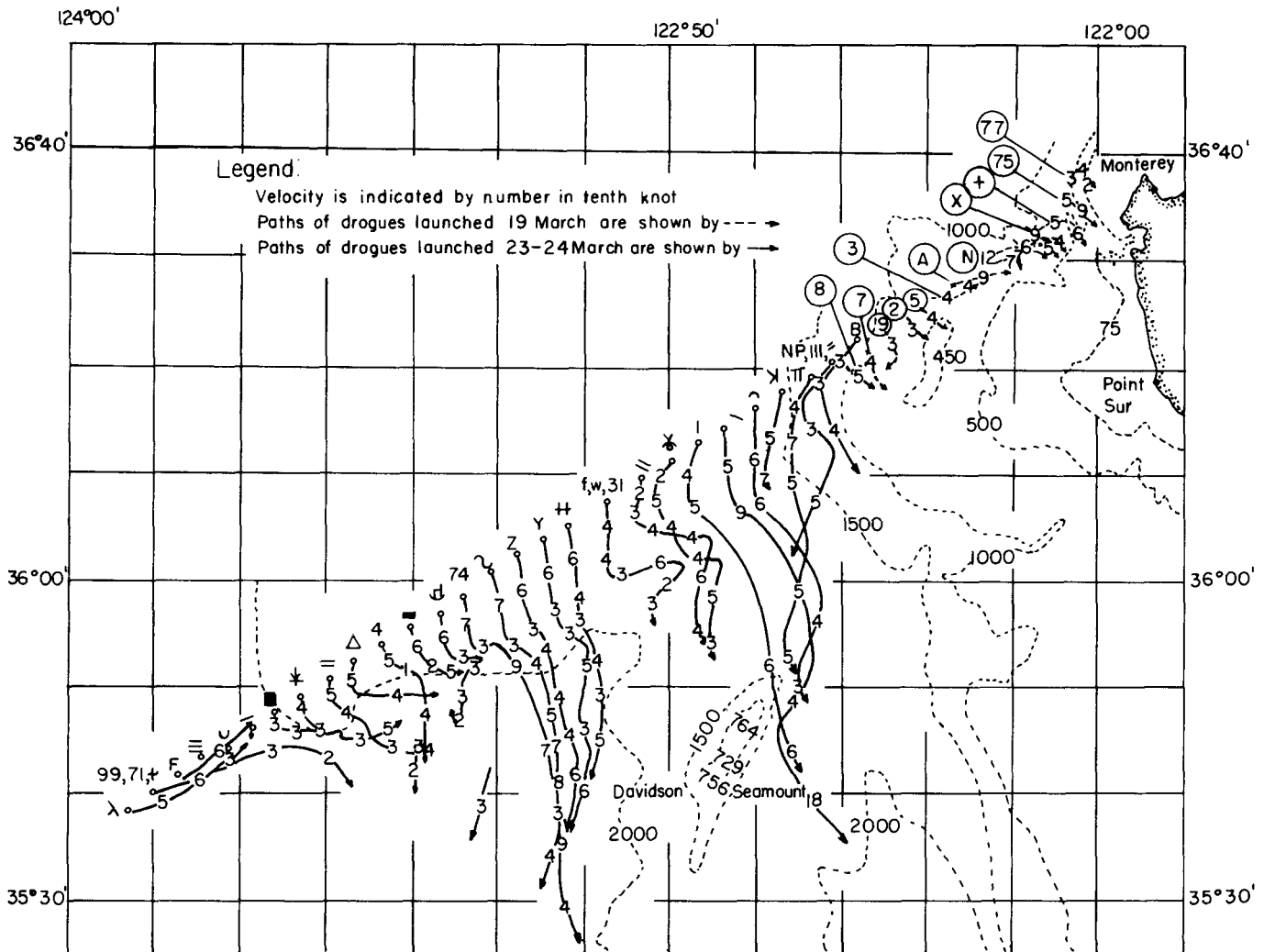


FIGURE 173. Current survey during March 1958, offshore from Monterey, California. The vessel determined the drogue positions as many times as possible during the survey. The velocities and direction of movement was determined between each positioning point for each drogue.

lowed for three days. There were two rapidly south-eastward-moving streams on either side of a slower, meandering stream, which was just to the north of the Davidson Seamount, almost as though this 700 fathom seamount affected the surface flow. At the end of the line of drogues the current was moving northeastward toward the coast. It later turned towards the south-east.

As long as we are discussing this countercurrent, I would like to mention one matter that appears to me to require explanation. All of the drift bottle recoveries above Point Conception are from bottles that were released within fifty miles of the shore. Some of these traveled over a thousand miles up the coast on the countercurrent! Many traveled several hundred miles. Of the hundreds of bottles released more than fifty miles offshore, there have been *no recoveries*. This curious matter seems to me to argue that there must be a very special structure to this countercurrent that can convey bottles along a route that has a "thinness ratio" of twenty. Some mechanism must keep these bottles offshore, but not more than fifty

miles offshore, and some associated mechanism prevents offshore bottles from entering the countercurrent. If some helicoidal flow exists in the countercurrent with the surface waters moving north and offshore, this possibly would account for the first of these problems. If so, surface species of organisms should be transported north more rapidly than shallowly migrating ones, and this is precisely what we have!

Berner: During the countercurrent period, the river outflows south of San Francisco are going to the north too. I think this is probably significant.

Murphy: I want to repeat something I said the other day with respect to those Hawaiian salinities, and this is, we have a south-north positive salinity gradient and normally in the spring, the axis of this gradient seems to shift to the north, and lowers the salinity at Hawaii. This last year, 1957, it failed to shift to the north, but the general aspects of the north-south profile remained the same but was located farther south than usual for the spring-summer period. At the same time, the heat budget in the Oahu

region resembled a typical heat budget farther north. In other words, one would say that during the summer of 1957, Hawaii was "farther north" than usual.

Revelle: Do we have any information for the oxygen?

Reid: Above the thermocline, being in equilibrium with the atmosphere, it varies with the temperature. Below the thermocline there is evidence of a small decrease in oxygen content from the normal in the region of the countercurrent. Offshore we have an increase in the oxygen content in the deeper layer, which again would be consistent with shoreward movement of offshore waters below the thermocline. The oxygen content normally decreases to the south and increases to the west off Baja California and California.

Revelle: Do you actually have a change of dynamic height?

Reid: Yes. At the surface with respect to 500 meters.

Revelle: When was that?

Reid: Beginning in the latter part of 1957 and continuing through January 1958, at least. The strongest we have examined are in January.

Revelle: Then it seems to me that my oil globule picture is not very satisfactory.

Reid: The main stream of the California Current seems to be slightly inshore. There seems to be a stronger countercurrent, but it also appears to be narrower.

Brinton: I would like to comment on an opposite situation. From recent observations made in January, 1958, in the Peru Current region at the latitude of Callao, about 12 degrees south, the oxygen in the minimum layer was practically zero. These low concentrations are associated customarily with waters north of the Equator. Is water with very low oxygen unusual as for south as Callao?

Wooster: Yes, it is normal for the oxygen content at the minimum to be extremely low in the Peru Coastal region.

Fofonoff: From the data at station "Papa" we have found that between 150 to 300 meters, the oxygen has been increasing quite steadily since we started measurements out there. This was from the summer of 1956 to the end of 1957, and involved oxygen just below the halocline. A temperature increase in the same depth range was found during the winter of 1957-58.

Fleming: This would be water from the south-southwest.

Reid: That would indicate a change in movement down to about 300 meters.

Fleming: It is critical here because of the nature of the T-S relationships. There is a sharp oxygen gradient at the higher level in the core of the Gulf, with higher oxygen where the core persists further out. I do not mean to imply that there is a current moving

south to north, but that there is water from lower latitudes.

Takenouti: I have no material on oxygen change.

Revelle: The only other evidence that I can think of, perhaps there is other, is from various kinds of biological information, and this is of two sorts: organisms that are drifted with the water mass, and animals that respond to the changes in the water and swim into the water that they like.

Berner: Biological evidence in late 1957 suggests that there was a small shift toward the coast of the species that possibly live in central water, which really is not unusual. This shift was not apparent in April of 1958 when there was a coastwise shift of *Nyctiphanes simplex* to the north.

Isaacs: So what do we get out of the biology then, a coastal countercurrent? But the biology is putting a restriction on what you can say about the flow of this Pacific water. I fail to see how the surface waters can merely spread coastward without carrying the organisms with it.

This is the reason that we made quite an effort to visit the stations farther north to see what was happening. It is not accidental that the last station touches the zooplankton boundary.

Sette: It must involve a fairly thick layer if judged from the vertically migrating Euphausiids.

Brinton: Direct evidence on the countercurrent is not sufficient to say that it was stronger off Point Conception this year than previously, although indirect evidence, from plankton species distributions, says that it was stronger. This is the first time in all the years for which there are data on *Nyctiphanes* and *Denticulatum* that these species have occurred north of Point Conception, except for one occasion in 1952. *Euphausia eximia* has extended in April 1958 to a line off Monterey, inshore, which is north of the previous most northern record. This is evidence of a quite positive nature because the offshore "central" population did not seem to be coming in.

Isaacs: Actually, microplankton seem to have been coming toward shore in a big swirl off Southern California and then drifting north in the countercurrent around Point Conception and up the coast.

Brinton: It does not seem to me that we can say that the biological information fully proves that the countercurrent is better developed than usual. The inshore temperatures also became more suitable for southern forms associated with this coastal region. From the temperature standpoint, these could exist in an environment farther north than usual. A persistent countercurrent in a cooler regime might not carry the southern forms north of Point Conception. The offshore animals whose presumed temperature tolerance would allow them to live in this coastal belt if they are introduced into it, have not conspicuously entered this area. But these alongshore forms have. They may be adapted to the "central" offshore region from the standpoint of food and other things, which

could, at the same time, keep them out of the area. I do not know whether we can actually conclude that the reason the "central" forms did not get into the coastal waters is that they did not have transport access to it.

Sette: May I ask a question? If the central water mass containing food suitable for its biota, expanded toward the shore, would not that provide food for those organisms as well as carry them in? I would think this would be true unless there were a rapid change in the character of the water. Stating it the other way, if you have water bringing in the conditions for the animal, why should it not bring the animals and their food also?

Brinton: It should. I would like to add that the eastern boundary of the "central" fauna did move somewhat shoreward, towards Southern California in February 1958, as compared with 1957.

Question: Has it shifted shoreward, at the latitude of San Francisco?

Brinton: Not a great deal, if at all. There is fairly good evidence from the plankton that there was a countercurrent inshore, and also it was a bit warmer there. A coastal environment usually confined to Southern California waters was introduced into the northern area by some means.

Haxo: This is not quite as clearly documented by the phytoplankton, but it is substantially in agreement.

Saur: Was it not said that the response to winds of water along shore is not necessarily the same as that offshore—why shouldn't there have been a very strong local surface countercurrent right along the California Coast?

Stommel: I am finding it a little difficult to keep the scales of the various phenomena under discussion straight in my mind. Perhaps this is partly because we are using all different sorts of projections, and scales, on the figures being presented.

I have been arguing here for the most part, that in the central part of the ocean there is not likely to be much response of the dynamic topography due to changes of the wind systems of a month or two duration. Moreover, I also think that the type of evidence presented here for Mid-Pacific temperature changes is mostly limited to surface temperature data. So far as I can see, we are not in a position to compare the theoretical idea with observation. However, there is at least one type of phenomenon—of a kind brought up by Margaret Robinson—that I think probably does actually produce a distinct local change in dynamic topography despite the predictions of the simplified theory, namely, in those regions where the western boundary current, the Kuroshio, pulls away from the coast and flows eastward. This strong narrow current can meander northward and southward rather quickly and thus produce significant local changes. Regions where this may occur may extend over much of the northern North Pacific. But south of 35°N latitude,

I should suppose that the simplified theoretical considerations ought to prevail.

Brinton: Temperatures in the Southeastern Pacific, from Downwind Expedition observations made by Worrall, included in the IGY report of the cruise, show certain differences from temperatures plotted in the charts in *The Oceans*.

At 200 meters, the warm 18 to 19° water in the eastern central South Pacific region was found as far east as Easter Island, near 105°W, in February 1958. Temperatures at this depth plotted in *The Oceans* show the 18 to 19° water present to 95°W. A similar change was noted for temperatures at 400 meters. Thus, the deep warm water was more restricted in its general spread during the past winter, if significance can be attached to the relatively scattered Downwind data.

On the other hand, the same 200-meter Downwind temperature chart showed temperatures in the Peru Current extension along the equator to be 1 to 2° warmer over a broad area. This was reflected in the surface temperatures in the eastern Equatorial region as well, where they were 1 to 3° warmer in February 1958 than in the average picture presented by *The Oceans*.

The most extreme temperature change was in the Peru Current proper between Callao (12°S) and Antofagasta (24°S), where February surface temperatures were several degrees higher than usual. When the Peru Current in this region ceases to be cold, a wide oceanic area to the west may be influenced by the greater warmth of the Peru Current and the South Equatorial Current.

Wooster: It is difficult to be sure about the changes at 200 and 400 meters. There is only one comparison possible, Schott's charts as reproduced in *The Oceans*, and these are not mean temperatures but rather are from isolated observations. If observed temperatures differ from those shown in these charts, it is not really convincing evidence of a temperature change.

Revelle: I see obviously that we cannot cover everything, but before we conclude our discussion of what happened, there are two quite important phenomena that I forgot to mention, one of which I was not aware of because I was not here on Monday. One of the regions we have not considered is the Equatorial region. Starting in the spring of 1957 and going all the way through to the present, there has been a warming of the entire mixed layer in the section between Hawaii and Tahiti and Samoa, and perhaps even a thickening of the mixed layer. This suggests a shift from west to east of the mixed layer, which is deeper on the western side. Another thing that is also suggestive of a west-east shift are those diagrams for the similar years of Dr. Takenouti (Fig. 64, 65, 66), which show an anomaly on the west coast of North America. Along the whole west coast of the Americas when we have a negative temperature anomaly, we have a positive temperature anomaly in Japan, and vice versa

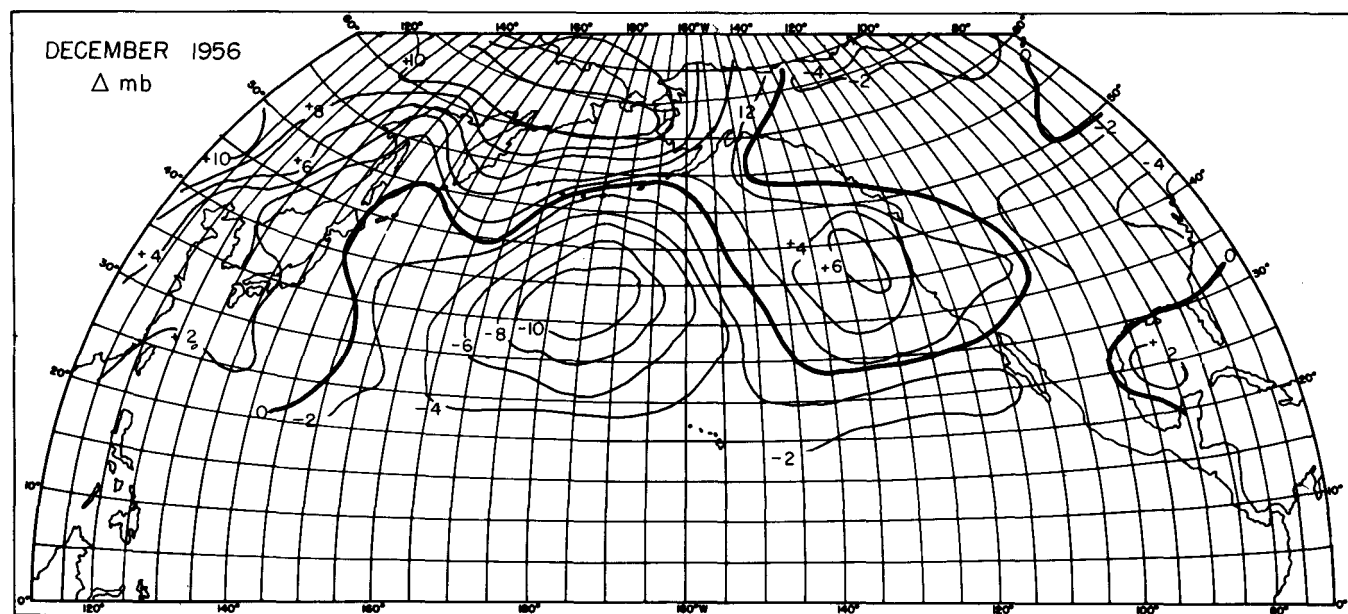


FIGURE 174. Sea level atmospheric pressure anomaly (Δ mb) in the North Pacific in December 1956, showing two cells. According to the advection theory, temperatures should be colder than average on both the Asiatic and American coasts, and quite warm in the Central Pacific.

(Figs. 64, 65, 66). These are for extreme years. In 1941 he said it was the reverse, and in that year, warm waters on this side of the ocean were accompanied by warm waters on the other side of the ocean. This is suggestive of events in general. For the present occurrence it is partly contradictory material, because from figure 52 and 56-59, the conditions in 1957 do not appear to be uniformly cold. Possibly this was not a typical year.

Isaacs: We could be confusing 1956-1957, 1957-1958, which might be two different cases.

Roden: A large single pressure anomaly in the center of the Pacific is likely to cause opposite temperature anomalies on opposite sides of the Pacific; two or more could produce a number of situations in the ocean, and we can only expect that the temperature anomalies will show a different pattern. In December 1956 there was a positive anomaly in the Eastern and a negative anomaly in the Western Pacific. One could expect negative temperature anomalies on both sides of the Pacific and positive anomalies in the center. This refers, strictly speaking, to air temperatures but is also valid for sea surface temperatures.¹

Revelle: I think we now ought to discuss the last three questions. How does the ocean affect the persistence in the atmosphere? Do we have possibilities

¹ Mr. Roden submitted figure 174 to the editors to illustrate the situation he describes.

of predictions? What more research needs doing? Let us take the number three question first, particularly concerning motion in the Ekman Layer and baroclinic adjustment as a function of changes in the wind stress. Charney, would you like to lead off on that?

Charney: As I see it, the problem is, how does the ocean respond to surface influences that vary both in space and in time? Several people, including myself, have attempted to analyze the transient motions produced by variable wind stresses. The trouble is that the models we have used have been so oversimplified that much of the reality has been simplified out. For one thing, we have analyzed the rather unrealistic infinite ocean.

If the periods of the exciting forces are long compared with a day, two kinds of free oscillation are excited in an infinite ocean: the first is essentially a barotropic Rossby wave in which the pressure and horizontal velocity are independent of depth; the second is also a Rossby wave in which the pressure gradients and motion are confined primarily to the water above the thermocline. We may call this the baroclinic mode. If the period of the driving force is comparable to the baroclinic free period, i.e., several years, the baroclinic mode will be excited. On the other hand, synoptic weather systems have such short periods that they invariably excite only the barotropic mode, as Rossby found in 1937. They do this by producing a horizontal mass convergence in the Ekman Layer,

which then gives rise to a pressure force and a slope current extending undiminished to the bottom.

Revelle: Of course I do not think it really happens. A barotropic current is quickly established in the mixed layer.

Charney: I am speaking of a highly idealized model in which the continuous density distribution, shown as the continuous curve, figure 175, is replaced by the

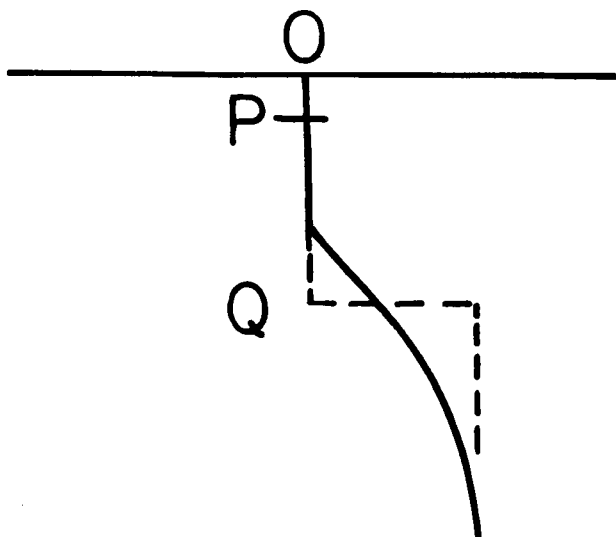


FIGURE 175. Idealized model of density distribution.

discontinuous distribution, shown as the dashed curve. A rapidly-moving wind system will excite currents reaching all the way to the bottom. (Note added in proof: The slope current in the layer O Q should not be confused with the drift current in the Ekman Layer O P. This is probably the source of my misunderstanding with Revelle. J.C.)

Revelle: I do not think this is so. When we were off the coast of lower California a couple of years ago, we actually had a series of drogues at different depths in the mixed layer and at a greater depth. During the time we were there, which was about two weeks, the wind shifted several times and the surface drogues in every case went in the wind direction. These drogues were at depths of 10-15 meters in the mixed layers. In other words, the current went in the direction of the wind. When the wind blew from the north, the drogue would go south; from east with west wind and west with east wind. This is like water sloshed back and forth in a bathtub.

Question: Why was that not more or less at right angles?

Revelle: We do not know. This was in the mixed layer at latitude of about 27°N. Below the mixed layer the drogues went almost continuously in the same direction regardless of the wind change.

Isaacs: In the final results, the deep currents did not quite do that. They made a long arc with no relationship at all to the wind, whereas this surface current described another big arc in response to wind

shift. Curiously, if the wind diminished suddenly, the surface drogues moved into it for a while!

Charney: I remain puzzled by the result that synoptic wind systems should excite barotropic currents. I think that Rossby's reasoning was essentially correct, but that it does not apply to a very real situation.

Revelle: This is Ekman's reasoning too, but they do not agree with actual drogue measurements.

Charney: If one ignores inertial effects the currents become quasi-geostrophic and the mass-velocity adjustment takes place instantaneously. Let me illustrate this point, which I think is important. Following Rossby, we consider an infinite homogenous ocean and

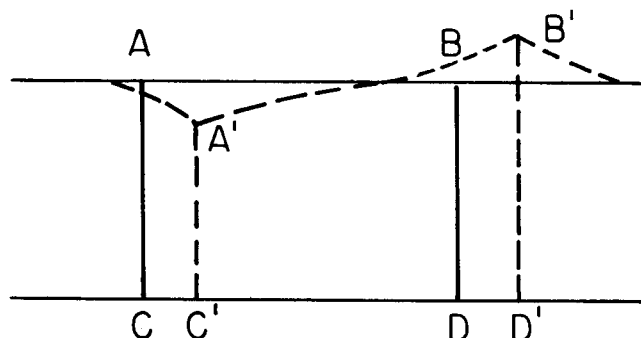


FIGURE 176. Rossby's Model.

imagine that an infinite wind stress directed northward (into the page) acts on the strip AB and instantaneously imparts a velocity U to the prism of water ABDC extending north and south to infinity (Fig. 176). The coriolis force acting on this volume will then deflect it to the right, so that there will be horizontal convergence and a rising of the free surface to the right of BD and divergence and a falling of the free surface to the right of AC. In the process the entire system will execute a series of gravitational-inertial oscillations, and some of its energy will be radiated off to infinity as a train of gravity waves. In the end the free surface will acquire a permanent deformation A'B' to produce the pressure force necessary to balance the current displaced from CD to C'D'. Rossby calculated the difference in energy between the initial and final states and found that very little energy is converted to inertial oscillations, even in this extreme case.

In reality the wind stress is applied slowly, so that even less energy goes into the short-period inertial-gravitational oscillations. Hence one may ignore them altogether and regard the motion as constantly in a state of geostrophic adjustment. This is the essence of the geostrophic approximation. It enables one to dispense with the extra baggage of the inertial motions.

Now, because of the variability of the Coriolis parameter, the forced motions will not be stationary but will propagate with the speed of Rossby waves. What we do not understand is how these motions behave in the presence of boundaries. Here, it may be that, as in the case of steady motion, frictional and vertical effects cannot be ignored, and complications will arise.

Stommel: Do you have some ideas about how the Rossby wave that is generated will actually be reflected at a coast?

Charney: In all probability they would bounce back and forth and produce standing barotropic oscillations. That is to say, they would produce an oscillating current extending all the way to the bottom. But this sort of thing is not observed.

Revelle: We observe it by the difference between the drogue measurements and the geostrophic current.

Munk: Who has this geostrophic versus observed current data?

Charney: The currents would be geostrophic, but not measurable by standard methods.

Munk: Did you not try to calculate the effect of pressure gradients?

Revelle: They are not measurable by a Nansen bottle, even though you should be able to arrive at their value by the departure of the drogue and the Nansen bottle.

Stommel: The velocities of water particles associated with the barotropic response of the ocean are so small as to be unmeasurable, and so it is very hard to see how we can compare the theory of Rossby with any kind of present day field observation. A further point I would like to raise—more in the nature of a question, really, is how shall we treat the boundary conditions at coastlines for reflecting Rossby waves? We might simply use the geostrophic approximation right up to the coast and assert that there is no flow normal to the coast. But in other oceanographic models—such as the steady ones with western boundary currents—we bring in higher order dynamical processes at the coasts like inertial terms or viscous stresses.

The theoretical investigations of Rossby and of Veronis and Stommel indicate that the response of geostrophic currents to large scale wind-systems of a period of a week or so, will largely be in the barotropic mode, and not in the baroclinic mode. In other words, the non-uniform distribution of wind-stress associated with a storm over the ocean will tend to pile up water in the surface layers, but this will mostly make itself felt by a rising of the top surface rather than a deepening of the density structure, and hence the horizontal pressure gradients produced by the local accumulation of water will not be attenuated with depth, and the geostrophic current system set up will be essentially independent of depth. The purpose of the calculations of Rossby and of Veronis and Stommel was not to study in detail the nature of the velocity field right at the surface, but to investigate the response of deep (let us say at the depth of the main thermocline) density surfaces to short-period fluctuations in the wind systems and to show that the geostrophic currents extend to the bottom. The model employed two layers of different density. In the numerical examples the top layer was considered to be much deeper than any likely subtropical Ekman Layer depth. The velocities referred to in all discus-

sions of the results of the theory are velocities in each layer.

If we now confuse these velocities with observable velocities in the top ten meters of the actual ocean—in an actual Ekman drift layer under a strong wind-stress—we are likely to arrive at a seeming contradiction between the theory and experience from drogues, and other direct measurements, because it certainly seems quite contrary to experience or physical intuition to assert that the transient wind-stress of a storm cannot move surface water without carrying along all the water beneath it,—all the way to the bottom. Although there are very few observations to make a convincing case either way, in the deep-ocean it certainly seems most reasonable to suppose that the winds are capable of driving a shallow surface layer quite freely back and forth across the sea surface. This is the point of view which Dr. Revelle is emphasizing when he speaks of the surface mixed layer moving about.

If we want to set up a schematic model of the ocean which exhibits such a thin surface layer skimming around over the surface of the ocean, we must reformulate the Veronis and Stommel model, introduce the wind-stress as a surface stress rather than as a body-force, allow for a vertical variation of the velocity within the top-layer, and include a vertical eddy-viscosity in the top layer. In this way we can find a shallow surface layer moving around as rapidly as our taste demands, but we will not need to modify any of the results of the Veronis-Stommel model, so long as we interpret the velocities that appear in the theory properly, as the average velocities.

Thus we can still be assured that the density structure of the ocean will not respond sufficiently to a large-scale wind-storm to permit any baroclinic adjustment in the geostrophic flow. This means that in Straw man I the movement of a thin surface lens of low density water will occur, by virtue of a storm, almost entirely as an Ekman drift. There will be no need to make any correction for changes in the geostrophic currents produced by shifting surface temperature patterns. There *will be* changes in the geostrophic currents, but since these extend to the bottom, the velocities will be much smaller than the velocities in the Ekman-drift layer and relatively negligible. For somewhat longer periods, a partial response in the density field will occur, and it will be necessary to allow for the development of baroclinic geostrophic currents which themselves have appreciable surface velocities and hence modify the picture obtained from consideration of the Ekman drifts alone. Of course it scarcely needs to be mentioned that the details of the Ekman drift as a function of depth, in contrast to the vertical integral of the Ekman drift, depend upon the nature of the turbulence in the surface layers and are therefore essentially uncomputable. This is one of the reasons why the theories of Rossby and of Veronis and Stommel deal with average velocities in each layer. The more I think about this,

the more I realize that the results of the theoretical models are rather susceptible of misinterpretation when applied to the real ocean, and so perhaps it has been worthwhile to have made these comments at length. I hope they will help to dispel any suspicion which I may have inadvertently fostered, that there is some deep and irreconcilable discrepancy between the well-known results of the theories, and the common-sense picture of the wind driving the surface few meters of water.

Charney: The difficulties in finding how the ocean responds to a variable wind stress so far appears to be connected with the boundaries. Perhaps simple reflection of the Rossby waves does not take place. As the traveling waves hit the boundaries, it may be that narrow boundary currents are created in which all kinds of peculiar physical effects occur. Thus these boundary currents are the only places where friction really counts, and it may be that appreciable energy is dissipated there, so that only part of the energy is reflected. Again, inertial effects might act to produce a kind of rectification of the currents, etc., etc. I think we have enough data and sufficient motivation to embark on a very careful experimental study of ocean currents based on simplified theoretical models. This will give some insight into the nature of the physical agencies at work. With such knowledge one might be encouraged to undertake a numerical approach to the solution of the problem, especially when one knows the wind stress much better than anything else.

Stommel: At any rate, you want to apply numerical techniques to more complete mathematical models, but not to real oceans yet. As I understand your remarks, it would be better to study a rather flexible idealized model than a rigidly formulated realistic one.

Charney: Yes, but I think that we should try to confront these models with experimental tests as soon as we can. The only way to do this is to deal with actual distributions of wind stresses over the oceans. I feel that you would be gratified by the results. We have done similar things in the atmosphere, using such crude models that an outsider would not anticipate that we would obtain such interesting conclusions as we do obtain.

Isaacs: It seems possible that the absorption process at the boundary could be mixing.

Fleming: May I take a minute? When we learn something about the response characteristics of the ocean, I think we *will* find the long-period wave responses, and lags.

Charney: This could very well be. I would not be surprised if you would find some really important lag relationships among ocean currents, which could be important for predictions. It is a possible problem for analysis.

Revelle: Dr. Fofonoff, have you any ideas about the possibility of estimating what happened in the water from the wind system, particularly from the transient winds?

Fofonoff: In an infinite ocean a uniform wind moves the surface water without piling it up in any region, and therefore does not produce barotropic motion extending to the bottom. This is also true for a limited time after a wind starts to blow over a finite region of real ocean. The wind has to move the water some distance before the secondary effects due to piling up of water can take effect. Now a ship working in a small region for a short period of time may see only the initial development of the surface flow. Thus, on the time scale of the actual observations the ocean may appear to respond just as though it were infinite in extent.

Munk: I am very much troubled because I think this is where oceanographers come out poorly.

Fleming: It seems to me that Dr. Sette's remarks and studies, and those of others, are very heavily dependent on these relationships. Looking at Namias' charts one does get the feeling that there is a lag in the connection of events in the air and those in the sea. And with only the present understanding, a good deal of sense may be lost between the wind picture and the oceanic picture. Yet I am most impressed that the quality of the data I have seen the last few days is very high. So let us start out with charts of these sorts from month to month, using dynamic methods, numerical if necessary, and predict, if that is what you wish, what will happen in the ocean. And as one reason why it should be used, we will just say that some day we shall learn about the atmosphere by this application. We have succeeded, as far as I know, in using computers to "learn from our knowledge" only in two ways:

(1) handling of data, production of data, which is not involved here;

(2) in making computations of problems where the base is physics, and wherein an order of magnitude of the factors at least was understood. Here one uses the computer to tie things down within the order of magnitude that you understand. We have a problem here of an order that we do not understand. What do we know about friction in the western boundary? We have no model. How do we go about computing this in the case for which we have no fixed picture at all?

Stommel: Let us use the computer to study more realistic models, not just yet the oceans.

Isaacs: Fleming is right in my opinion. The computer might help us as a computation means, but to go heavily at this time into computational problems of unreal models where we have not yet solved realistic and typical problems seems a very questionable step. I am afraid there will be a new generation of oceanographers growing up who are going to throw words around about computational instability, and electronic wind transients, and get wholly tied up in such things. If you accept Charney's suggestion you will need increasingly more observations, and these must be more direct, unequivocal, and critical

than present classical oceanographic observations, to enable you to check the success of the calculations. What can oceanographers do when they have only charts such as we have to check computations according to some model of what happened in the ocean? You are not going to check it against the geostrophic flow. You might check against temperatures. Temperatures, or against sea level, perhaps. This seems very dubious, but possible.

What I am trying to say is, how do we substantiate the veracity of a model calculation from oceanic data, the greatest part of which must be interpreted through the same model we are attempting to verify.

Charney: I am suggesting that we use the machine as an inductive device, as a means for testing theories and for discovering new interrelationships. It is clear that the machines will be of limited use until better physical understanding is obtained and more data are available, but this does not mean that we should not use them at all. To understand the highly interrelated events discussed in this Symposium, we shall not be able to dispense with devices for dealing with the complex data and the non-linear interactions.

I can cite one very good example, where the machines have been used to good advantage in the study of the general circulation of the atmosphere. This is Phillips' model.

Munk: I think that it would be very much worthwhile to demonstrate this.

Charney: Phillips took a numerical model that we had devised, put in simple energy sources and sinks, and calculated the evolution of the flow from a state of rest. He obtained circulations that very much resembled the circulation of the atmosphere. The advantage was that he was able to combine for the first time a variety of physical mechanisms which had been studied separately but never in combination. By varying parameters he was able to assess their relative importance and their role in the general circulation. In this respect the machine was used for the same purpose as a laboratory experiment. His work has already led to a number of purely theoretical investigations.

I would expect that a similar experiment could be carried out for the oceans, despite our lack of precise knowledge of turbulent processes. The agreement with observation might be close enough to support the next steps to be followed; and if this leads to verifiable predictions, I would not turn up my nose at them. Munk's success in explaining the gross features of the general circulation of the oceans with a very limited knowledge of the energy sources and sinks, leads one to expect that a similar success might attend an attempt to calculate the large-scale seasonal or extra seasonal variations.

Revelle: One thing about the computer business, there are not enough data to put into it, but if you have few numbers, you can analyze them with a few

people. It is only when you have large numbers that the computer is required. If you have a number of wind stresses, you do not have anything to check it against. It seems to me that it is quite clear, for one thing, we have a continuous time series of sea level data, which for the most part can be enclosed in a coastal booklet, particularly including the package we have assembled from the IGY. Munk was saying last night, and again today, if, for example, we can use the computers (it does not take a fancy one, the IBM system would be quite adequate), for the actual tide itself at sea level observation, then you would have a more or less continuous basis of coverage that would not be too far different from the meteorological maps.

Namiias: When it come to the problem of possible prediction of the surface water temperature pattern by the wind pattern, I have the impression that the problem is certainly no more complex than similar meteorological prediction problems. I do not mean to say that we always can interpret correctly circulation predictions in terms of weather phenomena, but, particularly for longer term anomalies of one week or more, it is profitable to develop objective methods translating these winds into associated temperatures. In fact, this has been done for quite a while with a fair degree of accuracy. At first this seemed to be beset with almost unsurmountable obstacles. Similarly it seems to me from the work that has been presented here, that there is sufficient order to make an attack on the prediction problem. According to the paper prepared by Mr. Reid, for example, one can find that 1931 had remarkably similar pressure and sea temperature anomalies (Fig. 84); there must be some fundamental reason common to both periods. While things are certainly a lot more complex than I suspected, fundamentally there must be some rather broad scale and clear-cut relationship between these anomalous patterns in the ocean; and fortunately they are of a large scale. I think these temperature anomalies will require some special smoothing that will bring out their large scale nature a little better.

This empirical attack already begun should certainly proceed with full vigor, and might provide fresh ideas and clear up and bring new facts to light.

You mention, Dr. Revelle, possible lag effects. Here again, one feels almost intuitively that such effects are present, and that they may be carry-overs from the oceanic conditions that some of the atmospheric circulations have impressed on the ocean surface. These lags could make oceanic prediction problems easier than those of the atmosphere. In other words, in the atmosphere we have to predict the whole circulation. We do not get much help from the past in forecasting this circulation. When I think of the slowness of motion in the ocean, prediction does not appear so remote. In my presentation I tried to indicate

a hypothesis which, if correct, could conceivably have led to a partially correct oceanic and atmospheric prediction for these abnormal seasons.

Revelle: It seems to me that it is a true statement that you just made, and I would be inclined to be much bolder about it. There does not seem to be any reason that any meteorologist could cite to explain this persistence in this weather system, unless he took the oceans into account. The ocean is so conservative it may very well be that a shift in the oceanic conditions really determines the whole persistence in the weather system.

Fleming: There is one other thing that comes into this, that is the fact that the oceans have boundaries. The atmosphere does not have similar boundaries. The first effect may be of warm waters moving toward the coast, and, whether or not it has been displaced northward, it is contributing to the development of the anomalies as just pointed out. I think this is associated with the time lag, which also affects the boundaries.

Revelle: The ocean cannot change too much because it is fixed geographically.

Fleming: It may anchor the meteorology geographically.

Revelle: One thing that might very well come out of the Symposium is the serious consideration of the long-term persistence in the atmosphere resulting from the ocean. Evidently persistence is bound up between the atmosphere and the ocean.

Namias: I feel this way myself. I have made a number of studies, statistical studies of the regional characteristics of persistence. From what I can determine, there is a significant difference between persistence over the oceans and persistence over land. You will find that over the oceanic regions there is greater persistence than over land areas during the cold seasons—especially at lower latitudes.

Revelle: This is probably to the second order. I am impressed by the fact that the shift of the ocean systems will determine the persistence in the whole system that you have there.

Isaacs: This seems very good to me, this discussion of large scale interaction as a further study. But I really am somewhat discouraged over our lack of a picture of what has taken place. I had hoped that we would indeed be able to at least qualitatively point out such relationships as you mention. We have talked about what possibly happened in these years from a great number of measurements of the ocean, and it is strange that we have to sit here and discuss whether or not we know which way the water was going. It appears to me that the way we are making oceanographic measurements has a real deficiency, as I have been saying. We are trying to determine the significance of the measurements we have made through a preconceived model that we have not even proven and yet through which model we interpret the data. I feel that, for the time being, we have to look

for more direct ways of going about the measurements supporting these less direct data and studies, and we have to do this for some time in the future. We must make the most thoughtful and least equivocal direct observations that we can.

Sette: Isaacs, have you any ideas how to carry out this program of direct measurements?

Isaacs: I believe that there are many very simple methods. At Scripps, we have been working on some methods that require only the further development of existing know-how and seamanship, the deep moored stations, for example. I have already described the drogue work we carried out from a Navy ship. With one of our ships we can carry out much more of such work. Certainly the drogues are an inelegant way to proceed, and the method yields very little data for the work involved but there is hardly any arguing with the answer one has obtained from the effort, and it constitutes a critical test if very extensive areas are covered.

Stommel: One of the possible future techniques for long-term monitoring of the ocean that I hope can some day be made a practical one, is the tracking of rather large numbers of Swallow-type floats over long distances by means of hydrophone arrays fixed on island listening stations. At present there are several obstacles to this scheme: (1) how can we make a repeating sound source of sufficient intensity to work over several hundred miles in the SOFAR Channel? (2) How can we keep a check on the depth of the floats? (3) How will we identify different signals when large clusters are being used?

Fleming: There are already the kinds of floats that Stommel wants, existing in the ocean, namely the organisms. They have a built-in clock, for they have a life cycle. I think this is probably the practical answer to the problem—careful selection of certain types of organisms.

Revelle: I would like to propose a different system. First of all, I now feel that the idea of time series will really give us something that we do not have. I think that we could do this. We now have the ability to plant a buoy in deep water and have it stay there for months and months. Even with a relatively small number of these, the amount of information you can get simply in terms of numbers, is relatively large. This is a simple measurement of current motion in the top 3 meters to 400 meters. I think we could do this, and can do this at half a dozen or dozen stations. We might predict conditions on the California Coast from one strategically-placed buoy.

Munk: How about the potential difference? What is to stop us from having those 1000 miles apart connected with cheap underwater cable and measuring potential difference. This will give us the transport.

Revelle: We should have time series at strategic points in the open ocean with actual measurements of the motion of water. Perhaps it is more important in the south, because everything else we can get by other methods.

Fleming: You are making point measurements. You are going to have presumably a tremendous lot of background noise at single points of observations. That is something you can only learn by doing. It is easier to put out four stations than one in an area, and the data would lend themselves better to a solution if we could essentially average them. I think you need both fixed stations and buoys.

Revelle: The fixed stations would put us much further ahead than we are now.

Fleming: I do not think so. What I am trying to say is, the fixed station is a lot easier to follow than the buoys, which is a job.

Isaacs: The drogue technique worked out quite well.

Revelle: Can you do this all the time? Are you going to be all one year trying to observe some sort of a transport? If we had been doing this for the last ten years, for example, we would have gotten rather discouraged by now. It is only because of this transient year that we think we have a new insight.

Isaacs: Perhaps you are correct, but we are still unable to say clearly what happened, and if we had carried out more direct measurements, I am sure we would be able to. I am astonished at how much these discussions have had to lean on such simple data as the drift bottles.