EDITORS' SUMMARY OF THE SYMPOSIUM

INTRODUCTION

The Editors feel that the principal contribution of this Symposium is the collection under one cover of much of the evidence of change in the Pacific in 1957 and 1958, and many of the ideas of the meteorological-oceanographic interactions in this extensive "experiment" of nature.

In this way, a scientist dealing with one aspect of variations of the ocean or atmosphere will have at his beck a volume of broadening data and provocative thought from other disciplinary approaches to these common problems.

The Editors feel that the product of this Symposium is not the ripened fruit, plucked from a mature and stately tree of knowledge; but, rather, the basic stuff, the fertilizer, food, trace elements and pollens by which seedlings struggling in many gardens, will thrive, hybridize, fruit and generate new capabilities.

In this summary, The Editors will not dwell long on the detailed evidence, but rather will attempt to assemble the broad picture of what took place, and to extract the advice and guidance expressed by the participants anent the development of inquiries, prescient of the causes of variations in the atmosphere and oceans of this planet.

The climatological and oceanographic events of 1957-58 are relatively describable, but the large-scale interrelationships of these events are by no means clear. That is to say that there is only *some* understanding of the relationships that enables one "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' " (Revelle, page 195).

The Editors are in hearty agreement with Mr. Namias, who, in one of the less formal of his many illuminative statements in this Symposium stated: "It is certainly much more complex than I ever dreamed."

THE EVIDENCE

The year 1957 terminated a remarkable and unprecedentedly monotonous decade (Isaacs, Introductory Statement), which involved low temperatures and high northerly winds in the coastal eastern North Pacific—(Reid, Figs. 74 and 75); a low stand of sea level (Stewart, Fig. 101); warmer conditions in the Western Pacific (Takenouti, Figs. 52 and 66); possibly cold conditions in the Equatorial Pacific (Murphy, Figs. 36-37) and in Hawaii.

Small perturbations are common in all regions of the Pacific, and some sequence of changes, at various places in the Pacific could undoubtedly be traced back in time from the year 1957 for any period of years for which adequate data were available. However, the widespread concerted changes in the Pacific in 1957-58 suggest, that at this time we look no further back than the onset of some conspicuous and widespread change that compels inquiry as to its being one of the early links that was forged in the chain of occurrences.

From the evidence presented in this Symposium, it appears that the first strokes occurred in 1956, perhaps as early as July of that year.

By July 1956 the temperatures at Christmas Island, almost on the Equator (Murphy, Fig. 37) had started on a departure from the previous seasonal record, a departure that was to be accentuated through the events of 1957-58. In the same month there began rapid and vacillating shifts in the axis of the Kuroshio quite unlike the previous record (Takenouti, Figs. 52, 54, 63). In August 1956, the sea temperatures off the Baja California Coast rose sharply without an associated decrease in wind velocity (Reid, Fig. 74), and by October a conspicuous anomaly existed in the meteorological conditions of the Central Pacific (Fig. 177, kindly supplied by Roden), quite unlike previous autumns of the decade.

Little information for the autumn of 1956 was presented in this Symposium, and the Editors have examined little outside of that presented. However, by November 1956 (when the Mariposa-Monterey observations were started) the equatorial crossings displayed a strong warming in the north Central Equatorial Pacific (Murphy, Fig. 36). This trend was well developed by January 1957 (Murphy, Fig. 14), and was associated with a drop in the trade winds by this time (Namias, Fig. 4; Sette, Fig. 159).

It is interesting to conjecture that the sudden decrease in the trades from their high spring level occurred in the summer of 1956, resulting in a sudden decrease in east to west flow (Revelle, page 203), which was associated with the disturbances in the Kuroshio and the California Current, similar to that suggested by events in 1957—Murphy (page 47).

These events of late 1956 can only be considered premonitory, however, without a clearly discernible connection with the later developments.

The first act of 1957 opened in the winter of 1956-57, with the occurrence of water much warmer than normal over much of the North Pacific (Murphy, Figs. 14, 37, 38; Reid, Fig. 74) and on the Peruvian Coast (Wooster, page 43), and a surprising drop in the velocity of the westerlies of the North Pacific (Namias, Fig. 4).

During this period, conditions remained much as before in the Gulf of Alaska, with possible evidence of some increased northerly flow into the Gulf as early as November 1956 (Fofonoff, Fig. 88).

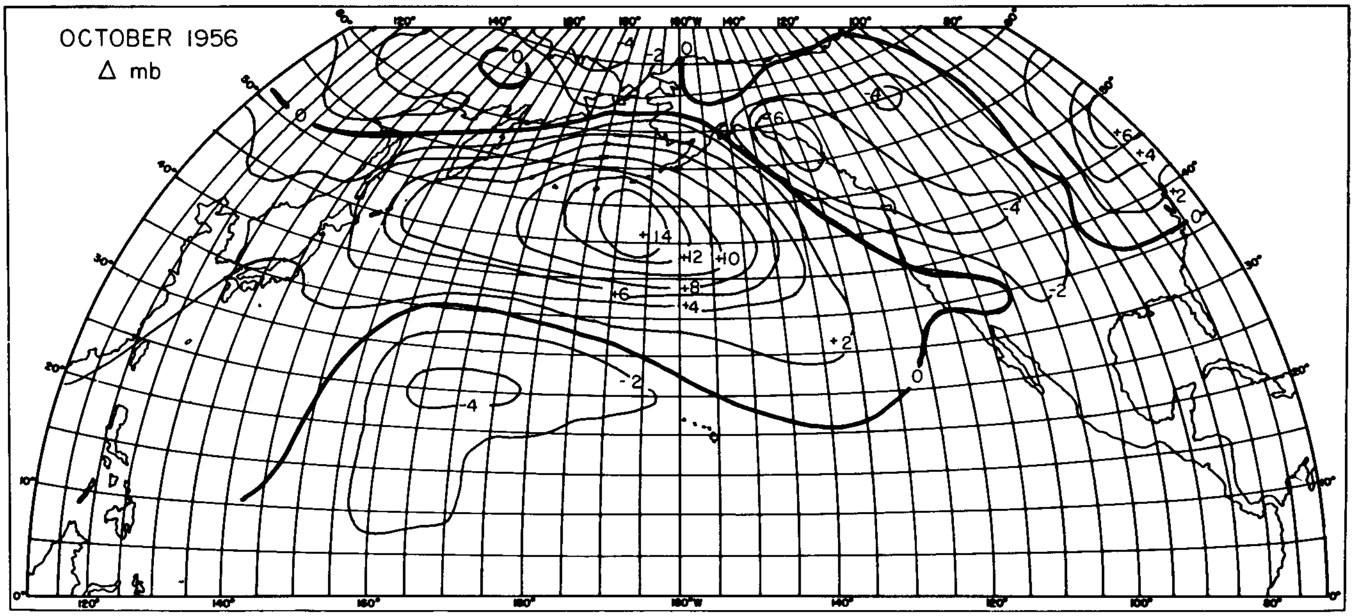

FIGURE 177. Sea level atmospheric pressure anomaly (Δ mb) in the North Pacific in October 1956.

By January and February 1957 sea level along the California Coast was rising at its greatest rate since 1941 (Stewart, Fig. 101), starting earlier and rising somewhat faster in the north than in the south (Stewart, Figs. 90 to 99).

During this period (winter of 1956-57), the North Pacific low was weak and to the west of its usual position and an actual high-pressure area occupied most of the eastern North Pacific. Actual south to north winds existed in the central North Pacific. This condition can be described as a very extensive high-pressure anomaly over the North Pacific with a low anomaly over the temperate Central Pacific (Namias, Fig. 4). Roden's December 1956 chart (Fig. 174) shows the extremely anomalous conditions in that month. These 1956-57 winter conditions probably were a major phase in the sequence of events.

By the spring of 1957, the anomalous high pressure over the central North Pacific had given way to a weak low anomaly, which slowly developed and moved eastward across the Pacific, reaching its greatest and unprecedented development in the winter of 1957-58 about 1000 miles off the Pacific Coast of the United States and arriving at the California Coast in the spring of 1958 (Namias).

The anomalous low appears to be associated with and preceded by abnormally high sea temperatures, and its wake appears to be associated with abnormally low sea temperatures (Namias, p. 31; Murphy, pp. 47 to 51).

The maximum of the abnormal coastal temperatures off the Pacific Coast of North America and the maximum of the abnormally high stand of sea level appear to be concurrent with this maximum development of the low anomaly rather than with its arrival on the California Coast (Stewart, Fig. 100).

In the meantime, the biota had been affected by these events. As early as the spring of 1957, there was evidence of a northward encroachment of warm water species of fish and possibly a retreat of the northern species along the California Coast (Radovich, p. 163) (Sette, Fig. 167).

This trend intensified over the year and into 1958, with southern fish migrating even into Alaska waters (Radivoch, p. 163).

Among the planktonic forms, warm water phytoplankton made its appearance off Southern California early in 1957. The tropical nature of the flora intensified until, in December 1957 and January 1958, the population was essentially tropical, with some tropical species extending to Monterey by March 1958 (Balech, Fig. 115).

A similar northward extension of warm water zooplankton occurred in the winter and spring of 1957-58. Although Central Pacific species apparently did not encroach on the California Current from the west, southern species extended at least as far as northern California close to the coast (Brinton, Berner).

Other evidence, such as the dynamic height and the movement of drift bottles (Reid, Figs. 81 and 82), indicated the development of a strong narrow countercurrent along the California Coast during the winter of 1958, although no countercurrent was directly observed in a drogue survey in March 1958.[1]

The pattern of spawning of sardines during 1958 was considerably altered, with spawning taking place both farther north and earlier than at any time in the previous decade (Ahlstrom). The same alteration was

[1] At the time of this Symposium, it was thought that the countercurrent had been missed by the direct survey, as there is evidence that the surface countercurrent ceases in or before March. This surmise probably was correct because similar measurements in January 1959 found a well-developed countercurrent.

true for the spiny lobster in 1957, as compared with the previous decade (Johnson).

The readers are referred to the individual presentations for details of the events, which have been summarized above.

It appears, however, that the onset of these changes was very abrupt. An inspection of data taken by M/V *Manning* in September 1956 shows no recognizably abnormal conditions in the north Central Pacific.

The unusual oceanographic and meteorological conditions must have developed by the winter of 1956-57. *No anomaly as great or as extensive as that of the winter sea temperatures in this region appears in the entire reported subsequent sequence.*

MODEL OF EVENTS

In the short time available to them for the general discussion, the participants in the Symposium attempted to erect simple models, hypotheses, or "straw men" to examine as possible *modi operandi* of the events.

These straw men were presented and discussed.

In Straw man I (Revelle) the thesis was that the thick globule of warm water that occupies the Central Pacific had simply thinned and spread out as a result of decreased winds and currents.

In Straw man II (Isaacs) the supposition was, in brief, that the Alaska Gyre had expanded in the fall and winter of 1957-58.

In Straw man III (Munk) the change is portrayed as a pair of anomalous cyclonic cells symmetrical about the Equator.

In their more prolonged (even if less profound) contemplation of the evidence, the Editors have been impressed with the probable veracity of *all three* of these models. There seems little doubt but that each of these mechanisms did indeed operate. Elements from all are necessary to explain events, though they probably are not sufficient. It remains to elaborate somewhat on the reasons for this Editors' opinion, and to point out some lacks in both evidence and theory.

Straw man I. Postulation of a warm water globule or lens in the center of the ocean surrounded by colder water implies a higher stand of water in mid-ocean than at its margin. This implies, further, a circulation around the margin of the lens clockwise in the Northern Hemisphere, counter-clockwise in the Southern Hemisphere. The thinning out of this lens and the slackening of this circulation would be necessary associated conditions. With slackened circulation, a lesser quantity of cool water from the north would be carried southward on the east limb of the gyre, and a lesser quantity of warm water northward in the west limb of the gyre. Anomalous thinning of the lens then would account for the anomalous warm surface waters of the Eastern Pacific and anomalous cold water in the Western Pacific, consonant with the inverse relation between temperature on the east and west side of the ocean.

The question is, did the lens flatten out, becoming thinner in the middle and thicker around the edges? Unfortunately, there is a paucity of sub-surface data with necessary time-continuity in the Central Pacific. However, there is a suggestion of thickening of the Eckman Layer of the tropical North Pacific (Murphy, Figs. 39 and 40) and there is a suggestion of deepening of the Eckman Layer at the one part of the lens margin, that is, in the Gulf of Alaska (Fofonoff, Fig. 86 and text). Although this evidence is somewhat sparse, it tends to support the model set up in Straw man I.

Straw man II. In many ways Straw man II resembles Straw men I and III, but places emphasis on changes in the northern gyres of the North Pacific, rather than upon changes in the central gyre, as the primary changes. Despite the fact that the meteorological anomalies were more intense in the region of the northern gyre, this distinction is perhaps artificial.

The original postulation of Straw man II was concerned only with the expansion of the Alaska Gyre under the influence of the cyclonic wind anomaly of the winter of 1957-58.

Extending this thesis back to the winter of 1956-57, Straw man II also must suppose a contraction of this gyre under the influence of the considerable anticyclonic wind anomaly of that season.

The assumed expansion and contraction of this gyre are supported by the sequence of temperature change in the central and eastern North Pacific, that is, by the observed time sequence and location of cold and warm anomalies.

Drift bottle results furnish direct evidence of the validity of the model of Straw man II. That is, they support it, if we may suppose that the trajectory (from "Papa") shown in figure 88 for the August 1956 releases, represent conditions that were normal for the previous decade, and that trajectories of bottles released from Ocean Station "Papa" during 1957 represent the changed condition.[1] According to this supposition, the "normal" split was at about the latitude of "Papa" and shifted south thereafter. Whether this represents evidence of a shift in the axis of the West Wind Drift, or whether the current axis remained at the same latitude but the split occurred farther south within the current, remains a question unresolved from this evidence.

Straw man III. In drawing inference from Straw men I and II, we have already indicated that separate gyres in the North and South Pacific must be recognized in any model. Munk extends this to include Namias' (page 31) recognition of similar atmospheric circulation in the two hemispheres and his supposition of some teleconnection between the two, such that changes in one hemisphere must necessarily involve changes of a similar nature in the other hemisphere. In a steady state, some transfer of energy takes place and if there is a change it should be evidenced in both hemispheres. The evidence presented by both Takenouti and Wooster support the idea that changes as large as those noted in the North Pacific are indeed accompanied by similar changes in the South

[1] The release of November 1956 seems also to represent the changed condition,—somewhat too early to fit the time sequence of other events.

Pacific, although the mechanism of the teleconnection remains unexplained.

While all three models, or at least elements of each, have their value in searching for the inter-relationships of some of the events in 1957 and 1958, they fail to bear on some of the other notable events.

Principal among these is the eastern North Pacific coastal countercurrent. That it was spectacularly stronger in 1957 and 1958 than in the immediately preceding years is eloquently asserted by the northward extension of the phytoplankton, and certain members at least, of the zooplankton community. Indeed, from the biological evidence, it appears that the countercurrent change was more vital in environmental change than temperature itself, although the latter undoubtedly increased the survival of the warm water forms north of their usual range. If an understanding of the physics of the ocean is to be of value in understanding the environmental effects on marine life, transient flows, and, above all, countercurrents must be taken into account.

Another area hardly touched by the models is the northernmost part of the North Pacific. If, as in Straw man I or Straw man III, a major change occurred in the central gyre of the North Pacific, it is reasonable to suppose that there must be associated changes in the Alaska gyre and the Bering Sea gyre with its strong western limb known as the Oyashio. Although Straw man II recognizes a connection between the central gyre and the Alaska Gyre, there was very little evidence adduced as to the changes in the Bering Sea, nor any postulated in the models.

A third lack is an hypothesis as to what event could have preceded and presumably triggered off the major changes in atmosphere and ocean. One of the Editors (Isaacs), inspired by Namias' belief that feed-back from anomalous surface water temperatures to the atmosphere might have important effects on the subsequent atmospheric circulation, noted in January and February 1957 (Murphy, Figs. 14 and 15) a large pool of anomalously warm water in the north Central Pacific at a time when it would be most likely to exercise profound influence on subsequent occurrences. That is to say that warm water at a high latitude in winter must release to the atmosphere vast quantities of heat and moisture.

Evidence as to how this anomalous pool of warm water came into being is rather tenuous, but there are a few indications. Anomalously warm water existed in the central Equatorial Pacific as early as the summer of 1956 (Summary, p. 211). What mechanism could translate this anomaly northward by the following winter? The West Wind Drift could hardly be expected to have given rise to the abrupt increase in temperature extending to the Aleutians. As already discussed, there seems to have been a deepening of the Ekman Layer in the Gulf of Alaska and a thinning in the tropical North Pacific at or prior to January 1957. On the other hand, (Namias, p. 31) one can conceive of an anomalous flow as rapid baroclinic response to the anomalously high pressure level in the North Pacific (Fig. 174). The anomalously warm water in February 1957 (Murphy, Fig. 15) occupies the area under the anomalously high pressure in this season (Namias, Fig. 4) and is to the *right* of the anomalous wind as would be expected if this were a transport anomaly.

Whether or not this is the cause of the appearance of this anomalously warm water pool at high latitudes, there is no question that the pool existed by January 1957. All subsequent major meteorological and oceanographic events appear to follow this initial excitation in describable and logical (if not calculable) order. Indeed we can consider this sudden dislocation of a heat source to have been the *conditio sine qua non* of the events.

A series of weak cyclonic disturbances appears to have followed the warm water as it moved easterly across the Pacific. These disturbances increased in intensity as they reached the warm areas and appear to have become relatively stationary there. This gave rise to a cold wake to the west arising either from mixing, divergence near the cyclonic circulation or, most probably, both. The anomalous high pressure was eroded by these disturbances and the meteorological process culminated in the great anomalous low pressure area that developed in the winter of 1957-58 (Namias, Fig. 8) in the eastern North Pacific.

This apparently regular sequence of events over the period from summer 1957 to spring 1958, Namias believes, might be explained by long period interactions wherein the cyclone-spawning, steering and developing mechanism (atmospheric planetary waves) are regulated both by normal climatological factors and abnormal sea surface temperatures; the latter being set up and slowly modified by the abnormal wind systems.

Thus the meteorological situation in the winter of 1956-57 could have been the triggering process for the ensuing events of the main sequences. This view on the onset of the change also is in harmony with Revelle's Straw man I in recognizing a mechanism for the slackening of atmospheric and oceanic circulation associated with thinning of the central warm water lense.

Having reviewed the proposed models, the Editors will summarize what, in their opinion, are some of the promising leads and conspicuous deficiencies in the research on the oceans and atmosphere that emerge from the discussions.

The following is not intended to be all-inclusive, and much will be lost if this is taken to be a complete résumé of the many perceptive comments, suggestions and thoughts presented by the participants throughout their foregoing contributions and discussions.

1. Perspective of Oceanographic and Climatic Changes

It appears to the Editors that one of the most valuable results of the Symposium is to have pointed out clearly and unequivocally, and from a wide range of evidence, that locally observed changes in ocean conditions, marine fauna, fisheries success, weather, etc., are often the demonstrable result of processes acting over vast areas. In the case of local Pacific conditions, the changes obviously often are only a part of changes involving the entire North Pacific if not the entire Pacific or the entire planet.

It appears that this realization should emancipate many provincial marine investigations and stimulate much thought and inquiry into these vast and critical events that so profoundly influence the local areas of the Pacific.

This is to say, for example, that a basic understanding and subsequent basic forecasting of the fluctuations of a coastal fishery, probably can be best achieved by a *thoughtfully limited* study of the entire ocean, in addition to concentrated concern with the immediate area of the fishery.

The same comment may be appropriately applied to waste disposal at sea, beach erosion, coastal agriculture and a variety of investigations directed toward practical ends.

2. *Transient Response of the Ocean to Winds*

Much of the evidence points to rapid response of water under the influence of transient winds. Indeed, the triggering of the entire phenomenon appears to have resulted from motions of this nature. An understanding of such transient response of the ocean does not exist. From the discussion it is evident that the participants felt that the problem should be attacked by:

a. Better and more direct measurements of transient flows and of the associated factors.

b. An adequate model or theory, such as an extension of the Veronis and Stommel theory to deal with shear stresses on the Ekman Layer.

3. *Thermal and Hygrothermal Interaction of Atmosphere and Ocean*

The sequence of atmospheric events may be influenced substantially by feedback of thermal and hygrothermal energy from translocated heat sources. Both meteorology and oceanography would be greatly advanced by a better theory and understanding of these interactions. In the body of these proceedings Namias, Stommel, Charney and others outline possible approaches.

Pending this, there clearly appears the less satisfying but nonetheless important possibility of "trajectory forecasting" for ocean conditions. That is, in the case of the ocean, because of the large areas and long time scale of its changes, empirical studies may permit forecasting of certain changes in areas where the initial excitation is remote.

4. *Persistence in the Previous Decade*

A review of the evidence (Isaacs, Fig. 2; Stewart, Fig. 101; Reid, Fig. 74; and, appended to this Summary, Fig. 178), emphasizes that the period of 1947-56 displayed a uniform monotony of conditions in at least the eastern North Pacific that is scarcely suggested by any similar series of years in this century. The record of serial salinities at Scripps Pier (Fig. 178) indicates in 1947 a cessation of some variations of large amplitude and many months persistence that had occurred in almost unbroken sequence since 1916.

Although the change in 1957-58 was conspicuous by any standards, it was not remarkably different from year-to-year changes in the fifty years previous to 1947, during which period, hot and cold years, and years with other types of variations, were common. Indeed, looking at the events of 1957-58, these climatic and oceanographic changes are not so remarkable *as is the monotony of the decade that they terminated.*

It appears from a variety of data that some process affecting *at least* the entire West Coast of North America ceased to operate about 1946-47, and it, or some other process, resumed operation in 1957.

The persistent period of conditions 1946-1956 holds alarming implications for certain types of engineering studies, where the "normal" is determined by brief surveys, for the design and guidance of such relatively irrevocable acts as locating sewage outfalls, disposing of atomic waste in the sea, jetty construction, etc.

5. *Coastal Currents and Countercurrents*

It appears that much remains to be discovered and understood about the nature and stability of coastal currents and countercurrents. Elements of this prob-

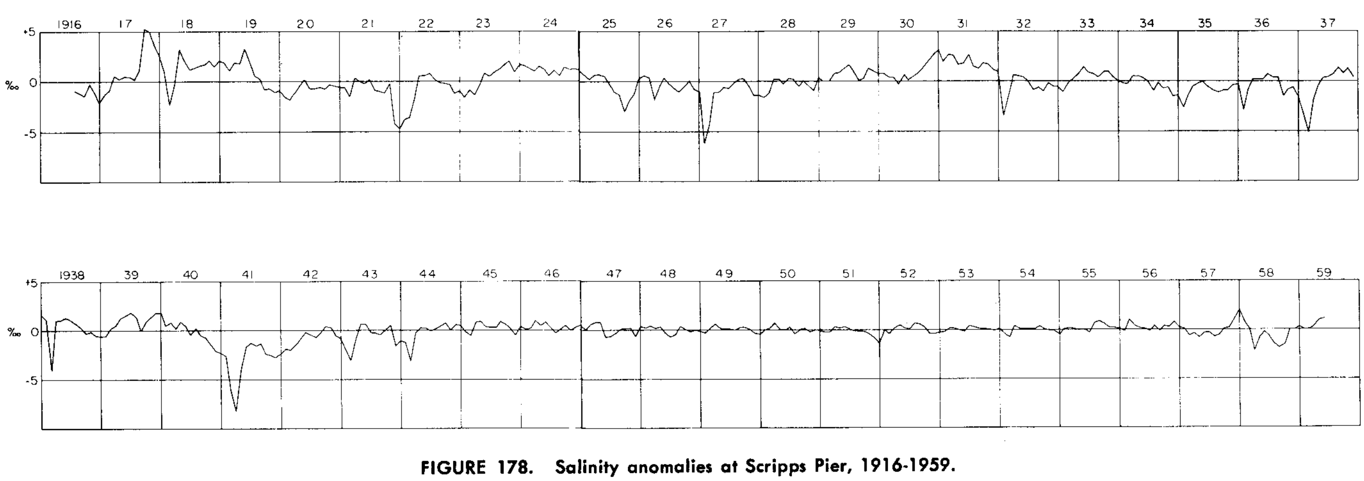

FIGURE 178. Salinity anomalies at Scripps Pier, 1916-1959.

lem were emphasized in the Symposium and included the following needs:

a. An understanding of the influence of coastal configuration on coastal currents, particularly the divergence of these currents from the coast.

b. An understanding of the factors controlling coastal countercurrents, their onset and strength.

c. A good description of and a coherent theory of countercurrents, particularly concerning their apparently remarkable narrowness.

d. Investigation of the effect of countercurrents in innoculating coastal water masses with organisms from sources other than those upstream, in the dominant current direction.

6. *Teleconnections*

One of the most provocative documentations of the Symposium was that of the apparent relationships between oceanic and atmospheric changes between the North and South Pacific. The apparently large changes in the North and South Pacific compared with the reportedly small change in the North Atlantic perhaps can argue for a teleconnection via the oceans rather than via the atmosphere in 1957.

Perhaps the expansion of a warm water lense in one hemisphere must of necessity invoke changes across the Equator. Certainly this teleconnection is an intriguing and important matter and should be understood.

7. *Biological Indicators*

It was abundantly evident from this Symposium that the strongest and most spectacular evidence of marked change in the coastal countercurrent and of the absence of encroachment of mid-Pacific water toward the coast came from biological, rather than physical, observations. The Editors are inclined to think that without this evidence quite a different view of what had taken place in the oceanic circulation in the Eastern Pacific would have prevailed at the Symposium.

The Editors feel that a widening of the taxonomic scope of indicator assemblages might lead to even more assured conclusions. This appears especially true in the case of zooplankton, where the evidence consisted of a few members of only two taxonomic groups. Surely examinations of entire assemblages or faunae of water masses should lead to more certain recognition of water mass identity and provide a much more powerful tool for following water mass movements and identifying the mixings, both horizontal and vertical, between water masses. To be sure, such comprehensive treatment of plankton samples is prohibitively time-consuming if done by traditional methods. There is opportunity here for some inspired planktonologist to devise a revolutionary time-saving system for plankton analysis.

8. *Environmental Requirements*

Much weight has had to be placed on the role of temperature as a limiting quality of the environment; and, for practically all plankton organisms, our notions about the point at which temperature becomes limiting have been derived from field observations by drawing conclusions from the correspondence of isotherms with the boundaries of the distribution of the organisms in some horizontal plane.

For many plankton organisms, if not most, the same isotherms do not coincide with their boundaries in the vertical plane. This apparent absurdity invites coordinated field and laboratory experiments to discover whether or not the real limiting condition is not imposed by some other physical, chemical or biotic quality or qualities of the environment and also at what stage of life it or they operate.

9. *Biological and Physical Investigation of Dynamics in the Mixed Layer*

Some of the evidence, as for instance drift bottle experiments, suggests that important quasi-systematic water movements occur within the mixed layer and that their nature is such as to cause important horizontal displacements (and, perhaps, "sorting") of vertically migrating organisms.

It appears to the Editors that these processes are important and should be attacked by a concerted study by physicists and biologists using direct measurements, as from drogues, and indirect inferences from movement of plankton organism, whose displacements, in a sense, integrate the movement in the mixed layer and the upper layers in general.

This suggestion is only exemplary of a large number of important and exciting possible investigations into the physical implications of the habits of marine organisms and the corollary physical processes in the ocean to be learned from the distribution of the organisms.

10. *General Comment on the Application of Dynamical Oceanography to Transient Conditions and Boundary Phenomena*

The previous findings and recommendations tend to point out a conspicuous disparity between the conceptual and descriptive understanding of transient conditions and boundary conditions of the ocean on the one hand, and the related dynamical understanding on the other.

It was apparent that the understanding of events was almost solely descriptive and that the contribution of dynamical theory to the understanding of the changes in the oceans, the physics and physical relationship with the atmosphere, coasts, and between currents was almost wholly lacking.

Indeed, the Editors felt that serious questions were raised as to whether or not accepted steady-state dynamical theory sheds *any* light on the transient and boundary dynamics of the real ocean, and whether or not the actual events are accommodated by *any* existing dynamical theory.

Important as are description and conceptual approaches, they soon reach strict limitations in the absence of that essential interplay with evolving dynamical theory, and their data then tend to appear fragmentary, uncritical and anachronistic, especially when considered as tests of the veracity of a variety of tentatively-proposed mechanisms of vastly differing requirements.

It appears to the Editors that this deficiency can be remedied and basic understanding acquired only by the true concert of observation, abstract thought, formulation of hypotheses, and tests by appropriate critical observation.

Several approaches to such a concerted attack on transient and boundary dynamics were suggested in the Symposium. *It appears to the Editors that some unifying approach must be selected and followed through by an adequate effort.*

The Editors are not suggesting a regimentation of the field, but rather a definition by some proper body, (a small symposium perhaps), to interest those in the field toward a definite direction of attack on the vital and unresolved basic problems of the transient and boundary dynamics of the ocean.

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