THE LONG TERM HISTORICAL RECORD OF METEOROLOGICAL, OCEANOGRAPHIC AND BIOLOGICAL DATA

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The title under which this contribution appears is much too comprehensive. The title not only takes in the whole field of meteorology, oceanography and biology of the sea, but also the multifold interrelated events occurring in these fields with the passage of time. I shall only be able to give you some samples of the kinds of records available, and of some simply derived time series extracted from these records. After a quick run through these to give an idea of the information available as long-term records, I shall propose some elements of a model that might relate some of the observed events, providing the assumptions involved are valid.

The group in our laboratory consisting of Ted Saur, Oceanographer, Larry Eber, Meteorologist, and myself are thinking of the events affecting the fisheries as originating in the atmospheric circulation, operating through their direct effects on the properties of sea water and indirectly by modifying the oceanic circulation, and these, in turn, affecting our marine animal populations, both directly and indirectly through the chain of plant populations. So we have a very long chain of events, each link of which is complex in itself.

As a first approximation, it will be helpful to simplify the historical picture by centering attention on only the major and dominant events. So I would like to ask you to ignore, if you can, the small-scale, short-period events which are recorded in time-series graphs and center attention particularly on the very gross features.

Our first approach in studying the atmospheric circulation was to construct a simplified and integrated description of the fluctuations of those parts of the wind system which we thought might have major effects on the ocean circulation. For source data we first discovered that Mr. Namias' group was producing monthly mean maps of air pressure at sea level, which summarize a great deal of the significant meteorological picture in monthly time units, and of which his group kindly furnished us copies. This series dates from the immediate postwar years. Later, there was found in the files of the U.S. Weather Bureau another series of such maps which extends back to 1898, a copy of which was kindly furnished to us by the Bureau. We have put the first approximately thirty years of these maps on the shelf, and have worked only on the last thirty years.

To reduce the information to a simple numerical form, we have selected pairs of fixed points distributed over the North Pacific Ocean in such a manner that the difference in pressure between the members of each pair will give pressure gradient over what we think are the major elements of the ocean circulation

system, adjusting a little so that they also tend to be, as nearly as possible, along the axis of the major winds of the North Pacific. As may be seen in figure 158, this arrangement provides a series along the axis of the North Pacific westerlies, a series in the northwesterly winds along the North American coast, a series in the northeast trades, and a series over the Kuroshio Current. These are intended to describe the major gyral system over the North Pacific Ocean. Additional pairs are provided to describe the tributary circulation: the gyre in the Gulf of Alaska and the gyre in the Bering Sea discharging as the Oyashio past Kamchatka.

The members of these point-pairs have been separated by a distance such that, according to geostrophic approximation, the difference in millibars is very nearly equal to the geostrophic wind speed in meters per second; and if you wish to think in nautical terms, you may multiply the number by two to get knots, though the knots will be a little undersized. In the trade wind area, in order to confine the pairs to the area in which observations were somewhat better and to the area in which the gradient lies, we halved the distance between paired points, and have multiplied the number of millibars by two in constructing the time series.

In figure 159 is a sample of some of the time series resulting from this treatment. In this figure we have combined months into seasons, as Mr. Namias has, using December, January and February for winter and plotting the value against the year that January falls in, so that the point at 1930 represents the mean of December of 1929, and January and February of 1930. We have drawn the mean line for the thirty-year period, so if you wish to think of the fluctuations as anomalies, you may refer them to the mean line instead of to the actual scale. However, the vertical scale is in millibars of pressure difference between points spaced at such a distance apart that the same numerical value expresses the geostrophic wind speed in meters per minute, or if multiplied by two, the geostrophic wind speed approximately in knots. The gap in the curves at about the middle of the thirty-two-year period is the result of disruption of the marine meteorological observations in various parts of the Pacific during the war years. The widest gap is between 1939 and 1943 in the tradewind series (lower period of figure 159).

To identify the four curves in figure 159 we may refer to the numbers by which the point pairs are designated in figure 158. The "Westerly" curve is the mean of pairs three to six, the "Trade" curve is the mean of 11 to 16, the "Oyashio" is the mean of
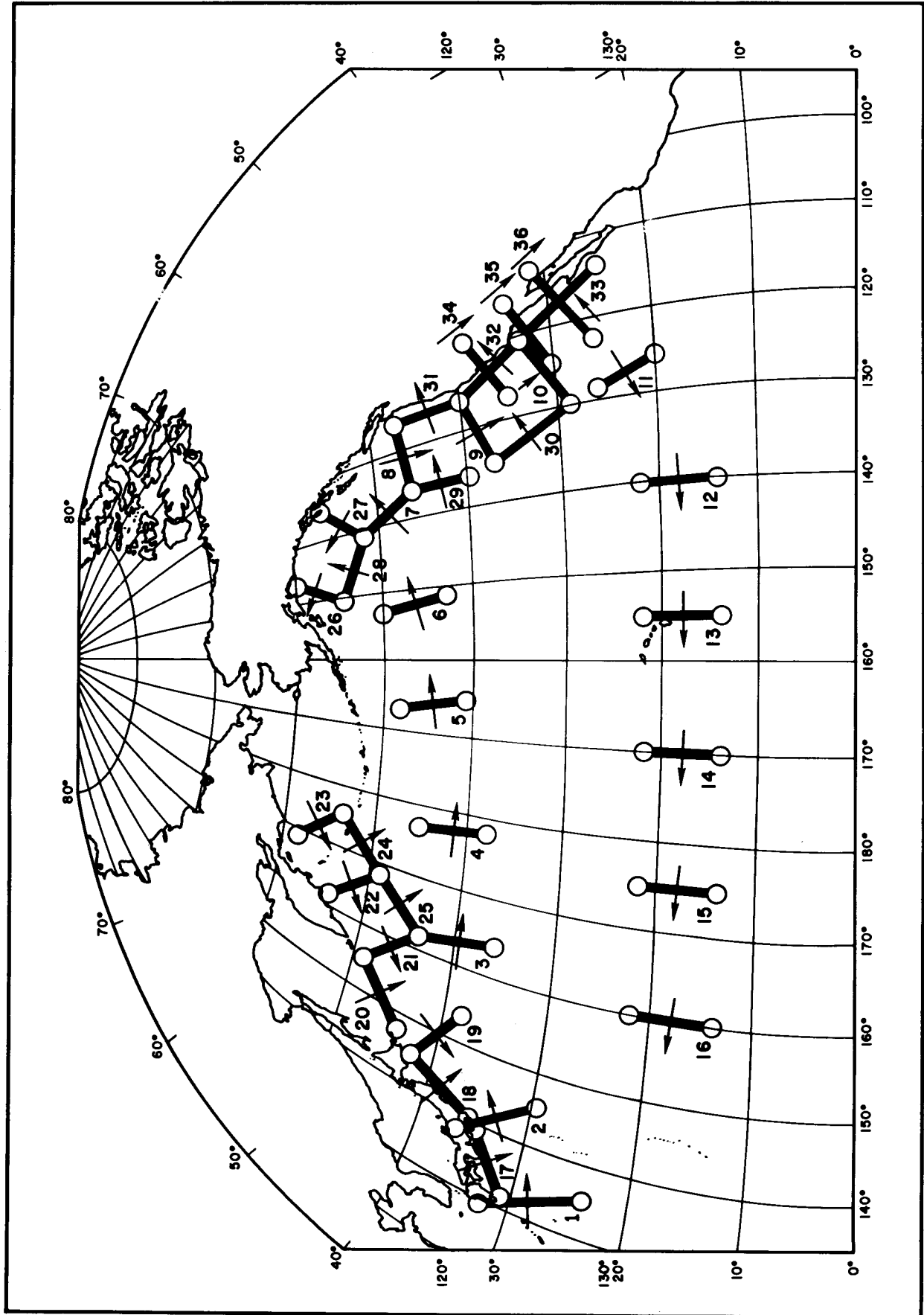
FIGURE 158. Location chart showing points, marked by open circles, between which pressure differences were read in deriving wind indices. Arrows indicate the positive direction of the geostrophic wind component associated with each point pair.
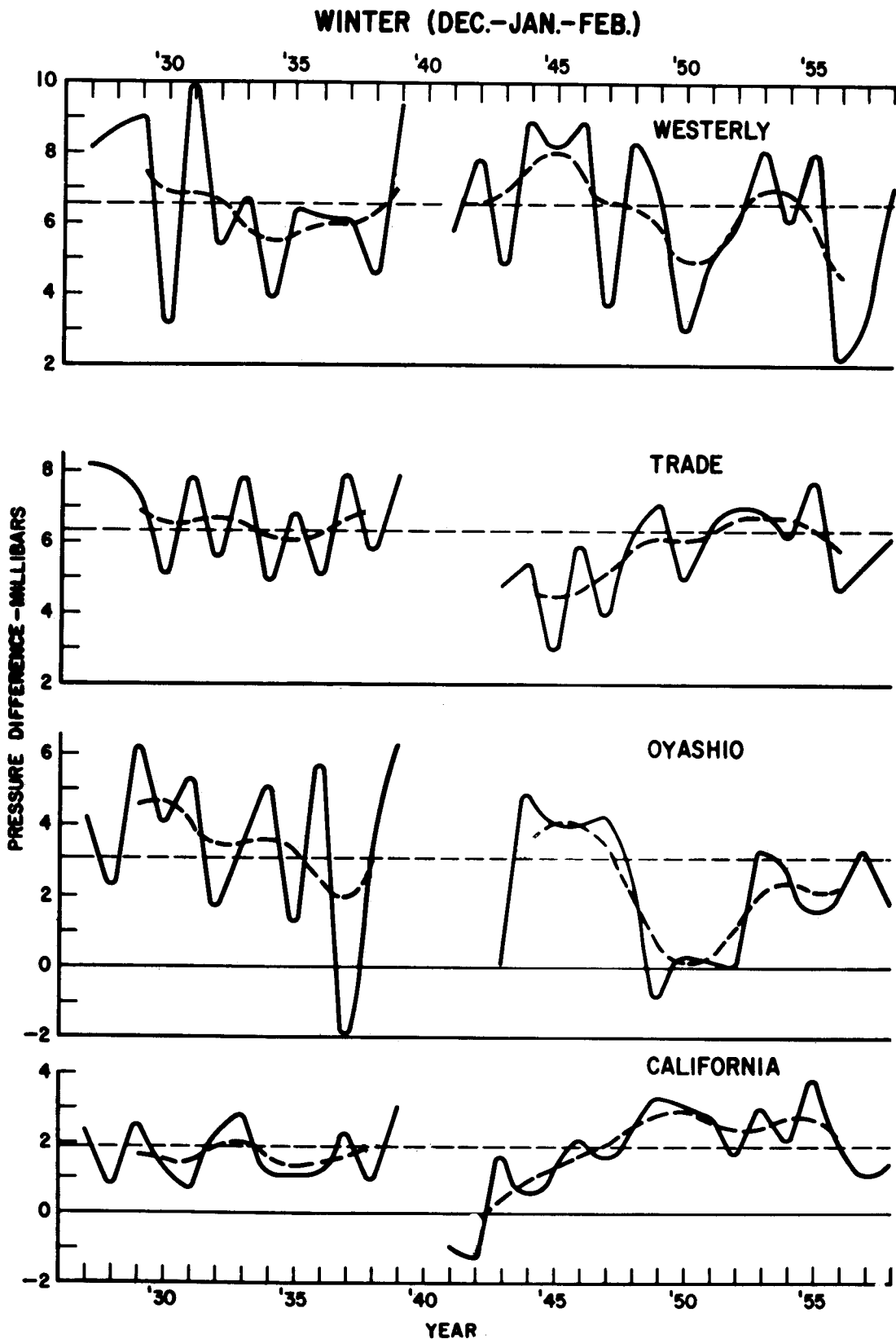
WINTER (DEC.-JAN.-FEB.)

FIGURE 159. Wind index values for westerly (locations 3 to 7), Trade (locations 11 to 16), Oyashio (locations 21 to 23) and California(locations 34 to 36) wind fields for the three winter months: December, January, and February, 1926 to 1958. Dashed line is an unweighted running average of three.

21 to 23 and the "California" is the mean of 34 to 36. The last series was originally to have been represented by eight to ten, but we found that these point pairs tended to straddle a portion of the dome of the semi-permanent North Pacific high cell too frequently to represent the axis of the prevailing coastal wind, so we shifted to the 34 to 36 set which gives us the winds flowing immediately along the coast.

With this sample of wind indices I wish to point out only two things: first, that we have very wide variations from year to year, for instance, the low point in the westerlies in 1956 has a value only a little above two, the high point (in 1931) has a value of nearly ten,—a variation of nearly five-fold in the three-month mean geostrophic wind. Second, that the variations do not appear to describe fluctuations around a constant level. We appear to have quite marked changes in levels. The winter westerlies have quite a long period of, let us say, weakness, in the middle 1930's, a period of strength in the middle 1940's followed by another period of weakness during most of the 1950's. In the coastal curve there has been a rather extended period of average winds during the 1930's up to the gap which ends with 1939, then a sudden change to a very low level in 1941 and 1942, but rising to a high level generally above the thirty-year mean for the decade beginning with 1947. Incidentally, this decade includes all of the period of the California Cooperative Oceanic Fishery Investigations suggesting that the entire period of these investigations, until 1957, has been during anomalously high winter northwesterlies.

Stewart: Would you say that the northerly components were strongest?

Sette: Not exactly northerly, but the component parallel to the coast and parallel to the California Current has been, generally speaking, above normal as measured by this particular field for a considerable number of years. You might say, offhand, it was from 1949 to 1957. Then our change in 1957 and 1958 reverts to lighter winds at about the level of the 1930's but not abnormally low,—though I should not use the terms "normal" or "abnormal," because there would be quite a difference in the "normal" depending upon which reference period is used, even with a reference period as long as thirty years.

Namias: Your point for the California coastal area in winter 1957-1958 corresponds directly to the pressure chart given in my figure 8, which gives the anomaly in respect to a normal which compares with the entire normal period you have. In other words, in this area the pressure anomaly is not far from the normal of a long period. A little way from the coast, however, and a little further north, the pressure anomaly is distinctly much below the normal.

Sette: Thank you. It is comforting to find that the index series, for the limited field it covers, is consistent with your analysis of the entire pressure field. The evidence of the larger negative anomaly farther offshore and to the north suggests that it may be advisable to expand the index series to include additional fields if these be critical to the ocean circulation.

With regard to oceanography, long-term records with continuity are lacking except for the observations of sea-surface temperatures taken at coastal stations along the margin of the sea, which may or may not represent the open ocean conditions and the tide-gage records of sea level height. These have already been discussed extensively during this Symposium. However, we shall refer to the sea temperatures at one of the coastal stations later in connection with the model proposed to relate events in the atmosphere, ocean and fisheries.

There is one other source of oceanographic data with time continuity and with considerable space continuity in the form of sea temperatures reported by U.S. merchant and navy ships. Although this source has been drawn upon to construct charts of the long-term average sea temperature (H.O. 225), it has not been exploited for the study of fluctuations and trends in the North Pacific Ocean. Our laboratory has acquired listings of these data and preliminary examination suggests that the density of observations is sufficiently high over much of the North Pacific to permit time series analyses. However, the amount of processing required for such use is formidable, and our work on these data is not yet far enough along to offer any results.

For the biological phenomena we are similarly embarrassed by lack of fully appropriate data. The results of large scale commercial fishing provide the only time series covering a relatively long period, and for many species the total annual catch is the only record that extends far enough into the past to be regarded as long-term. Unfortunately, the quantity of catch is sensitive to many things besides the weather and the sea; most important among these things are economic influences. For instance, in figures 160 and 161 giving the annual catch of several species, there is a dip in every curve in the early 1930's undoubtedly caused by the economic depression of that time.

Another thing to be noted, is that our largest California fisheries have relatively short histories. A large portion of the record reflects growth of the fishery rather than trend in abundance of fish. For the sardines the growth period extends from about 1915 to about 1935; the growth period for the tuna fishery for yellowfin and skipjack, from about 1920 to about 1945 or 1950.

The albacore, being the first species of tuna to be taken in quantities commercially, has a somewhat longer post-growth history. The fishery for this species began several years earlier than shown on the graph and had nearly passed through its early growth phase by 1915, the earliest year shown in the graph. After about a decade the albacore practically disappeared from the catch record. The negligible quantities reported between 1926 and 1938 did not have an economic cause. There was a very real failure of the fishermen to find the albacore in our Pacific coastal waters during this period. Some dozen or fifteen years later the albacore "came back." The fishermen again found them along the coast and we have had an albacore fishery continuously since then.
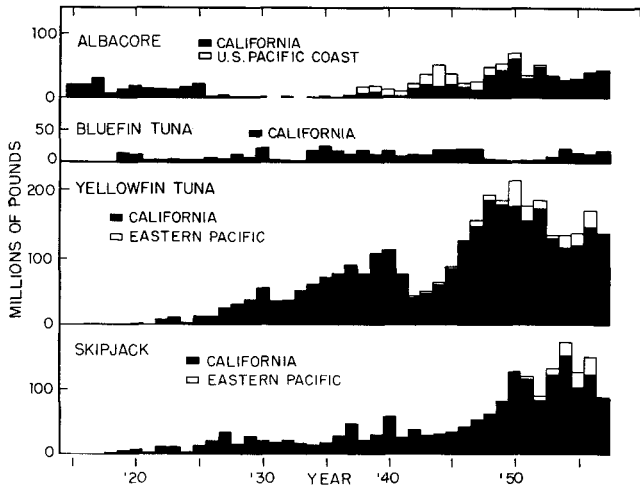
FIGURE 160. Total commercial catch, in millions of pounds, of the four principal coastal pelagic fish species of the Pacific Coast, 1916 to 1957.
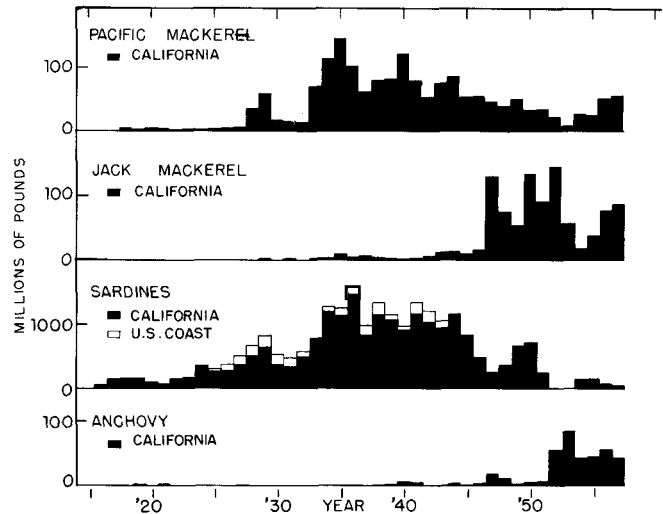
FIGURE 161. Total United States commercial catch, in millions of pounds, of the four principal coastal pelagic fish species of the Pacific Coast, 1916 to 1957.

Whereas the fishery before the albacore disappearance in 1928 was confined to the California coast, after their return in 1938 the catch north of California became very substantial. Whether or not the albacore was abundant along the coast of Oregon and Washington during the 1920's and 1930's is an interesting question. The statistical record only tells us that catches began to be made there in 1937 and rapidly increased thereafter. The absence of albacore from the catch record for Oregon and Washington prior to 1937 may mean that the albacore were absent, or it may mean only that there was no fishing for them along the coast of Oregon and Washington. The former alternative seems the more probable, because the coastal waters north of California were trolled for salmon for a considerable number of years before 1937, but not until that year did the salmon trollers begin to catch albacore in quantities large enough to appear in the statistical record. On the other hand, the decline and practical disappearance from the catch record after 1950 must reflect absence of albacore from the Oregon and Washington coastal waters, because at that time the albacore trolling methods were well developed and commercial concentrations of albacore would not have escaped the attention of the fishermen.

For another instance where some of the changes in the catch record probably reflect something real in nature, we may look at the bluefin tuna record. We think that the years when there is a low catch shown on the graph were years when there really were not many bluefin in Southern California waters. The tuna fleet during the thirty-year period regularly traversed these grounds and would not have passed by bluefin schools if they occurred in commercial quantities.

For the yellowfin tuna and the skipjack the fishing effort and the resulting catches are quite well documented for the years since 1933, thanks to the Inter-

American Tropical Tuna Commission. So instead of looking at the total catch record which is subject to economic influences, we can look at the catch per unit of effort given in figure 162. Inasmuch as economics in-
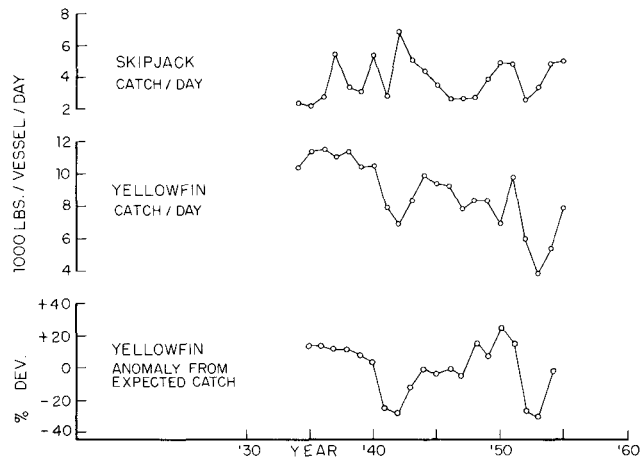
FIGURE 162. Annual mean catch per standard vessel per day of skipjack and yellowfin tuna by the California live-bait fishery and the anomaly from "expected catch" of yellowfin tuna, 1934 to 1955.

fluences the catch mainly through altering the amount of effort, these curves have had the effects of economics removed. They should show mainly the abundance or availability, or both. To reflect the uncertainty as to the relative influence of abundance and availability, I shall use the term "apparent abundance," and say that these are curves of fluctuations in apparent abundance. The plotted values are the catch per standard boat per day fishing derived from the experience of the tuna bait boat fleet and cover all of the geographical area of the eastern tropical Pacific Ocean fished by this fleet. Since fishing expanded through more

area during the period of the record, the statistics plotted in figure 162 may not refer strictly to the same tuna population or complex of populations year after year. Except for this qualification, they probably represent the apparent abundance in the eastern tropical Pacific.

According to Dr. Schaefer's studies, fishing on skipjack is so light and the size of the catch is so small in relation to the size of the population, that fishing itself has had no discernible effect on abundance. Accordingly, the skipjack curve probably represents, faithfully, the natural fluctuations in abundance.

For yellowfin this is not true. Fishing is sufficiently heavy to have a discernible effect on abundance and the curve for this species represents the effects of fishing as well as natural fluctuations. However, Dr. Schaefer has studied the density-dependent dynamics of the yellowfin population as fished since 1933 and, among other things, has computed the expected catch as a function of effort based on the relationship prevailing from 1935 through 1954. Scaling from his graph (Schaefer 1957, fig. 3) the actual catch and the expected catch values and expressing the deviations of the former from the latter as percentages, gives the lowermost curve in figure 162. This curve may be taken as reflecting the changes in yellowfin abundance due to natural causes, though of course some of the simplifying assumptions necessary in formulation of the equation for computing expected catch may have exerted some influence also.

The skipjack and yellowfin curves are shown together to see if there is a correspondence in the fluctuations of these two tuna species. Both are warm water species. Both have very similar feeding habits as far as we can tell. Both are found in the same general area. In fact the fishermen often catch both on the same trip. But there is hardly any correspondence in their fluctuations. Obviously the two species react differently to the environment, at least in respect to their abundance or availability, or both.

Figure 161 gives the annual catches for four species of fish—the Pacific mackerel, the jack mackerel, the sardine and the anchovy, which are grouped naturally from two aspects. They are all found more or less in this same region, all being what you might call neritic-pelagic fish, excepting possibly the jack mackerel, which is more oceanic—and they also are caught by the same fleet of boats.

Within this group the sardine has been quantitatively so dominant in the catch that it was necessary to reduce the plotting scale to one-tenth of that used for the other three species.

For this group we again have the initial growth periods, that of the sardine and Pacific mackerel ending in the middle 1930's. The outstanding event since then was the decline in the sardine catch in the middle 1940's. This species practically disappeared commercially from the Washington, Oregon and Northern California Coasts, leaving only the Southern California waters producing a catch. This was not caused by any economic event, but was a real change in abundance or availability or both. However, the recent and

sudden development of the jack mackerel and anchovy fisheries after the middle 1940's was a response to the economic crisis brought on by the dearth of sardines. The anchovy and the jack mackerel tended to partially replace some of the shortage of sardines and Pacific mackerel respectively in the economy of the fishing fleet and of the processing-distribution industry.

There is some difficulty in comparing fluctuations of catch in this group of curves. As before stated, the sardine supported such a large fishery in relation to the others, that it was placed on one-tenth of the scale of the others. For some purposes, a better way of comparing series occurring at different orders of magnitude, is to plot them on the logarithmic scale. This also has the advantage that the slopes of the lines representing the rates of proportionate increase or decrease are the same regardless of the level at which they occur. This has been done for Pacific and jack mackerel in figure 163 and for the sardine and anchovy in figure 164. It is very striking that all four curves converge at the same general level going from left to right on the charts. Although the catches of the four species began at different orders of magnitude, during the last several years all four have been caught in quantities having a similar order of magnitude. This may be an indication that the area in which the fishery takes place supports more anchovy and jack mackerel than
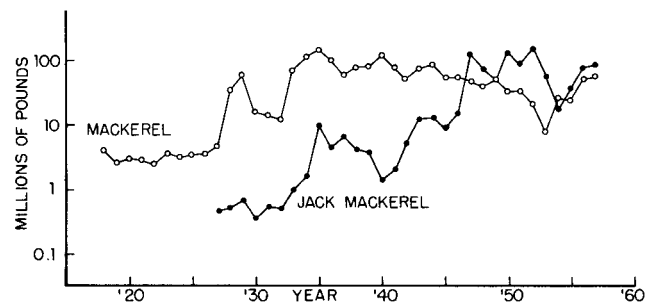

FIGURE 163. Logarithmic plot of the Pacific mackerel and the jack mackerel catches shown in figure 161.
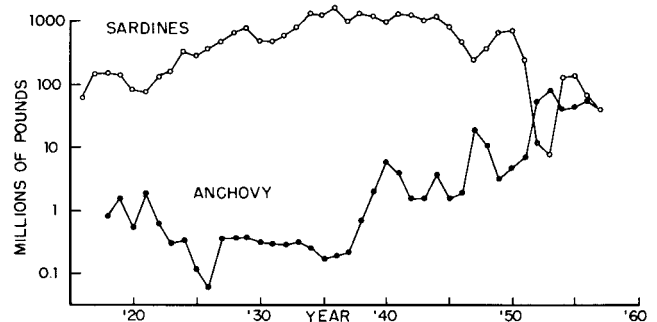

FIGURE 164. Logarithmic plot of the sardine and anchovy catches shown in figure 161.

we thought some years ago. Whether or not this is true, this group of curves serves to illustrate how the statistical record results from a blend of processes, some taking place in nature, as probably is true for the sudden failure of the sardine fishery and others result from man's technological and economic adjustments to such events. They serve in this Symposium mainly to point out or point up the complexities involved in attempting from the fishery record, to extract the natural events in the sea as distinct from man's response to such events.

To pass away from the California scene for just a moment, we should look for some northern fish, preferably some that are not complicated by anadromous habits, as is the salmon. The only one for which we have had a substantial fishery over a long-term period, is the pelagic herring. Figure 165 gives the historical record of the herring catch along the Pacific coast of North America from California to Western Alaska. The record runs from 1915 to 1957 and all of this is growth period. Even in recent years, particularly in British Columbia, additional fisheries have opened up. It is very difficult to ascribe much significance to the humps in the curve, as they mostly represent economic development of certain areas at different times and at different places. The oldest fishery is in Alaska. Most of the catch is Alaskan in the first half of the chart. In the middle years the Canadian fishery became substantial. The last area to be developed is in the northern portions of British Columbia.
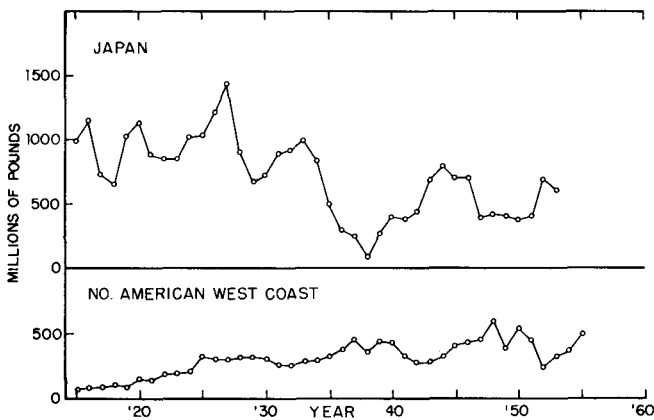

FIGURE 165. Total annual commercial catch, in millions of pounds, of Pacific herring by Japan, 1915 to 1953, and by Canada and the United States, 1915 to 1955.

The Japanese fishery, plotted on the same scale, has even a longer history. We can see, as far as catch is concerned, that there have been some large fluctuations, also changes of level. I do not know what they imply. Perhaps just as is true of our own fisheries, the Japanese herring catch is strongly influenced by economics. There is one thing that does seem to ring a bell, faintly at least, and that is after 1930 since our fishery in the Eastern Pacific reached most of its development, there seems to be an inverse correlation in the major changes between the Japanese and North American west coast fisheries. On the basis of these gross catch statistics, I would not attach any signifi-

cance to this, but I think it invites an effort to get some more refined data that would indicate whether those two series reflect economical or biological fluctuations, and whether they are, indeed, inversely correlated.

Takenouti: Yes, the big drop (in the 1930's) was biological in Japan. Canning is very important in Hokaido and fishermen always do their best to catch as much herring as possible. Therefore, the decline is biological, not economic. Even through the war there was no change in fishing effort.

Davies: Is it possible to bring the element of catch per unit of effort into these series?

Sette: Yes, it is. I have been working on the Alaska records but am not far along with it. British Columbia people brought effort into their analyses for a while, but later became convinced that it was not giving them an index of population size as distinct from availability, so they lost interest, I understand, from speaking to Dr. Taylor. I think it would be good if they would resume these analyses to give the kind of records which might be analyzed to reflect the elements of abundance and availability.

I would like to pass from the herring to the one instance where we have information on the biology of the fish, the record of the fishery, and some elements of the physical environment from which might be fashioned a model, relating climatic, oceanic, and fishery events. This fish is the California sardine. It has been stated, anonymously, that the California sardine is the most investigated and has the best documented record of the numbers and distribution in the egg stage, of any marine fish. This is evidenced by Dr. Ahlstrom's contribution to this Symposium. What I wish to propose is a very simple, very crude model or hypothesis accounting for a portion, at least, of the causes for fluctuations of year-class strength of this fish. This is proposed more with the idea of stimulating work in the direction of disproving this hypothesis, if you like, or substituting some other hypothesis, than to give a final answer.

In proposing this hypothesis I should say that it is not composed of entirely new concepts. The first element, that of temperature on the spawning grounds, particularly, has long been recognized as a significant condition for year-class success. This subject of course has been under constant consideration and investigation by Mr. Marr and his associates in the CCOFI program, and has been incorporated by him in an hypothesis relating to year-class strength (Marr, in press).

Dr. Ahlstrom has given charts showing annual changes in the site of spawning. These are very obvious in the Southern California area. To supplement these I shall have to draw, also, on some personal experience going back to 1939, 1940 and 1941, when I had my feet wet with the same water that the sardines were spawning in. At that time we had no such extensive surveys of sardine spawning as are now being made. By a joint arrangement between Scripps Institution of Oceanography and our Bureau, we had the E. W. Scripps available for the spawning seasons of

1939, 1940 and 1941. In 1939 we tried to cover a very broad area, and you have seen some of the oceanographic charts resulting from this. In 1940 and 1941 we concentrated on what you might consider an area of postage stamp size just south of the Santa Barbara Channel Islands and reaching westward a little beyond the longtitude of Point Conception.

Dr. Ahlstrom has discussed the temperatures at which the sardines spawn. If we ignore the high-temperature spawnings reported by Ahlstrom in central Baja California, which may relate to a different stock of sardines, we can say that sardines spawn at about 15°C off Southern California. This corresponds to our 1939-1941 experience. In 1940 and 1941 in Southern California waters we had those temperatures quite early in the season. My recollection is that we had 15° temperatures early enough so that there was spawning in March, which is the earliest we went out. At any rate, we took sardine eggs in plankton tows in March and we suspected from our catch of larvae that there had been spawning in February. In those years it continued through June. If we had been able to make enough cruises, I think the record would resemble Ahlstrom's plot of the time scale for spawning in upper or central Baja California. However, such a long spawning period has not been observed during the CCOFI investigations except possibly in the last year or two.

Ahlstrom: I can give you two figures. In 1940 the average temperature of spawning was 15.1; in 1941 it was 16.1°C.

Sette: Thank you for confirming my recollection.

For my model I hypothesize that we need to have 15-degree water in this area, not just some short time, but for a long enough period, in order to have an extended spawning season from March to June. This is far different from a brief May-June spawning season, especially when you remember that there is very good evidence that these fish are capable of spawning more than once.

In saying the sardines require that temperature, I would like to say further that I do not think it is the temperature *per se* that is required. It was very noticeable in 1940 that there was a cold tongue extending down from Pt. Conception into our spawning area and that the spawning tended to be concentrated around the periphery of this cold tongue. Allen's counts of diatoms from some of these cruises indicated that this cold area was well filled with diatoms, and I suspect that the sardine has a habit pattern or a built-in set of reactions, perhaps genetically impressed through evolution, that brings them to spawn in a place near, but not in, waters containing the beginning of the phytoplankton-zooplankton cycle. In our small area of survey this seemed to be the condition around the periphery of the cold tongue. It may be that the temperature in such places happens to be around 15°C most of the time; if so, temperature is probably just an indication of a complex set of conditions that the sardine has some way of sensing, maybe through temperature itself, but possibly by sensing some other property.

Based on this reasoning, let us say that the opportunity for spawning is one element involved in our model. The opportunity will be greater the larger the area and the longer the time that conditions are suitable in the area. For Southern California waters, suitability is indicated by temperature not departing greatly from 15°C.

The other element of our model concerns the early stage of life from the time of hatching until the time the young sardines show up in the bait catch. I believe the work done by John Radovich and his associates in surveying the abundance of the sardines after they left the planktonic existence and have reached bait size, has shown that the years of high abundance after the end of the planktonic stage proved later to have contributed good year-classes to the adult stock. This indicates that the fluctuation-producing mortality occurs before bait size is reached and points to events during the planktonic egg and larval stages as the ones which determine year-class strength. Therefore, in addition to the requirement that there be the opportunity for spawning, there is the critical element of survival through egg and larval life.

To examine this I will again go back to 1939 and 1940. Speaking from rather dim recollection, but of matters that left a strong impression, in the area of maximum sardine egg abundance, well offshore, we caught a correspondingly large number of young newly-hatched larvae. Well inshore, we had a substantial representation of late larvae. Relatively few intermediate larvae were taken anywhere in our survey area. The distribution of larvae as to stage of life and as to location appeared to be consistent with the conclusion that shortly after hatching from the egg in offshore waters the larvae were drifted southward out of the survey area and some time later were drifted toward shore and northward into the inshore portions of the survey area. This would be consistent with the existence of the Southern California semipermanent eddy described earlier in this Symposium by Mr. Reid. If such an eddy were well developed in 1940 and 1941 and in the location shown by Reid's charts, the newly-hatched larvae in those years would tend to be drifted southward in the offshore limb of the eddy, then shoreward somewhere south of the survey area, and then northward again into our survey area by the inshore limb of the eddy.

We may, for purposes of our hypothesis, consider that such a circulation would tend to populate the alongshore waters of Southern California with late-stage larvae, that this favors their survival, and that the end result is to contribute a strong year-class to the sardine population in Southern California.

On the other hand, if the eddy in some years is less well developed, more larvae would be drifted a greater distance southward, either to perish or to populate waters so far to the south that they would not become members of the Southern California population.

In other words, in addition to a long period of suitable temperature (or the biotic conditions indicated by temperature) for spawning, a second requirement is

a suitable circulation of the water during the period of larval drift.

To see whether the physical picture is consistent with these two elements of the hypothesis I have assembled several time series in figure 166. Having been
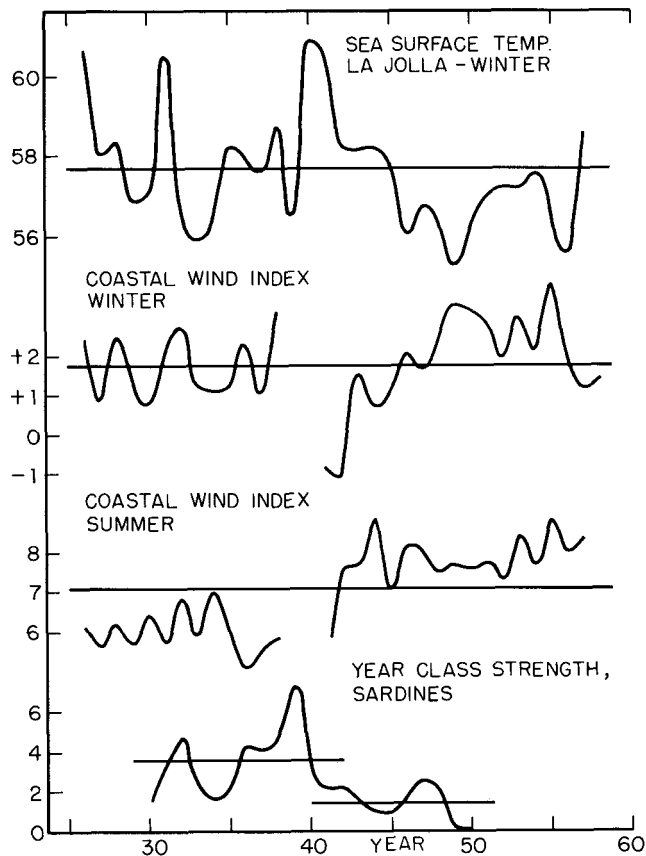

FIGURE 166. Mean winter (December, January and February). surface sea water temperature (° F.) at La Jolla, 1925 to 1957, mean winter (December, January and February) and mean summer (June, July and August) wind indices (millibars) for the relative year-class strength of sardines as indicated by the total number, in billions of sardines over two years old, landed during the life of each of the year-classes 1930 to 1950.

prepared for other purposes, these are not the most appropriate data series, but from among those now available, these are the ones most nearly suiting our present purpose. The upper curve gives the mean temperature for winter (December, January and February) as taken at the end of the Scripps pier over a thirty-year period. These give the temperature along the shore line rather than that of the waters of off-shore spawning grounds and apply to a set three months earlier than the sardine spawning season. If we may assume that the Scripps pier temperatures in general reflect the temperature fluctuations of the adjacent ocean, and if we may assume that winter sea temperatures tend to persist into the spring, we may consider that this curve, in a general way, should reflect the year-to-year fluctuations of temperature on the Southern California sardine spawning grounds.

Note that there was a progressive lowering of sea temperature from 1941 to 1946 and that they continued low through the next eleven years, not rising above the thirty-year mean until 1957. Below the temperature curve is shown the same coastal wind index as was shown at the bottom of figure 159. This shows the association of stronger than average winter winds with the lower than average winter temperatures during the eleven years preceding 1957. The association between the wind and the seawater temperature, of course, is the expected one—stronger northwesterly winds tending to increase transport by the California Current of cold water southward and also intensifying the upwelling of cold water along the coast.

We have hypothesized that for spawning it is necessary to have a set of conditions where the temperature is around 15°C or 59°F, not necessarily because of the temperature itself, but perhaps also because of other things associated with temperature. From the two curves we have discussed, it is obvious that if the temperature and winds during the pre-1942 years reflect this set of conditions, those of post-1942 years did not. Since 1942 the winds were much stronger and temperatures much cooler up until 1957.

We may turn now to the Southern California eddy as an element in our model. For lack of direct long-term information on the currents, I would like to propose the possibility that this eddy or a counter-current, tends to be better developed with weak spring or summer northwesterlies than with strong north-westerlies. The latter would tend to obliterate or weaken the countercurrent much of the time. The basis for accepting this as a reasonable hypothesis is described in general terms by Sverdrup, Johnson and Fleming (1942). According to them, an inshore countercurrent develops at the surface, if it develops at all, in the wintertime when the northwesterly winds are less strong and less constant; it does not appear at the surface so frequently, if at all, in the summer-time when the northwesterlies are prevalent.

In the third curve of figure 166 is our record of the summer wind index for the waters along the California Coast as an average of June, July and August. As with the other series, this does not coincide precisely with the period of larval drift, but perhaps covers enough of the period to generally reflect the major year-to-year changes in strength of the winds during much of the larval drift. It is very noticeable that the summer winds were weak during the pre-1942 years, as compared with the summers of the last fifteen years. If the association of weak winds with a strong eddy or countercurrent is valid, it appears that the physical events during the critical larval history of the past thirty seasons were consistent with the hypothesis that weak northerlies during the part of the year when the larvae are drifting are necessary to the survival of the larvae.

But this is not alone sufficient because there must have been an adequate amount of previous spawning. Further, it probably is not sufficient to have the proper temperatures in the regular spawning areas

in the proper time, and suitably light winds during larval drift. Other conditions also may be necessary as we all know, the larvae must feed in order to survive and may be limited to certain specific elements of the biota for their food; they have to compete with other plankters for this food; they must survive the inroads of predators and so forth. So survival of year-classes is a very complex thing. Nonetheless, I propose that the two physical elements in this model probably can determine the general level of year-class survival over relatively long periods. Upon this level would be superimposed short-term fluctuations caused by the other environmental elements. These could perhaps be conditioned by suitable or unsuitable sequences in the various upwellings during the season and so forth.

At the bottom of figure 166 is the record of year-class strength as scaled from a figure given by Clark and Marr. The vertical scale represents the total number of fish over two years old that the commercial fisheries landed from each year class throughout its life. The two horizontal lines represent the average year class size in the period 1930 to 1940 and 1941 to 1950 respectively. Clark and Marr drew attention to the fact that year classes averaged twice as large during the 1930's as during the 1940's. I should add that the period of small year-classes has persisted into the 1950's. Earlier in this Symposium we were given indications that the 1957 class may be a more successful one.

If we look at the year-by-year oscillations in the four curves of figure 166, we find little consistency between temperature, wind and sardine year-classes. But if we look at the general levels, it appears that the winters in the early half of the thirty-year period were generally characterized by warmer sea temperatures and weaker winds, and summers even more strikingly by weak winds. During the early half of this period a succession of good year-classes maintained the sardine population at a high level of abundance. In contrast, during the last half of the thirty-year period, the winter winds were stronger, the water cooler, and the sardine year-classes poorer.

Although this does not prove a cause and effect relationship, the long-term events are consistent with the hypothesis that with weaker winds in winter, the waters on the Southern California spawning grounds more often warm earlier in the spring, permitting a longer period of spawning to seed the water with sardine eggs and with weaker summer winds, conducive to better development of the Southern California eddy or inshore countercurrent, the larvae hatched from the eggs are more likely to be retained in the Southern California area and to be carried inshore and northward and survive to populate the California fishing area.

That there is not a similar correspondence of events among the year-by-year oscillations within the long-term suggests that the physical events, while necessary, are not sufficient, separately or in combination, to determine the amount of spawning and rate of sur-

vival. They leave out of consideration the complex interrelations among the biological elements in the sardine environment,—the density and distribution of organisms that form the food of, that compete with, or that are predators on the sardine during the planktonic stage of its life. Fluctuations in abundance and distribution of these biological elements of the environment may also depend basically on physical conditions, but these would operate through a train of physical, chemical and biological events affecting different trophic levels at different times and in different ways. It is hardly likely that simple indices such as mean temperature over a three-month period at a single station, or a three-month average geostrophic wind field over a large area would reflect the particular event or the particular succession of events or the coincidence of several events critical to the production of successful year-classes.

Having given this one example indicating how events in meteorology, oceanography and biology as given in long-term records might be related to elucidate a major event in one of our large fishery resources, I would like to summarize by noting two things. One, that in oceanography and marine biology there is a dearth of adequate and appropriate long-term records. Two, that where appropriate records exist or can be developed, and where enough is known about the biology and bionomics of a species, there is promise that some important events can possibly be "explained."

And I would like to close with an apology for not having brought specific enlightenment to the meaning of events during 1957 and 1958, except, perhaps, to suggest that the long-term record seems to identify the recent change as being toward, rather than away from, long-term average conditions.

DISCUSSION

Johnson: I would like to mention something that I think you may have implied more or less but did not actually state, and which may give added importance to the variation of the temperature. It may be necessary to have suitable temperatures of a certain duration for the development of eggs in the ovary prior to the actual spawning. This is quite true in the invertebrates and I believe it may be an important element also for the sardine.

Sette: If so, the use of the winter index might still be pertinent. December, January and February are pre-spawning months. The hypothesis regards the conditions during these months as predetermining the time for earliest attainment of spawning temperature. Perhaps they also precondition the sardine for spawning.

Namias: You indicated that the data for the indices during the later period were the material we use as the basis of our extended forecast program. Are the data for the earlier period taken from hemispheric weather map records? Do you know the sources?

Sette: We secured full-sized photocopies of a series of microfilmed maps from the Weather Bureau for the period from 1899 to 1939. Does that identify them for you? I have suspected that the change of level in the summer index might be due to lack of homogeneity. Could you comment on this?

Namias: There is an embarrassing element in this historical weather map series. For example, I investigated some long-period changes in other areas, particularly the Arctic, and found the material very nonuniform from the standpoint of data analysis. In that area there were tremendous errors because of spurious analysis. For this particular area (the North Pacific) I have not studied the material, but we have found in some subtropical areas a systematic bias. The user must realize that preparation of the historical maps was a crash project during the war years. We had many people drawing maps and we employed systems for assuring homogeneous analysis. One of these naturally involved text book models, which at times can be misleading. It is possible that the climatic break you have indicated is a little more too pronounced than it is in reality.

Sette: I should say that this whole scheme of wind indices is a very crude attempt to get a broad connected picture of what happens. We still want to examine a number of things. We want to look at observed winds, in certain areas at certain times, as compared to the geostrophic winds, and we want to look at the fields of temperature associated with certain fields of pressure. Our indices reflect the geostrophic wind component that runs parallel to the ocean current system. Crosscurrent indices might also be studied.

I think this attempt to get a general picture of the atmospheric circulation influencing the ocean has to have a growing and developing period. This is the first attempt for the Pacific. For the Atlantic, Chase (1954) constructed a geostrophic wind index for the "Westerlies" and "Trades" for a run of about three years and correlated its fluctuations with sea level variations associated with Gulf Stream oscillations.

Namias: Of course the description of a weather map in terms of an index is bound to be somewhat unsatisfactory because it is putting things into straight jackets. The thing that worries me about the curve labelled Coastal Wind Index, Summer, in figure 166 is that it has a characteristic that one hardly ever finds in long period records, namely that there is practically no overlap of values in the early and late portions of the period. There is a complete break in the population of values. If you compiled a frequency distribution, you would probably get a bimodal distribution. You also have this curious two-year cycle, in which there is practically no overlap of odd and even years.

Sette: I was worried about that, that is why I brought up the question.

Namias: My guess is that the general nature of your results is probably correct. But I think the actual change was not as abrupt as it appears to be in figure 166. Correction of the error might raise the level in the early period and perhaps make a more gradual trend. I suspect its general character is all right.*

Roden: If you were to go back to before 1927, you could find again, periods of above average winds such as 1919-1925.

Question to Namias: Were all of these charts analyzed by a crash program?

Namias: Analyses were done in three places. I think the broad scale aspects are quite reliable. All I am objecting to is the abrupt transition indicated here. I say this because some studies of the subtropical anticyclones indicate a distinct bias in the analysis of the older charts. I do not know if there is any easy way to remove that.

Sette: One thing I think we can do is to look at the observed winds, compared to the geostrophic winds. We might find some sources from which to get observed winds from both periods.

Revelle: The Japanese herring fishery varies inversely with the Japanese sardine fishery.

Sette: This has been reported in the literature, and I believe some investigators have attributed it to changes in ocean currents,—further penetration southward of cold water from the north favoring the herring at the expense of the sardine and vice versa. However, the long-term record of annual sardine catch in Japan compared with the annual herring catch does not show an inverse relation very clearly. But the sardine catch record includes also anchovies and round herring, to complicate things. Also the inverse relation might hold for a limited sea area, but be obscured in the overall total catch.

If data fully suitable for reflecting abundance of sardines on the western side of the Pacific were available, and if it turned out that there are almost simultaneous changes of the sardine fishery in Japan and in California over several decades, and if these appeared to be responses to similar elements in the ocean circulation, then there would be tremendous assurance that the observed correlations have a real physical and biological basis. It would provide, in effect, a different set of data to test the hypothesis. This type of comparison, however, would depend on the genetical struc-

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* Subsequent to this Symposium I reviewed this problem further with Larry Eber. The eastern margin of the pressure field represented by point pairs 34, 35 and 36 in the summertime lies in a semipermanent "thermal low." It appears quite probable that with respect to this thermal low, the conventions employed for analysis of the pressure charts for the earlier period differed from those employed for the later period, and this may account for the markedly lower level of the California index in the earlier half of the series presented in figure 166. The index for the pressure field defined by point pairs nine and ten, lying entirely over the ocean and not involving the thermal low, displays no such marked change in index level in the middle of the thirty-year series. In my opinion this confirms Mr. Namias' suspicion that the marked change of level shown in figure 165 is a spurious one introduced by analysis methods. The alternative index based on point pairs nine and ten does not invite the same suspicion and probably reflects, in general, the geostrophic wind field over the area of the California Current, even though the outer margin of this field may be somewhat affected by including a larger portion of the high pressure "dome" than would be ideal. According to this index the summer winds of the last half of the series, and especially the last seven years, have been more frequently and more pronouncedly above the thirty-year mean than during the earlier period. Thus the weight of evidence still remains in favor of the mechanism hypothesized in the model which connects ocean circulation, as inferred from atmospheric circulation, with sardine year-class survival.—O.E.S.

ture of the two stocks being identical. If they are not, the reaction pattern of responses might well be different and one would not find the same response in the two different situations. Just as Garth Murphy has pointed out, albacore are caught at different temperatures in Japan than in California.

Murphy: I do not think the albacore is reacting to temperature on either side, but to associated conditions. There is one important factor in the environment which co-varies with temperature and appears to vary differently depending upon the particular oceanic system under consideration. This factor is turbidity or clarity along the California Coast, and it co-varies with temperature very strikingly. These are areas where very slight decrease in temperature is associated with a marked rise in turbidity. In the offshore areas this could be important to the fish because this turbidity probably comes principally from small living things,—phytoplankton. This is important from the point of view of food, and it also may be important from the viewpoint of predation—the larvae finding this food, other things finding larvae. Actually, a few data suggest that some of these differences in supposed optimal temperatures for albacore depend at least in part on the turbidities that are associated with these temperatures.

Sette: I have been hoping that the instrument people working on automatic recording devices for buoys would add a gadget to measure turbidity or transparency—something that is associated directly with the biota.

Isaacs: Could I ask an undiscriminating question? How much of this fishery evidence can be explained in a sort of crude way, by just saying we have had a shift in environment,—a shift of the northern environment south over a period, followed and preceded by a shift of the southern environment to the north?
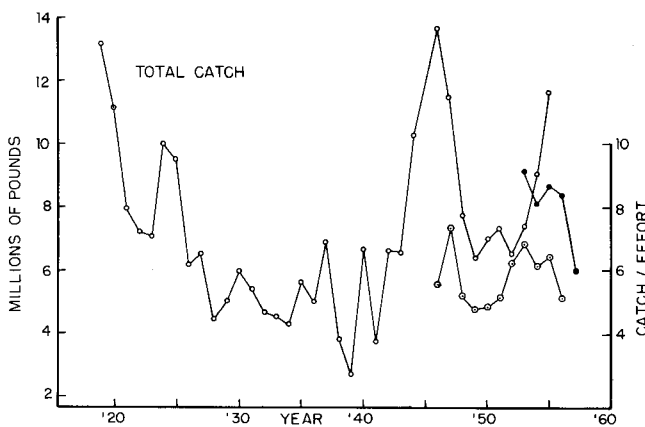

FIGURE 167. Total commercial catch in California of chinook salmon, in millions of pounds, 1919 to 1955 (open circles); mean catch of chinook salmon per boat per season, in thousands of pounds, by California trollers, 1946 to 1955 (dotted circles); and mean catch of chinook salmon per boat per day, in hundreds of pounds, by California trollers, 1953 to 1957 (filled circles).

I was particularly struck by the chinook salmon, which we think is particularly sensitive. The chinook represents the one instance of a cold-water fish possibly withdrawing from an area of anomalously warm water; the previous instances were of warm-water species invading an area with anomalously high temperature.

Sette: I did not mention the chinook salmon at all because I was running over my time, and Mr. Radovich had already discussed the same species under the name "king" salmon. Since you bring it up, however, I should explain that figure 167 gives three curves: the total commercial salmon catch off California, the catch per boat per season, and the catch per boat per ten days. The last curve, based on data kindly furnished by the California Department of Fish and Game, is the most refined measure of apparent abundance and the only series continuing through 1957. On the other hand, it does not carry far into the past. The other curves, however, permit an approximate basis for relating this last series to the long-term levels. From this it might be concluded that the apparent abundance of salmon did indeed become substantially lower in California in 1957. Since this is a cold-water species and is at the southern end of its range when in California, this might be interpreted as a partial withdrawal northward in the warm year of 1957. There could also be other interpretations of this apparent decline in 1957, such as descent to deeper cooler waters not customarily reached by troll line, or a real reduction in numbers of the salmon population stemming from poor survival in former years.

Marr: I am greatly impressed by this sardine model. One reason for this is that it agrees to a substantial extent with a model I proposed at the 1957 Sardine Conference, at the Ninth Pacific Science Congress (Marr, In Press), and in the most recent *Progress Report of the California Cooperative Oceanic Fisheries Investigations* (California Marine Research Committee, 1958), as Dr. Sette has already mentioned. It is, of course, always a great pleasure to find oneself in agreement with someone like Dr. Sette.

I would like to elaborate somewhat on my model since it contains more of the background information, both on the fishery and on the biology of the sardine, that Dr. Sette may have included in his thinking but did not present this morning.

Naturally enough, I was led to my model by considering features which any realistic model must explain, or at least with which it must be consonant. These include:

1. *The location and magnitude of the fishery.* The fishery, which formerly existed off San Pedro, Monterey, San Francisco, Astoria, Grays Harbor, and Vancouver Island, suffered a differential decline with latitude. It declined first, and fell to zero in the Pacific Northwest. The decline proceeded to the south and at the present time the only fishery is off San
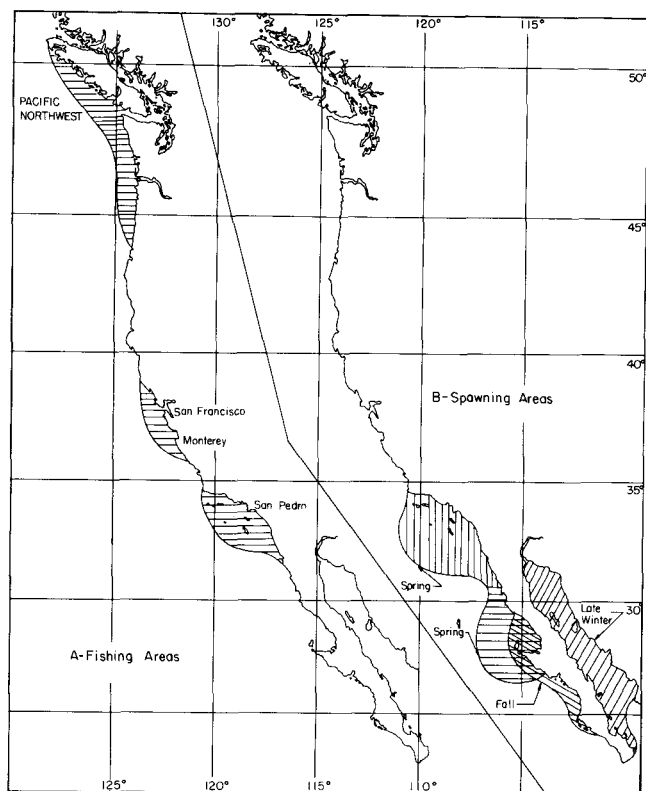
FIGURE 168.   Schematic representation of: A) fishing area; B) spawning areas of the Pacific sardine.

Pedro, and that at a rather low level. (The location of fishing areas is shown in figure 168A.)

2. *The year-class composition of the catch.* This was not the same at all ports and some features, at least, are not what one would expect with a simple diminution of population size. In British Columbia, for example, the last year-class to appear in the catch in any abundance was the 1943-class. Thereafter the catch there was dependent on year-classes already in the fishery; there were no new additions.

3. *Movements as revealed by tagging.* Pre-World War II tagging experiments, carried out by California, Oregon, Washington and British Columbia, showed that fish tagged in any of these areas tended to be recovered in any of the other areas, as well as in the area of tagging. However, fish tagged off Baja California did not conform to this pattern. About three-fourths of the recoveries of these fish were made off San Pedro and one-quarter off Monterey and San Francisco. (Since there were no recovery facilities in Baja California, the experiments yielded no informa-

tion about the possible movement of fish from the north into Baja California waters.)

4. *Early growth.* Sardine growth during each year of life may be inferred from annular marks on the scales. If one considers, by year classes, only the growth during the first year of life, a striking discontinuity is evident. The first year's length of the 1936-through 1943-classes varied around an average of about 103 mm. The first year's length of the 1944-through 1952-classes varied around an average of about 129 mm. The year-classes of these two periods obviously experienced different growth histories during their first year of life. The latter group is that which made no contribution to the Pacific Northwest fishery.

5. *Spawning areas.* In an earlier contribution to this Symposium, Dr. Ahlstrom discussed the spawning areas of the sardine. These may be idealized (and are so shown in figure 168B) as (a) the area off Southern California from Pt. Conception to northern Baja California (spring spawning), (b) the area of central Baja California (spring spawning), (c) the inshore area from Sebastian Viscaino Bay south along the coast of Baja California (fall spawning) and (d) the Gulf of California (late winter—early spring spawning). These areas represent opportunity for isolation in space or time; i.e., they represent opportunities for the existence of subpopulations.

A realistic model must take these features of sardine biology and the fishery into account. My model violates this dictum at the outset by considering only the two offshore spring spawning areas. My model states:

1. Sardines which are produced off Southern California migrate as far north as the Pacific Northwest, support the fishery there, and contribute to the fisheries of San Francisco, Monterey and San Pedro.

2. Sardines which are produced off central Baja California migrate as far north as Central California and enter into the fisheries of San Francisco, Monterey and San Pedro (especially the latter).

3. Lack of spawning success on the Southern California spawning grounds since 1943 could account for the observed changes in the fishery.

4. Coincident with this postulated lack of spawning success on the Southern California spawning grounds there was a period of below average (with respect to the long-term mean) surface temperature.

Like Dr. Sette, I am hesitant about suggesting a connection between year-class size and water temperatures *per se*. This possibility should not be overlooked, however, as there might well be a connection through time of maturation of sex products and zooplankton succession (larval food items). Dr. Sette's suggestion

of the possibility of a relationship between counter-current (or eddy) development and year-class size is also intriguing and should certainly be investigated.

I have two final remarks. First, I do not believe that fish which spawn in the two off-shore areas represent two subpopulations. Rather, I believe they represent one genetic unit whose spawning area is influenced by environmental conditions in any particular year. The area in which the fish are produced, however, influences their subsequent history, as postulated above. Second, I think of my model as specifying, in general, conditions necessary for the production of successful year-classes. But the existence of such conditions do not guarantee the production of successful year-classes.

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