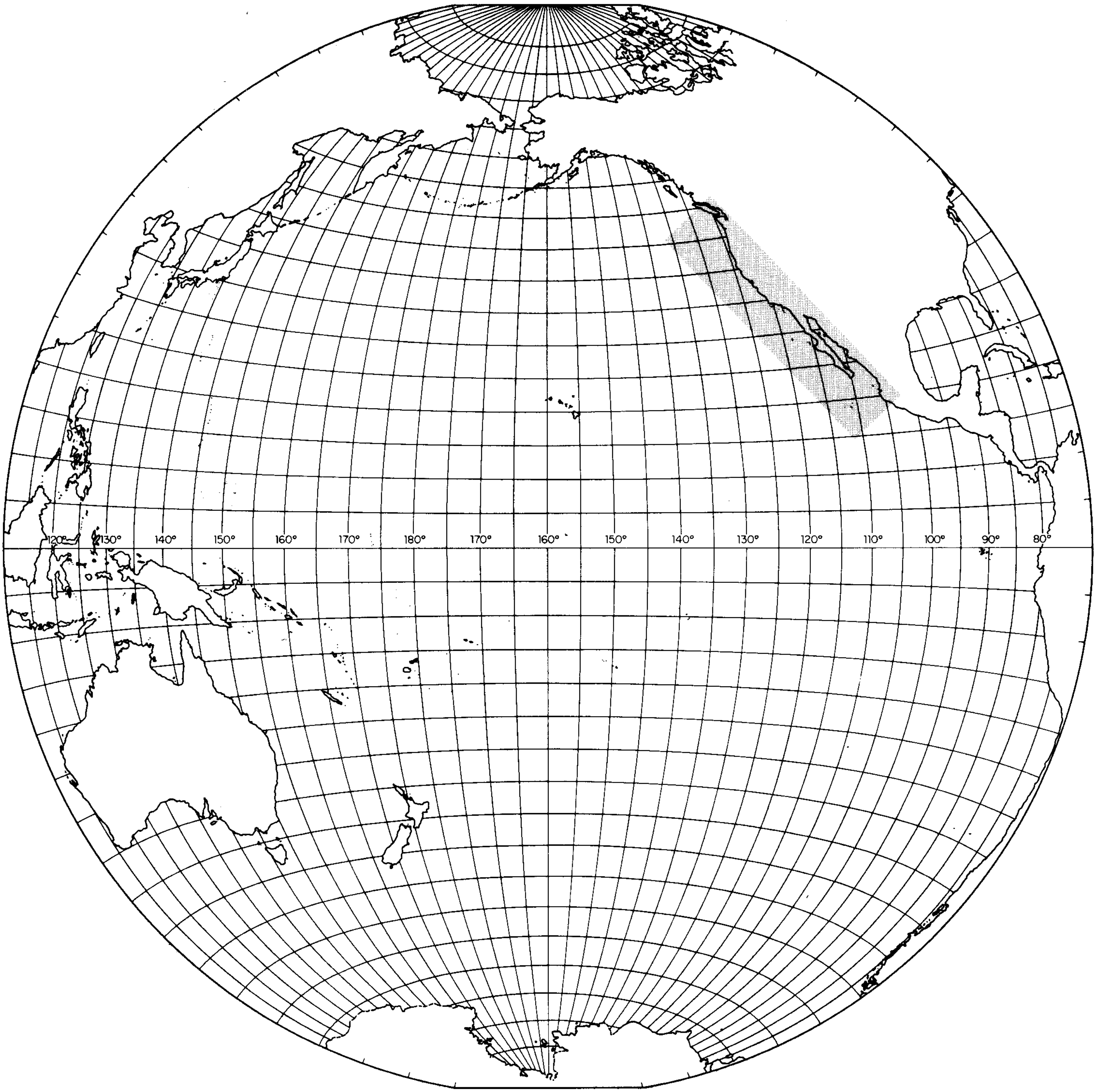
STATE OF CALIFORNIA
MARINE RESEARCH COMMITTEE



CALIFORNIA COOPERATIVE OCEANIC FISHERIES INVESTIGATIONS

REPORTS

VOLUME VII
JANUARY, 1960

STATE OF CALIFORNIA
DEPARTMENT OF FISH AND GAME
MARINE RESEARCH COMMITTEE

CALIFORNIA COOPERATIVE OCEANIC FISHERIES INVESTIGATIONS

Reports

Volume VII

1 January 1958 to 30 June 1959

Cooperating Agencies:

CALIFORNIA ACADEMY OF SCIENCES
CALIFORNIA DEPARTMENT OF FISH AND GAME
STANFORD UNIVERSITY, HOPKINS MARINE STATION
U. S. FISH AND WILDLIFE SERVICE, BUREAU OF COMMERCIAL FISHERIES
UNIVERSITY OF CALIFORNIA, SCRIPPS INSTITUTION OF OCEANOGRAPHY

1 January 1960

LETTER OF TRANSMITTAL

January 1, 1960

EDMUND G. BROWN
Governor of the State of California
Sacramento, California

DEAR SIR: We respectfully submit the seventh report on the work of the California Cooperative Oceanic Fisheries Investigations.

The report consists of two sections. The first contains a review of the present operational organization of the investigations, a brief review of the research underway during the period January 1, 1958, to June 30, 1959, a description of the partial resurgence of the sardine fishery during the 1958-59 season, and a list of publications arising from the program. The second section is a report on a symposium held in June 1958. This symposium was designed to review the unusual changes in the ocean circulation off California during 1957 and 1958. Scientists from many disciplines and several countries were assembled for this purpose. Their contributions and the attendant discussion are worthy of the attention of everyone concerned with the problems faced by the people of California in understanding and utilizing the resources of the Pacific.

Respectfully,

THE MARINE RESEARCH COMMITTEE
J. G. BURNETTE, Chairman
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W. E. STEWART
G. C. VAN CAMP, SR.

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PART I

REVIEW OF ACTIVITIES

January 1, 1958-June 30, 1959

GENERAL

Though the essentials of the research objectives and programs were little changed during the period, there was an important change in the directional organization of the California Cooperative Oceanic Fisheries Investigations. In June 1957 a Special Technical Committee was appointed by the Marine Research Committee to examine the CalCOFI research program and organization. As a result of their report the Marine Research Committee moved as follows on December 19, 1957:

Now, therefore, be it resolved, That the Marine Research Committee accept the report of the Special Technical Committee and act as follows:

1. The Technical Advisory Committee, having fulfilled its purpose, is hereby dissolved.
2. CCOFI leadership, direction, responsibility and authority will be vested in a four man committee (to be known as the CCOFI Committee), comprised of a representative from each of the following: California Department of Fish and Game, Marine Research Committee, Scripps Institution of Oceanography, and the U. S. Bureau of Commercial Fisheries. The members of this committee will serve in equal status, with the Marine Research Committee representative acting as chairman. The members of the CCOFI Committee shall serve at the pleasure of the Marine Research Committee and the heads of their respective parent organizations.
3. The Marine Research Committee representative will be employed by the Marine Research Committee as a full-time scientific member of the CCOFI Committee. The individual selected shall be a broad and practical senior scientist acceptable to the other members of the committee. It is intended that his functions include, (a) to act as an integrative force in the CCOFI COMMITTEE, (b) to advise the Marine Research Committee on research progress, staffing, budget and other matters, (c) to represent the Marine Research Committee on the CCOFI Committee in the expression of policy, interest, and in the allocation of funds. In the discharge of these functions he shall receive the full support of the Marine Research Committee. In order to fulfill these functions he must frequently visit all cooperating organizations including the California Academy of Sciences and the Hopkins Marine Station, in order to become completely familiar with all aspects of the research work under way.
4. The representatives of the other three organizations will be appointed by their respective heads. They shall be the full-time working heads of the program in each parent organization. They should have authority within their own organizations to carry out commitments made to the CCOFI Committee.
5. The CCOFI Committee shall meet at least once a month. The Chairman shall be responsible for preparing agenda and calling regular meetings of the Committee. Any CCOFI Committee member or the Chairman of the Marine Research Committee may call additional meetings by notice to the Chairman of the CCOFI Committee. The Chairman shall prepare and distribute minutes to the members of the CCOFI Committee, the Marine Research Committee, and the co-operating organizations.
6. Some matters will of necessity have to be referred to the Marine Research Committee and the parent organization heads. These include, (a) questions that cannot be satisfactorily resolved within the Committee because of disagreement or lack of authority, and (b) policy matters on which the Committee needs guidance. However, differences of interpretation of scientific matters probably indicate insufficient understanding and should be resolved by obtaining additional understanding through outside advice or additional research. Decisions reached on matters within the authority of the Committee should be acted on without further referral.

The motion was not fully implemented until January 1959. In the interim the newly formed committee functioned with the chairman of the Marine Research Committee serving as their representative. Beginning in February, 1959, the CalCOFI Committee has held regular monthly meetings as well as a number of special conferences. About half of their deliberations have involved solving current administrative problems, the balance being devoted to program review.

One recommendation stemming from the committee involves this report. In the past the report has been construed as and treated as a progress report. With the maturation of the program the concept of a progress report has outlived its usefulness, and, in fact, recent progress reports have contained original scientific papers. With this report formal recognition is given to an accomplished fact, in that the word "progress" is dropped and a serial numbering system adopted. The contents of this and future reports will be Part I, consisting of very brief progress reports, and Part II, containing original signed scientific contributions. These will be contributions arising from the research programs, and in general will consist of large monographic papers, or series of interrelated papers such as the symposium contained herein.

AGENCY ACTIVITIES

California Academy of Sciences

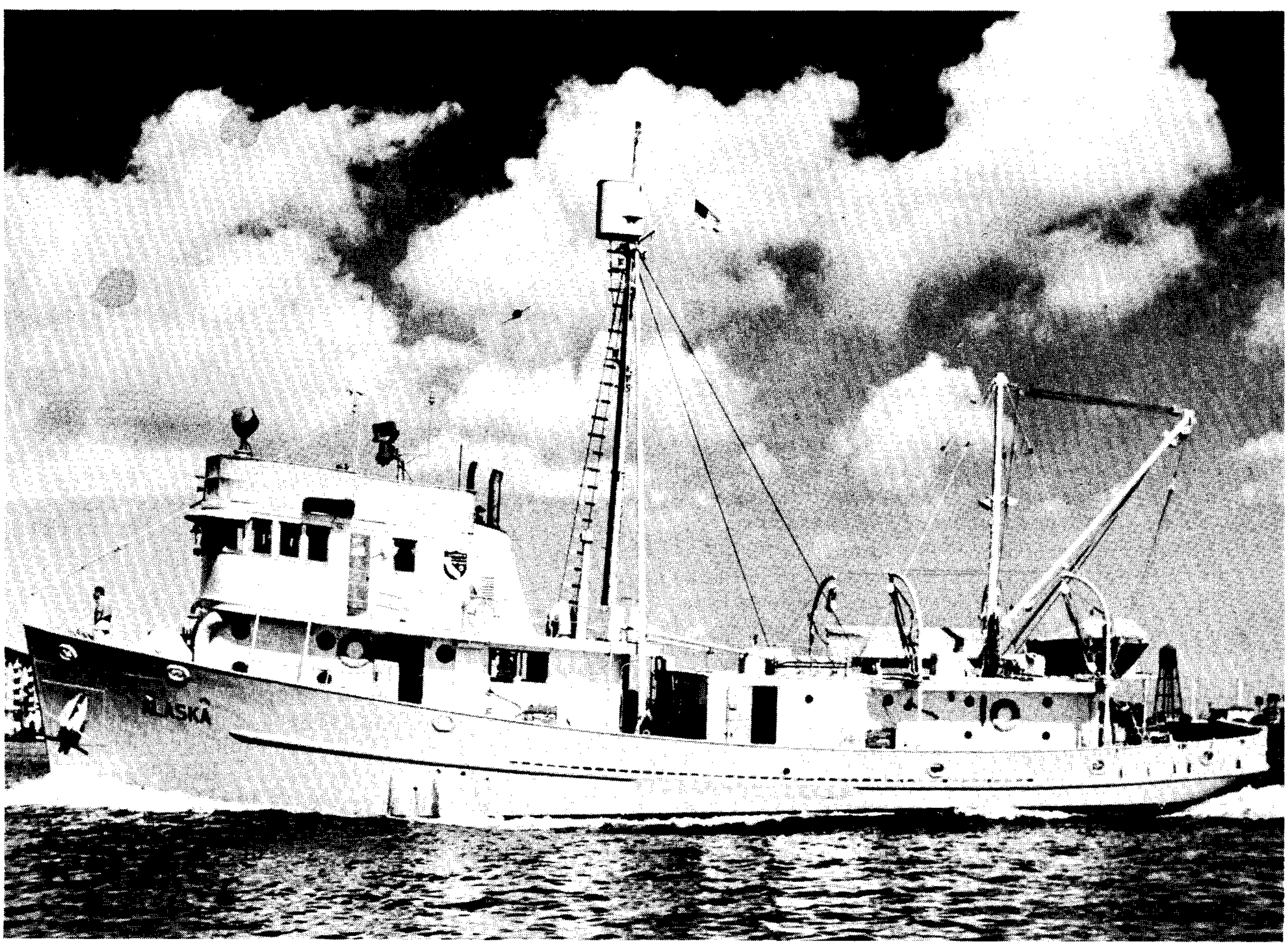
Studies on the behavior and reactions to stimuli of sardines and ecologically related species were continued, utilizing one investigator and assistance as needed. These studies are directed towards developing the intimate knowledge of the sardine needed to understand its reactions in nature as an individual and as an aggregational entity. A conceivable product of such investigations is reduction of the costs of harvesting the resource.

California Department of Fish and Game

The department conducted its portion of the investigations through its organizational unit called the Pelagic Fisheries Investigations. In addition to collecting the basic catch statistics on sardines, anchovies, jack mackerel, Pacific mackerel, and squid, the department undertook several important investigations. These include: (1) measurements of the relative abundance of year classes based on field surveys on the high seas; (2) measurements of the sizes of the year-classes in the commercial fishery in cooperation with the U.S. Bureau of Commercial Fisheries; (3) systematic field sampling for morphometric and genetic studies; and (4) sampling of the commercial catch, live bait catch, and airplane surveys of the population. In the laboratory emphasis has been laid on the production of reports and analyses predicated directly by the nature of the field work itemized above, as well as specialized studies such as the re-distribution of fishes during the past two warm years, and the population dynamics of the Pacific sardine. The staff of the Pelagic Fisheries Investigations has been strengthened, and an unfortunate, but unavoidable lag in analysis and reporting will soon be closed.

Hopkins Marine Station, Stanford University

Hopkins Marine Station continued to monitor the physical, chemical, and biological environment of Monterey Bay, formerly the largest and most colorful center of the sardine industry. An approximately



weekly schedule is maintained. In addition personnel from the station collect shore data from points to the south of Monterey Bay. The formal program is essentially one of monitoring the bay, but the collected data and samples have been and are utilized extensively by investigators in and out of the formal CalCOFI array.

Division of Marine Resources—Scripps Institution of Oceanography, University of California

Scripps Institution has since the inception of the CalCOFI program been the major participant. Its portion of the program can be roughly separated into oceanographic survey and monitoring, and fundamental studies relating to the basic problems of utilizing the living resources of the ocean. The first portion of the program is in part a service function in that Scripps endeavors to measure the ocean climate as background for other studies, principally biological. In addition, these oceanographic data are fundamental and of great interest in their own right, and are the basis of descriptive and analytical studies of the California Current system. The ship time involved in the oceanographic studies is also used to collect the field data needed by other projects such as the egg and larva studies of the Bureau of Commercial Fisheries.

The second phase of the Scripps research involves a number of basic programs some of which draw extensively on the materials and data collected during the field work and others that are independent of the field work. In general these studies are concerned with aspects of the biology of marine organisms, and the ecology of the sea not generally included in the traditional fishery program. Examples are the zoo-geography of chaetognaths, salps, and euphausiids, studies of micro-nutrients in the sea, the genetics of *Tigriopus,* and climatological studies of the eastern North Pacific.

A secondary but nonetheless very important program at Scripps is the development of better tools for looking at the ocean. Work of this nature is haphazardly supported throughout the world, despite the fact that many, if not all of our conventional methods are either exceedingly expensive, or very inadequate. A striking example of this work is the development of free vehicles, by which instruments or devices can be placed at any depth in the sea and recovered later, without a vessel standing by during the operational period. One of these free vehicles is a simple fish trap. One of the results of using this trap was the discovery that sablefish could be caught in 746 fathoms off La Jolla. Sablefish had not been previously reported from over 400 fathoms, and fishing for sablefish off southern California has generally been conducted between 100 and 300 fathoms. Deep moored instrument stations are another development bearing important implications to marine research. The recording skiffs have been moored in 2,800 fathoms.

The Scripps program has been strengthened by clearly associating the CalCOFI research with the Division of Marine Resources, to stimulate studies into the interrelationships of the various disciplines, fisheries, and oceanography. In dealing with these subjects the scientist must synthesize diverse facts into models, either conceptual or formal that resemble nature as a functional whole, in addition to the strict disciplinary approaches that tend to isolate certain phenomena or classes of phenomena for study. It is for this synthesis that many of the research problems, including the CalCOFI program, have been assembled in the Division of Marine Resources.

U. S. Bureau of Commercial Fisheries (BCF)

Investigations conducted by the BCF center around the sub-population problems, egg and larva surveys, sampling of the commercial catch of sardines in Baja California, composition of the catch, early survival of sardines, availability, population dynamics, and physiology of the sardine. In general the Bureau conducts the kinds of studies normally considered a part of a fisheries investigation, and their work complements that of the California Department of Fish and Game. Many of their studies are jointly conceived and executed with the latter agency.

The BCF continued to operate the motor vessel Black Douglas, which together with the equivalent of two vessels from Scripps and one from the Department of Fish and Game made a total of about four research vessels on the CalCOFI program. Among the highlights of the BCF activities during the year was finding the first indications of genetic diversity in the Pacific sardine, initiation of badly needed physiological studies on the sardine, and development of a practical way to examine the detailed distribution of the plankton organisms comprising sardine food.

REVIEW OF THE PARTIAL RESURGENCE OF THE SARDINE FISHERY

During the 1958-59 Season

The most exciting event in the past several years of the sardine fishery was the catch of 102,000 tons made during the 1958-59 season. The catch created considerable interest because it was the largest since 1951, and because for the first time since 1951 sardines were taken in appreciable quantities off central California, that is captured and landed in the vicinity of Monterey and San Francisco. The table below gives the landings in recent years in thousands of tons.

Season	*Thousands of tons*	*Season*	*Thousands of tons*
1949-50	339	1954-55	68
1950-51	353	1955-56	74
1951-52	129	1956-57	34
1952-53	6	1957-58	22
1953-54	4	1958-59	102

The full story behind this event cannot be related now because the resurgence must be subjected to the tests of time and to further scientific analysis. Nevertheless, there is enough information on hand to permit a somewhat detailed description of the 1958 fishery, and to suggest some of the factors responsible for the increase in the catch.

THE FISHERY

The fishing season officially opened on August 1, in central California and September 1, in southern California. The fishing fleet totaled 150 vessels, 30 from Monterey and 120 from the San Pedro area. Fishing was delayed until August 18, in the central California or Monterey area because of price negotiations but for the first time in many years began promptly on the opening of the season in the southern California or San Pedro area.

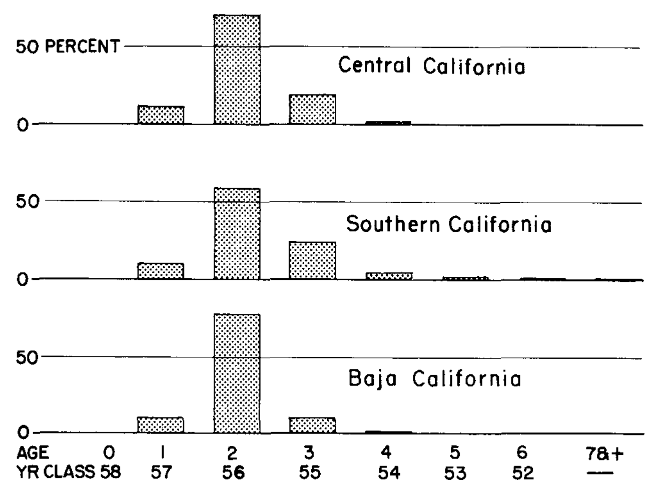
During the initial part of the season the ex-vessel price of sardines was $60 a ton. In late September and early October there was a two weeks stoppage of fishing because of price negotiations. Commencing October 5 a reduced price of $50 a ton was agreed upon, with a 40,000 ton minimum set for the balance of the season. This minimum was allocated among the several canneries. In addition, a 40 ton nightly boat limit was imposed on the fleet. Quotas were reached by some canners in mid-November when they stopped taking fish for a time, then resumed under lower limits toward the last of the season.

As might be expected, the complicated situation described above was the result of a relatively poor market for canned sardines related to the recent decade of scarcity of sardines, which had reduced the domestic usage and demand, and because the highly competitive South African and Japanese fisheries had expanded to fill the voids created by the recent California shortage. (In recent years including 1958 canning has been the only primary use of the sardine catch.)

The restricted fishery, then, failed to realize the potential catch. What the catch might have been is speculative. Fish continued in fair abundance through the end of the fishing season (Dec. 31), so it is not unreasonable to suppose that an unfettered fishery might have taken an additional 25,000 to 50,000 tons. Despite the overall oversupply of sardines, the distribution of the fish along the coast resulted in something less than an adequate supply off Monterey and San Francisco where canners had to rely on trucked fish from Morro Bay for about half of their raw material.

COMPOSITION OF THE CATCH

During the season approximately 25,000 tons were delivered to Monterey and San Francisco canneries and 78,000 tons were processed in Southern California. The Mexican fishery in Baja California landed 7,800 tons during the California season, and 11,600 during the earlier part of 1958. As indicated in the figure below, the catch was dominated by the 1956 year class taken as two year olds. These two-year-old fish were smaller than the usual two-year-old fish of recent years. For instance the mean size of two year



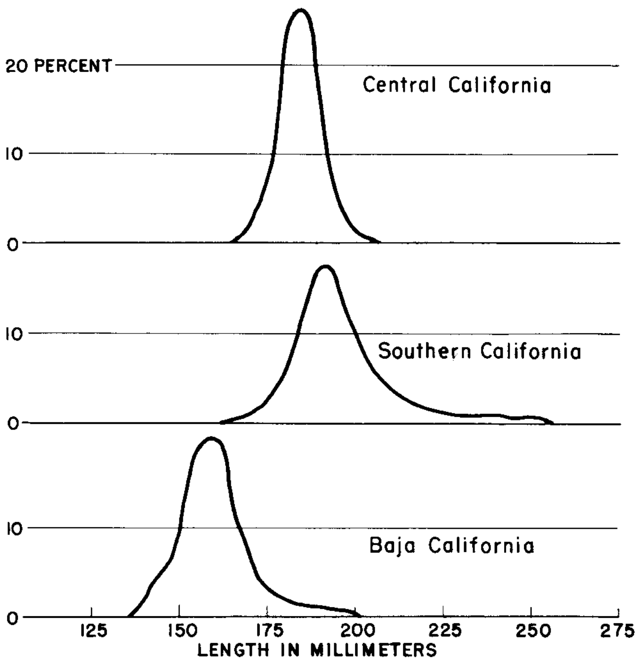
Age Composition of the Commercial Sardine Catch in Three Main Fishing Areas

olds at San Pedro during 1958 was 191 mm (7.6 inches), which is 12 mm (.5 inches) smaller than the previous recent minimum (1954-55), but is not much smaller (3 mm, or .1 inches) than the 1939 year class as two year olds, an outstanding year class.

AVERAGE SIZE OF TWO-YEAR-OLD SARDINES AT SAN PEDRO

Season	*Average size*	*Season*	*Average size*
1950-51	205 mm	1955-56	206 mm
1951-52	205 mm	1956-57	224 mm
1952-53	222 mm	1957-58	204 mm
1953-54	213 mm	1958-59	191 mm
1954-55	203 mm		

The Baja California fish were even smaller, in fact they were so much smaller that it suggests that the fishery was not utilizing the same stocks of fish as the Southern California fishery. The average size of their two-year-old fish during comparable portions of the fishing season was only 162 mm (6½ inches), that is 1.1 inches smaller than the San Pedro fish. The two year olds landed at Monterey were also smaller than the San Pedro fish, indicating somewhat more population heterogeneity than usual.



Length Composition of the Commercial Sardine Catch in Three Major Fishing Areas

The composition of the 1958 catch was also unique in that it was the first year since 1950 that two-year-old fish have dominated the catch. During the intervening years three year olds were generally the most abundant year class in the fishery. At the risk of oversimplification this could have been the result of one of three alternatives or combinations thereof (1) There had been very heavy mortality, natural and/or fishing on the 1955 year class. (2) The 1956 year class is outstandingly large, and (3) there was a northward shift of the population which might place a relatively larger portion of the 1956 year class in the area of the California fishery. There is considerable evidence against the first alternative, and little for it, so it need not be considered further in this brief discussion. A review of some of the events preceding and external to the fishery sheds some light on the validity of the remaining two suggestions.

HISTORY OF THE 1956 YEAR CLASS

The 1956 year class had an inauspicious start. The number of eggs (256×10^{12}) was modest, being only about 1.6 times the numbers of eggs spawned in 1955. As in 1955, the majority of the eggs were deposited off Baja California with only light spawning in the Southern California spawning center. The total spawning area was, however, only 0.7 as large as that of 1955. Oceanographic conditions during the spawning period in 1956 were even colder than the previous several "cold" years that produced mostly small year classes. However, the first indications of the shift to the "warm" ocean conditions of 1957-58 can be detected in the final months of 1956.

Tracing this year class onward, tabulation of the numbers of large larvae in the plankton samples suggest that survival to this stage was almost twice as high as in any of the previous four years. This, in itself, suggests a somewhat larger, but certainly not an outstanding year class. Actual surveys of the fish population that are conducted annually furnished estimates at ages "6 months" and one year but neither of these indicies suggested that the 1956 year class was anything but typical of recent years. Thus, if one sets aside the evidence provided by the fishery there is nothing in the record to suggest that the 1956 year class is unusual with respect to size, so it is logical to examine the other possible causes of the upsurge in the fishery.

The decline in the sardine fishery involved an absolute reduction of the population. The size of this reduced population is known with considerable certainty. The size of the pre-decline population is not known with similar exactitude, but it was probably not more than 10 times larger than the reduced population. Even this is not enough to account for the reduced catch which fell to less than 1/100 of its former amount. Hence, superimposed upon the effect of the reduced population in producing the dramatic decline of the sardine catch is a further significant factor, reduced availability, as a result of the population being located to the south, essentially placing it out of reach of the California fishery.

Over the last decade these events have been concurrent with the onset and persistence of uniformly cold water in the California Current, the result of vigorous upwelling and stronger southward flow. This could have had an effect on population size, and almost certainly had an effect on the distribution of the sardine, that is the cold water and increased flow could affect both population numbers and distribution.

Conversely, the partial resurgence of the fishery in the 1958 season need not have involved an upsurge in the population, but could easily result from a northward shift of the population in response to a warming of the oceanic climate associated with a reduction in currents. This appears to be precisely what happened, and probably accounts for most of the change.

It was mentioned earlier that near the end of 1956 there were weak indications of the warming of the ocean climate. This trend, instead of reversing itself as was usual during the past 8 years, strengthened through 1957, and persisted through 1958 and 1959. As indicated, the most dramatic and perhaps important change was a rise in the temperature of the seas off California of about two to four degrees Fahrenheit. This is a remarkable change in an area with a seasonal variation of only ten or twelve degrees Fahrenheit. Associated with this, "southern" plankton appeared far to the north and close to the coast. Judging from their biology, at least some of these organisms were actually transported into northern areas, thus providing evidence of changes in the water flow along the coast. The associated appearance of ''southern'' sportsfishes, such as the yellowtail, in large numbers is probably the ''warm years'' change noted by and appreciated by the most people.

The sardine population also responded to the change, and spawning in 1957 was farther north than the previous year. However, the estimated number of eggs deposited was less than during earlier years so there are no grounds for supposing a significantly larger population. The 1957 catch was not impressive, however, for a variety of reasons. In 1958 oceanic conditions were, if anything, warmer than 1957, sardine spawning was again northerly oriented, and the fish remained north through the fishing season. Thus, the most significant and best documented event underlying the 1958 catch was the persistent northward shift of the sardine population.

In summary the moderately successful 1958 season may be attributed first of all to a fishery that began much earlier in the year than 1957 thus reducing the effect on the fishery of the usual pattern of the sardines migration to the south in winter; second, to the presence of a year class (1956) probably larger than the several preceding classes, but certainly no larger than ''moderate sized''; and third, and most important, to continued high availability through the fishing season. Future catches and surveys should serve to substantiate or revise this tentative appraisal of the best year in the recent history of the sardine fishery.

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PART II

SYMPOSIUM ON

"THE CHANGING PACIFIC OCEAN IN 1957 AND 1958"

Edited by OSCAR E. SETTE, JOHN D. ISAACS

Rancho Santa Fe, California

June 2-4, 1958

DEDICATION

This Symposium is dedicated to

TOWNSEND CROMWELL
and
BELL M. SHIMADA

associates in research of many of the participants in this Symposium, who lost their lives, June 2, 1958, in an airplane crash near Guadalajara, Mexico, while en route to join the research vessel *Horizon* to make further observations on the changing conditions in 1958.

PREFACE

In presenting these discussions of the surprising events of the years of change, the editors have attempted to retain the informality of the Symposium, altering the speakers' words only for the purposes of clarity.

In so doing, the editors have, of necessity, preserved not only informality, but a conspicuous irregularity of style. A few of the papers were read, some given from extensive notes, and some essentially extemporaneous. All unprepared papers were recorded as given, and all papers and discussions were submitted to the authors for review and revision. Some authors saw fit to delete vernacular expressions, others permitted them to remain. The editors have striven to see only that the thought was expressed clearly, and have made no attempts to alter the authors' decisions in regard to the choice of degrees of formality.

Impossible to retain in full was the spirit of a group of outstanding investigators from a wide range of disciplines attempting to understand the message from the incoherent mutterings of nature stirring with obscure excitations.

Following the Symposium the changes in the sea and atmosphere have continued to manifest themselves in various ways—the authors and the editors have resisted the temptation to liberally sprinkle the presentations with pertinent footnotes of these subsequent changes.

OSCAR E. SETTE
JOHN D. ISAACS

ACKNOWLEDGMENTS

On behalf of the participants of the Symposium, the editors wish to extend their sincere thanks to those who made the Symposium possible:—to Dr. Roger Revelle, Director, and to the Scripps Institution of Oceanography at La Jolla, California, for their sponsorship and for the many arrangements for travel and accommodations; to Mr. Julian Burnette, Chairman, and to the Marine Research Committee of the State of California, for their sponsorship and their broad recognition of the importance of the subject of the Symposium; to Mr. Reginald Clotfelter, Manager, and the staff of the Inn at Rancho Santa Fe for their fine arrangements and for their hospitality to the group; to our secretaries and recorders, Mrs. Lorrayne Buck, Mrs. Patricia Bridger, Mrs. John Wyllie, and Miss Barbara Edwards and to Mr. Richard Schwartzlose for his valuable assistance in the arrangements for the Symposium and for his devoted help to the editors during the preparation of these proceedings.

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INTRODUCTION

By the fall of 1957, the coral ring of Canton Island, in the memory of man ever bleak and dry, was lush with the seedlings of countless tropical trees and vines.

Two remarkable and unprecedented events gave rise to this transformation, for during 1957 great rafts of sea-borne seeds and heavy rains had visited her barren shores.

One is inclined to select the events of this isolated atoll as epitomizing the year, for even here, on the remote edges of the Pacific, vast concerted shifts in the oceans and atmosphere had wrought dramatic change.

Elsewhere about the Pacific it also was common knowledge that the year had been one of extraordinary climatic events. Hawaii had its first recorded typhoon; the seabird-killing *El Niño* visited the Peruvian Coast; the ice went out of Point Barrow at the earliest time in history; and on the Pacific's Western rim, the tropical rainy season lingered six weeks beyond its appointed term.

The meteorology of the North Pacific was most unusual, with intensification of the North Pacific low and slackening of the winds along the California Coast. In regions of the Pacific where intensive oceanographic measurements were being carried out, investigators were sharply aware of changes. Over much of the eastern North Pacific water temperatures were as much as three degrees centigrade higher than normal, and in the California current, more than four times the solar heat actually received, would have been necessary to account for the warming.

This widespread variation in the weather manifested itself dramatically on a local scale. At La Jolla, for example, the temperature of the sea surface reached the highest averages during July, August, and September of 1957 in 21 years. Southern California had one of its rainiest autumns in several years. Throughout the summer reports came in of the appearance in quantity of fishes that in recent years had been caught only as stragglers: by the end of September 1957 the party boats off Southern California had landed 2,805 dolphinfish against a previous high of 15 in 1947. Some of these events, related as anecdotal, might forever remain so, were it not that recent years have seen an upsurge in man's interest in the atmospheric and oceanic environment. This interest has been expressed by growth in research organizations motivated to record, study and understand the environment and its perturbations.

Beginning with organizations nearest the site of this Symposium, the University of California, Scripps Institution of Oceanography (SIO), in addition to its studies of the sea in all its aspects, has its Marine Life Research Program (MLR) devoted particularly to understanding the sea as the environment for important marine food resources. This division participates in the California Cooperative Oceanic Fishery Investigations (CCOFI)—a group effort under the aegis of the Marine Research Committee of California, in which also participate the South Pacific Fishery Investigations of the U.S. Fish and Wildlife Service (USFWS), the Pelagic Fish Investigations of the California Department of Fish and Game (CF&G), the California Academy of Sciences and Stanford University's Hopkins Marine Station. Of particular significance to the Symposium are the CCOFI observations of physical oceanography, plankton populations and fish spawning in the waters along the eastern margin of the Pacific Ocean abreast of California and Baja California by monthly visits to the grid of stations charted in figure 1.

With interest to the south, in the eastern tropical Pacific, is the Inter-American Tropical Tuna Commission (IATTC) with headquarters on the SIO Campus and coastal field stations in Costa Rica and Panama. Further south is the Consejo de Investigaciones Hidrobiológica of Peru.

To the north is the Department of Oceanography of the University of Washington and the Pacific Oceanographic Group (POG) of Canada, carrying on extensive observations in the subarctic waters of the Pacific, supplemented by further observations made in connection with the North Pacific Fishery Convention by the U.S. Fish and Wildlife Service from its Pacific Salmon Investigations at Seattle and by the Fishery Research Board of Canada from its Biological station at Nanaimo.

In the central North Pacific, the Pacific Oceanic Fishery Investigations (POFI) has been studying physical and biological oceanography from the tropics to the subarctic; while from the western margin of the Pacific, the Japanese Meteorological Agency, together with the central and prefectural fishery agencies have been recording and studying events in the Northwest Pacific ocean. Less directly oriented to the Pacific but adding to the store of Pacific information are the survey and observation programs of the U.S. Coast and Geodetic Survey. Of special pertinence to this Symposium have been the records of the U.S. Weather Bureau and the studies by its Extended-Forecast Section.

Where programs of these organizations required cognizance of and coordination with each other's activities, the scientific leaders of projects concerned have met annually as the Eastern Pacific Oceanic Conference (EPOC). These meetings gave rise to joint oceanographic surveys in 1955 of the Pacific Ocean north of the 20th parallel of north latitude (NORPAC) and of the eastern tropical Pacific (Eastropic) and in 1956 of the central and western equatorial Pacific (Equapac).

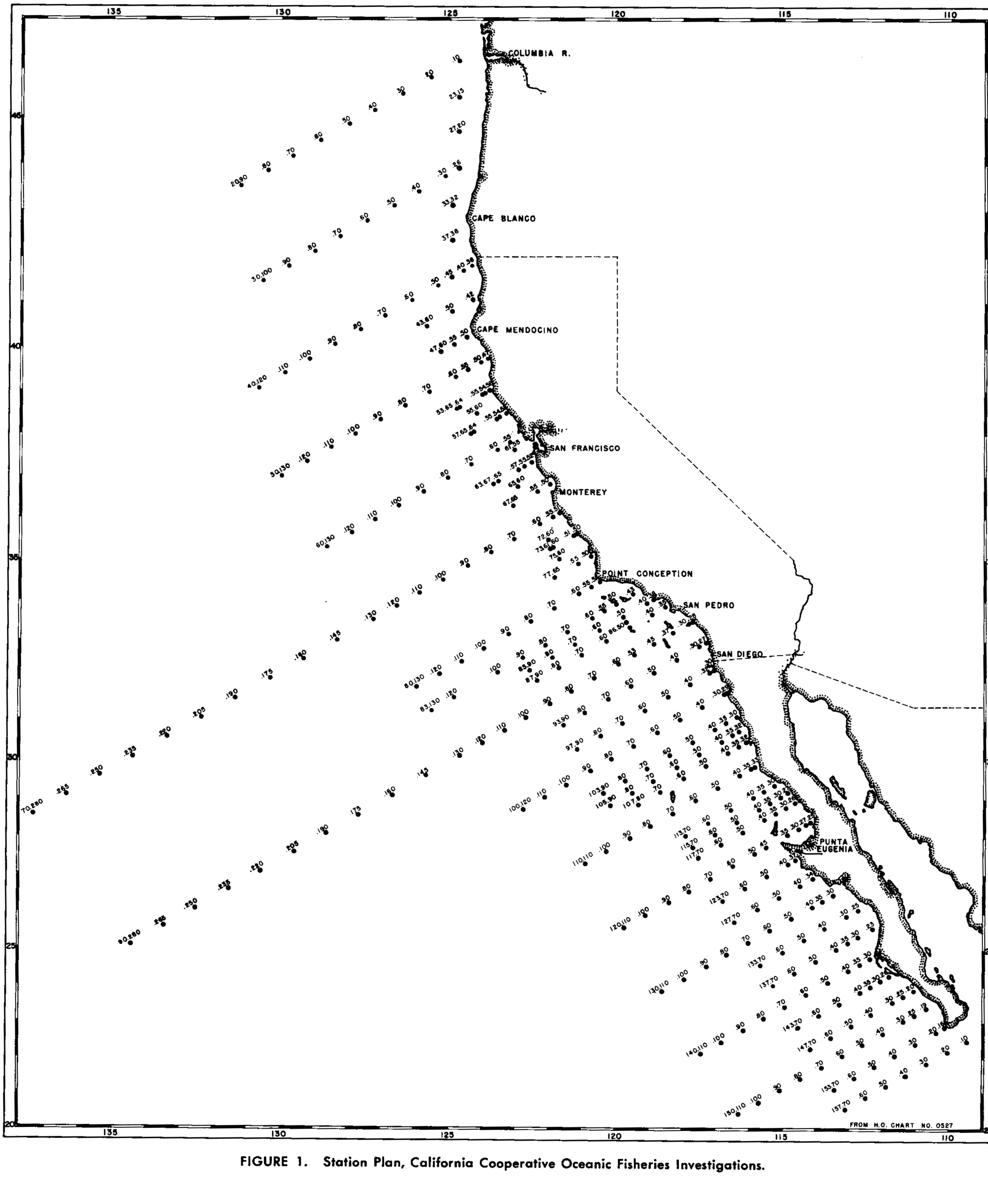


FIGURE 1. Station Plan, California Cooperative Oceanic Fisheries Investigations.

With the information gained from the widespread activities of these organizations by probing the ocean properties and sampling the ocean biota over vast areas, it was thought that, for the first time perhaps, there may exist a background adequately comprehensive and having depth and resolution sufficient to discern the grand pattern underlying the remarkable events of 1957. This Symposium was conceived to bring together the evidence for joint examination by scientists from disciplines of biology, zoogeography, oceanography, meteorology and even astrophysics.

Much of the evidence consists of observations recorded just prior to the Symposium, many of which had not been fully processed and none of which had been comprehensively studied. Therefore, the findings must of necessity be regarded as preliminary. These proceedings, documenting the evidence and the ideas brought together in this Symposium, and bringing about interdisciplinary awareness of interrelationships between atmosphere and hydrosphere and between living organisms and their environments, should increase the effectiveness of further research as to what happened in 1957. This in turn should prepare the way for more discerning observations of future changes and a more penetrating study of their causes and implications.

PROCEEDINGS

INTRODUCTORY STATEMENT

JOHN D. ISAACS

It is very good to see all of you here. You have received some summaries of the changes that the oceans and atmosphere have undergone in the last year. I have been looking forward to seeing the day when this group discusses these important events.

Everyone invited has been able to come with the exception of Dr. von Arx, who is on an expedition. He originally planned to row ashore, fly back here and attend, but it was a bit too complicated for him, I take it. I have a message from him and a short paper, which Fritz Fuglister will read.

Dr. Revelle is not able to be here today, as he had to fly back to Washington on a congressional hearing on IGY. We expect him back tomorrow.

We hope you will have a pleasant, stimulating, and prosperous session, and that an informal spirit will prevail.

I will begin immediately with introductions, using the somewhat alphabetical list on the agenda, which I think each of you have.

First is Dr. Elbert Ahlstrom, Assistant Chief of South Pacific Investigations of the Fish and Wildlife Service on the Scripps Campus. He is associated with much of the work at Scripps, and particularly in the last ten years with the distribution of fish spawning, eggs and larvae in the Eastern North Pacific with the Cooperative California Fisheries Investigations.

Dr. Robert Arthur is next on the list, Associate Professor of Oceanography at Scripps, and Chairman of the Department of Marine Geophysics, teaching and doing other very important work in meteorology and oceanography.

Felix Favorite is from the Seattle Laboratories of the U.S. Fish and Wildlife Service, and has been studying the variations in the locations of the northern fishes, mainly the salmon, and the related oceanographic and meteorological conditions. There is a sort of international problem, I understand, as to the Asiatic and North American salmon population, which the salmon do not quite understand.

Dr. Leo Berner is a member of Scripps staff in Dr. Martin Johnson's Division. Leo is a zoogeographer studying the distribution of the salps, and tomorrow he will discuss the distributions of the salps in the Eastern North Pacific and their variations.

Dr. Edward Brinton, a zoogeographer also, is studying the distribution of the euphausiids as related to the physical conditions of the oceans. He will tell us tomorrow of some biological evidence from the distribution of the euphausiids.

I am sure that Dr. Jule G. Charney is known to many of you as Professor of Meteorology at Massachusetts Institute of Technology and recently of the Institute of Advanced Studies at Princeton. Dr. Charney is a native son of California—one of the few who has wandered to the East Coast. He is the chairman of our session today.

Dr. David Davies came from South Africa a little over a year ago to join the Scripps staff, and has a long history of important work in fisheries as related to oceanography. He has carried out a comprehensive study of the South African pilchard, a fish closely related to our sardine.

Dr. Grant Athay is a guest from the High Altitude Observatory of the University of Colorado. Some time ago I inquired of Dr. Roberts, Director of the Laboratory, hoping that he or one of his staff could address an evening session on solar events as interrelated to meteorology. In this letter, I suggested that quite possibly the stretch from biology to astrophysics was a little wide, and that they might not want to participate in the whole Symposium. However, I found that they felt no barrier, and thought it would be a valuable Symposium from all standpoints.

Dr. Carl Eckart is known to most of you I am sure. He is Professor of Geophysics at Scripps. To enumerate his honors would sound as if I were bragging that I knew him.

Dr. Nicholas Fofonoff is a guest from the Pacific Oceanic Group at Naniamo, British Columbia. He is concerned with dynamic oceanography and he will tell us of the north Eastern Pacific oceanographic conditions in today's session.

Dr. Richard Fleming is certainly known to all of you. He is Professor of Oceanography and head of the Department of Oceanography at the University of Washington. He won his medals as co-author of *The Oceans* and other important contributions.

Fritz Fuglister, from Woods Hole, has long been interested in dynamic oceanography. I am glad that the Atlantic is not acting up to such an extent that the Woods Hole Oceanographic Institution staff is unable to spend its time worrying about the Pacific.

Dr. Harris Stewart is with the U.S. Coast and Geodetic Survey in Washington. He has been studying Pacific sea levels and tide records and will report on this. He is a former student of Scripps, and was a shipmate of mine on Capricorn Expedition.

Dr. Francis Haxo, member of the Scripps staff, Associate Professor of Marine Botany and head of the Division of Marine Botany. Tomorrow Francis is going to tell us about Dr. Balech's findings as to what the phytoplankton has been doing off the California Coast.

Dr. Carl Hubbs, head of the Division of Marine Vertebrates at Scripps, has been keeping tab on the Pacific for many years; fishes, birds, whales, temperatures, and prehistoric man. Some years ago he initiated a temperature survey and carried out extensive studies of coastal temperatures along the Baja California Coast. Today he is going to discuss the Quaternary history of Pacific climates.

Next is John Isaacs, Associate Professor of Oceanography and Program Director of Marine Life Research. A naive, enthusiastic sort of person.

Dr. Martin Johnson, well known to many of you, also is co-author of *The Oceans*, and Professor of Marine Biology in the Division of Marine Invertebrates. He has stimulated the work on zoogeography that Dr. Berner and Dr. Brinton will discuss tomorrow. His own work on the phyllosoma larvae of the spiny lobster, is important evidence for us to consider.

John Marr, Chief of South Pacific Investigations of the U.S. Fish and Wildlife Service, has a long history of thoughtful research in oceanography as related to fisheries. With John Radovich and myself, he sits on an advisory committee of the California Cooperative Oceanic Fisheries Investigations.

Dr. Walter Munk is Professor of Geophysics at Scripps, and at this moment is writing a book on his subject. He does not like to be called the "world's greatest living oceanographer," which reminds me of the Pacific Proving Grounds where they have a regulation that you cannot send a congratulatory message home, so we commonly send a message saying "Regulations forbid me to send a message congratulating you on your birthday."

Garth Murphy from Honolulu, is Director of the Fish and Wildlife Service's Pacific Oceanic Fisheries Investigations. Garth is responsible for some of the charts that I enclosed with the prospectus. He is going to discuss Central Pacific conditions.

Jerome Namias of the Weather Bureau, Chief of the Extended Forecast Section, will lead off the session this morning with the meteorological picture of 1957-58. In a recent letter, he said that he has become more and more excited about what is going on in the Pacific.

John Radovich of the California Fish & Game is head of the Pelagic Fishes Investigations at Terminal Island. I am sure that John is going to try to show that fishes are better oceanographers than we are when he tells about the redistribution of fishes in the last year.

Joseph Reid, member of the Scripps staff, has for some years been oceanographer for the Marine Life Research group, carrying out important work in the eastern North Pacific. He will tell us about the conditions in this area over the last decade.

Dr. Roger Revelle cannot be here today, as I have said.

Gunnar Roden is associated with Joe Reid in the studies on the meteorology and oceanography of the eastern North Pacific. Joe and Gunnar have collaborated in a comprehensive paper on this subject.

Ted Saur is from Dr. Sette's laboratory in Palo Alto, and has a long history of important work in oceanography at Scripps, and in the Navy as Aerological Officer, and with the Naval Electronics Laboratory as a Research Oceanographer.

Dr. Elton Sette is with us as sort of a father confessor for all those who think they have found a relationship between meteorology, oceanography, and fisheries in general. He already has thought of all these relationships many years ago. Dr. Sette, the head of U.S. Fish and Wildlife Service's Ocean Research at Stanford, has spent his time exclusively with these relationships in the last few years. It is a compliment to the Symposium that he has come here.

Dr. Milner B. Schaefer is in charge of Inter-American Tropical Tuna Commission, a Pan American organization, located on the La Jolla Campus, and is studying oceanography in association with high seas fisheries—mainly tuna.

Henry Stommel is a staff member of the Woods Hole Oceanographic Institution, whom we can think of as an East Coast Walter Munk.

Dr. Yositada Takenouti is a guest from the Japanese Meteorological Agency. Dr. Takenouti will tell about what is happening on his side of the ocean.

Another visitor, Dr. Warren Wooster, Oceanographer at Scripps, has just retired to Estados Unidos after a couple of years with the Consejo in Peru.

I would also like to introduce Dick Schwartzlose, secretary of this Symposium, and an important member of the Marine Life Program; and our fair recorders, Barbara Edwards and Virginia Wyllie. Mrs. Buck, who is hostess, will take care of any letters you want typed, arrangements, telephone calls, etc.*

Shortly I will turn this session over to Dr. Charney, but I wish to say one or two words about this Symposium. You have listened to these introductions and, of course, realize that there is a very wide interdisciplinary representation of participants. This is really not a requirement that we imposed, but one that was dictated by the oceans. I hope we can, indeed, look across these interdisciplinary boundaries and find all existing shreds of evidence that might place restrictions upon the model that we erect.

I hope that we can have a free and easy session here. People can wander up and look at the charts and feel free to ask for explanations that will make things clearer, and I hope that discussion will be general.

I think to a great extent that this change we are looking at may be a matter of increased attention in the last 15 years. The amount of attention given to the oceans, to meteorology, indicates a world-wide interest in these matters. Reports of unusual happenings come from wide places. More people are in more strange places, writing letters about more strange

* The following staff members of Scripps Institution also attended the Symposium on the 2d and 3d days: Dr. June Pattullo, Assistant Research Oceanographer; Margaret Robinson, Chief, Bathythermograph Section; and Hans Klein, Head, Data Collection and Processing Group. Eds.

happenings. We easily could break this Symposium into a series of anecdotes about these changes. But much of this is the result of interest and attention. Despite the unusual appearances of many of these changes, I feel that this is not a thousand-year change nor a fifty-year change, but perhaps a ten-year change.

Before I turn this over to Dr. Charney, I wish to mention another bit. I have been looking at air temperatures along the coast with the idea that for selected marine stations they were a rough integration of the oceanographic temperatures, which were not so much determined by the conditions at one point, but rather reflected the conditions over a rather larger area.

This chart (Fig. 2) starts in 1926 and continues to the present. It gives the air temperature at San Diego as fall means, annual means, and spring means. We see that in the last ten years, spring means and annual means have been singularly unchanging. Before then we had wide fluctuations year after year up to 1948. The other marine stations, figure 2, display similar trends. Thus we are looking in this Symposium from the base line of a recent decade that was relatively changeless. That is, it so happens that the most intensively investigated decade was a monotonous one, year after year, compared with the previous two decades, and possibly compared with the last five.

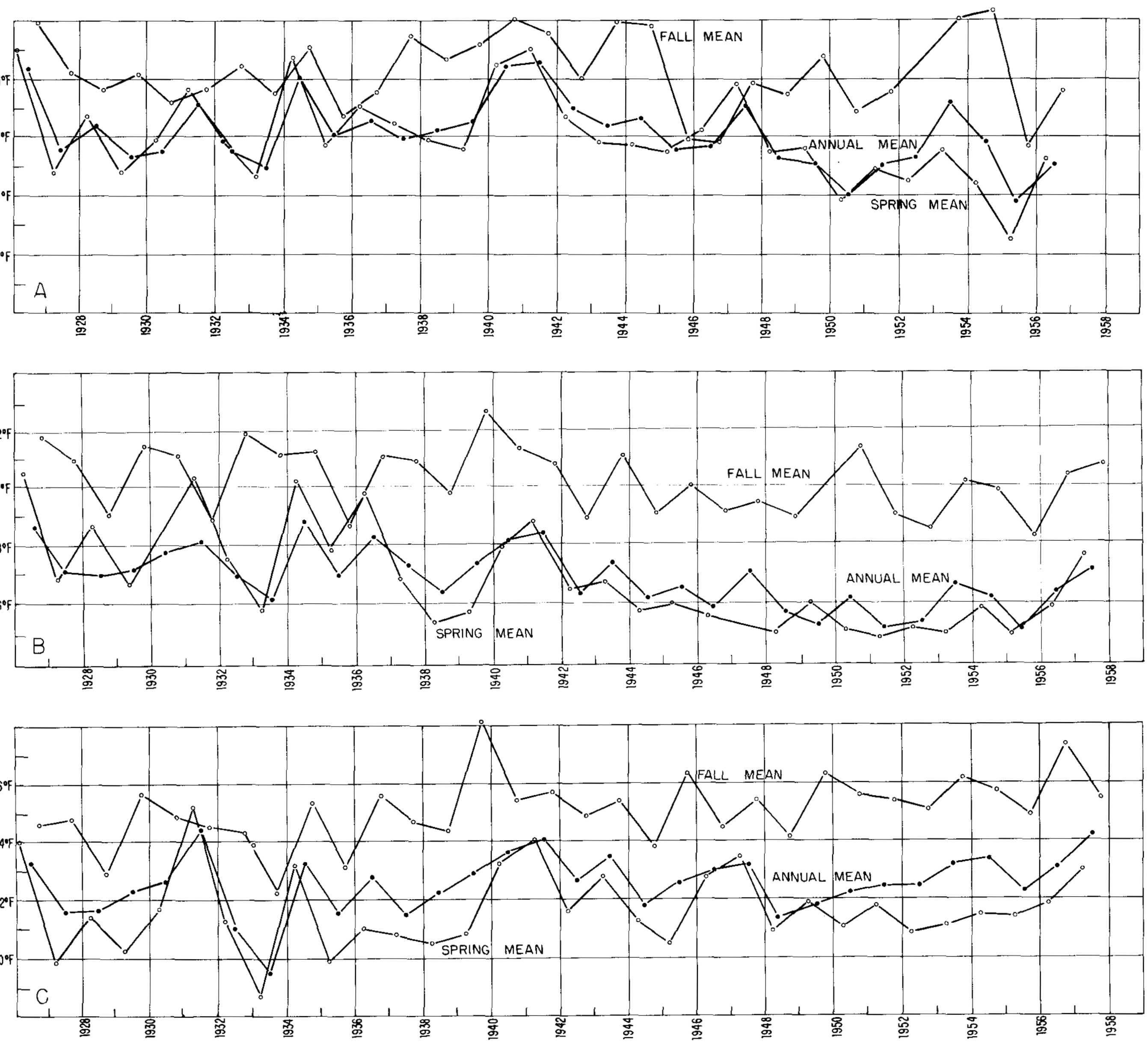


FIGURE 2. Mean Air Temperatures (Spring, Fall, Annual). A) Tatoosh Island, Washington. B) San Francisco, California. C) San Diego, California.

SECTION I

THE PHYSICAL EVIDENCE

CHAIRMAN'S STATEMENT

JULE G. CHARNEY

I consider it a very great honor to be invited to be chairman of this session. I have been searching for the rationale which lay behind the invitation, and the only explanation I could possibly find is that as a non-oceanographer, I am probably the most naive person here, and perhaps it was hoped that out of the mouths of babes the truth would come. At any rate, I shall derive courage from my naiveté to make some comments.

When one looks at the program of the meetings, one cannot fail to be struck by the extreme diversity of the subjects to be discussed. Every conceivable time and space scale is represented. We are going to talk about phenomena with periods of the order of days, weeks, months, years, and even, I notice in Dr. Hubbs' title, millenia. Although the spectrum is very broad, it seems to have a maximum intensity at about a year. As for the space spectrum, the preliminary data indicate that it is equally broad, ranging all the way from narrow boundary phenomena to phenomena embracing an entire ocean. Of course the atmosphere is also involved, as is the solid earth, and this evening we shall have a lecture on solar events.

So that this makes me feel that oceanography is one of the few fields of human endeavor in which the Renaissance spirit persists, where it is possible in a finite time and in a finite way to encompass a subject so vast as the one we propose to treat here. As in a Renaissance court of the 15th or 16th century, we shall talk about biology, physics, mathematics, astronomy, and, I'm sure, philosophy and ethics.

Yet, I think that the intention has certainly been, despite all the diversity, to discover a unity. I do not know what the unity is going to be, so I shall not attempt to influence the course of the discussion, but I do think that the most desirable method of procedure is to present the facts as concisely and as briefly as possible, and as soon as possible to bring the audience into the discussion, because I think that we should avoid too great a preoccupation with one or another of the facets of a given phenomenon in order not to lose sight of the features it has in common with other phenomena. We may thus hope to discover the causal factors that hold them all together.

It is appropriate that the first speaker is Mr. Namias, who will discuss the atmospheric events that are presumably responsible for the oceanic changes. I wish to say here that we meteorologists have been particularly at fault in regarding oceans as a kind of passive body which merely responds to the motions of the atmosphere. It is clear, if one considers long-period changes, that this cannot be the case. Indeed, for long-term changes, it is probably more the other way around. Ultimately we shall have to consider the oceans and atmosphere as a coupled dynamical system. I would hope to see as a result of these meetings a more fruitful collaboration among meteorologists and oceanographers, resulting perhaps in the discovery of causal relationships between changes in ocean and atmospheric circulations.

A last remark—we have eight speakers today and approximately five hours of time to devote to formal presentation and discussion. I think that the most fruitful method of procedure is to keep the formal presentations brief and to bring the audience in as soon as possible. Then if the speaker has more facts that he wishes to communicate, he can let them come out during the discussion. In keeping with the informal intention of this Symposium, members of the audience should feel free to break in at any point during the discussion. To prevent my own from becoming longer, I will introduce the first speaker, Mr. Jerome Namias, of the U.S. Weather Bureau, who will give us the meteorological picture.

THE METEOROLOGICAL PICTURE 1957-1958

JEROME NAMIAS

This particular problem of change, climatic change, is really quite an old and familiar one. It arises in many branches of geophysics, and in meteorology it is especially common. As John Isaacs said, the correspondence from the public on this subject is quite amazing, and the Weather Bureau gets more than its share of these letters. People want to ascribe abnormal weather to almost anything one can think of. Each generation seems to espouse some theory as to the changing climate. Of course, the present vogue is to blame things on nuclear tests. I have one personal correspondent—a one-way correspondence—who makes it a habit to ascribe each and every extreme weather phenomenon to bomb tests. Of course, his reasoning is entirely *post facto*.

Actually, from the standpoint of meteorology, the weather is always changing, never static. The atmosphere is a restless medium undergoing all sorts of transitory variations—not only on an hourly or daily scale, but also on a weekly, monthly, yearly, decadal, and greater scale up to the ice ages. Those of us engaged in modestly attempting to detect and predict some of these variations work with periods of the order of a week to a month. We would indeed be surprised if there were no major changes taking place —even of the order of a year, two years, or even a decade. This does not mean to say that we have illusions about the ease of interpretation or explanation of these climatic changes. We simply do not know the answers now, although there are a few tantalizing leads. I was particularly gratified to be invited to this Symposium because I believe that we can no longer ignore transitory fluctuations of the oceans in treating atmospheric motions. Normally, most meteorologists are content to treat ocean and land surface as fixed and unchanging terrain, which modify the air masses in transit above. It is usually assumed that the only oceanic variations that affect atmospheric motion are the normal seasonal changes of the ocean temperature; and all surface temperature variations are more or less tossed aside as being small or insignificant—meteorologically insignificant, that is. The relatively large variations in air temperature, pressure, and wind tend to make meteorologists feel this way. However, to me the ocean temperature fluctuations or anomalies seem striking and important for it is conceivable that they may exert rather important feedbacks on the atmosphere.

I will mention one example of such a feedback. In 1955 New England had two hurricanes, Connie and Diane. Normally a hurricane produces very heavy precipitation as it moves inland, but these two storms produced over some areas precipitation equalling almost one-half the normal annual total. Now in examining that particular summer's weather, one finds that the early summer was characterized by marked stagnation and warmth of the atmospheric circulation over Northeastern United States and adjacent waters. This was also associated with a positive temperature anomaly of the ocean surface off the East Coast. As the two storms moved northward off the coast they were imbedded in tropical air from a tropical source region essentially displaced northward because of the higher ocean temperatures. The storms were fed by increased moisture, and I believe this had something to do with the increase of precipitation over and above the amount one might normally anticipate. This case may be an example of a feedback mechanism where atmospheric conditions set up over a long period of a summer season influenced the ocean, which in turn had a subsequent effect on atmospheric behavior. It is quite conceivable that similar events proceed on a larger scale. Therefore, I hope the material brought to light in these meetings will encourage further studies of ocean temperatures, so that meteorologists also might be able to study them in relation to atmospheric phenomena.

While the central problem at this meeting is oceanic variations, I would like to make a few preliminary remarks about the character of persistent recurrences that take place in atmospheric phenomena, because it is likely that this sort of thing also occurs in the oceans. In the first place, whether or not events are highly correlated from day to day, from week to week, and even from year to year, this correlation makes for long-period trends. This sort of thing is not a simple serial correlation, but one where a regime established during one period comes back in much the same form later on. The period of recurrence is not fixed; it may be a few days, a week, or perhaps two weeks, but with some consistency. For example, last winter recurrent cold waves into Southeastern United States were the rule. Recurrence accounts for a lot of the persistence observed in meteorological events, and this, of course, has its influence on the ocean. Figure 3 highlights the nature of this recurrence tendency.

From forty years of historical daily-weather maps, the standard deviations of daily and monthly mean pressures averaged by latitude were computed for the forty Januarys. The striking feature of the figure is that standard deviations of the daily values are about the same as those of monthly means (between the forty Januarys). We get variations between Januarys that are fully as great as between individual days during a given January. This points up the nature of the problem we are up against, and the answer to this riddle will go far toward solving the long range problem. A second point is that weather is not only correlated in time but also in space. If a large scale aberration in atmospheric circulation is happening in one area, say over the Northeast Pacific, it will soon influence circulation in other even remote areas both up and down stream. Each time that an event happens, whatever the cause, it will set off a series of disturbances down stream, and will also have

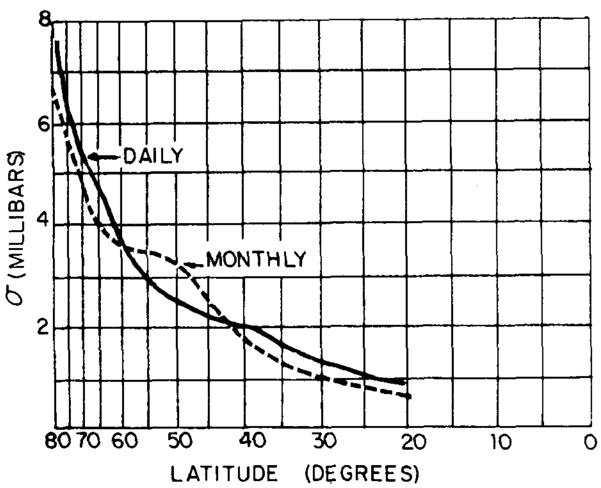


FIGURE 3. Standard deviation of mean sea level pressure along latitudes for daily (solid) and monthly mean (dashed) values for the Januarys from 1899 to 1939; from data prepared by G. W. Brier.

certain up-stream propagating effects. Thus in meteorological phenomena there is a high degree of interdependence in space as well as in time. We understand much more about the interdependence in space than in time.

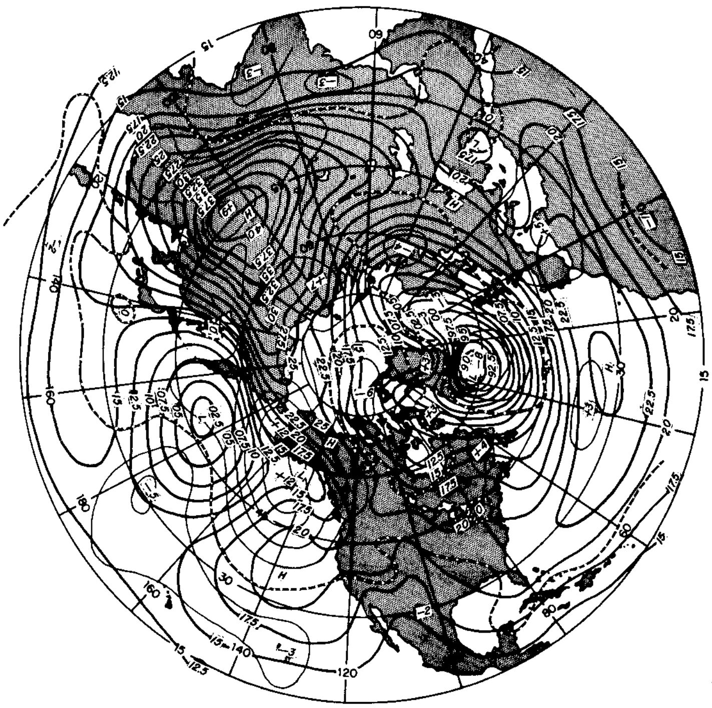
This special interdependence means that if some abnormal form of circulation and weather is occurring in one area, we cannot attribute it solely to peculiarities in that general geographical region. While it might be associated with some abnormality in ocean temperatures of the region, it could be a completely resonant phenomenon due to something which has happened thousands of miles away. More likely, it could be due to several events in several remote areas, each ganging up or reinforcing one another so as to produce specific abnormalities in the concerned zone. Unfortunately, no one so far has been able to determine which circulations are forcing which others in climatic fluctuations of the order of a month or more. When we add to this complex the oceanic system, things must become even more difficult.

In spite of all this confusion, I feel that the water-temperature fluctuations over the Pacific to be discussed here can be rather definitely associated with large-scale atmospheric systems. The scale of systems of which I speak are very large—several thousands of miles—and the energies involved are tremendous. Small-scale patterns like cyclones and anticyclones are superimposed and transitory. The large-scale systems are the great centers of action which are coupled with so-called planetary waves in the upper-level westerlies of the general circulation. An easy way to shed light on these is to average over a long time interval, purposely suppressing the small scale "noise." This has been done for the seasonally-averaged charts for sea level and 700 mbs (Figs. 4-9). These figures are three month means. Winter is considered as December, January, and February; while spring is March, April, and May; summer, June, July and August; and so on. On these charts are also shown (as light solid lines) the departures from normal—that is, deviations from seasonal averages of many years. These isopleths indicate the anomalous component of the total mean flow, which, of course, is composed of the normal flow plus the anomalous component. We also have the departures from normal of thickness between 700 and 1,000 mbs. These charts essentially indicate the mean departures from normal of temperature in the lower troposphere.

Let us now review the highlights of the general circulation of the atmosphere during 1957 and 1958 with the help of these mean charts. The most consistent abnormality of the general circulation since the beginning of 1957 seems to have been the remarkable tendency for weak westerly winds in temperate latitudes. In fact, in only one month since the beginning of 1957 have the temperate latitude westerlies (zonal component between 35°N and 55°N) been appreciably above normal. Weak westerlies or low index conditions are not associated with the same displacements of the centers of action in summer as in winter. In the cold season pressure distributions accompanying low index generally involve an excess mass of air (with respect to normal) over northern latitudes with a more or less compensatory deficit at low latitudes. In such cases the peak strength of the westerlies is usually found farther south than normal, a condition which can be seen from the winter and spring wind-speed profiles at the lower left of the 700 mb figures 8 and 9. In these for half the hemisphere lying between the Greenwich Meridian and 180° we have plotted the zonal component of observed mean and normal wind as a function of latitude. Also in low-index cases during winter the subtropical anticyclones are weak or absent and displaced south of their normal positions. This was especially noteworthy during the winter and spring of 1958. In summer, on the other hand, low index is frequently accompanied by northward displacements of the subtropical anticyclones and a corresponding northward shift of the westerlies.

If we examine in greater detail the anomalous features of the mean seasonal circulations over the North Pacific we see that central and eastern areas have been dominated by a large negative anomaly during the period from summer 1957 through spring of 1958. Moreover, the East Pacific high-pressure area during this period has been much weaker than normal. Note especially the pronounced anomalous southerly components over the Eastern Pacific during winter, 1957-58 (Fig. 8).

Now, if we turn to the water temperature anomalies that Mr. Murphy's Bureau of Commercial Fisheries laboratory have prepared (see Figs. 14-30 McGary paper) we might try to relate them to our meteorological anomalies. At first glance the surface water



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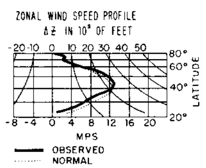
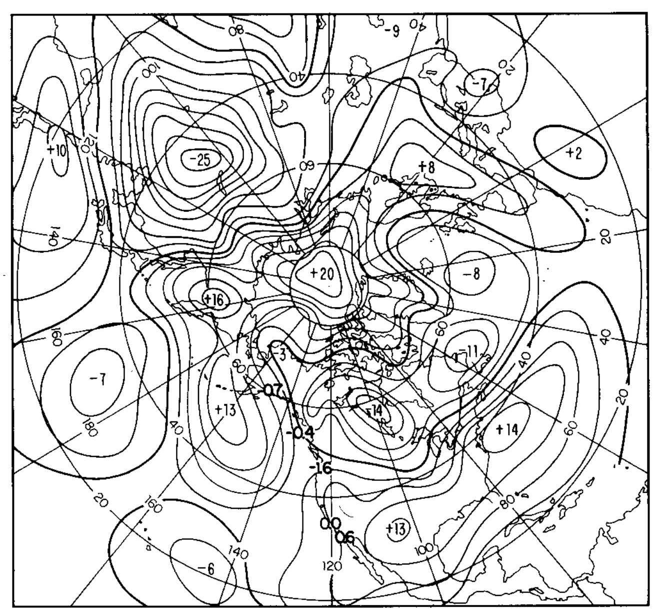


FIGURE 4. WINTER 1956-1957

① *Sea Level*—Isobars (heavy solid) are drawn at intervals of 2.5 mbs. Departures from normal are shown by isopleths (light solid) drawn for every 2½ mb for winter and 1¼ mb for other seasons. The broken line represents zero departures. Numbers represent highest and lowest values in centers.

② *700 mb*—Contours (heavy solid lines) are generally drawn for 200-foot intervals. Isopleths of departure from normal are drawn as light solid lines for each 50 feet, the centers of maximum and minimum being labeled in tens of feet. The broken line represents zero departures. At the lower left of the 700 mb chart is a zonal wind speed profile where the zonal wind speed (for 0° westward to 180°) is plotted against latitude as a heavy solid curve, and the normal as a dotted line.

③ *1000-700 mb Thickness Anomaly*—Isopleths are drawn for every 30 feet, with maximum and minimum values shown by numbers at center. Large numbers along North American West Coast show seasonal departures from normal of surface sea water temperatures (°F).



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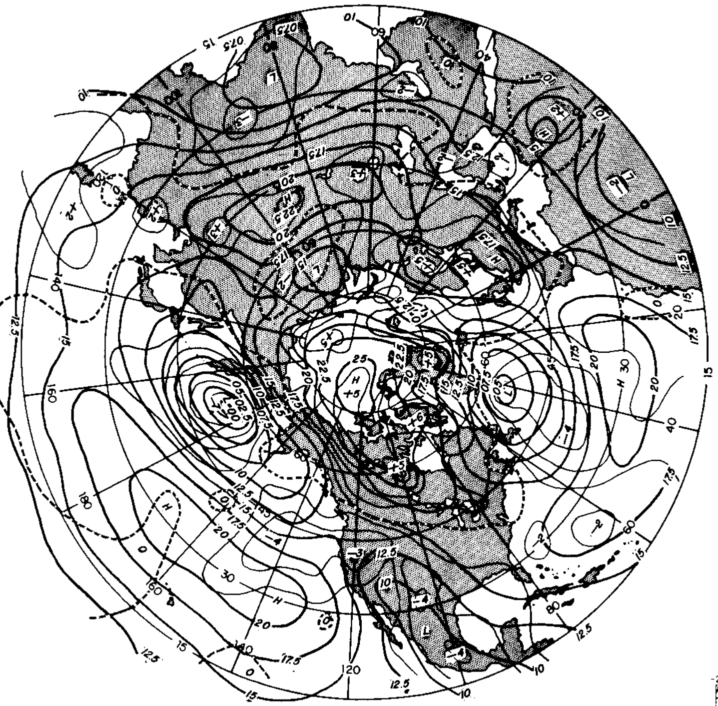

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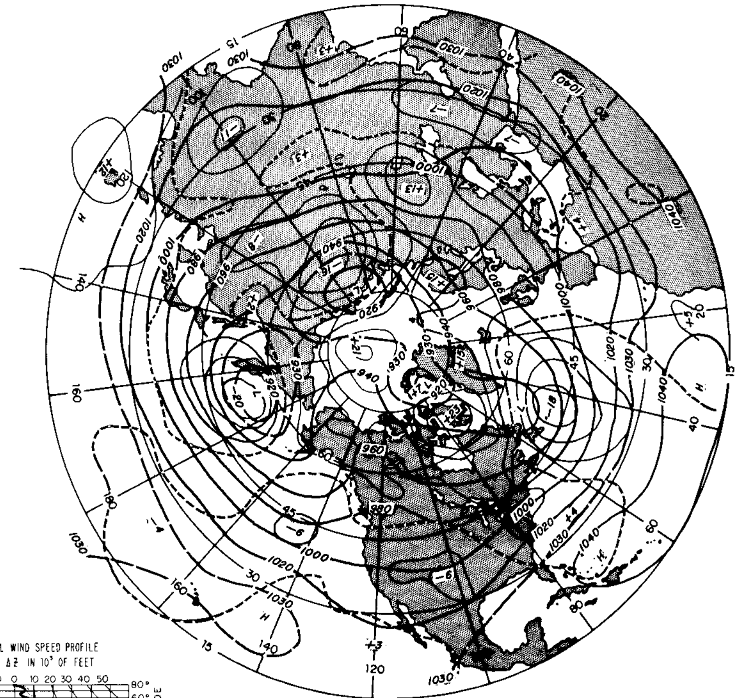

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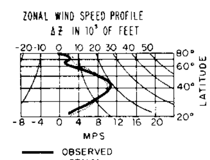
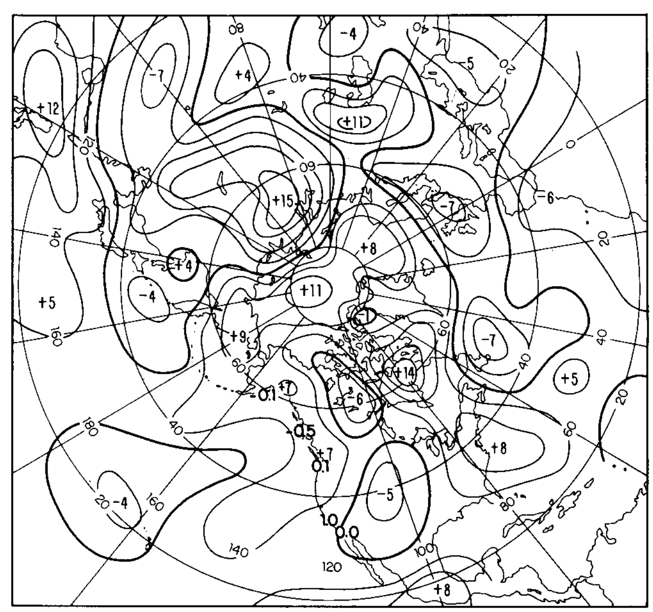


FIGURE 5. SPRING 1957.

① *Sea Level*—Isobars (heavy solid) are drawn at intervals of 2.5 mbs. Departures from normal are shown by isopleths (light solid) drawn for every 2½ mb for winter and 1¼ mb for other seasons. The broken line represents zero departures. Numbers represent highest and lowest values in centers.

② *700 mb*—Contours (heavy solid lines) are generally drawn for 200-foot intervals. Isopleths of departure from normal are drawn as light solid lines for each 50 feet, the centers of maximum and minimum being labeled in tens of feet. The broken line represents zero departures. At the lower left of the 700 mb chart is a zonal wind speed profile where the zonal wind speed (for 0° westward to 180°) is plotted against latitude as a heavy solid curve, and the normal as a dotted line.

③ *1000-700 mb Thickness Anomaly*—Isopleths are drawn for every 30 feet, with maximum and minimum values shown by numbers at center. Large numbers along North American West Coast show seasonal departures from normal of surface sea water temperatures (°F).



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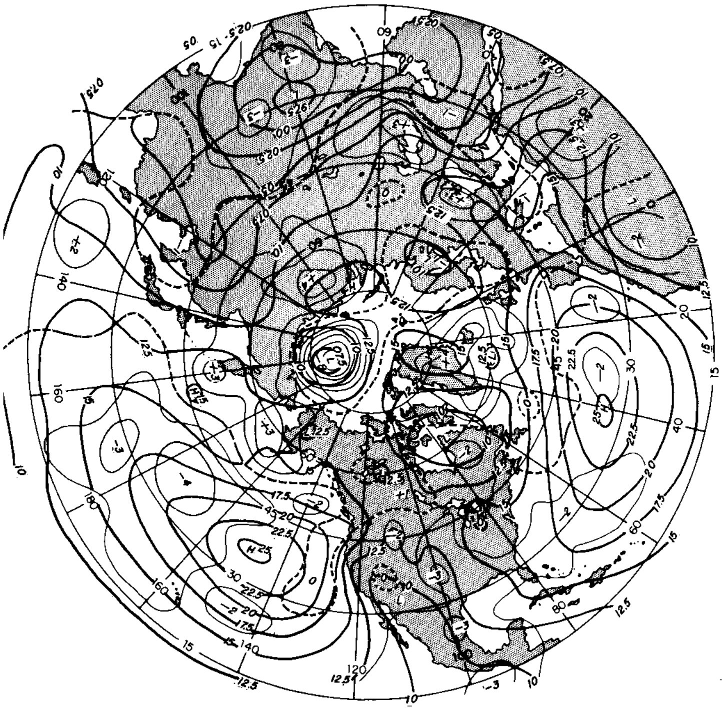

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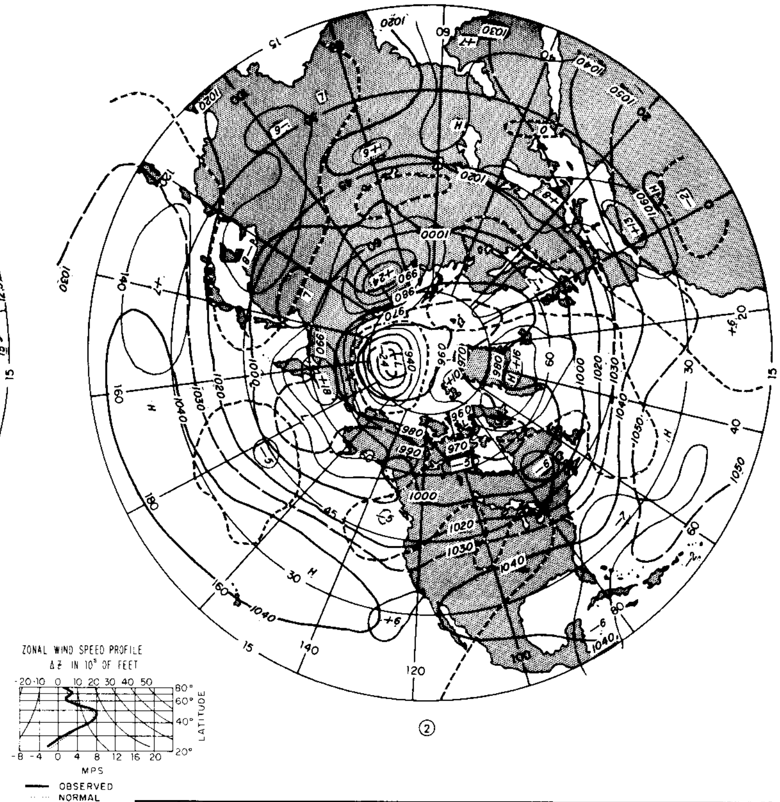

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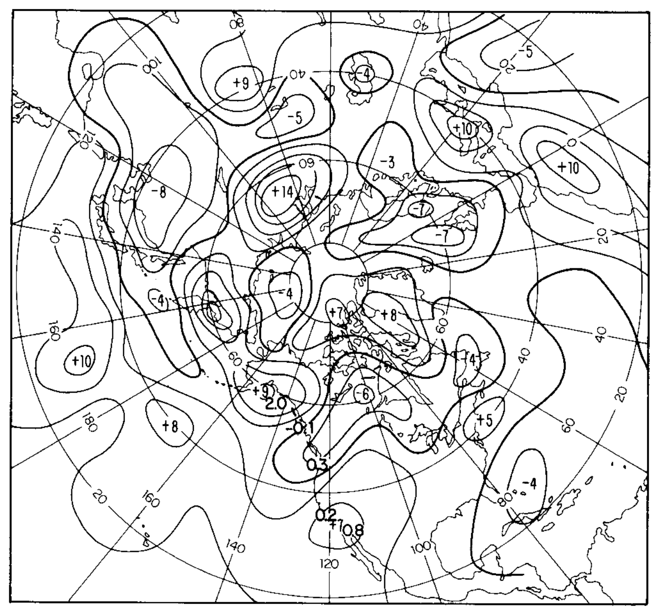
FIGURE 6. SUMMER 1957.

① *Sea Level*—Isobars (heavy solid) are drawn at intervals of 2.5 mbs. Departures from normal are shown by isopleths (light solid) drawn for every 2½ mb for winter and 1¼ mb for other seasons. The broken line represents zero departures. Numbers represent highest and lowest values in centers.

② *700 mb*—Contours (heavy solid lines) are generally drawn for 200-foot intervals. Isopleths of departure from normal are drawn as light solid lines for each 50 feet, the centers of maximum and minimum being labeled in tens of feet. The broken line represents zero departures. At the lower left of the 700 mb chart is a zonal wind speed profile where the zonal wind speed (for 0° westward to 180°) is plotted against latitude as a heavy solid curve, and the normal as a dotted line.

③ *1000-700 mb Thickness Anomaly*—Isopleths are drawn for every 30 feet, with maximum and minimum values shown by numbers at center. Large numbers along North American West Coast show seasonal departures from normal of surface sea water temperatures (°F).



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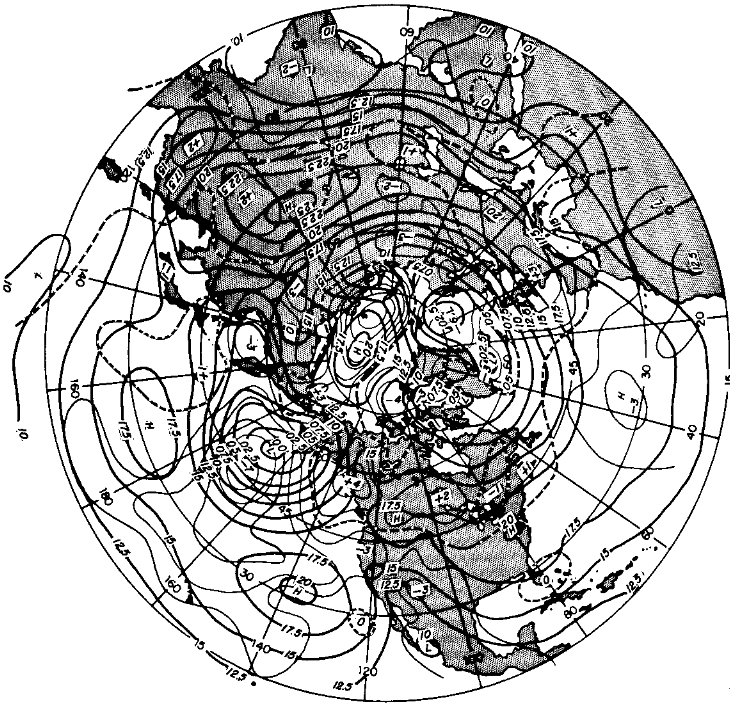

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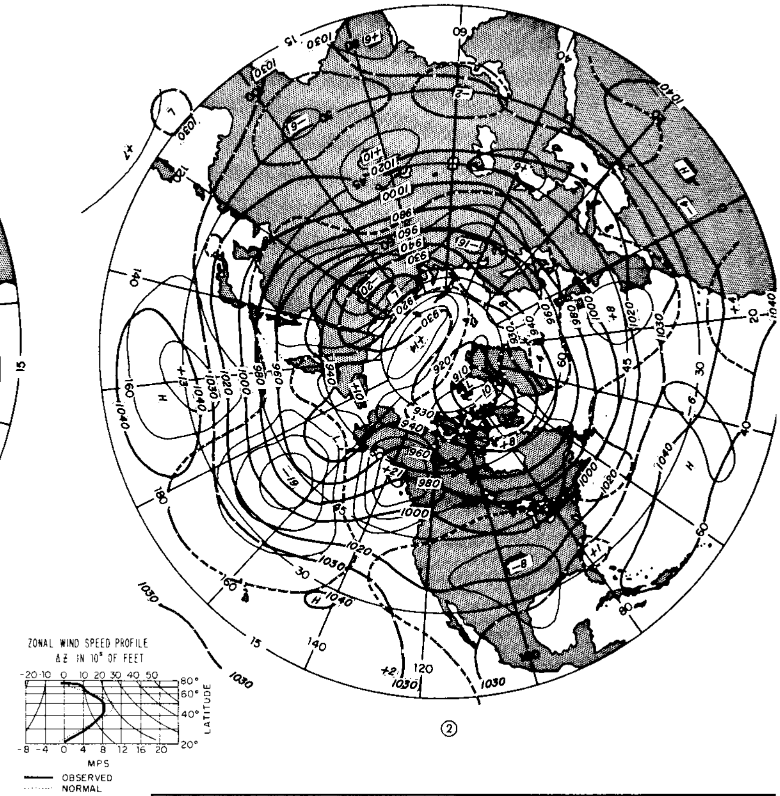

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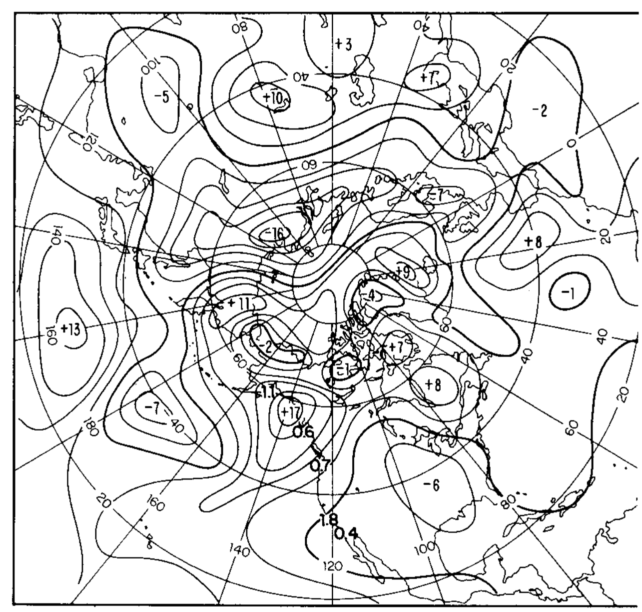
FIGURE 7. FALL 1957.

① *Sea Level*—Isobars (heavy solid) are drawn at intervals of 2.5 mbs. Departures from normal are shown by isopleths (light solid) drawn for every 2½ mb for winter and 1¼ mb for other seasons. The broken line represents zero departures. Numbers represent highest and lowest values in centers.

② *700 mb*—Contours (heavy solid lines) are generally drawn for 200-foot intervals. Isopleths of departure from normal are drawn as light solid lines for each 50 feet, the centers of maximum and minimum being labeled in tens of feet. The broken line represents zero departures. At the lower left of the 700 mb chart is a zonal wind speed profile where the zonal wind speed (for 0° westward to 180°) is plotted against latitude as a heavy solid curve, and the normal as a dotted line.

③ *1000-700 mb Thickness Anomaly*—Isopleths are drawn for every 30 feet, with maximum and minimum values shown by numbers at center. Large numbers along North American West Coast show seasonal departures from normal of surface sea water temperatures (°F).



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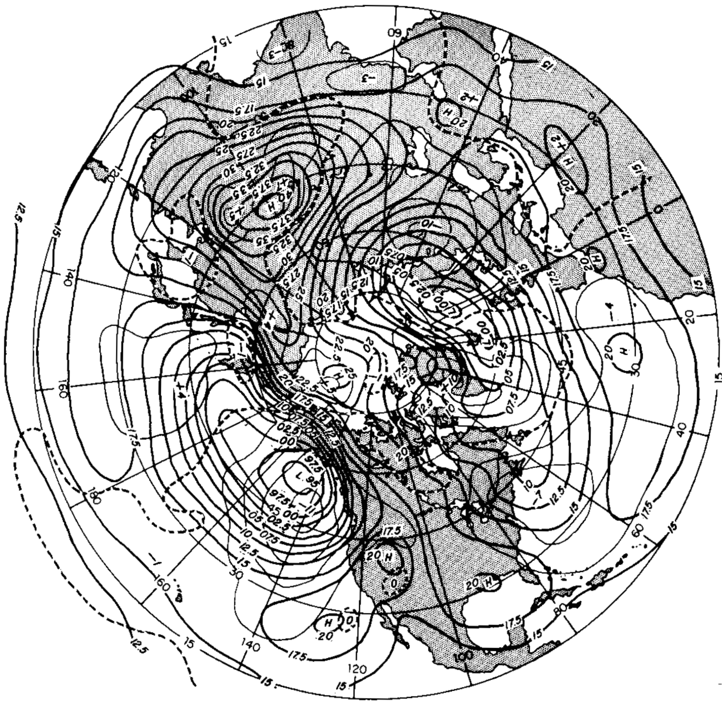

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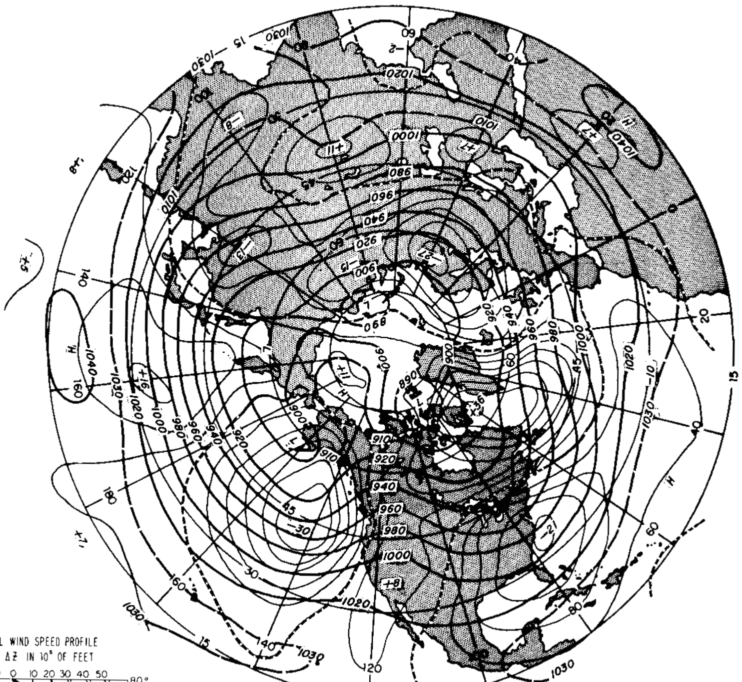

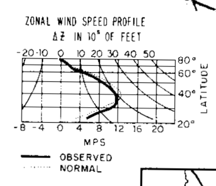

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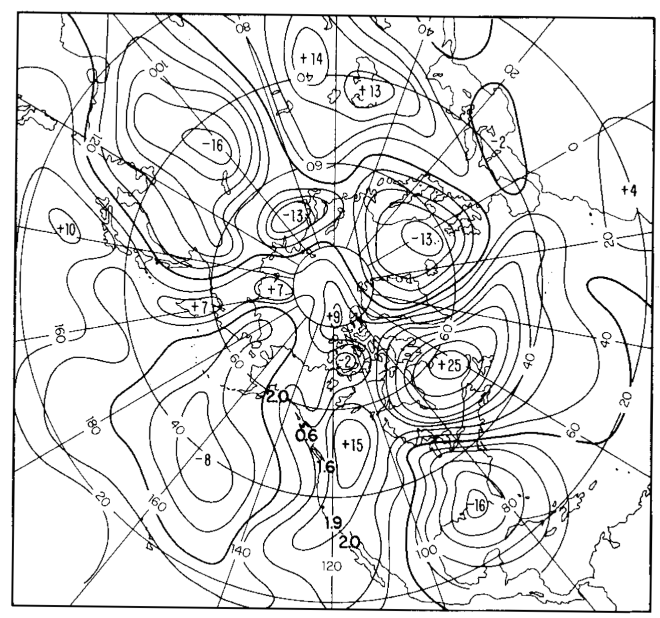
FIGURE 8. WINTER 1957-1958.

① *Sea Level*—Isobars (heavy solid) are drawn at intervals of 2.5 mbs. Departures from normal are shown by isopleths (light solid) drawn for every 2½ mb for winter and 1¼ mb for other seasons. The broken line represents zero departures. Numbers represent highest and lowest values in centers.

② *700 mb*—Contours (heavy solid lines) are generally drawn for 200-foot intervals. Isopleths of departure from normal are drawn as light solid lines for each 50 feet, the centers of maximum and minimum being labeled in tens of feet. The broken line represents zero departures. At the lower left of the 700 mb chart is a zonal wind speed profile where the zonal wind speed (for 0° westward to 180°) is plotted against latitude as a heavy solid curve, and the normal as a dotted line.

③ *1000-700 mb Thickness Anomaly*—Isopleths are drawn for every 30 feet, with maximum and minimum values shown by numbers at center. Large numbers along North American West Coast show seasonal departures from normal of surface sea water temperatures (°F).



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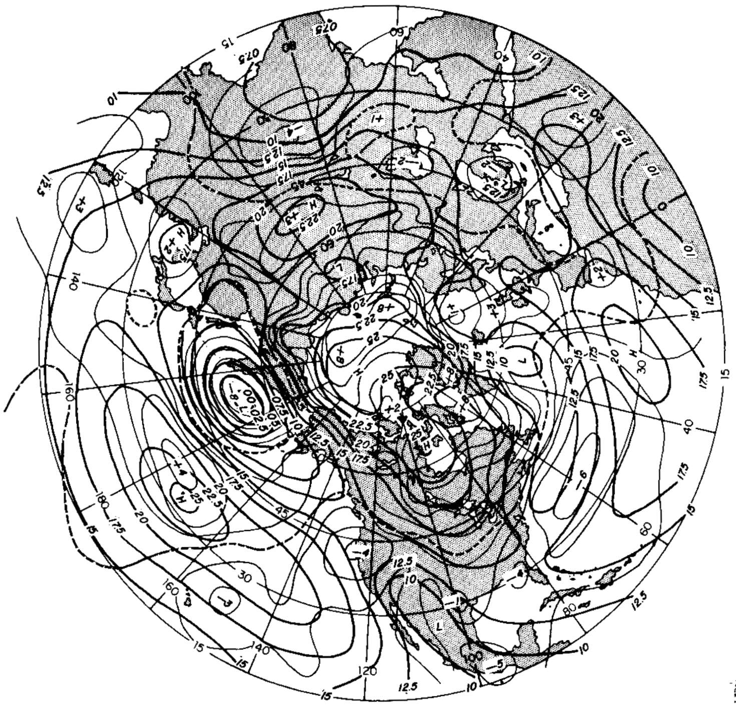

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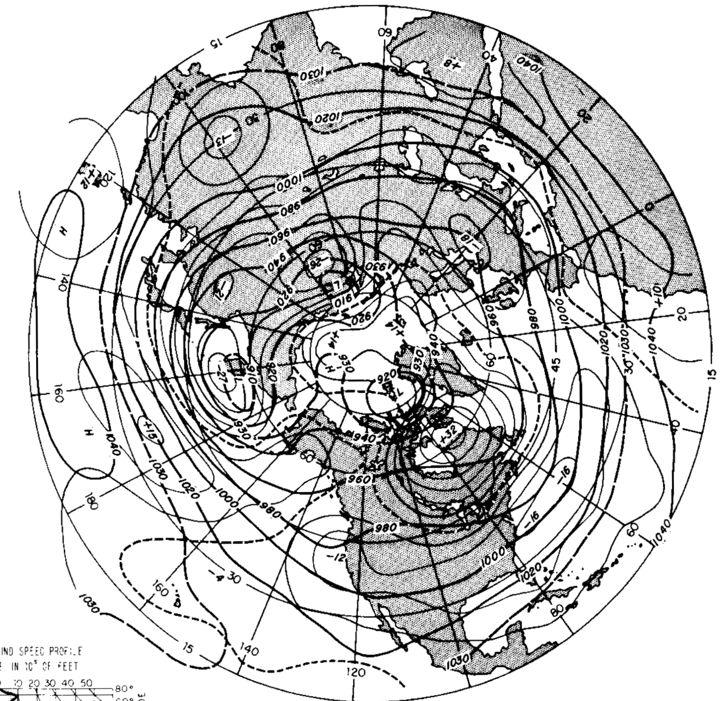

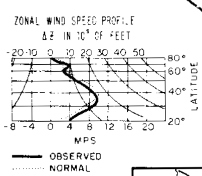

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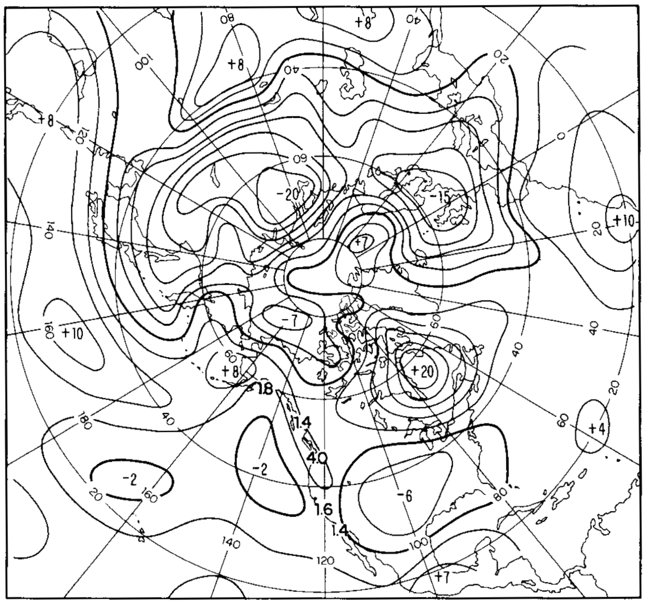
FIGURE 9. SPRING 1958.

① *Sea Level*—Isobars (heavy solid) are drawn at intervals of 2.5 mbs. Departures from normal are shown by isopleths (light solid) drawn for every 2½ mb for winter and 1¼ mb for other seasons. The broken line represents zero departures. Numbers represent highest and lowest values in centers.

② *700 mb*—Contours (heavy solid lines) are generally drawn for 200-foot intervals. Isopleths of departure from normal are drawn as light solid lines for each 50 feet, the centers of maximum and minimum being labeled in tens of feet. The broken line represents zero departures. At the lower left of the 700 mb chart is a zonal wind speed profile where the zonal wind speed (for 0° westward to 180°) is plotted against latitude as a heavy solid curve, and the normal as a dotted line.

③ *1000-700 mb Thickness Anomaly*—Isopleths are drawn for every 30 feet, with maximum and minimum values shown by numbers at center. Large numbers along North American West Coast show seasonal departures from normal of surface sea water temperatures (°F).



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temperature anomalies appear to have little or no organization. In fact, if we look at them too closely we might get the impression that they consist of completely chaotic islands of anomalous warmth or cold. However, if we stand back and try to get the big picture there are undeniable macro-scale features which embrace large portions of the ocean. For example, during September, October, and November 1957, the entire southern portion of the Eastern Pacific (east of 170°W) south of about latitude 40°N was above normal (Figs. 22, 23, 24 McGary paper). North of here the ocean was characteristically warm over eastern portions but with marked changes over the west. In December (Fig. 25 McGary paper) a pattern similar to September arises although with larger departures. In January 1958 (Fig. 26 McGary paper) the field becomes chaotic but by February a more organized state seems to emerge with relatively cool temperatures in central and western portions and marked warmth in the east.

Now the seasonal meteorological picture for fall indicated especially by the 700 mb mean contours and anomalies shows a deep trough with an anomaly of —190 feet over the Central Pacific with ridges and positive anomalies on either side (Fig. 7). The form of these isopleths means that over much of the area east of 165°W the prevailing and resultant wind was composed of stronger south-to-north components than normal. West of here the reverse anomalous components are implied. These mean anomalous components are composed of persistently-recurrent northward (east of 165°W) and southward (west of 165°W) thrusts of air in advance and to the rear of rapidly-developing cyclones moving into a central Aleutian Low, and with sharp polar-front systems persistently found in mid-Pacific during this particular season. It is likely that on each occasion when the southerly-wind component sets in, surface water is set in motion and a general east to northeastward drift becomes established.

If we consult an atlas of maps of normal sea-surface isotherms during winter over the North Pacific, it looks to a non-oceanographer that it does not require much of an anomalous southerly-wind component operating for an extended period of time to effect a material local warming, particularly if we assume that the water conserves some of its heat as it moves. At any rate, it is conceivable that sustained anomalous transports of water might have been induced by the anomalous winds and thereby help to explain the broad-scale sea-temperature anomalies reflected in the U. S. Bureau of Commercial Fisheries charts. Of course, all of this is rather vague and is really more of the nature of a suggestion for further investigation.

An alternative explanation might involve deeper phenomena brought about by some sort of mutual adjustment process between induced current and pressure field and involving density changes of consequence. It seems that this long-period wind-ocean interaction problem is a beautiful one for the dynamic oceanographer. The relationship between anomalous wind and ocean temperature seems to be even clearer after the circulation has settled down in winter of 1957-1958 (Fig. 8). Here the negative air-circulation anomaly is very strongly developed and somewhat farther east than in fall. Note the strong southerly anomalous fetch over the Eastern Pacific. In this case, a strong prevailing southerly wind is represented. I believe that this persistent and recurrent condition was responsible for the extreme warmth of water masses found off the West Coast during February 1958.

Another peak in the curve of surface-water temperature just off the West Coast occurred during the summer of 1957. However, we cannot employ the same reasoning that was used to account for the winter warming. This is so because the mean circulation anomalies are too weak to effect much change. The Pacific anticyclone was not appreciably weaker than normal and the strength of the offshore northerly components upon which upwelling depends do not appear to be much weaker than normal, although they are slightly so. Experience with other summers' maps suggests that there was no great aberration in the northerly-wind component, and possibly the reduced upwelling may account for only a small part of the positive water anomaly. On the other hand, there is a striking anomaly in the upper level trough off the West Coast. For example, there is a +60 foot anomaly off Southern California during summer 1957 (Fig. 6), implying failure of the West Coast upper-level trough to develop in a normal fashion. This was also associated with a summer that was characterized by pronounced warmth along the entire California Coast.

Now it is well known that positive anomalies imply less cyclonic vorticity and less ascent of air than normal. Therefore, it is possible that the stratus cloud deck was somewhat thinner and more disintegrated than is normally the case in this area. I understand that during calm conditions in summer the uppermost layers of the ocean respond more than at other seasons to solar radiation and I propose that some of the warming here may have been associated with increased absorption of enhanced radiation through weaker screening by the poorly-developed stratus cloud-deck. Here again I am merely tossing out a suggestion for further study where numbers must be employed.

At any rate, the implication is that the long period of warm water off California may result from a combination of different mechanisms. The summer and early-fall warmth might have been associated with increased absorption of solar radiation, diminished evaporation, and diminished upwelling, while the late fall and winter warmth may have been produced by some sort of a surface-transport process as I indicated.

If we now make a rough comparison between mean-thickness fields and the corresponding charts of sea-surface temperature anomaly, it seems that a definite positive correlation exists. For example, during winter the warm water off the West Coast of North America can be associated with warm air in the lower troposphere, whereas in the Central Pacific anomalously cool air is found over anomalously cool water (Fig. 10). Then again, positive correlation exists in fall, especially in the Gulf of Alaska, the Central and

Eastern Pacific. Of course, some postive correlation might be anticipated, first, because low layers of air are in contact with the sea and are modified by the underlying supply of heat especially in the free ocean.

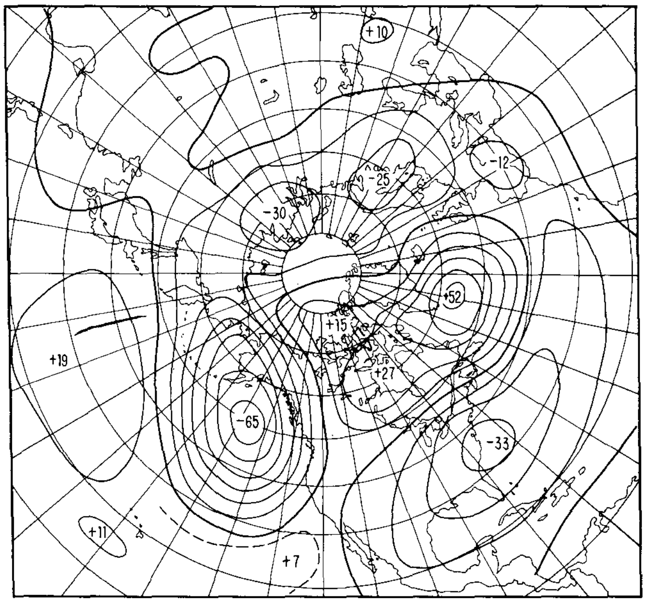

FIGURE 10. 1000-700 mb thickness anomaly change chart. Change of Winter 1957-1958 from Winter 1956-1957 in tens of feet.

Secondly, patterns of thickness anomaly are related to the anomalous atmospheric flow—that is, anomalous southerly currents of air not only drive water northward and eastward but they also reflect advection of warm air masses from the south (or they diminish the northerly component) and this almost always results in positive air temperature anomalies. If you take time to look at these thickness charts and compare them with the circulation anomalies you will see this general relationship. In other words, warm-air temperatures frequently accompany warm-water temperatures because both may be responses to abnormal forms of the general circulation. I prefer to believe this is the primary reason why it has been so warm along the West Coast during the past winter, rather than to ascribe the air warmth chiefly to the sea warmth. Apparently, there is a three-way relationship between anomalous patterns of mean height, or pressure, mean thickness, and mean sea-surface temperatures. This relationship could be helpful in oceanographic-atmospheric analysis. I should think that the analysis of sea-surface temperature fields could be done much more efficiently if the atmospheric thickness fields were considered. In other words, the use of the thickness field might help detect errors in sea-surface temperature observations and it might help in constructing smoother temperature patterns, thereby eliminating some of the apparent chaos.

So far I have talked only about the passive role the ocean plays in atmospheric-ocean interactions. However, one of the chief reasons why this general topic fascinates me is the possibility that, once disturbed in its thermal structure for a long period, the ocean surface may play an active role in modifying or even generating atmospheric-circulation anomalies. This is an old idea that has been resurrected and dusted off on many occasions, and I do not have much new to add to the inconclusive scraps of evidence presented up to now. However, in studying over the figures presented here, a sliver of light appears regarding the long-period evolution of mean patterns from summer of 1957 through spring of 1958. This is hopeful since it is usually impossible to detect continuity of features on mean maps for consecutive seasons. In this case it seems we can follow the course of the negative anomaly in the 700 mb circulation from its initial summertime position at about 180° south of the Aleutians, gradually eastward in the following fall, winter and spring, arriving off California in spring. These negative anomalies and associated troughs are almost invariably associated with increased cyclonic activity. If the long-period continuity suggested is real, two questions arise: (1) What accounts for the long lifetime of such systems? and (2) Why should such an area move with some sort of regularity in seasonal means?

Let us take the second question first. As summer goes into fall there are two climatological phenomena over the North Pacific and adjacent areas that are very highly probable—that is, they take place almost every year. (1) the westerlies increase in strength, and (2) an upper level trough along the east coast of Asia becomes established. Both these phenomena are associated with the increased cyclonic activity developed off the Asiatic Coast with the onset of the Asiatic cold season monsoon, and with increased baroclinic developments of these cyclones as they approach the Aleutians. Now, if we first assume that some factor imparts a long lifetime to the negative anomaly we may roughly explain its eastward motion like this: The trough found in the vicinity of Korea and Eastern Asia in summer becomes more or less locked into position along the Asiatic Coast in fall. As the westerlies increase in strength in fall, the preferred position for the next downstream trough would be expected farther to the east than in summer. This reasoning qualitatively employs Rossby's idea of the stationary wave length of planetary waves which, as is well known, increases with the zonal wind speed. A similar trend in winter may explain the further progression of the trough and negative anomaly. However, the continued eastward motion in spring is not so readily explained, although certain studies of so-called blocking phenomena (where a low latitude trough is surmounted by a high latitude ridge) indicate a preference for the Eastern Pacific area in spring, especially when a fast westerly current exists upstream as it does in this case off the east coast of Asia.

Now, we come to the first question; namely, why such an anomaly center should enjoy a long lifetime. Here we speculate that in this case it is possible that the warm water developed to the east of the negative anomaly and the general contrast between this water and cooler water to the west might be of consequence. The warmer water would provide a better heat and moisture supply for cyclones to feed on and the horizontal contrast might assist in developing thermal asymmetries favorable to cyclone development. But since the stationary wave length is in itself changing, the area of influence would itself be changing position and might be expected to shift.

Of course, this is the sketchiest type of hypothesis and it is only the delightful informality of this meeting that encourages me to suggest it. But the history of meterology and perhaps oceanography indicates that such imaginative excursions are probably initially required for progress. Perhaps some dynamic meteorologists and oceanographers may explore such a simple hypothesis and formulate mathematical models that could either expand upon it or completely negate it.

DISCUSSION

Isaacs: Can you say from the historical maps how often these anomalous situations arise?

Namias: That question puts me on the spot. It seems easy to break records in meterology, so I will say this: I have not found any circulation of this kind as persistent as this one has been. In other words, in about 25 years of record, I have not seen as strong a long-period aberration of this precise kind and strength over the North Pacific.

Question: Do 1931 and 1940 stand out?

Namias: Yes, looking over past maps over the North Pacific, the winter of 1940-41 seems to have had some marked similarities, but it is not as intense. It also had similar characteristics when viewed as the general circulation from the Eastern Pacific on to the Eastern Atlantic. In 1931 adequate upper air data were lacking, but perhaps the sea-level weather charts could be studied.

Saur: They show it to be similar.

Stewart: These similarities also show up on some long-range monthly charts of sea level.

Schaefer: Do you know whether there are internal oscillations in this system?

Namias: I think Charney can answer that better than I. It is rather difficult to conceive of internal oscillations with this time period. We have been able to disentangle some internal oscillations of the order of a week or possibly up to a few weeks, but this latter seems pretty long. Sometimes one can follow something that suggests a general redistribution of vorticity over a week or so. Anything longer than that, nobody really understands. There is some evidence that there can be long-period trends such as the so-called index cycle in which the position of the westerlies migrate southward and then northward over a period of roughly four weeks. Anything longer than that we simply have little idea about. But certain interactions between circulations over widely separated areas, as between the eastern North Pacific and the Western Atlantic are fairly well recognized. For example, the intense cyclonic activity off the East Coast of United States and the cold waves in Florida last winter can be associated with the abnormal East Pacific trough.

Stommel: How does the Atlantic meteorological abnormality compare in magnitude with this? As I remember it, it would be almost of this order.

Namias: Yes, it would.

Question: Was it connected with the Pacific, or was it in the Atlantic alone?

Namias: It was associated with the Pacific, but in the Atlantic case there was a great blocking anticyclone. No two cases are identically alike.

Charney: Everyone is concerned with the catastrophic importance of this change. Is that really very important? Is it not important to try to explain changes whether they are catastrophic or not?

Schaefer: Certainly these things do have a certain recurrence. These same periods that you have mentioned tally with the abnormalities off Peru in 1941, for example, almost exactly. One interesting thing was that 1953 was an abnormal year in the Peruvian situation though not so extreme as this last year.

Namias: It would seem to me logical that there was some interconnection between the abnormal wind systems of both hemispheres, but no one has been able to document this idea. Maybe with the help of IGY data we can piece things together.

EL NIÑO[1]

WARREN S. WOOSTER

One of the most celebrated of oceanic disturbances is that known as *El Niño*, an occurrence of the first half of the year which is reported at irregular intervals from the coast of Northern Peru. Conspicuous outbreaks were reported in 1891 (Schott, 1931) and 1925 (Murphy, 1926) and more recently in 1941 (Lobell, 1942), 1953 (Posner, 1957) and 1957-1958. Similarities between this phenomenon and conditions observed off the California Coast and in other coastal upwelling zones suggest that the underlying causes of the observed abnormalities are the same in such regions.

Unfortunately it is not easy to discuss the Peruvian *Niño* in quantitative terms. The lack of a long record of systematic observations throughout the year makes it difficult to determine satisfactory averages with which to compare observations believed to be abnormal. Although it has been possible to establish a crude picture of the seasonal march of surface temperature along the Peruvian Coast (see later), little is known about changes in surface salinity; subsurface conditions are even less well known. In order to look for meteorological changes which might produce *El Niño* one would like a set of average pressure charts such as Dr. Namias has prepared for the Northern Hemisphere—but no such information is readily available.

With such a paucity of quantitative information it is not surprising that the characteristics of *El Niño* have never been well defined. One symptom is common to all reports—the presence of unusually high sea surface temperatures. Other frequently mentioned features include a southward coastal current, heavy rainfall, red tide *(aguaje)*, invasion by tropical nekton, and mass mortality of various marine organisms including guano birds, sometimes with subsequent decomposition and release of hydrogen sulfide (known as *El Pintor*).

Before attempting to define *El Niño* or to hypothesize as to its origin, it is of interest to examine some of the conditions observed during the events of 1957 and 1958.

As early as December 1956 unusually high sea temperatures were observed off Northern Peru. The summers (Southern Hemisphere) of both 1957 and 1958 were marked by heavier than usual rainfall in the north, and as far south as Lima (12°S) the winter of 1957 was warmer and less overcast than usual, this condition lasting until early September. There were, to my knowledge, no reports of a southward coastal current, but there were some indications that the northward flow of the Peru Current was much reduced. The guano birds, which were nesting on the northern islands in December 1956, abandoned their fledglings as they did again during the summer of 1957-1958. During the winter of 1957 large numbers of guanay *(Phalacrocorax bougainvillii*, the principal producer of commercial guano) died along the beaches, apparently from starvation. This did not happen in 1958, but adult birds were uncommonly scarce. Anchoveta *(Engraulis ringens)*, the main food of the guano birds, although caught by fishermen, were unavailable to the birds. There was no evidence of excessive mortality of these fish, but rather indications that they were no longer present at the shallow depths where they are usually abundant. Coastal waters were invaded by tropical forms, the yellowfin tuna fishery extending farther southward than usual, and hammerhead sharks, manta rays, and dolphin fish were common at least as far south as Lima.

Evidence for the conditions described above, although credible, is difficult to document. However, early in 1958 it was possible to make some measurements from *Bondy*, a Peruvian naval vessel attached to the Servicio Hidrográfico. The scientific work was carried out by scientists of the Consejo de Investigaciones Hidrobiológicas. During the period 24 February to 9 March a systematic survey was made of the region between 12°S and 4°S, extending offshore about 60 miles. Observations included 78 bathythermograph and surface-salinity measurements, and hourly surface-temperature readings were made along the track of the vessel.

As is usual along the Peruvian Coast, lowest temperatures (21.5-23.0°C) were found just offshore. However, the areas of cold water were small and isolated, and most of the region was covered with water whose temperature was greater than 25°C. When these values are averaged by one-degree squares they can be compared with the February average latitudinal variation of surface temperature in the same squares, as computed from data in the period 1939-1956 published in the *Mapas Mensuales* of the Compañia Administradora del Guano (Fig. 11). During this period lowest February temperatures were observed in 1950, highest in 1941. Averages from the 1958 *Bondy* cruise are similar to those of 1941, or from 2°C to 4°C higher than the long term mean.

Off upwelling coasts, such as that of Peru, subsurface temperature measurements usually show a moderately strong and deep thermocline offshore, which becomes shallower and less intense as it approaches the coast (Wooster and Cromwell, 1958). Weakening of the thermocline near the coast is associated with the enhanced vertical-mixing characteristic of upwelling. During the February *Bondy* cruise, most of the region was covered by a shallow (less than 30 meters) layer of warm (greater than 25°C) water underlain by a strong thermocline lying offshore a

[1] Contribution from the Scripps Institution of Oceanography.

distance of 20 to 60 miles, and only in a few places were the ascending isotherms and weak thermoclines of upwelling encountered.

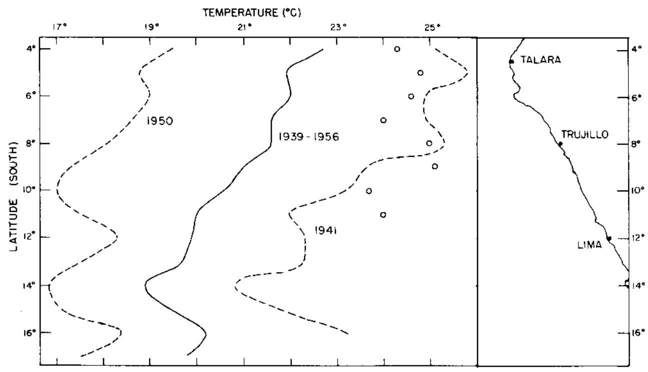

FIGURE 11. February values of average sea surface temperature along the coast of Peru. Curves are based on one-degree square averages from *Mapas Mensuales*. Circles are values for the same squares computed from *Bondy* data of February-March 1958.

Surface salinities in most of the region were 35°/₀₀ or greater, with lower values (to 34.92°/₀₀) only off Talara (4°30′S) and Paita (5°S). South of 7°S, there was a general increase of salinity with increasing temperature (Fig. 12), and also with increasing distance

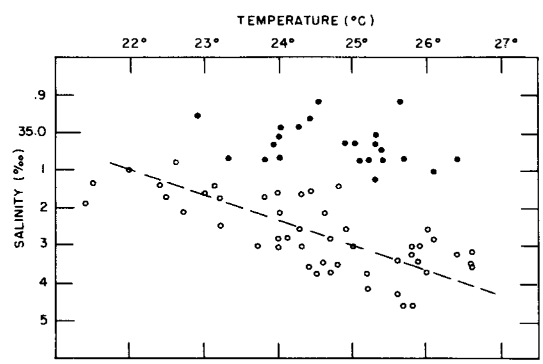

FIGURE 12. Surface temperature-salinity diagram for *Bondy* cruise of February-March 1958. Observations north of 7°S shown by filled circles, south of 7°S by open circles.

offshore. Only north of 7°S, and especially off Talara, were low salinities associated with high temperatures. Therefore it appears that the warm waters observed off northernmost Peru were of different origin than those present south of 7°S.

I would like to propose the following hypothesis concerning the cause of *El Niño*. Usually a southerly wind blows parallel to the Peruvian Coast, causing a net transport of surface water away from the coast, with replenishment from greater depths. For some reason, as yet unknown, the Peru Current swings westward a few degrees south of the equator. To the north are found the warm and relatively low saline waters of Panama Bight (Wooster, 1959).

Each year during southern summer highest temperatures are found along the Peruvian Coast from

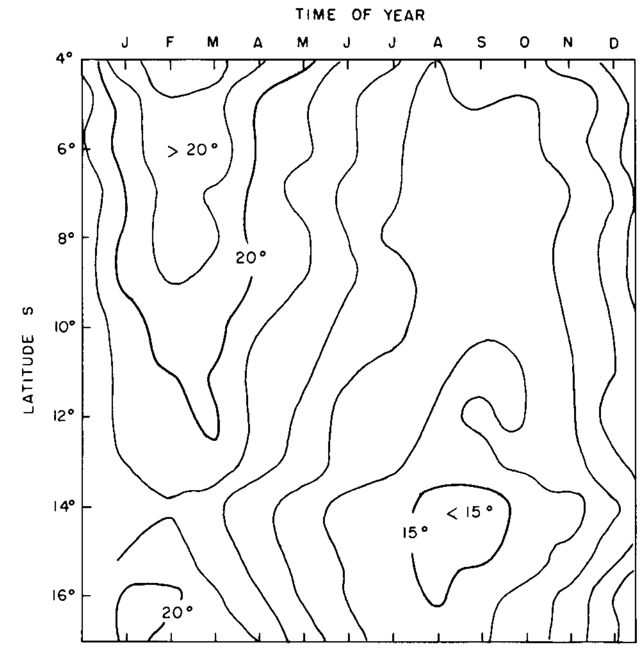

FIGURE 13. Average sea surface temperature along the coast of Peru, based on one-degree square averages for the period 1939-1956 from *Mapas Mensuales*.

4° to 17°S (Fig. 13). During *Niño* years, there is a general weakening of the atmospheric circulation, and reduction of the wind-stress component parallel to the coast is responsible for a weakening or cessation of upwelling. Higher than normal temperatures then result from *in situ* heating (as suggested by Sears, 1954) and from a coastward drift of open-ocean surface waters. In Northern Peru, the northern boundary of the Peru Current lies farther south than usual, and the tropical waters usually found north of this boundary may reach as far south as 7°S.

It has been suggested that red tides *(aguaje)* are more frequent during *Niño* periods (Schweigger, 1958). This would be consistent with the model if one accepted the theory that red tides are associated with reduced horizontal and vertical circulation, and thus with reduced dispersal of rapidly growing flagellate populations. This theory is implicit in the discussion of Kierstead and Slobodkin (1953).

In the light of this hypothesis, *El Niño* can be defined as the set of conditions developing off an upwelling coast when reduction of the wind stress causing upwelling during an extended period of time leads to weakening or cessation of vertical mixing. The resulting conditions include an increase in surface temperatures, the development of a thin layer of light water offshore, a modification of the surface circulation depending on the nature of the variation in the wind stress, and the biological changes associated with the altered environment. At the same time, in a region such as the Peruvian offing, where a cold coastal current turns away from the coast at low latitudes, the well-defined zone of transition to warmer waters may be at higher latitudes than usual.

El Niño as thus defined is a generic term applicable not only to Peruvian coastal waters but also to similar regions off California, Southwest Africa, Western Australia, and the coast of Vietnam.[1] In the last two regions the monsoonal changes in wind stress lead to annual *Niños.*

Testing of the proposed hypothesis requires a much better set of oceanographic and meteorological observations than are presently available from Peru. Although the *Niño* phenomenon is less well developed in California waters, the extensive body of data from that region might make it possible to examine the hypothesis during years such as 1957 and 1958.

DISCUSSION

Stommel: What I have to say is only to offer some more or less obvious remarks based on Schott's charts of mean summer and winter winds and sea surface temperatures in the *Geographie des Indischen und Stillen Ozeans* (February and August). First we note that there is a considerably greater variation in the winds over the California Current than over the Peru Current. In February the high pressure area over the California Current is weaker than in August; whereas over the Peru Current the high pressure region is strong in both seasons. Looking now at the corresponding charts of the surface temperature, we see that the isotherms are bent toward the Equator on the eastern side of the Pacific in both seasons off Peru, but off California, they are bent toward the Equator only during August. In February, when the atmospheric high is not strong, they extend almost parallel to latitude circles much of the way to the California Coast. If we can assert that the distortion of the isotherms is produced by the winds, then we can see that there is a powerful "thermostatic" action off California, which reduces the seasonal variability of temperature but that this is not so developed off Peru, because there the winds distort the temperature field throughout the whole year. Depending upon how we like to talk of things, therefore, we might say that there is normally an *El Niño* off California every winter, when the winds die down as a rule each winter, whereas in the Peru Current, the *El Niño* is by definition, an abnormal event, because there the winds do not normally change much with season. Of course, this is only a playing with words, but it does point up, I think, a rather important difference between the two current systems. I wonder if more recent studies of oceanic climatological data substantiate the general features of Schott's Charts?

Namias: Although I am not ready to propose anything deserving the designation of a theory, I believe that there are interactions between the North Pacific Anticyclone on the one hand and its Southern Hemisphere counterpart, the South Pacific Anticyclone, whose air circulation affects Peru and its coastal waters. We recognize interrelationships of this sort (called teleconnections) between the semi-permanent high pressure areas of the Northern Hemisphere, and it is quite conceivable that there are similar coupling mechanisms operating between the Northern and Southern Hemisphere cells. In the present case, especially during winter of 1958, the dislocation of the North Pacific High and the westerlies as well (both far south of normal over the eastern North Pacific) was so great it would surprise me if the doldrum belt as well as the position and strength of the South Pacific High were not influenced. In fact, if the southward displacement of wind systems observed in the North Pacific carried into the South Pacific one could account for the weakened (or absent) prevailing southerly winds off the Peruvian Coast which appear to be responsible for *El Niño.*

[1] Some readers of this manuscript have objected to the use of the name *El Niño* to identify the general phenomenon, feeling that previous usage restricts the term to the Peruvian Coast. If a more appropriate generic term can be found, I would recommend its use. I have used *El Niño* in a broad sense to emphasize that the Peruvian *Niño* is *not* a unique phenomenon, but is rather merely a striking example of a wide-spread occurrence.

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RECENT OCEANOGRAPHIC CONDITIONS IN THE CENTRAL PACIFIC

PRESENTED BY GARTH I. MURPHY

(EDITORS' NOTE): *Mr. Murphy in his presentation briefly reviewed material that he distributed to all members of the Symposium. It seemed best that rather than presenting the review by Mr. Murphy that the material distributed should be presented in total. The discussion has been included at the end.*

SURFACE TEMPERATURE ANOMALIES IN THE CENTRAL NORTH PACIFIC, JANUARY 1957-MAY 1958

JAMES W. McGARY

The surface-temperature monitoring program for the Northeastern Pacific utilizing ship's weather report data which was described at the EPOC meetings at Lake Arrowhead last fall was started in October 1957. Since that time we have prepared a complete series for the middle ten days of each month from January 1957 through May 1958. The final form consists of a series of four plots for each month, the actual temperature contours and the anomalies from the 30-year mean (H. O. 225), the tabulation of all temperature data available for the Pacific east of 180° and between 20°N and 30°S., and since January 1958 the anomaly of 1958 from 1957. The procedure used in constructing the temperature charts of the Northeastern Pacific was basically the same as that used by Dr. Takenouti's group at JMA for the Northwestern Pacific and which was verified by a check against research vessel temperature data by Franceschini of Texas A & M. This procedure is quite simple, the temperatures are decoded, averaged by one-degree square, and contoured assuming they represent the average for the center of the squares. The anomaly charts are drawn by overlaying the monthly and normal charts on a light table.

The anomaly charts appear to have the most promise for tracing major changes in temperature in spite of the fact that they contain many small cells which have no month-to-month continuity. Actually many of the small changes in the anomalies from the 30-year mean are the result of the difference in techniques used in their construction. The 30-year mean charts were interpolated from H. O. 225 and hence represent data that has had a maximum of smoothing while the monthly charts are drawn strictly on the distribution of temperature indicated by averages for the one-degree squares after the obviously bad values have been culled out.

Most of the large features in the 30-year anomaly charts persist from month to month but their change in size, shape, and geographical position is more amoeboid in nature than can be accounted for by advection by the normal ocean currents. This leads to speculation as to the need for the computation of new mean-temperature charts. Examination of the major features of the area to the north and northeast of the Hawaiian Island chain will illustrate the manner in which these changes occur.

In January 1957 (Fig. 14) except for a few small cells the major portion of the area west of 145°W. longitude was 2°F. or greater, warmer than normal. By February (Fig. 15) in the area north of 35°N. the warming had persisted and the anomaly had increased in many places while to the south of 35°N. the positive anomaly had decreased in intensity. For March (Fig. 16) there were only minor changes in the distribution of positive and negative anomalies in the mid-ocean area but the amount of the positive anomaly had decreased. In April (Fig. 17) except for a few areas along 45°N., the return towards normal conditions had continued although most of the area was still slightly warmer than normal.

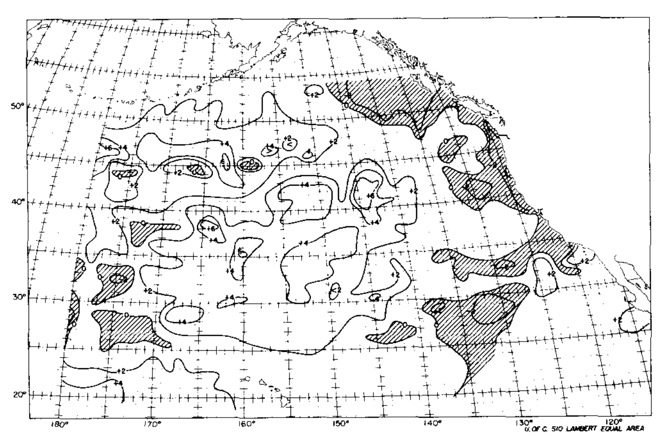


FIGURE 14. January 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

In May 1957 (Fig. 18) the dominant anomaly of the area was still 0 to 2°F. warmer than normal but there had been a slight increase in the areas of cold anomaly. This increase became pronounced in June (Fig. 19) and a tongue of cold water extended east between 35° and 60°N. latitude from 180° to 135°W. but outside this area the areas having positive anomalies of +2°F. or greater increased. This was the basic distribution throughout the summer with the tongue of cold water in the west or northwest merely changing shape and retreating or expanding (Figs. 20, 21).

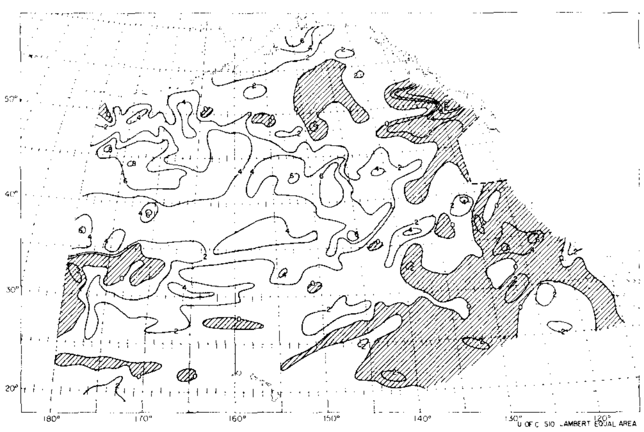


FIGURE 15. February 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

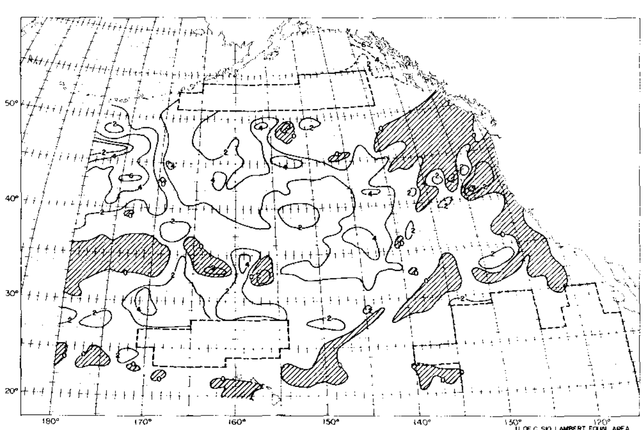


FIGURE 16. March 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

In the fall and early winter (Figs. 22-25) basically the same conditions existed; cold anomalies appear in the extreme west and positive anomalies reaching a maximum of greater than 6°F. dominate the eastern part of the charts.

In January 1958 (Fig. 26) the warm 0 to +2°F. anomaly was still dominant but there was really no distinct pattern of cold (<0°F.) and warm (>+2°F.) areas.

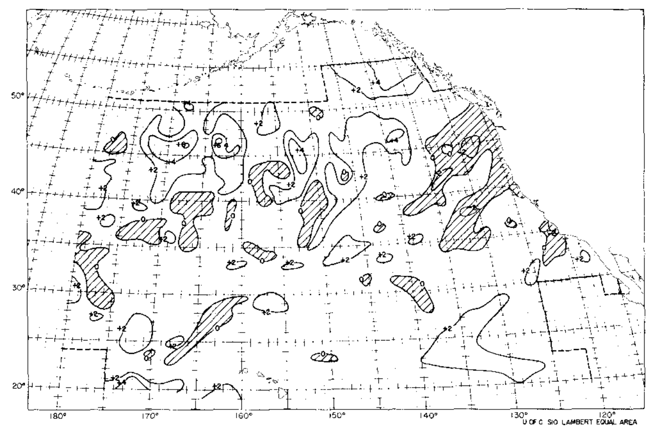


FIGURE 17. April 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

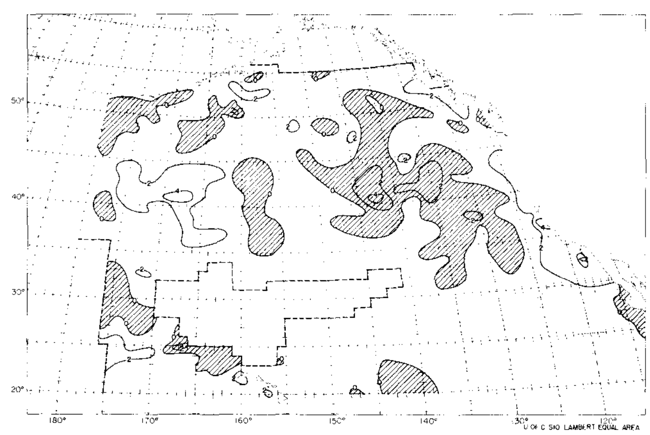


FIGURE 18. May 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

In February 1958 (Fig. 27) the areas of cold anomaly began a distinct increase in size and the percentage of the areas having surface temperatures below normal continued to increase through May (Figs. 28-30). However, there appeared to be no definite pattern to their increase.

The yearly (1958-1957) charts (Figs. 31-35) appear to have the most persistent features and in most areas exhibit month-to-month changes that would be expected from the accepted circulation pattern in the Northeastern Pacific. For example the January-April 1958 charts show a steady encroachment of the area of negative anomaly from mid-ocean towards the coast. It reached the coast in March at 38° to 40°N. where the monthly temperature charts indicate the divergence in the currents occur and in April it continued to spread north and south.

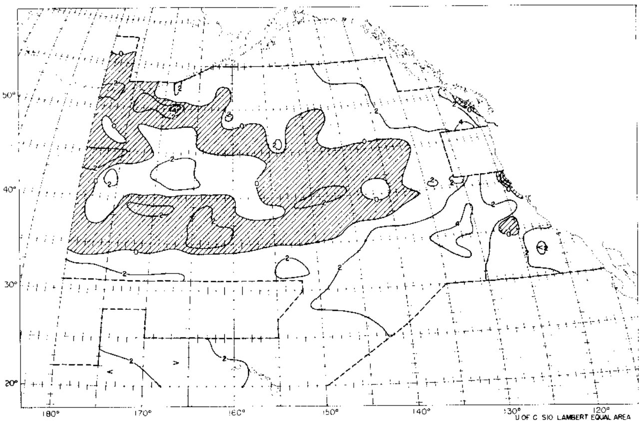


FIGURE 19. June 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

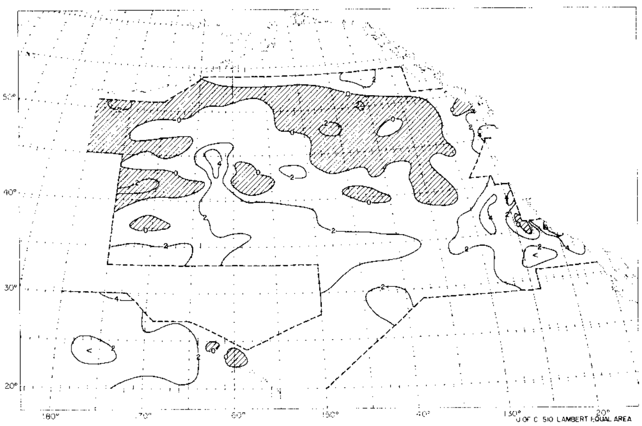


FIGURE 20. July 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

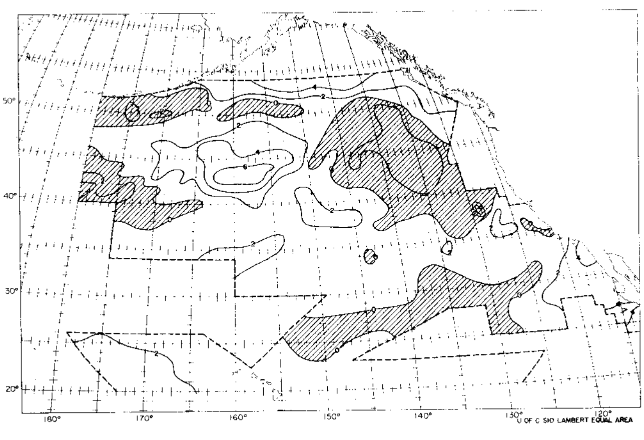


FIGURE 21. August 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

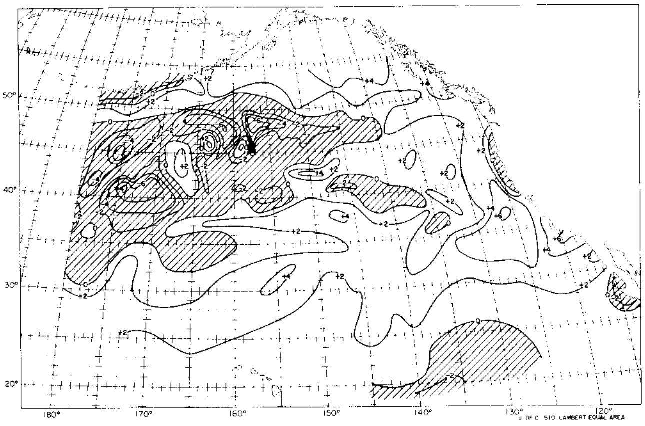


FIGURE 22. Sept. 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

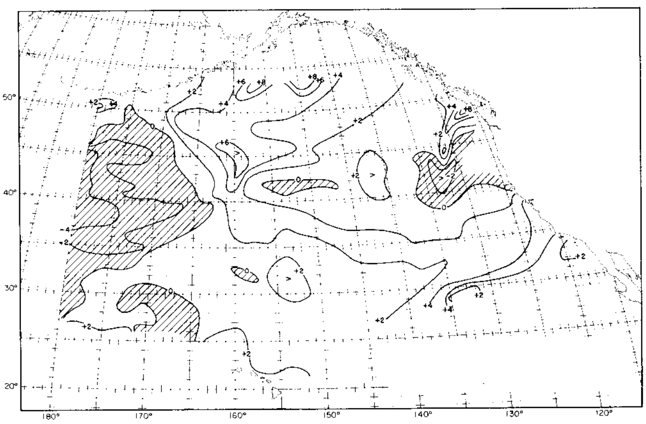


FIGURE 23. October 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

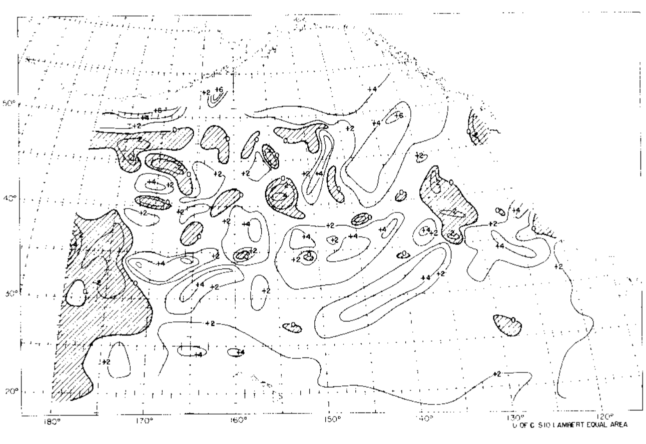


FIGURE 24. Nov. 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

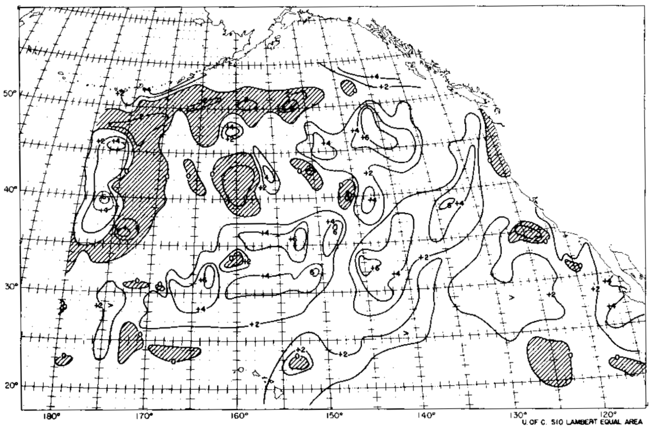


FIGURE 25. Dec. 11-20, 1957. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

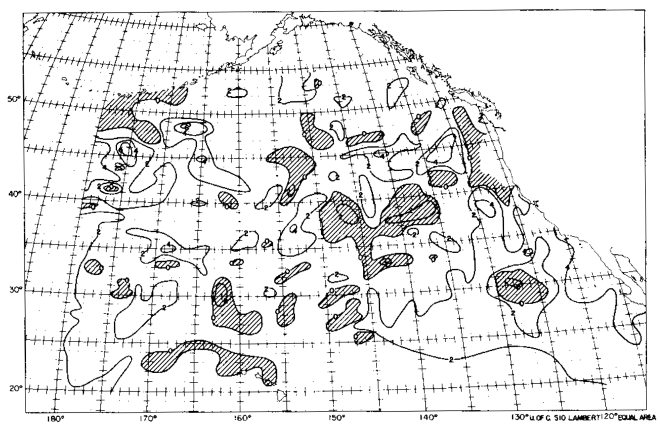


FIGURE 26. Jan. 11-20, 1958. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

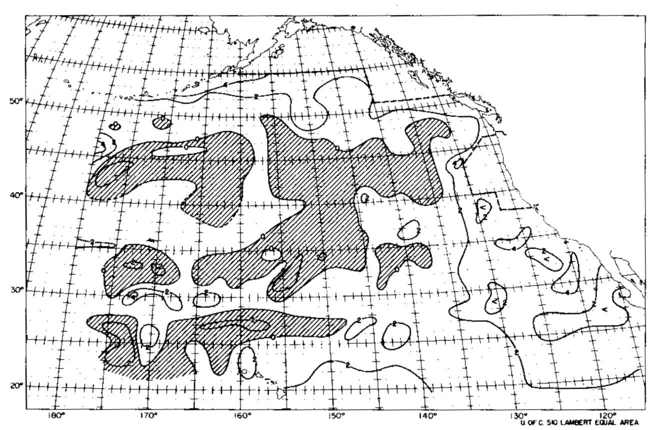


FIGURE 27. Feb. 11-20, 1958. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

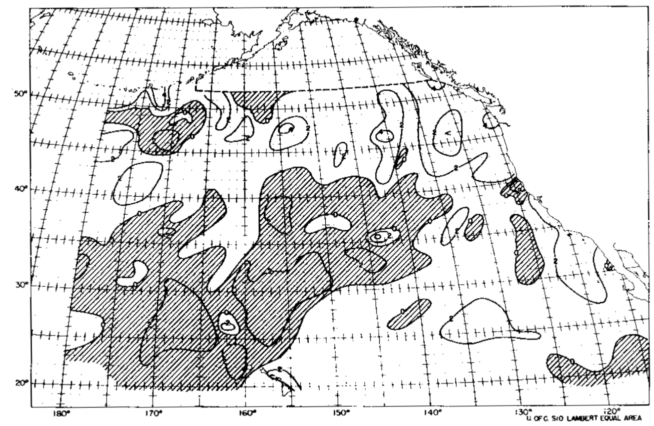


FIGURE 28. Mar. 11-20, 1958. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

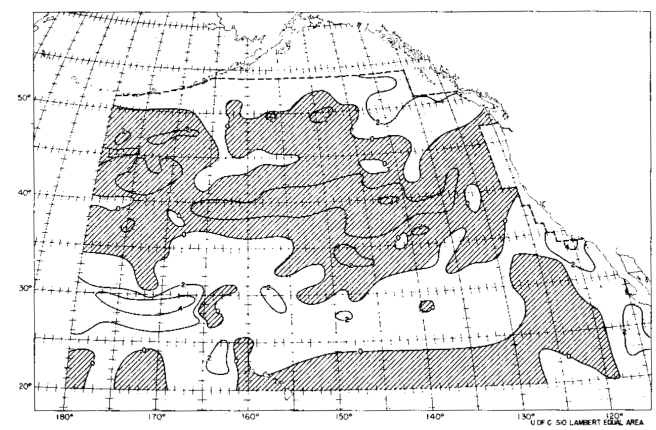


FIGURE 29. Apr. 11-20, 1958. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

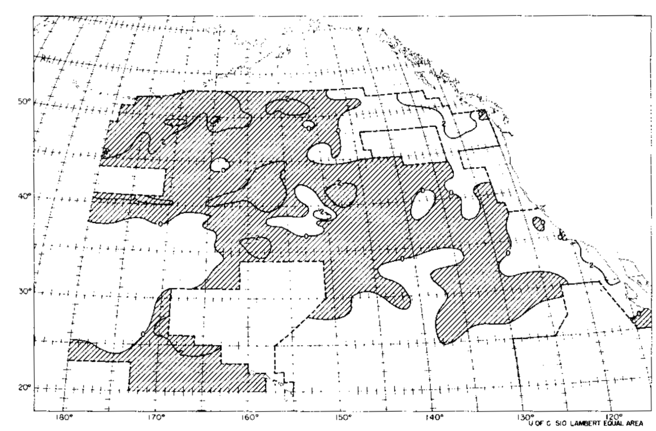


FIGURE 30. May 11-20, 1958. Anomaly of sea surface temperature (°F) from 30-year mean charts of H.O. 225. Hatched areas colder than average.

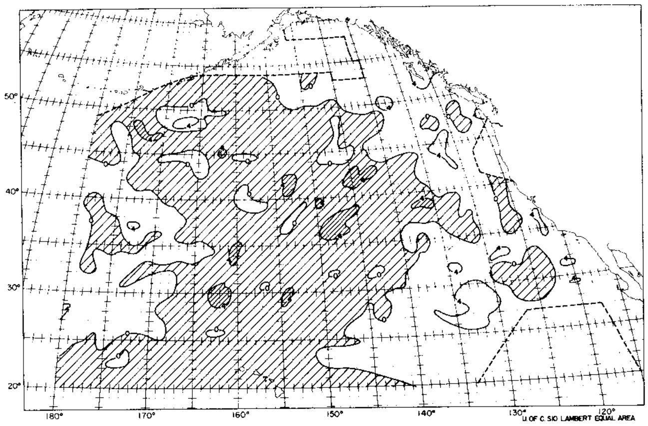


FIGURE 31. Jan. 11-20; 1958 minus 1957. Surface temperature change. Hatching indicates areas colder in 1958 than 1957.

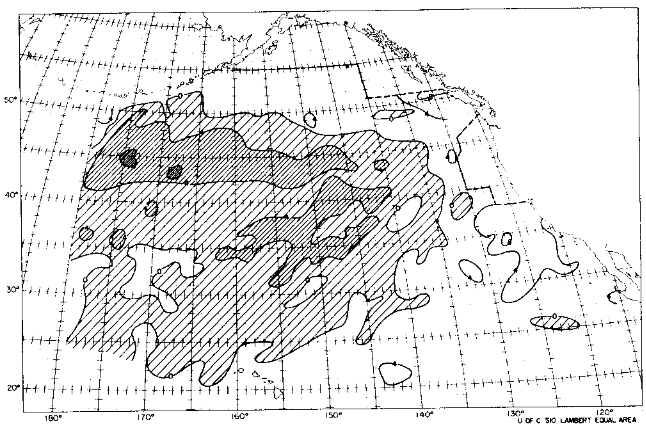


FIGURE 32. Feb. 11-20; 1958 minus 1957. Surface temperature change. Hatching indicates areas colder in 1958 and 1957.

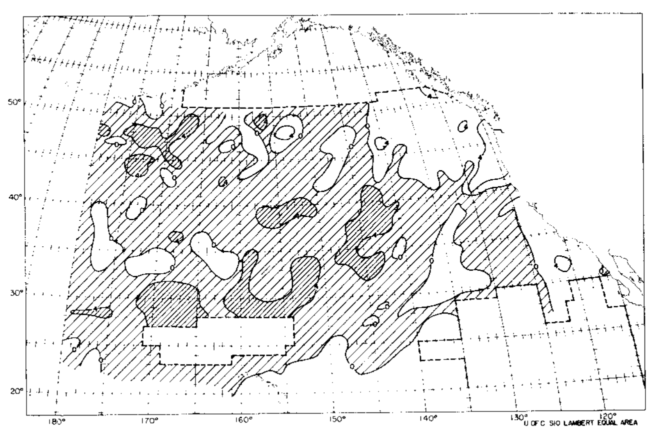


FIGURE 33. March 11-20; 1958 minus 1957. Surface temperature change. Hatching indicates areas colder in 1958 than 1957.

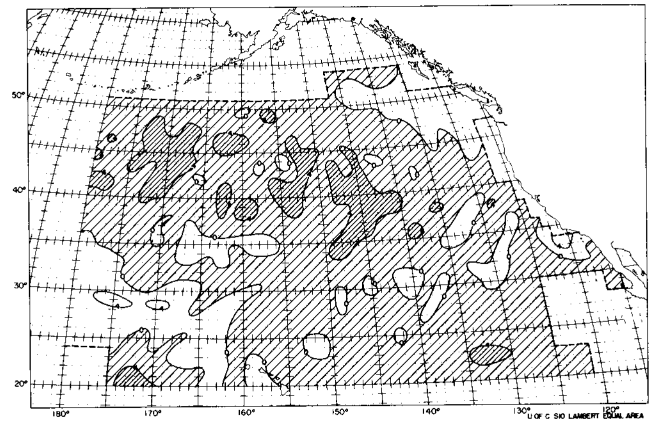


FIGURE 34. April 11-20; 1958 minus 1957. Surface temperature change. Hatching indicates areas colder in 1958 than 1957.

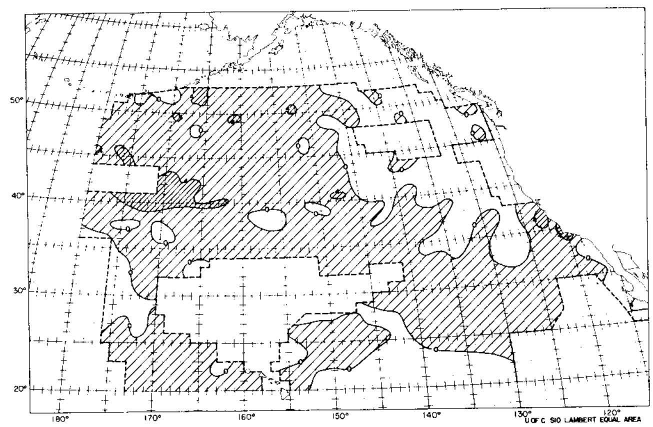


FIGURE 35. May 11-20; 1958 minus 1957. Surface temperature change. Hatching indicates areas colder in 1958 than 1957.

SUMMARY, 1955-57 OCEAN TEMPERATURES, CENTRAL EQUATORIAL PACIFIC

THOMAS S. AUSTIN

PACIFIC OCEANIC FISHERY INVESTIGATIONS

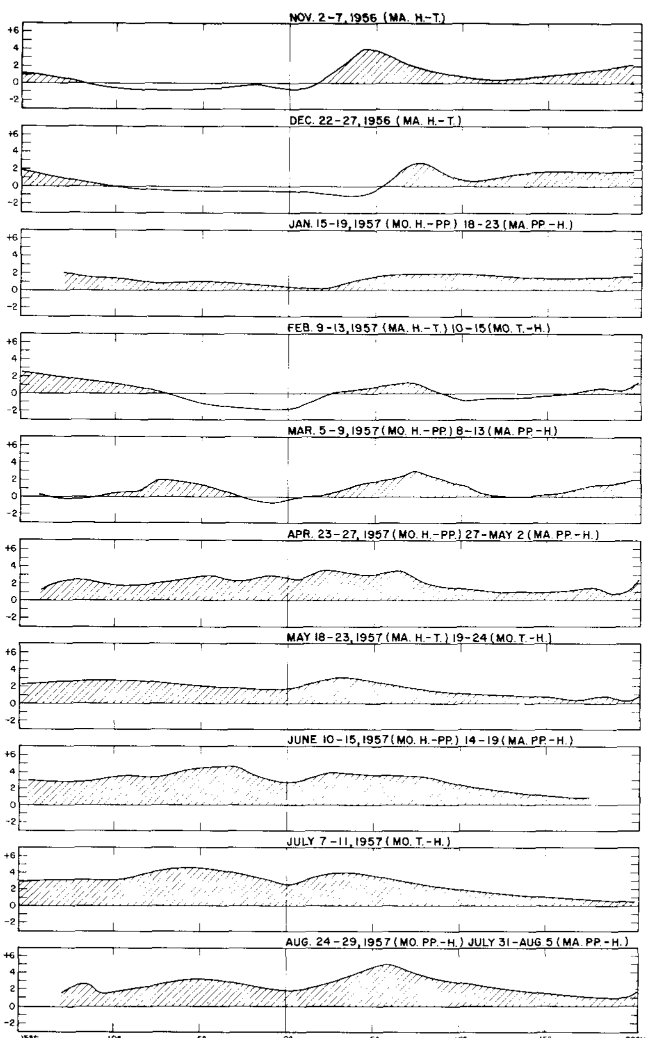
MONTEREY AND MARIPOSA DATA

Late in 1956, POFI made arrangements whereby two passenger liners, the SS *Mariposa* and the SS *Monterey,* were to collect surface temperature data and salinity samples once each watch (four-hour intervals) during their passage, Honolulu to Tahiti and/or Pago Pago and return. As a result of these arrangements, at least one surface temperature and one salinity section, along the courses traversed by these vessels, are available for each month, November 1956 to present.

In figure 36, the temperature data have been expressed as anomalies from the mean; positive anomalies have been shaded. Along the top of the panel for each month, the inclusive dates for the section, the vessel(s) (MO-*Monterey,* MA-*Mariposa*), and the ports of call (H-Hawaii, T-Tahiti, and PP-Pago Pago) are given. When both vessels made the run during any one month, their observations have been averaged and a single curve drawn. Mean temperatures, to which the observed temperatures were algebraically compared, were taken from the Monthly Meteorological Charts of the Western Pacific Ocean.[1]

When departing Honolulu (right end of each panel) (Fig. 36) and proceeding south, the vessels crossed the

[1] British Air Ministry, Monthly Meteorological Charts of the Western Pacific, MO-484, 120 p.



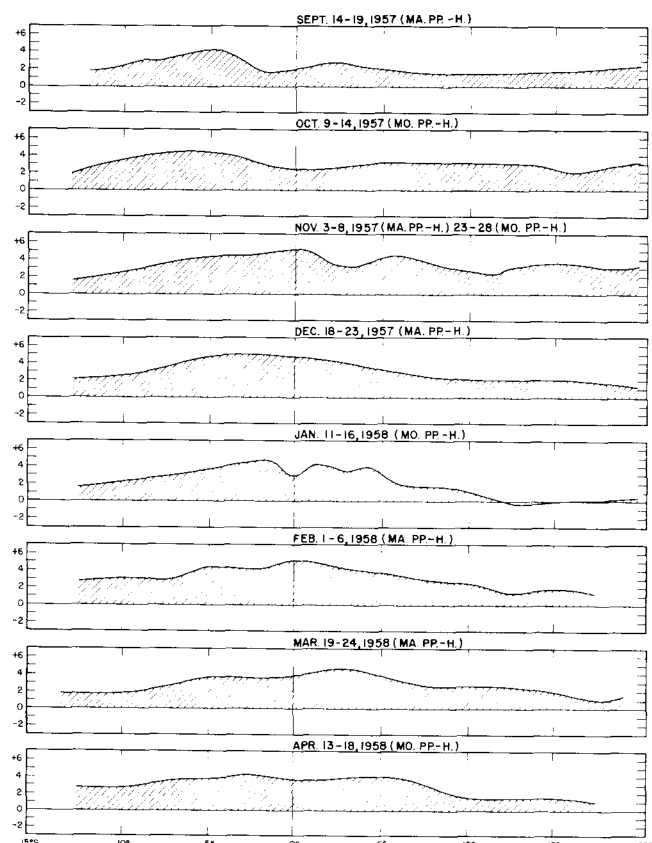


FIGURE 36. Sea surface temperature anomalies calculated from observations taken aboard the SS *Monterey* (MO) and the SS *Mariposa* (MA) during passage between Honolulu (H) and Pago Pago (PP) and Tahiti (T) and return. Inclusive dates for each voyage are given at the top of each panel. Positive anomalies are shaded.

westerly flowing North Equatorial Current to about 10°N. latitude, the easterly Equatorial Countercurrent, 10°N. to 5°N., and then the westerly South Equatorial Current, 5°N. to Tahiti or Pago Pago. At or near the Equator, they passed through the surface waters which are normally cooled by the upwelling from subsurface layers.

Referring to figure 36, April 1957 was the first month during which the anomalies were positive for the entire passage. With the exception of the northern portion of the leg for January 1958, the anomalies remained positive for each month of the year, April 1957 through April 1958. It would appear that, in general, the equatorial surface waters in the Southern Hemisphere warmed somewhat more slowly, but once warmed, maintained the positive anomaly at a consistently higher level than for the surface waters north of the Equator.

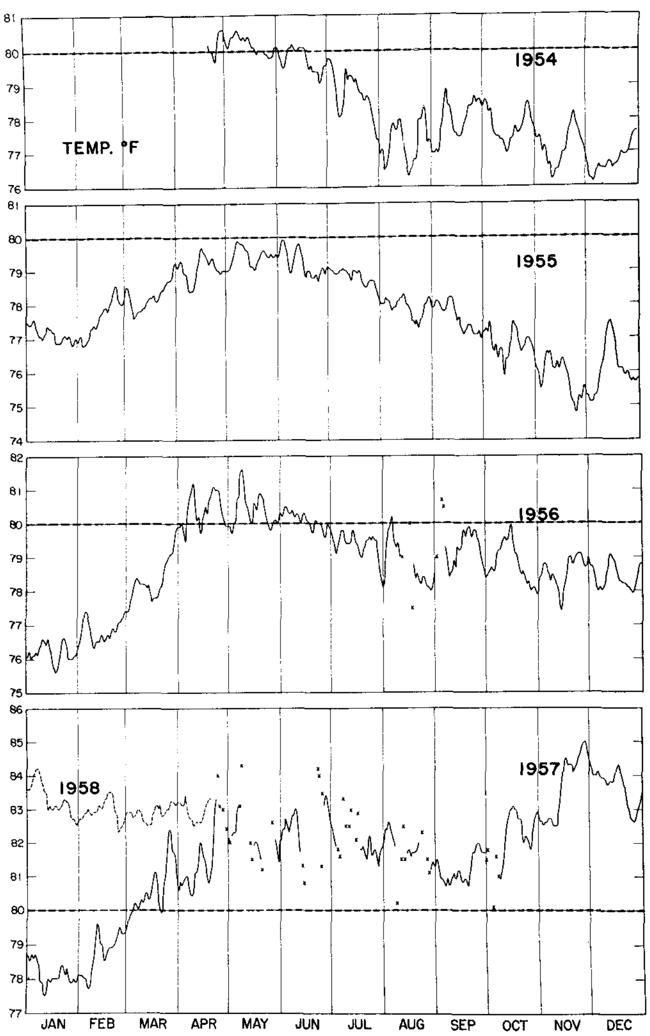


FIGURE 37. *Five-day moving averages for daily sea surface temperatures recorded at Christmas Island (2°N., 157°W.), one of the Line Islands group. Observations are taken along the lee shore near the seaward edge of the reef. An "x" denotes observed temperature for days within periods during which the five-day moving average could not be calculated.*

CHRISTMAS ISLAND DATA

In early 1954, POFI, in cooperation with the U. S. Weather Bureau, established a weather station on Christmas Island--an atoll of the Line Islands group centered near 2°N., 157°W. At the same time, arrangements were made for daily sea-surface temperature observations and weekly salinity samples to be taken at a position near the seaward edge of the reef, lee (western) side of the island.

The results of the daily temperature observations have been depicted as a five-day moving average in figure 37; as monthly means in figure 38. In figure 37

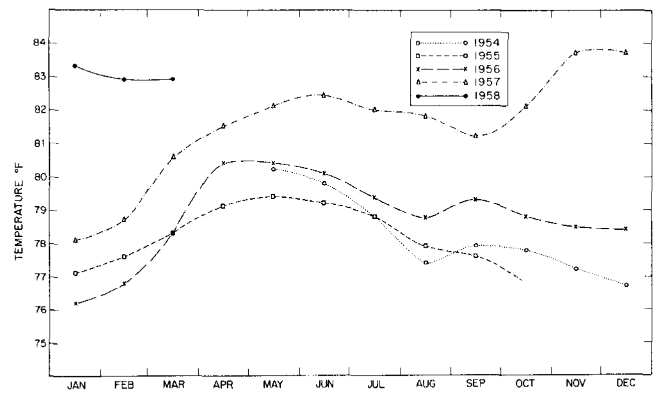


FIGURE 38. Thirty-day mean sea surface temperatures, Christmas Island station.

the small "x's" denote the observed temperature for days within periods during which a break in the record did not permit calculation of the five-day moving average.

The comparatively warmer surface temperatures observed at the Christmas Island station during late 1956, all of 1957, and early 1958 are quite evident on both figures 37 and 38. If we start with November-December 1955, we note that the temperatures were the coolest during the period of the observations. This situation, according to Rodewald (1956) was characteristic for the winter of 1955, Alaskan waters south to the coast of Peru. The surface temperatures then increased in a rather normal fashion, reaching the 1956 maximum during April and May. Subsequently there was cooling, but the 1956-57 winter minimum (78.5°F.) never did reach the same low as in 1954 (76.5°F.) or 1955 (75.0°F.). The 1957 spring warming continued until the surface temperatures reached 82° to 83°F., 2°F. warmer than for the same period during the years 1954-1956. Although the anticipated summer cooling was evident, June through September 1957, the normal trend was marked by reversal towards further warming (October), and the midwinter minimum was absent during 1957-58 (Figs. 37 and 38). Comparison of the dashed curve (1958), lower panel, figure 37, with the solid curve (1957) for the months January through April, shows that the January 1958 temperatures were approximately 6°F. warmer than those for January 1957.

Referring to figure 38 which shows the mean monthly temperatures, the curves for 1954, 1955, and 1956 show a reasonable degree of similarity. During the latter half of 1956 and all of 1957, however, the surface temperatures at the station were consistently higher than for the previous years of the series. The question arises: how indicative of subsurface conditions are these surface temperatures? Frequent meridional temperature-depth sections, at no more than monthly intervals, would be required to adequately answer such a question.

Some data for consideration of variations in temperature-depth distribution, 1957-1958 compared with previous years, are available from BT sections, Honolulu south to the Equator or to the Marquesas. 140°-150°W. longitude. Two sets of such sections have been compared in figures 39 and 40. The 60°, 70°, and 80°F. isotherms for each BT section are included; those for the particular month in 1957 as a solid curve, those for the chosen previous year (1955) as a dashed curve.

The September 1955 and October 1957 sections are very similar; the December sections comparatively dissimilar. On the December 1955 section, the only surface and subsurface waters observed to be 80°F. or above were those of the Equatorial Countercurrent.

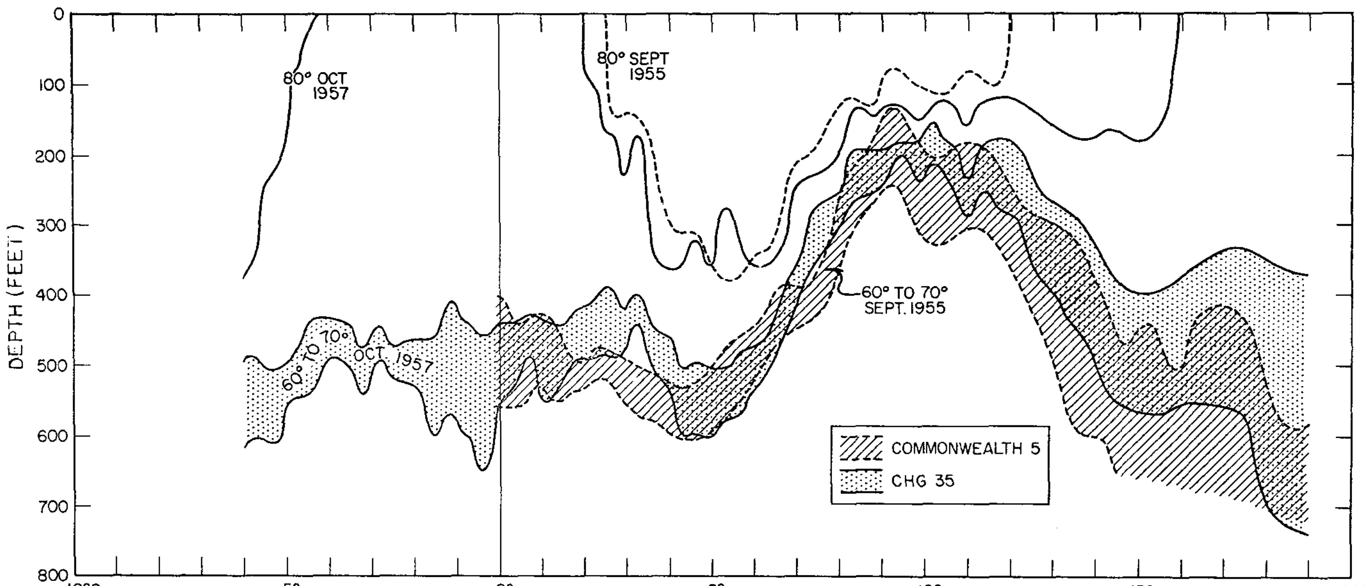


FIGURE 39. Vertical temperature distribution (80°, 70°, and 60°(F) isotherms) from BT sections made during *Commonwealth* Cruise 5, September 1955 (dashed contours) and *C. H. Gilbert* Cruise 35, October 2-7, 1957 (solid contours), 140°W.-150°W. longitude.

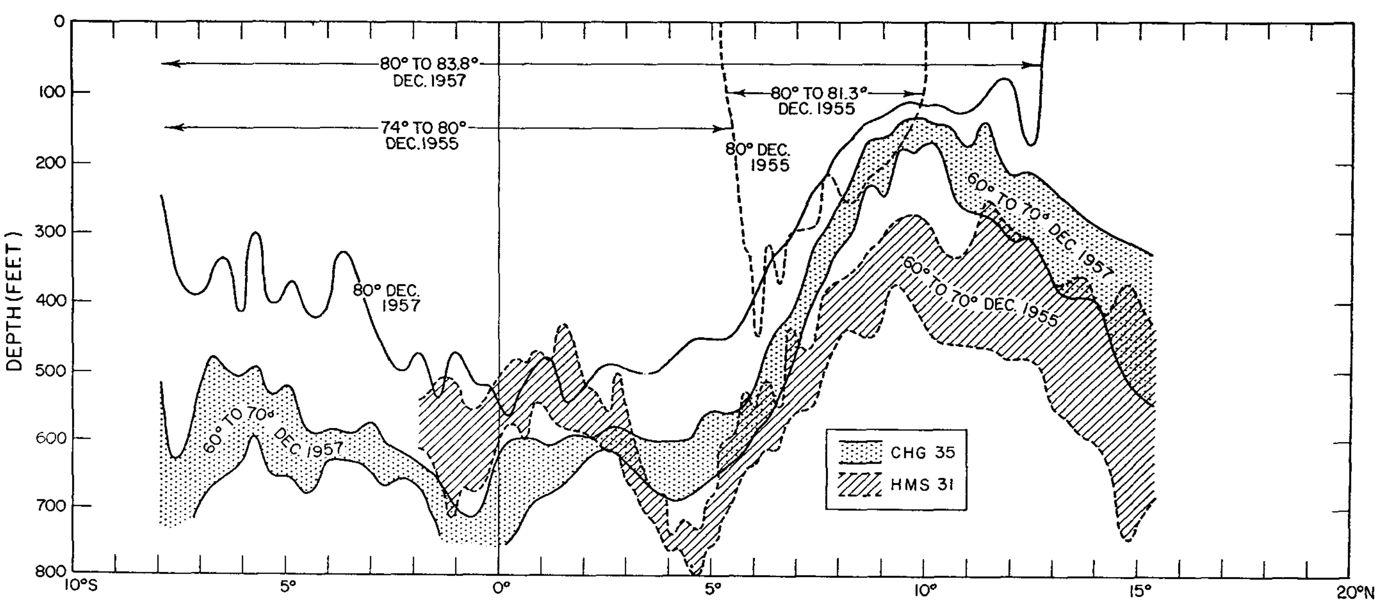


FIGURE 40. Vertical temperature distribution (80°, 70°, and 60°(F) isotherms) from BT sections made during *H. M. Smith* Cruise 31, December 1955 (dashed contours) and *C. H. Gilbert* Cruise 35, December 1957 (solid contours), 140°W.-150°W. longitude.

South of the countercurrent, the temperature of the waters above the thermocline were generally 74° to 80°F. In contrast, along the December 1957 section, temperatures in the surface layer, from 12°N. to at least 8°S. latitude were 80°F. or above, reaching a maximum value of 83.8°F. in the countercurrent. The 80°F. isotherm reached depths of 500 feet or more near the Equator. From consideration of surface temperatures at Christmas Island, figure 38, the means for early October and for December 1957 were 3°F. and 6°F. warmer, respectively, than the same months, 1955. From consideration of the subsurface distribution (Fig. 39) the subsurface temperature conditions were very similar during September 1955 and early October 1957, not clearly reflecting the apparent warming, while in a similar consideration of the two sections for December 1955 and 1957, figure 40, the surface and subsurface differences are strikingly evident.

If we compare the October and December 1957 sections (shaded areas and solid curves, figures 39 and 40, at least two interesting points may be seen. First, within approximately 90 days, a large quantity of warm water had apparently moved into the area, too large a quantity to be attributable to local effects such as that of insolation. In general, with persistent southeast trades, advection through 150°W. by the westerly South Equatorial Current would transport cooler waters normally found to the east of 150°W. Upwelling would cool the surface waters near the Equator. This situation, as inferred from variations in depth of the 60°F. and 70°F. isotherms, is illustrated on both the December 1955 and the October 1955 sections. These isotherms reach a maximal depth near 5°N., decreasing in depth toward the Equator, then increasing again in the Southern Hemisphere. The resulting ridge centered near the Equator reflects both westerly transport and upwelling. However, in the December 1957 section (Fig. 40), the same isotherms deepen continuously, 5°N. to the Equator, then slope upwards in the Southern Hemisphere. The trough thus formed near the Equator suggests, in contrast to the situation for December 1955 and October 1957, easterly transport through 150°W. of the warmer surface waters from the west and little or no upwelling. That such transport may have been the case is supported by an observation of a 5-knot easterly set, 00°30′N. to 02°30′N.; 160°W. reported by the SS *Monterey* on November 25, 1957.

Available data are too few to permit any more than speculation as to what processes may have been involved in causing these variations between the October and the December meridional, temperature-depth distributions. In general terms, there is a progressive east-west warming of the surface waters in the Equatorial Pacific, with the most pronounced east-west gradient to be found east of the 180th meridian. The sea surface in the Western Pacific is 0.8 to 1.0 dynamic meters higher than in the east; the thermocline progressively shallows, west to east. The existence of and variations in these situations are related to the presence of and variations in the trade wind system.

We mentioned above that vertical distribution of temperature for the October BT section suggested westerly flow of the surface waters, 5°N. to the southern limit of the section and upwelling centered about the Equator. This is the "normal" situation with the easterly component of the trades. With a relaxation of the trades, and particularly with a comparatively high frequency of westerlies, one could postulate a reversal in flow of the surface waters (and the undercurrent reaching the surface), with the warmer waters from the west becoming evident through the 150°W. section. A second factor leading to warmer waters in the surface layers—westerly winds are *convergent* in terms of surface flow at the Equator, thus there would be no divergence of the surface waters and no upwelling of the deeper, cooler waters. From advance indications of the available data, this may have been the situation during the December 1957 section.

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THE OCEANOGRAPHIC SITUATION IN THE VICINITY OF THE HAWAIIAN ISLANDS DURING 1957 WITH COMPARISONS WITH OTHER YEARS

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Pacific Oceanic Fishery Investigations

During the past three years POFI initiated a number of temperature and salinity monitoring stations in the Central Pacific as follows: Oahu, Hawaii; Johnston Island; French Frigate Shoals; Midway Island; Wake Island; Christmas Island; and Weather Station "Victor." The purpose of this network is to develop time series that can be related to variations in the oceanic circulation which, we anticipate, can in turn be related to variations in the distribution of skipjack and other tunas. At the present only the Christmas Island and Oahu stations have been established long enough to provide material suitable for discussion. The Christmas Island station is reported on elsewhere, leaving the Oahu station and other pertinent observations in the vicinity of Hawaii for consideration here.

SURFACE TEMPERATURE AND SALINITY AT OAHU

The Koko Head station is occupied each Tuesday morning about 9 a.m. A bucket of water is dipped from the cliff, temperature measured and the salinity sample returned to the laboratory for titration. There are no coral reefs at the station; the depth drops off abruptly to about thirty feet below the cliff and then quickly falls off to deep water. There are no nearby streams either, and thus the location is close to ideal with respect to non-interference of shallow water warming and run-off. Offshore surveys conducted simultaneously with the shoreside observations give us further confidence that water samples taken there are representative of general conditions off that portion of Oahu.

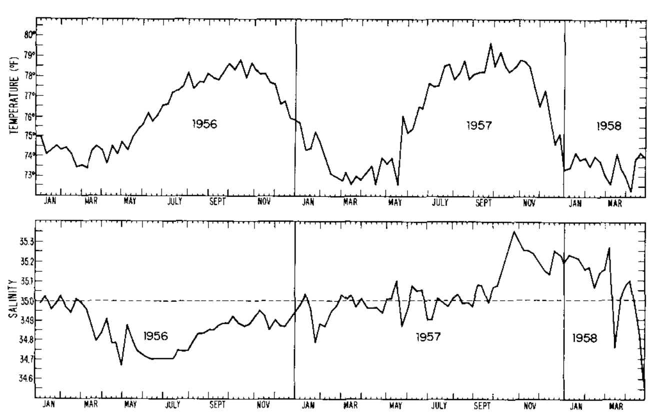

FIGURE 41. Surface temperature and salinity at Koko Head, Oahu.

Two and one-half years' records from this station are plotted on Figure 41. Since primary comparisons will be between 1957 and 1956, it is of some interest to consider the representativeness of the 1956 material. Though not herein documented, our accumulation of records suggests that 1956 was very close to an average year with respect to temperature and with respect to salinity. Weather patterns and biological patterns were also reasonably representative of average conditions during that year. We can then with some confidence treat 1956 as a base and proceed with intercomparisons between 1956 and 1957.

With respect to surface temperature, the pattern in 1957 was similar to 1956 insofar as maximum and minimum temperatures are concerned. However, it is clear that spring warming was accelerated in 1957 and the winter cooling was also more accelerated. These data in themselves are not clear evidence of a significant change in the advection pattern.

Surface salinity (Fig. 41) shows 1957 to be markedly different from 1956. The March-May decline in salinity followed by a gradual rise in salinity from about the first of July onward did not occur in 1957. Instead, salinity in 1957 remained almost constant until mid-September when it rose abruptly to levels higher than those ever recorded. This was followed in the spring of 1958 by an abrupt decline to what might be regarded as normal spring salinities. The decline however was short-lived, and during the first week of April salinities again rose to about 35.0‰ and remained at that level, at least through the third week in May (not included in plot on Fig. 41).

The major trends in the salinity data can only be accounted for by advection; for instance, the March-May decline illustrated by 1956 occurs at a time of decreasing rainfall. Each of the other major trends illustrated either were contrary to what might have been expected from rainfall and isolation, or could not be accounted for by these factors. However, at least some of the short-term changes in salinity can be correlated with rainfall, the most graphic example being the mid-March, 1958 decline; this occurred coincidental with a rainfall of about two feet within a 24-hour period.

From the data on figure 41 we can safely conclude that there are seasonal changes in advection in the vicinity of the Hawaiian Islands and that the typical seasonal changes did not occur in 1957; rather, the island area was brought under the influence of water masses that do not normally influence the area.

The data on figure 41 replotted in smooth form on figure 42, graphically illustrate the difference between the two years. The treatment on figure 42 is in essence a T/S curve, time being the third and non-linear variable, rather than depth. It is interesting to note that many features of the basic pattern were similar during the two years, very possibly reflecting similar patterns

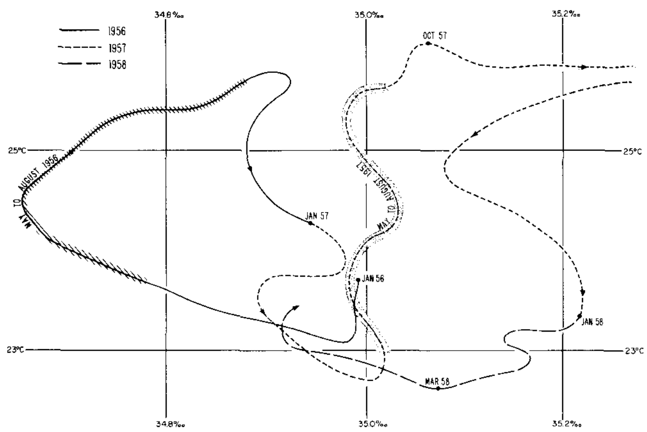

FIGURE 42. Surface temperature and salinity at Koko Head, Oahu (data smoothed by eye).

of temperature and rainfall between the two years. Many details are, however, different between the years and it is clear that the basic temperature-salinity relationship was very dissimilar between the two years.

In summary, the normal seasonal pattern in Hawaii involves high salinity and low temperatures in October-February, and low salinity and high temperatures during the summer. In 1957 the temperature regime resembled the normal except that the rates of increase and decrease associated with the September-October maximum were greater. In 1957 the salinity regime was very different from normal. The May-July low salinity did not develop and the ensuing winter saw salinities rise to higher than normal levels. The regime for 1958 has not clearly established itself. These differences in salinity and temperature can only be explained on the basis of changes in advection pattern in the island area.

DISTRIBUTION OF SALINITY NORTH AND SOUTH OF OAHU

One of the authors (Seckel) is in the process of summarizing and analyzing all available oceanographic data from the portion of the Central Pacific outlined in figure 43. This chart depicting certain surface isopleths shows that the mean position of the 35.0 isopleth during the period, April to July, is about $2\frac{1}{2}$ degrees north of Oahu. During the contrasting season, November to February, this isopleth lies about three degrees south of Oahu. This migration is quite compatible with the results of the salinity changes with time at Koko Head during 1956, which we believe to be close to an average year but is quite dissimilar from 1957.

A north-south profile of surface salinities during 1957 (April to July) figure 44 shows the 35.0 isopleth to be almost coincidental with the latitude of Oahu, rather than $2\frac{1}{2}$ degrees north of Oahu, Interestingly enough, the surface salinities that should have prevailed during 1957 at Oahu were located almost exactly $2\frac{1}{2}$ degrees south of Oahu. Figures 43 and 44 suggest that the normal spring north-south profile of surface salinity had about the same shape in 1957 and 1956, but was simply displaced about $2\frac{1}{2}$ degrees south in 1957.

The sum total of our observations suggest that normally during the winter the vicinity of Hawaii is under the influence of cool, high-salinity water of northern origin. Normally during the summer this high-salinity water seems to be displaced to the northward and the islands are influenced by lower-salinity, warm water that possibly originates to the southeast. Perhaps the high-salinity water is downstream, or is influenced by downstream Kuroshio flow, and signals advection from the west. During March-May this easterly-flowing water is displaced to the north and tends to be replaced by lower-salinity water from the southeast. Perhaps the water is related to California Current water. In 1957 this warm, low-salinity water persisted through spring, summer and the subsequent winter.

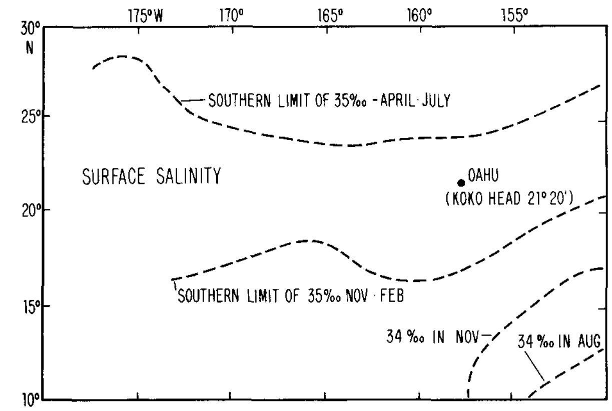

FIGURE 43. Mean Seasonal Locations of 35⁰/₀₀ and 34⁰/₀₀ Isopleths (Approximately 700 surface observations 1957 data not included).

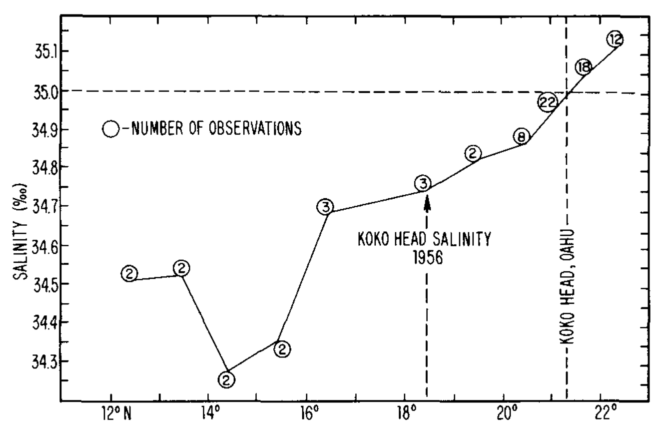

FIGURE 44. Mean surface salinity by latitude at the longitude of Oahu during the period April to July 1957.

SKIPJACK

The skipjack season in Hawaii undergoes marked seasonal, as well as annual variations. Though the annual variations may be large, they have not during any year of record been great enough to suppress the seasonal pattern. July is almost invariably the peak month and December, January and February are almost invariably low points in the annual cycle. Typically, landings begin to rise in April, rise precipitously in May and June, peak in July, and fall off fairly precipitously to October. Variations in effect may serve to accentuate the peaks and valleys, but by and large effort is constant throughout the year so the catch must be interpreted in terms of immigration and emigration of fish through the Hawaiian region. (The Hawaiian tuna fishery generally operates within 20 miles of land so that fishery is in effect a point fishery, unlike the Southern California tuna fishery, which roams over a vast expanse of ocean.) In addition to the seasonal shift in the magnitude of the catches there is a change in the size composition with winter or off-season catches generally comprised of small individuals, five to ten pounds in weight, and the bulk of the season or summer catches comprised of large individuals ranging from 15 to 25 pounds in weight. This change in the composition of the fishery lends further weight to the assertion that the basic changes in landings are functions of the movements of the fishes rather than changes in effort or economic factors.

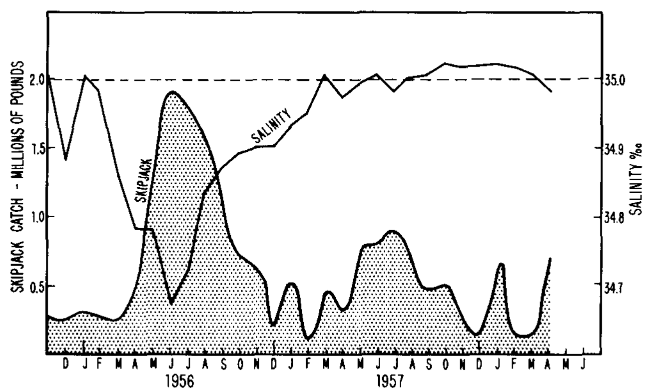

FIGURE 45. Monthly Skipjack landings and mean monthly salinity at Koko Head.

There is now a substantial body of evidence that shows the development of the season, that is the immigration of skipjack to Hawaii is related to the impingement of the warmer, less saline waters from the southeast into the island area. The first indication we had of this came in 1956 from a comparison of our Koko Head monitoring records and the skipjack landings. These data are shown on figure 45, together with similar data from 1957. As already demonstrated, and again indicated on figure 45, the low-salinity water did not impinge on the islands in 1957, and this is associated with failure of the fishery in 1957. The 1957 catches also differed from the 1956 catches in respect to size of fish; the large 15 to 25 pounders were almost entirely absent, the fishery being supported by smaller, typical winter skipjack. This sequence of events led us to examine the catches, that is the summer catches, during earlier years together with such records on salinity as we could assemble. These are tabulated below. It is clear from these figures that, at least during the five years' records, good or average season catches were associated with the presence of lower salinity water while poor catches, including the lowest on record since the war, were associated with higher salinity water.

< 35.0 ‰	> 35.0 ‰
1951—9.9 million lbs.	1952—4.8 million lbs.
1953—7.1 million lbs.	1957—3.2 million lbs.
1956—7.2 million lbs.	

If the skipjack are actually associated with low-salinity water, this association should be particularly sensitive during the spring, that is April and May, when the fishing season is just developing. Figure 46

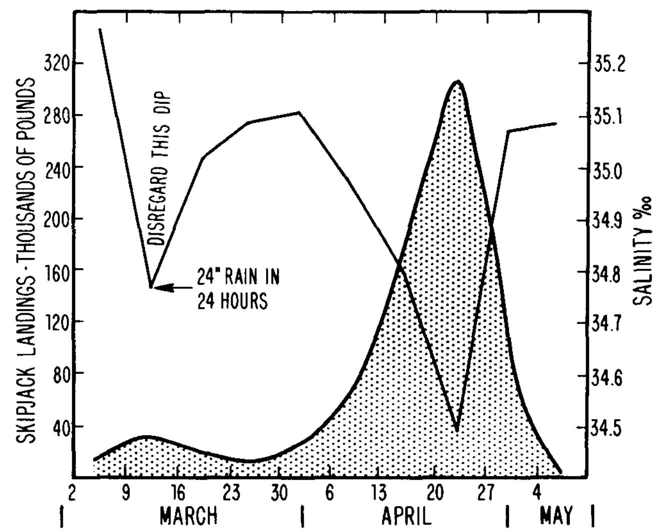

FIGURE 46. Spring of 1958. Weekly Skipjack landings and weekly salinities, Koko Head, Oahu.

illustrates the weekly records for the spring of 1958 during which time the ocean apparently formed a neat experiment for us. As may be seen salinities fell to an extraordinarily low level during the last week in April and this was exactly coincidental with a marked upsurge in skipjack landings; in fact, the rate of landings during the peak week was quite high for April and coincides with a typical June-July week's landings during the normal season. Following this, salinities rose abruptly to winter levels and the catch dropped to winter levels.

If the immigration of skipjack is associated with the advection of low-salinity water from the southeast, there should be corresponding evidence of directional movement of the fish. Data are available from two sources: (1) the pattern of landings along the island chain with the advance in the season and (2) the results of tagging. Unfortunately for a variety of reasons, the data from both of these sources do not provide a critical evaluation of the direction of movement of the skipjack. However, there are several indications that the fish do move into the island area from the southeast and no contradictory indications.

If we assemble all of this information into juxtaposition, it is possible to erect a relatively simple hypothesis to account for seasonal variations in skipjack landings in Hawaii, as follows. The skipjack that enter the Hawaiian fishery are a portion of a population migrating from east to west in a body of low-salinity water suggestive of downstream California Current water. If this low-salinity water impinges on the island area, skipjack in large numbers appear in the island fishery. The season is terminated either by the passage of the last wave of migrant fish, or simply by retreat of the low-salinity water, or both. The migration of skipjack entering the island area may be disrupted and delayed in response to special feeding situations that they encounter. This hypothesis relegates the islands to the role of an operating base for the fleet, rather than a base for the skipjack, and is in accord with our failure to find significant seasonal differences in the standing crop of biota in the Hawaiian Islands and our observation that during a typical season skipjack are abundant far south of the islands, well beyond the possible influence of the islands, whereas indications are that they are not abundant very far to the north of Hawaii. The problem of change in size of the skipjack with the onset of a typical season is also susceptible to explanation for many populations of tuna to arrange themselves geographically by size, the most typical picture being the small fish in a population tending to dominate the extremities of the environment. If we interject this additional factor, we can understand the absence of large fish during the 1957 "season."

ADVECTION—A CLIMATIC CHARACTER IN THE MID-PACIFIC

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In an attempt to discover oceanographic changes in the Hawaiian Islands region which may be associated with the seasonal nature of the skipjack fishery and its annual fluctuations, we have been looking into what might be described as the "oceanographic climate" of the region. This involves a study of the time and space distribution of surface variables which by means of budget considerations will yield some information regarding the processes associated with these distributions. The following notes describe partial results of this study and may be of interest during the second portion of this Symposium.

Since the temperature is of primary interest in any climatic study, the following will deal with the heat budget of the surface layer. On the basis of conservation of heat one can say that at any locality in the ocean the net heat exchange across the sea surface must be balanced by the change in heat content of the water column, heat diffused through the sides of the column, and the heat carried in or out of the column by means of currents. Such an expression can become rather complicated. However, since this is to be a climatic study, interest lies with the gross seasonal changes and therefore some simplifying assumptions can be made for the Hawaiian survey region (10°-30°N., 150°W-180°).

In this area the mixed surface layer is generally well defined and since it has neutral stability one can say that heat exchanged across the sea surface is uniformly distributed throughout this layer. Furthermore, because of high stability in the thermocline just below the mixed layer, and small horizontal temperature gradients, vertical and lateral diffusion are assumed negligibly small compared to advection and heat exchange across the sea surface. With these assumptions the heat budget and the volume budget of a column of water of unit cross sectional area can be expressed by

$$\frac{\partial z}{\partial t} = -\nabla(z\bar{v}) \text{ and } \frac{\partial(C_p z\theta)}{\partial t} = H - \nabla(C_p z\theta\bar{v})$$

which after expanding and combining reduce to

$$\frac{\partial\theta}{\partial t} = \frac{H}{zC_p} - \bar{v}\cdot\nabla\theta$$

In the above equations H is the net heat exchange across the sea surface (insolation less evaporation less back radiation less conduction), θ is the surface temperature, C_p is the specific heat at constant pressure which can here be considered constant, z the depth of mixed layer, $\bar{v}$ the velocity, and ∇ the operator

$$\bar{i}\frac{\partial}{\partial x} + \bar{j}\frac{\partial}{\partial y}$$

The last equation, the temperature budget, states that the time rate of change of temperature and not the absolute temperature is of importance when processes are considered. The equation also points out that in order to understand the advection term $(\bar{v}\cdot\nabla\theta)$ the horizontal distribution of temperature about the locality of interest must be known. In other words, by knowing the horizontal temperature gradient, one can obtain a measure of the velocity component perpendicular to the isotherms. Finally, heat advection cannot give any information about the velocity component parallel to the isotherms.

A similar expression for the salinity budget in the surface layer is as follows:

$$\frac{\partial s}{\partial t} = \frac{s}{z}(E-P) - \bar{v}\cdot\nabla s$$

Here s is the salinity and $(E-P)$ the evaporation minus precipitation.

The above discussion as well as the approximate values of $\frac{\partial\theta}{\partial t}$ and $\frac{H}{zC_p}$ used below, will be presented with more detail in the climatic atlas which is in preparation for publication.

Of interest now are the graphs obtained when $\frac{\partial\theta}{\partial t}$ and $\frac{H}{zC_p}$ are plotted versus time as shown in figure 47A. The solid line shows the mean seasonal variation of the rate of change of surface temperature in the vicinity of Oahu, Hawaii, and the dashed line the seasonal rate of change of surface temperature due to the net heat exchange across the sea surface only. The difference between the two curves indicates advection.

Thus, when $\frac{H}{zC_p} > \frac{\partial\theta}{\partial t}$, the component of flow across the isotherms is from cold to warm, indicating cold advection. When $\frac{H}{zC_p} < \frac{\partial\theta}{\partial t}$, warm advection is indicated. No advection $\frac{H}{zC_p} = \frac{\partial\theta}{\partial t}$, indicates either flow parallel to the isotherms or no flow.

Figure 47A shows what may be called characteristic advection features for the vicinity of Oahu: low advection from February to May, and considerable advection for the remainder of the year.

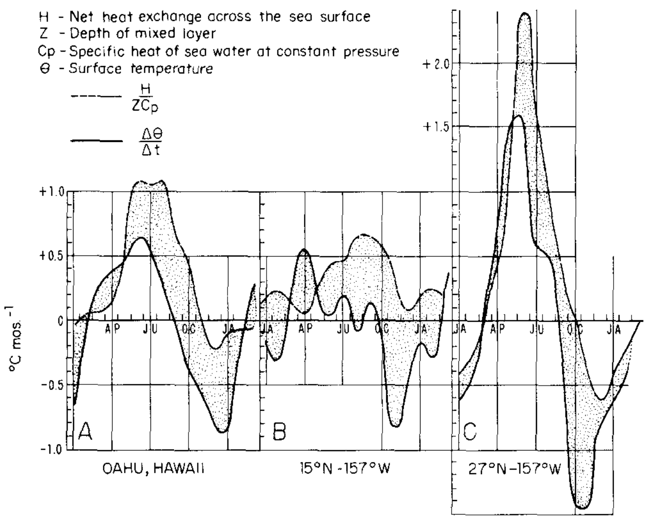

FIGURE 47. Characteristic heat advection curves.

Similar graphs can be drawn for other locations. For example, figures 47B and 47C show the "characteristic heat advection curves" for 15°N. and 27°N. to the south and north of Oahu, respectively. At 15°N. warm advection occurs during March to May, the $\frac{\Delta\theta}{\Delta t}$ max and $\frac{H}{zC_p}$ max are four to five months out of phase, and even the $\frac{\Delta\theta}{\Delta t}$ fluctuations between June and October are believed to be characteristic of the location.

The characteristic heat advection curve for 27°N. (Fig. 47C) shows that the period of low advection extends from February to May and high advection from October to December, approximately as in the vicinity of Oahu. However, the curves also reveal that the heat exchange across the sea surface plays a greater role in determining the surface temperature than it does farther to the south. In other words, advection is relatively less important in determining the temperature than is the heat exchange across the sea surface.

To illustrate how the characteristic advection curves may be used to interpret local temperature anomalies, the mean Oahu temperatures and rates of change of temperature (solid lines) are drawn in figure 48 together with the Koko Head values for 1956 and 1957 (dashed line). Of interest in figure 48A are the below normal temperatures February to June 1957, and again from December 1957 to April 1958. Also of interest are the below normal temperatures during November and December of 1955.

Figure 48B deviations from the mean pattern are brought out in terms of processes. For the sake of clarity the mean $\frac{H}{z}$ curve has been omitted from the graph. The $\frac{\Delta\theta}{\Delta t}$ curve shows that in December 1955 and January 1956 it was higher than the mean curve which can be interpreted as less cold advection. Then from February to April of 1956, the curve is below the mean which indicates an increase of cold advection during that period. September and October of 1956 indicated reduced cold advection. The $\frac{\Delta\theta}{\Delta t}$ curve for 1957 shows colder than normal advection between January and April and then, during May and June, considerable warmer than normal advection. Finally, during November and December colder than normal advection is again indicated.

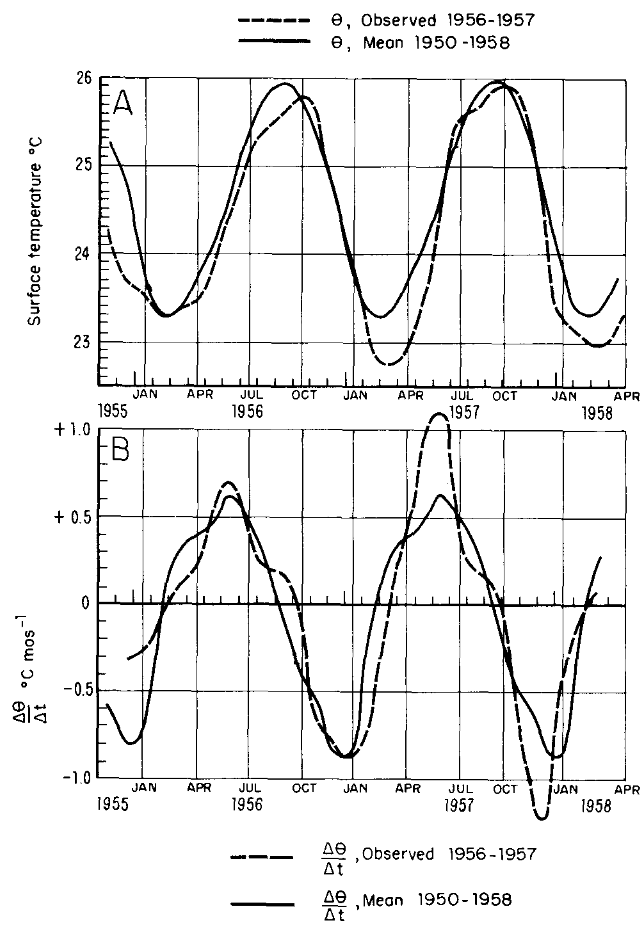

FIGURE 48. A. Mean and observed surface temperatures, Oahu, Hawaii. B. Characteristic and observed rates of change of surface temperature, Oahu, Hawaii.

When the 1957 $\frac{\Delta\theta}{\Delta t}$ curve is now compared to the mean or characteristic curve at 27°N. (Fig. 47C), a striking resemblance in the shape of the two curves becomes apparent. This suggests a southward shift of the oceanographic climate. Or, during 1957, Oahu found itself in an oceanographic climate normally to be found a few degrees of latitude to the north. This observation is in agreement with the salinity observations presented by Murphy et al.

Similar salt advection curves could add significantly to the climatic picture of a region. Unfortunately, our salinity data are too incomplete to enable an interpretation of the 1956, 1957 salinities in terms of salt advection at this time.

To summarize, characteristic heat advection curves illustrate two important climatic features:

1. Deviations from the mean $\frac{\Delta\theta}{\Delta t}$ curve as in 1956 can explain temperature anomalies in terms of heat exchange and advection, and, if the temperature gradients are also known, changes in the component of flow normal to the isotherms.
2. Changes in the characteristic $\frac{\Delta\theta}{\Delta t}$ pattern as in 1957 can reveal shifts in the oceanographic climate.

GENERAL REMARKS

The shortcomings in the above discussion lie in experimentally unverified interpretation of the $\frac{\Delta\theta}{\Delta t}$ curves and not in the use of such curves as tools in interpreting monitoring data. This is primarily due to the fact that one or more members of the budget equation are usually absent.

In order to be able to interpret characteristic advection curves with confidence, temperature and salinity measurements are insufficient to begin with. In the Hawaiian Islands region, the magnitude of the year-to-year variation of H and z must be determined as well as changes in the horizontal temperature and salinity gradients. In order to be able to verify the results, the velocity must also be determined.

We may discover that because of the buffering action of the sea the heat exchange across the sea surface shows little annual variation. We may also discover that the same is true for the horizontal gradients in many areas. We may find that although diffusion is not entirely negligible, the year-to-year variation of diffusion would be. Thus, characteristic advection curves could be used to interpret temperature and salinity anomalies in terms of shifts in oceanographic climate or changes in flow perpendicular to the isopleths. They could also be used to interpret past records where adequate time series are available.

ADDENDUM

In the discussion of figure 48B the deviation of the 1956-1957 observed $\frac{\Delta\theta}{\Delta t}$ curve from the mean $\frac{\Delta\theta}{\Delta t}$ curve was explained in terms of advection. This implies that $\frac{H}{z}$ and diffusion do not show year-to-year variations which, of course, is not reasonable. There are, however, data which suggest that year-to-year changes in these are insufficient to account for the $\frac{\Delta\theta}{\Delta t}$ deviations during 1956 and 1957.

In the vicinity of the Hawaiian Islands examination of the vertical temperature gradient below the mixed surface layer reveals no seasonal change. This suggests that the stability also remains constant and that therefore significant changes in the vertical diffusion are unlikely even if the diffusion is not negligible as assumed in the above discussion.

The seasonal range of the calculated heat losses from the sea surface (evaporation, back radiation, and conduction of sensible heat) is only about 5 percent. It is expected that the year-to-year changes in the heat lost from the sea surface are less.

Remaining is the incident radiation which can vary considerably due to changes in cloud cover. Although no observations are available at sea, the incident radiation has been measured on Oahu by the Hawaiian Sugar Planters Association and the Pineapple Research Institute.

Between 1943 and 1957 the greatest deviation of the incident radiation from the mean for the month of June has been less than ± 10 percent. Therefore, in June one might expect a change in $\frac{\Delta\theta}{\Delta t}$ of .1°C. due to a change in incident radiation. In winter when $\frac{H}{z}$ is small the year-to-year fluctuations in $\frac{H}{z}$ would result in a negligible change of $\frac{\Delta\theta}{\Delta t}$.

Going back to figure 48B, the deviations of the observed from the mean $\frac{\Delta\theta}{\Delta t}$ curve were approximately as follows:

December 1955—January 1956	+ .5°C.
February—April 1, 1956	— .15°C.
September—October 1956	+ .3°C.
January—April 1957	— .4°C.
May—June 1957	+ .5°C.
November—December 1957	+ .5°C.

These are well in excess of changes to be expected from changes in $\frac{H}{z}$ so that advection remains as the most likely process responsible for the 1956-1957 sea-surface temperature changes in the vicinity of the Hawaiian Islands.

DISCUSSION

Charney: Before opening the discussion from the floor, I wonder if I might use my prerogative as chairman to ask a number of questions. Since I do not have the proper background for absorbing oceanographic data in such large quantities, I feel that I have just about reached my saturation point and should like to hear some explanations. Quite a number of events have been reported and, as has been pointed out, we may be talking about phenomena on different scales. The examples presented by Mr. Murphy are a very good documentation of oceanographic changes. Attention has been concentrated on the temperature anomalies. May I ask questions about these anomalies? It seems to me that temperatures in the ocean can be changed by a number of mechanisms, some of which have already been mentioned by Mr. Murphy. Anything that would interfere with the net heat flux at the ocean surface would produce temperature anomalies. But variations in surface-heat flux do not in themselves determine the anomalies. You have to know what is happening beneath the surface. In the central regions of the oceans the heat that is being absorbed at the surface is at least partially balanced by the downward flux into deep water: if you increase the downward flux of heat beneath the surface, the temperature will fall, and conversely, if you decrease the downward flux of heat, the temperature will rise.

Anything that would decrease the anticyclonic curl of the wind stress would immediately, everything else being constant, decrease this downward flux of warm water and produce a warming. This is one mechanism. How important is it?

The next question is suggested by the data that Professor Isaacs so kindly provided beforehand. At Woods Hole we looked at the NORPAC current velocities and temperature distributions and asked ourselves, "Can one account for the temperature increases, which are of the order of 2° or 3° F., in a relatively short period, by advection of warm water?" I gather from Mr. Murphy's remarks that there is a possibility of this too.

A third question is suggested by Mr. Namias' reference to a possible correlation between anomalous temperature changes in the oceans and anomalous temperature changes in the atmosphere, although he carefully avoided the implication of a direct causal relationship. One can at least ask the question: to what extent are the temperatures in the ocean determined by the atmospheric temperatures? I am under the impression that the direct transfer of heat between the atmosphere and the ocean is small and could not account for the changes. If this is so, the influences must be more roundabout.

I do not presume to answer my own questions. They are questions, however, that I think are pertinent to the problems with which we are dealing here. And, I wonder if it makes any sense at this point, before we try to assimilate more data, to try to get some answers. Those who have presented the data have, of course, thought about answers to these or similar questions. By observation they have tried to establish direct physical relationships or to suggest the possibility of some mechanism or another. After all, we do have something to explain: enormous changes—anomalies of up to 6°F over a very wide area in the Pacific. These changes are rather sudden. From December to January, there are changes of 4 to 6°F. How does one account for these changes? Can one account for them by advection?

Fleming: Can I point out here that, in terms of total heat budget for the year, the changes in the heat of water are very, very small, compared to the other process of heat exchange. In other words, back radiation is so large compared say, to heat in the water, that I think you have to look for some process that is altering incoming radiation or evaporation. As has been said before, many of these are feedback mechanisms. I think you have to remember that you are looking at something that is probably a consequence, more than a cause.

Reid: Referring to one of your mechanisms—the temperature change which can be accounted for by wind curl—we do have measurements of temperature change but no computations of wind curl. We do have an index of pressure changes. When we consider the circulation of the winds and the known temperatures, we have found what appears to be in many cases, a relation between wind variations and temperature variations. We have a theory in mind but it is not necessarily a correct one.

Stommel: When you strengthen the California Current you do it by decreasing the wind curl.

Reid: And increased temperatures might be caused, among other things, by weaker winds as well as decreased wind curl.

Stommel: When you increase wind you have an increase in current and therefore a convergence to the right, and both of these could possibly change the water surface temperature.

Reid: Increasing the thickness of the mixed layer by stirring would cause cooling. In part of the Northern Pacific region, the wind strength is *inversely* related to the temperature, but in the Kuroshio it is *directly* related, in some seasons.

Charney: It would depend on how far down the mixing went.

Reid: We do have vertical sections across the California Current which show that in 1957 there was great warming in the mixed layer, and in some cases there appears to be a maximum in the extent of the warming just below the thermocline. The heat has not been merely redistributed so that the water is cooler below the thermocline than above. Enough additional heat has come in by some advective process to have warming below the thermocline as well as above.

Charney: Why do you say some advective process?

Reid: Because I have in mind a tentative hypothesis involving advection, which is consonant with the recent changes in the California Current system.

Charney: What about the stirring downwards of the heated layer?

Munk: I was going to say that maybe to some extent you can reason from an analogy with the seasonal effect as we are talking about a three months abnormality of weather. Dr. June Pattullo, for a doctor's degree, tried to work out a heat budget of the world on a seasonal basis and found out that for most of the world, the changes in heat content are limited to the upper layers. One can use the flux and ignore advection, and come out quite well. That tends to bear out Dr. Fleming, in that there is plenty of flux in and out.

Fleming: Also, I think while we are discussing this, a number of people have tried to evaluate these processes and they never succeed. You really can not evaluate all the factors. We have tried to account for the annual cycle of temperature. Gunter Seckel has been working with data around Honolulu, but you really cannot evaluate these things because there are too many unknowns. For example, in terms of the advection, you nearly always have horizontal gradients, but you do not know the direction of the flow well enough to know what component to put in to evaluate the advection.

Charney: But I gather from those remarks that if you simply made the assumption that there was a little advection, you could account for the changes.

Fleming: If you take the whole North Pacific.

Munk: If you could take the heat content (not temperature) of all oceans, then, of course, you have no advection. Only the flux through the surface is important—evaporation, radiation, etc. For even small units, advection might be a comparatively small factor. Dr. Pattullo took one gyre at a time and did pretty well by simply assuming no advection in and out of the gyres.

Charney: If you consider the seasonal mean, would it be possible to take the wind data and compute the mean Ekman convergence, which would give the flux of heat through the bottom of the wind-stirred layer? It seems to me that other things being equal, this would then give the anomalous temperature changes as well as the heat content changes. Of course we are only talking about superficial aspects. In other words, I do not think of this mechanism as determining the heat supply but only as determining changes in the surface layers. It may have little to do with the overall heat budget of the oceans.

Schaefer: If you know the change in heat content because you have measured it, from what do you compute this flux?

Charney: I may be all wrong and I hope someone will correct me if I am, but here is my reasoning: assume that the Coriolis force in the wind-stirred layer is balanced by the pressure force and the force of friction due to vertical eddy transfer of momentum:

$$f \vec{k} \times \rho \vec{v} = - \nabla_h p + \frac{\partial \vec{\tau}}{\partial z}.$$

Take the curl and integrate through the entire depth h of the windstirred layer. One obtains approximately

$$f \int_{-h}^{o} div_h \ (\rho \vec{v}) \ dz = \text{curl } \vec{\tau}_o;$$

From continuity

$$div_h \ (\rho \vec{v}) \cong - \frac{\partial(\rho w)}{\partial z}.$$

Whence

$$f(\rho w)_{z = -h} = \text{curl } \vec{\tau}_o,$$

which states that the mass transport ρw at the bottom of the stirred layer is equal to the curl of the surface wind stress divided by the Coriolis parameter. If the temperatures at this level are assumed to be known, the heat transport is thereby determined. This is essentially a mid-ocean up or downwelling effect.

Schaefer: Are you going to measure this quantity on the right from the pressure charts, and compute the vertical flux of heat?

Stommel: From a dynamic point of view, would it not be more constructive if you could use mixed layer depth rather than surface temperatures? That is what Seckel did.

Isaacs: But there was an increase in heat below the thermocline off California, and one of Seckel's assumptions was a negligible heat transfer vertically through the thermocline.

Schaefer: Rather than depend on a wind stress to compute heat transfer, Seckel used the total heat content of the water column. He measured this directly. One factor is bothersome here. You have three processes—advection, changes in incoming and changes in back radiation. The bothersome thing is back radiation. You either have to assume it to be a constant or measure it. For incoming radiation and back radiation, you wind up using some average for the Northern Hemisphere—values that do not apply to the particular area you are working with.

Charney: Is this a small difference between two large terms?

Schaefer: It is the difference between two relatively large terms. You do not know them for the exact data you are working with, and you assume some sort of average.

Saur: Roden, did you not make some calculations on the heat transport of the California Current? And did these not show a loss of up to 400 gram calories per day per square centimeter?

Roden: On an annual average the incoming radiation is about 400 cal per cm^2 per day. The back radiation is about 100 cal per cm^2 per day. The loss of heat through evaporation is about 100 cal per cm^2 per day.

This leaves 200 cal per cm^2 per day for the advection term, and the advection must be of cold water. Since radiation varies only by a factor of two in different years, and wind by as much as a factor of five (and stress as the square of this, Eds.), I think that heat changes are more likely to be caused by wind changes than by radiation changes.

Namias: One of the effects perhaps resulting from vertical motion, particularly in some of the areas of figures 21 and 22, was a particular dramatic change to a sharp negative anomaly in a short period of time. Associated with this was a sharp increase in cyclogenesis, which might account for a lowering of surface temperature. Over the easternmost area the temperature rise is confined, as I see it, mostly to the east side of a negative air pressure anomaly (Fig. 7) where there are these anomalous southerly components of wind presumably, resulting in warm water throughout the mixed layer. On the other side of the negative anomaly no such increase is true, as the anomalous components are from the other direction.

Roden: I think that the negative pressure anomaly will have opposite effects on opposite sides of the ocean. We have observed an increase in surface temperatures in the Northeastern Pacific and a decrease in the Northwestern Pacific.

Takenouti: North of the Kuroshio zone in Japan in 1955, there was also high temperature at this point. (1955 was a year of cold in the eastern North Pacific. Eds.)

Munk: In Hawaii do you observe great changes in salinity? I thought they were greater than temperature. In 1958 there was a marked rise in temperature, and a rise in salinity more marked than the rise in temperature. It must mean advection does it not?

Murphy: This was our interpretation.

Reid: And, further strengthening our belief in advection off California, we had significant decreases in sub-thermocline oxygen. There were extremely low oxygens off the coast of California, about 0.5 milliliters per liter less than average, and it is difficult to account for these subsurface changes in oxygen except by advection.

Favorite: I am wondering if insolation would account for the change you attribute to advection. Do you have that much confidence in the insolation values?

Reid: What we have here is too primitive for that. We have temperature measurements which allow us to make temperature anomaly charts over a certain area of the ocean. And there exists a somewhat similar distribution of the pressure anomalies for the period. We put these two sets of charts together—the pressure anomalies and temperature anomalies, and conclude that the pressure anomalies indicate an anomalous wind, which might result in moving water to effect the observed temperature anomalies.

Schaefer: The supposed changes in advection are so great that I do not think that they could be accounted for by errors.

THE 1957-1958 OCEANOGRAPHIC CHANGES IN THE WESTERN PACIFIC

YOSITADA TAKENOUTI

LONG TERM OCEANOGRAPHIC CHANGES NEAR JAPAN

During the earlier period of measurement the Kuroshio flowed closely along the south coast of Japan. In 1937 however, suddenly a cold region appeared off the Kii Peninsula. It became very cold in 1938 and remained so until 1946, although there were fluctuations in the location and the magnitude of the cold region during this period. Since 1946, this cold region has sometimes been present and sometimes not. Therefore, it is quite clear that there are two types of flow in the Kuroshio off the south coast of Japan, one in which a region of remarkably cold water appears and one in which the warm waters predominate everywhere. Figures 49, 50, and 51 show two typical types of the Kuroshio.

The changes of coastal water temperature at some stations along Pacific coast of Japan are shown in figure 52. As you see in the figure, the temperature at Tomisaki was very cold from 1934 to 1936. Then the cold water region off Kii Peninsula (34°N 136°E) appeared and the surface water temperature off Northern Japan became warm.

As Dr. Uda had already pointed out, when the region off Kii Peninsula is remarkably cold, the water temperature off Northern Japan is generally higher than average. Therefore, the two types in figures 49, 50, and 51 are to be considered as representing two models of warm and cold year.

SURFACE WATER TEMPERATURE IN THE WESTERN NORTH PACIFIC

The surface water temperature in the northern part of the Western North Pacific had been high from the summer to the winter of 1955-1956, but by the spring of 1956 it had decreased, and in January of 1957 it was lower than normal. It continued to be lower than normal until the end of the year. In the spring of 1958, however, it became a little warmer than normal, figures 53-61. In years of low surface water temperature the climate of summer in Northern Japan is generally cooler than usual.

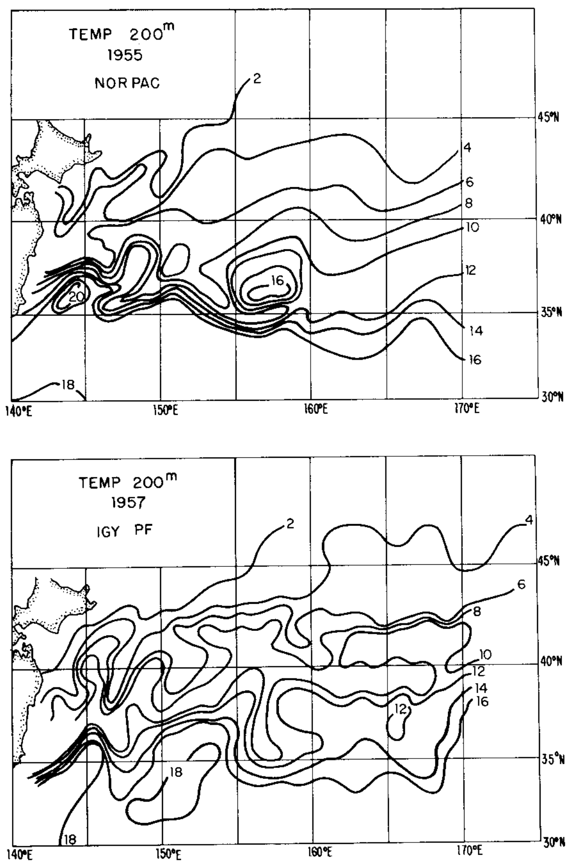

FIGURE 49. Comparison of the two types of Kuroshio Current 1955 warm, 1957 cold. 200 meter temperature.

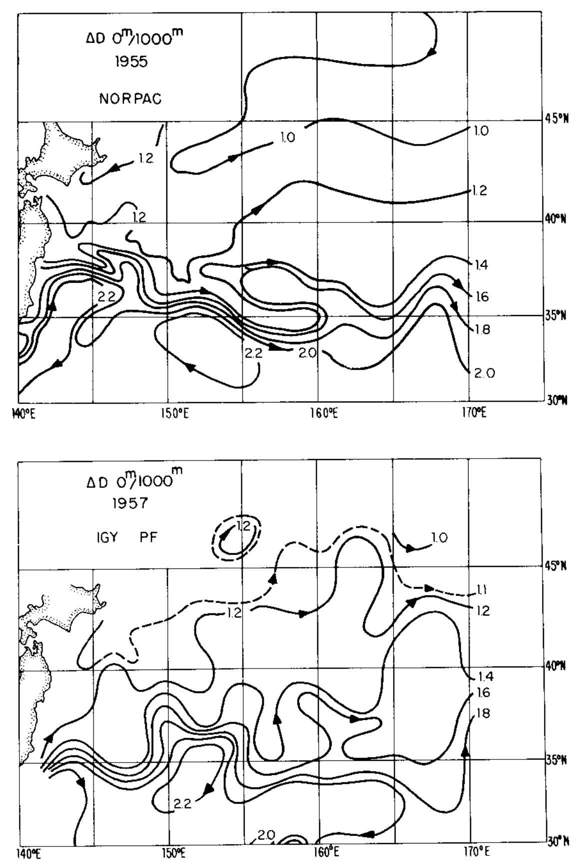

FIGURE 50. Comparison of the two types of Kuroshio Current 1955 warm, 1957 cold. Dynamic height anomalies (0 over 1000 decibars).

THE CHANGES IN 1957-1958

In 1957 the IGY program for polar front survey was held in the area from 30°N to 46°N and from 170°W to the coast of Japan. In figures 49, 50, and 51, some results of the expedition are reproduced as well as similar figures for NORPAC in 1955. It is considered that the year of 1955 was a year of warm water and 1957 was that of cold in the western side of North Pacific.

The most striking difference between them is the change in the axis of Kuroshio off east coast of Japan. In 1957 the Kuroshio was flowing along 35°N while in 1955 it was along 38°N, except east of 155° where the Kuroshio was flowing along 35°N in both years. Comparing the temperature distributions at 200 m layer, we find that temperature was lower near the coast in 1957 than in 1955, but no remarkable differences are seen offshore. The T-S diagrams along 144°E of both years (Fig. 62) show almost no difference, and it may be concluded that the difference in the temperature distribution is due to the change of area occupied by the Kuroshio water mass and the Oyashio water mass, not the changes in the characters of the water masses.

As for the cold water region off Kii Peninsula, it was very obvious in 1955 and very faint in 1957.

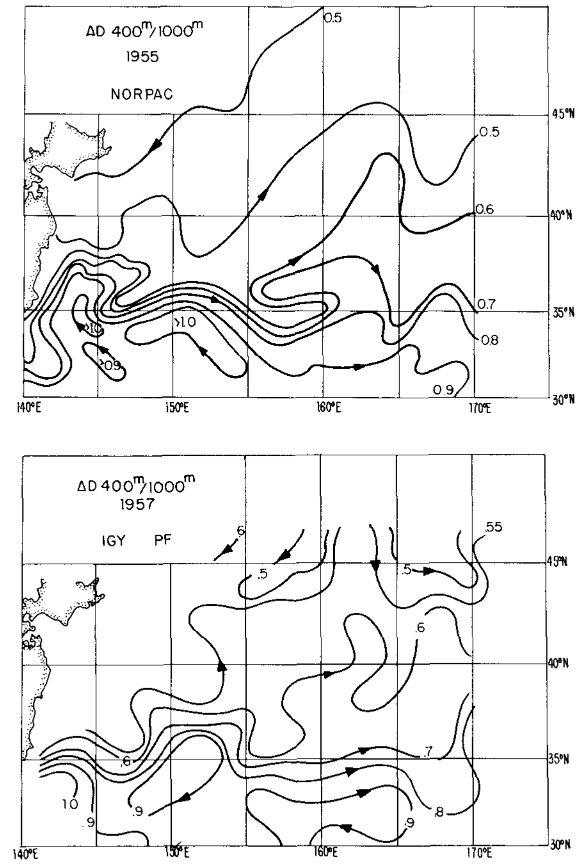

FIGURE 51. Comparison of the two types of Kuroshio Current 1955 warm, 1957 cold. Dynamic height anomalies (400 over 1000 decibars).

PERIODIC CHANGES OF THE KUROSHIO AXIS

The locality of the Kuroshio axis at the longitude of 144°E is shown in figure 63. It shows that the axis changes periodically with a period of four to five years; the average is about four and one-half years. From 1942 to 1948, the position of the axis could not be fixed exactly owing to the sparseness of data, but from some oceanographic and meteorological data there is reason to infer that the axis was in the north in 1943 and 1947, and in the south in 1944 or 1945. It is hardly necessary to explain that when the axis is in the south, surface water temperature off Northern Japan is lower than average, especially in summer, and when it is in the north, higher than average. But I should like to emphasize when the axis is in the south, the climate in Northern Japan is cool in summer and warm in winter.

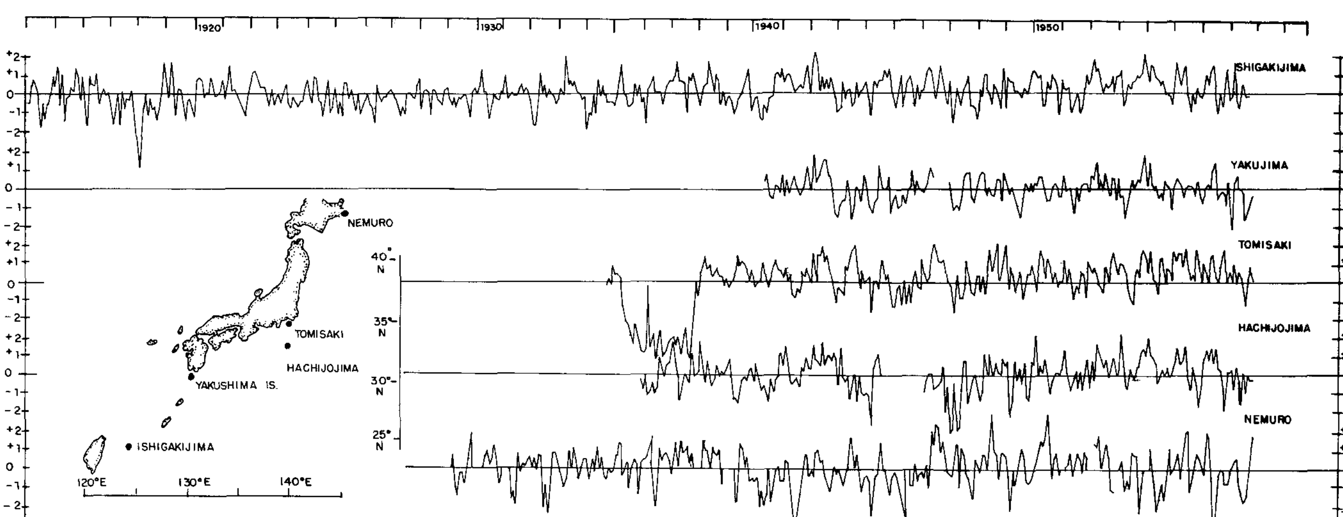

FIGURE 52. Monthly differences from averages sea surface temperature (degrees Centigrade).

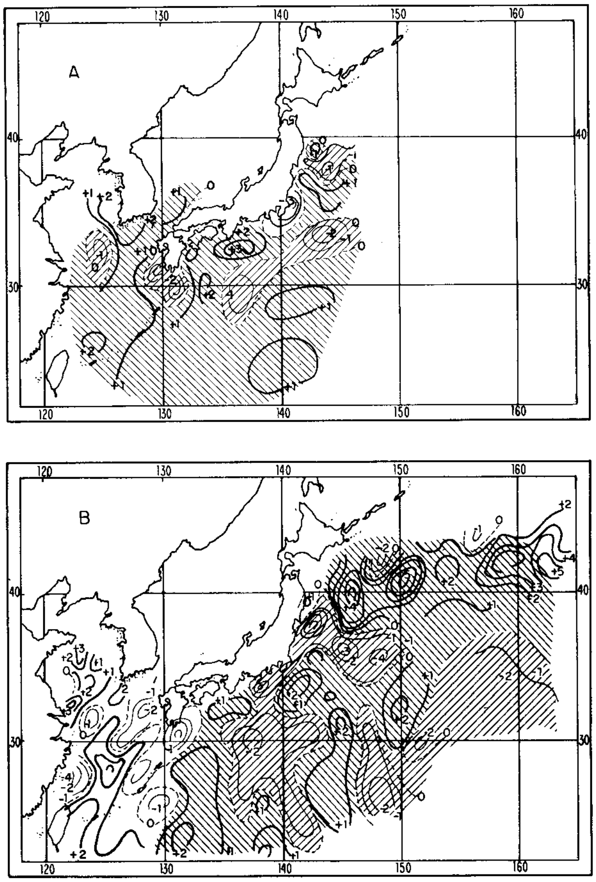

FIGURE 53. Anomaly of sea surface temperature for the second 10 days of April 1956 (A) from the mean of all data previous to 1942, and (B) from April 1955.

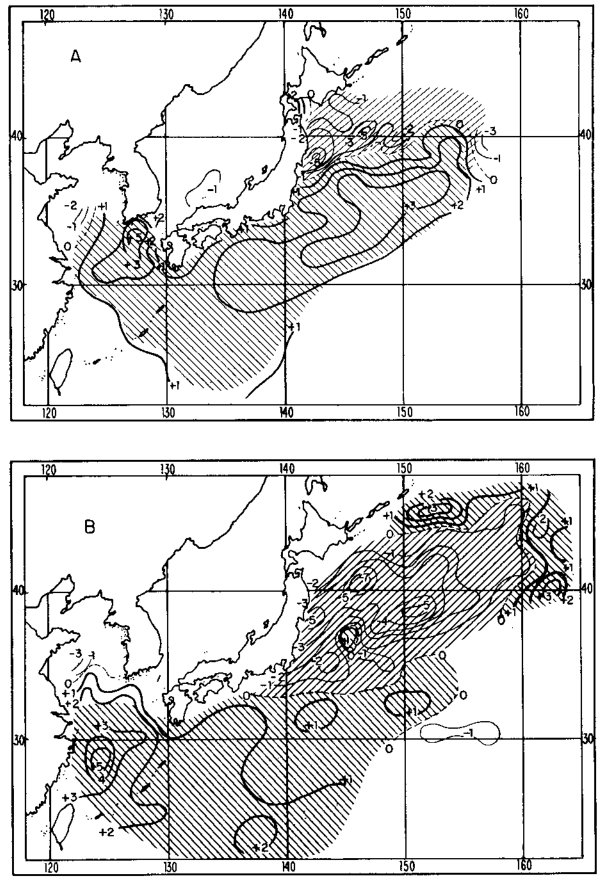

FIGURE 54. Anomaly of sea surface temperature for the second 10 days of July 1956 (A) from the mean of all data previous to 1942, and (B) from July 1955.

CHARACTERISTIC METEOROLOGICAL PHENOMENA IN 1957-1958

The winter of 1957-1958 in Japan was slightly warmer than usual. It was reported that when the surface water temperature was lower than usual the summer climate in Northern Japan was also generally cooler, and the climate of the preceding and/or succeeding winter was warmer; therefore the warm winter in 1957-1958 conforms to the general rule mentioned above.

In 1957 the rainy season was long as is the general rule in years of low water temperature. However, heavy rains were most conspicuous. From July 25 to 29 heavy rain was observed in Western Japan at Saigo, a small town in the west of Kyushu. More than 1,000 mm fell in 24 hours, and in Oomura more than 600 mm fell in one hour. During the long rainy season heavy rains were observed a couple of times in various localities along the southern coast of Japan.

RELATIONS BETWEEN THE TEMPERATURE CHANGE IN THE EASTERN AND WESTERN SIDES OF THE PACIFIC

Some years ago Mr. Hanzawa had pointed out that in the year of cool summer in Japan, there are two types in the surface water temperature distributions. For example, in 1934 a cold region of surface water was observed only at the west of 160°E, but in 1941 it was observed as far as the west of 135°W. Figures 64-66 show the relations between the anomaly of monthly mean coastal water temperature in 1926, 1931 and 1955, a vertical axis for the western side and a horizontal axis for the eastern side. In these remarkable years we can find reversal of temperatures on the two sides of the Pacific. In the figures you will find that the points lie in the second and fourth quadrants. Therefore, it is considered that when the Oyashio is flowing strongly southward (i.e. coastal water is colder than usual) the water temperature

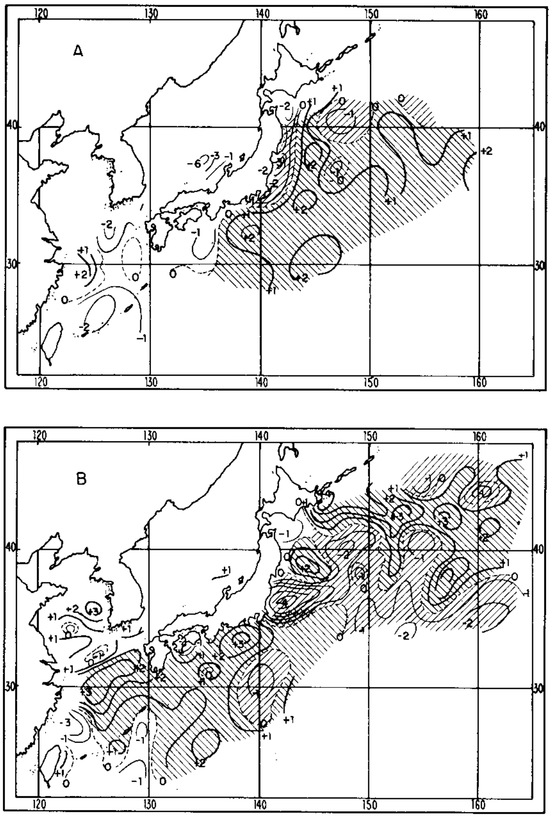

FIGURE 55. Anomaly of sea surface temperature for the second 10 days of October 1956 (A) from the mean of all data previous to 1942, and (B) from October 1955.

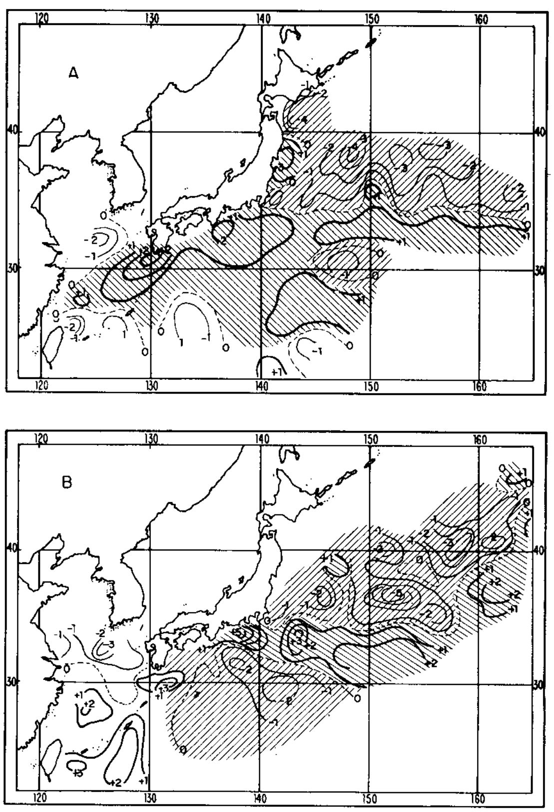

FIGURE 56. Anomaly of sea surface temperature for the second 10 days of January 1957 (A) from the mean of all data previous to 1942, and (B) from January 1956.

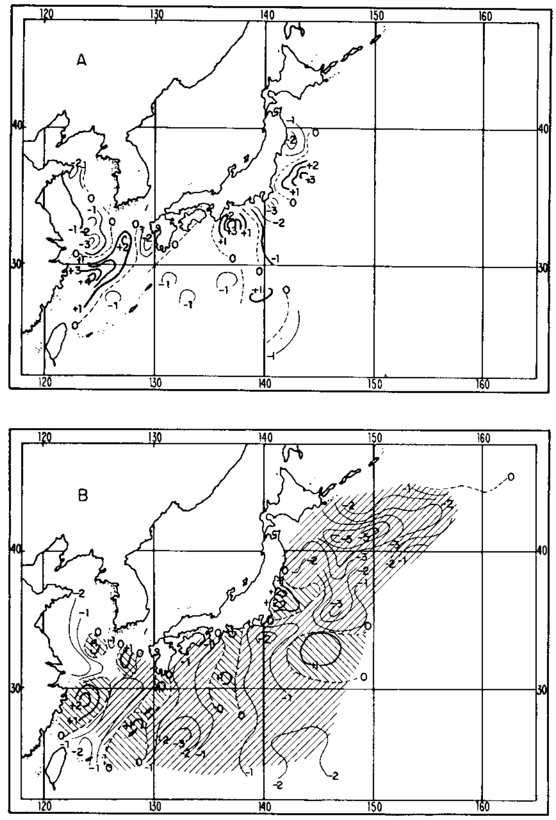

FIGURE 57. Anomaly of sea surface temperature for the second 10 days of April 1957 (A) from the mean of all data previous to 1942, and (B) from April 1956.

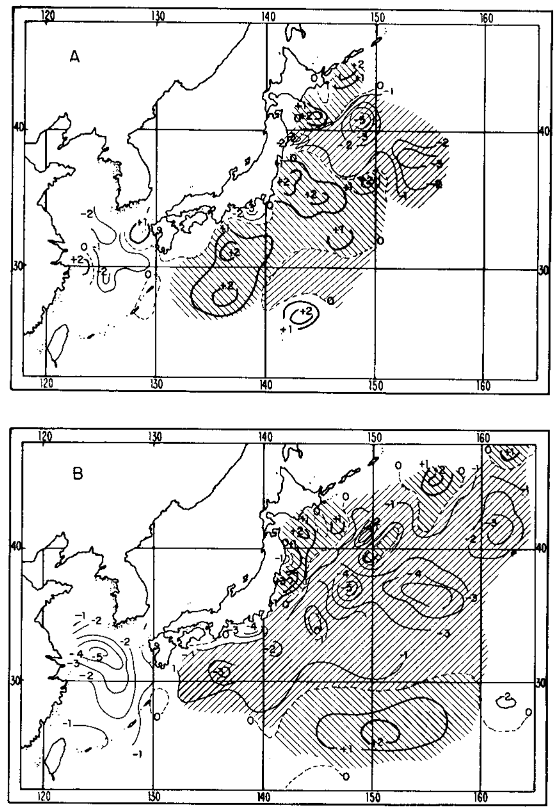

FIGURE 58. Anomaly of sea surface temperature for the second 10 days of July 1957 (A) from the mean of all data previous to 1942, and (B) from July 1956.

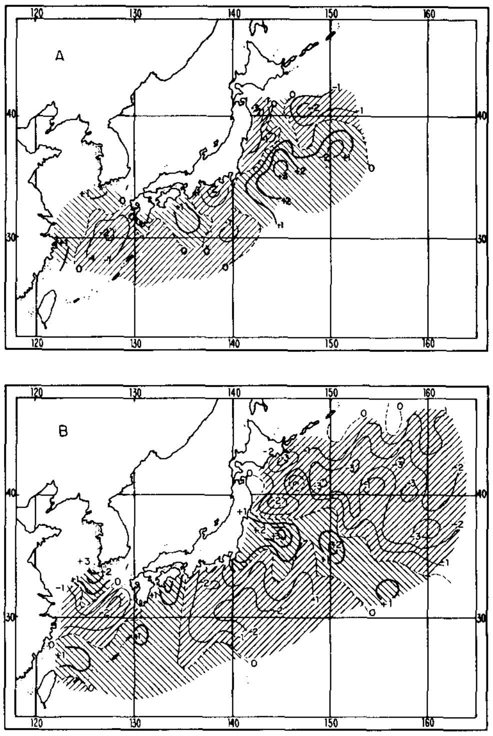

FIGURE 59. Anomaly of sea surface temperature for the second 10 days of October 1957 (A) from the mean of all data previous to 1942, and (B) from October 1956.

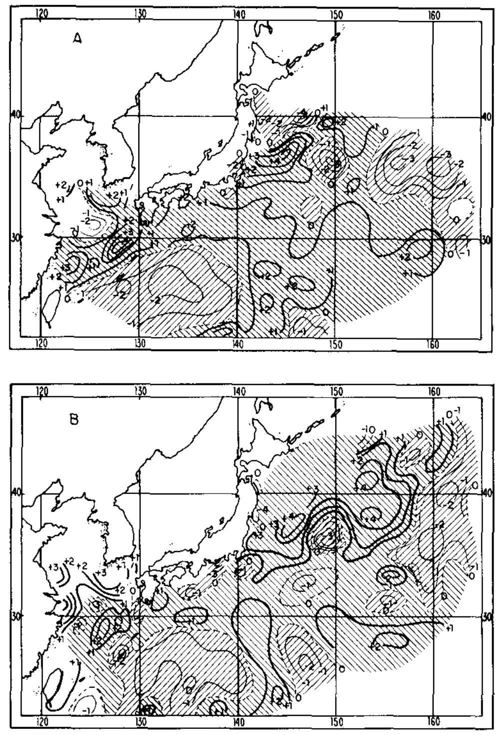

FIGURE 60. Anomaly of sea surface temperature for the second 10 days of January 1958 (A) from the mean of all data previous to 1942, and (B) from January 1957.

along the West Coast of the American Continent becomes high.

During the year 1957-1958 in the western side of the Pacific, the Kuroshio had only small meanderings. The axis of Kuroshio at 144°E was located rather in the south and the eddies producing northward spread of warm water were also less developed. The surface water of northern part of the Western North Pacific was cooler than usual but not so remarkably as in the year 1931 or 1934.

DISCUSSION

Namias: Excuse me, to what did you relate the cold summers and warm winters?

Takenouti: I would like to indicate the 1957 water temperature was cold in summer, was cool generally in Japan. And also the winter was warm. This is one of the correlations between the summer temperature and winter temperatures. My point—in 1957 the year is one of cool water temperature and cool summer, but I hope to indicate that such an abnormality is not so striking, a small anomaly, I would like to indicate. But most striking anomaly of the region in 1957 was very heavy rain in rainy season of Japan. In the summertime last year we had most striking heavy rains in July 25 to 29. The small town of Saigo was almost washed into the Pacific Ocean with 1000 mm of rainfall per hour. In another small town there were 600 mm in one hour. Many lives were lost. Such heavy rains occurred several times and this is most striking anomaly of 1957 in Japan. Thank you.

Charney: I am intrigued that you are not surprised that you found this correlation between the cool summer and warm winter. This is surprising to me. Why isn't it surprising to you? I would appreciate if you would explain why.

Takenouti: I do not know that I should be surprised. I am not a meteorologist.

Wooster: In comparing 1955 with 1957 data in the surface dynamic topography, the surface circulation appears to be much weaker and much more diffuse in 1957 than it is in 1955. The horizontal gradients are weaker in 1957 than in 1955.

Fleming: However, the total relief in the surface dynamic topography across the current is about the same in the two years—from 1.2 meters in the north to 2.0 in the south. There is nothing unusual in the 1955 chart, there is no indication of a double current, whereas there is in the 1957 chart.

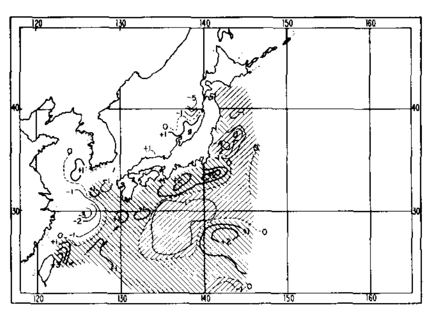

FIGURE 61. Anomaly of sea surface temperature for the second 10 days of April 1958 from the mean of all data previous to 1942.

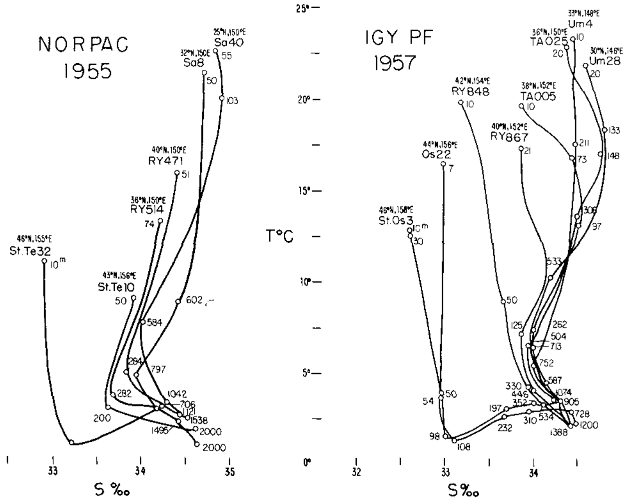

FIGURE 62. Temperature-salinity diagrams along 144°E for 1955 and 1957.

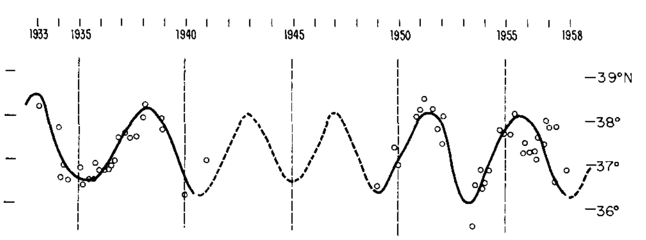

FIGURE 63. Location of the axis of the Kuroshio at the longitude of 144°E from 1933 to 1958.

Wooster: This also shows in the 200 meter temperature charts. There is a grouping of the isotherms both in the north and in the south in 1957. You do not have that in the 1955 data where the principal gradient is in the south.

A point I want to make is that if the 200 meter temperatures have any relationship to circulation, as some people believe, it seems to me that the currents must have been stronger in 1955, as the isotherms are much more closely packed for a considerable distance out from the coast of Japan than they are in 1957. My question is, do you have current measurements and GEK measurements in these years?

Takenouti: We do have for both.

Wooster: Do you find any difference in the measured surface velocities from the general picture of the circulation in the two years?

Takenouti: I understand that they do agree (i.e., the dynamic topography and the current measurements, Eds.).

Charney: Have you made any attempt to associate these rather remarkable fluctuations of the axis of the current with the atmospheric circulation?

Takenouti: Some Japanese scientists have tried to associate them, but I do not know how significant the results have been, but it is not always so that there is any clear association.

Charney: You have shown a very marked four-year periodicity (Fig. 63) in the position of the Kuroshio axis. It does not appear one chance in a million that this could happen by chance. Is there anything corresponding to this four year period in the atmosphere?

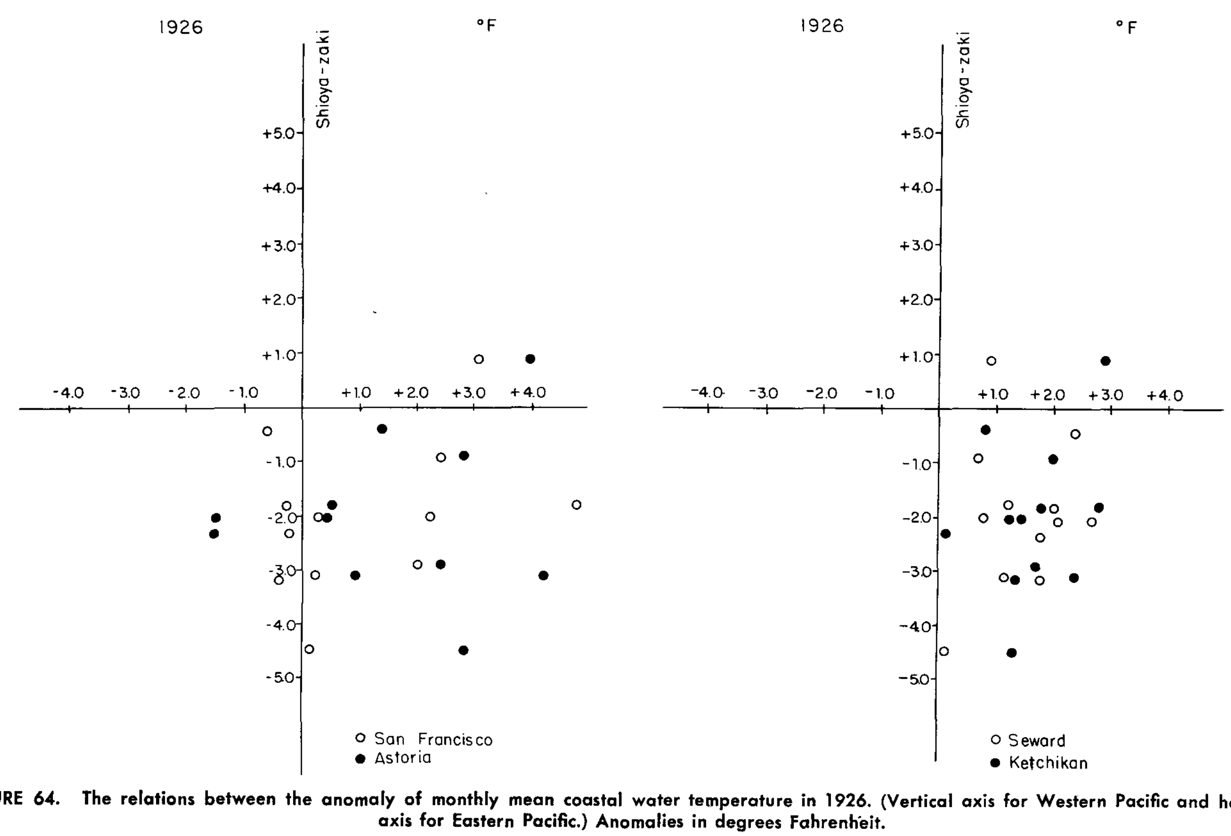

FIGURE 64. The relations between the anomaly of monthly mean coastal water temperature in 1926. (Vertical axis for Western Pacific and horizontal axis for Eastern Pacific.) Anomalies in degrees Fahrenheit.

Takenouti: This period in the ocean actually has not been documented sufficiently.

Schaefer: How do you measure the latitude of the Kuroshio axis?

Takenouti: We draw these charts (Figs. 49, 50 and 51) and then wherever the isobars are closest packed, we assume this point to be the axis of the Kuroshio.

Fuglister: Could I ask a question about something a little bit different? We have this interest in the area of changing temperatures, but what about along the southern part of your figure 49? You seem to have temperatures around 17 degrees at 200 meters, between 16 and 18 degrees, anyway. I was wondering how much that varies? It is about the same temperature we find in the Gulf Stream, within a degree probably. I have never seen much variation in this contour in the Gulf Stream data that I have looked at. It varies little from year to year. Over a period of time the changes are rather small in these temperatures on the seaward side of the Gulf Stream. This water has a remarkably constant temperature for a period of years—so constant that the variation is not more than plus or minus 0.2°C.

In a region in the Atlantic that presumably changes from season to season, there is a remarkable constancy in the 200-400 meter layer as far back as the time of the Challenger Exposition.

Stommel: On 100 meter charts if you pick some sort of characteristic point on the temperature sounding where you can identify a layer—for instance where the isotherm spread a lot, and if you place your attention on some feature like that rather than on a map or chart of temperatures, then you find a remarkable constancy and you can follow the layer over a big area.

Saur: This layer that is so constant in temperature, is it also one of constant thickness and vertical position in time?

Fuglister: Well, I am almost certain that there are variations in those parameters, but they must be very small as far as we know. This has not been measured exactly because it is very difficult to measure the thickness of the layer. There is no abrupt top or bottom to it. It seems to occupy the whole western basin of the North Atlantic, getting thinner as you approach the center and stopping or disappearing as you reach the mid-Atlantic ridge. Going west, the left hand edge of it is near the Gulf Stream and there it occasionally is interrupted by currents.

Question: Does it maintain its geographic position within narrow limits, that is, a relatively constant position?

Fuglister: I know of no shifting at all, but I think you could never get enough observations to show if it were shifting. It goes to about 20 degrees north, just north of the Antilles chain and then stretches out. It always seems to disappear as we come to mid-Atlantic. I am fairly sure these limits vary, moving in and out, but not much.

Question: Is there a great deal of mixing in that area?

Fuglister: There must be in the longitude of Cape Hatteras to mid-ocean in winter. Why is it with all

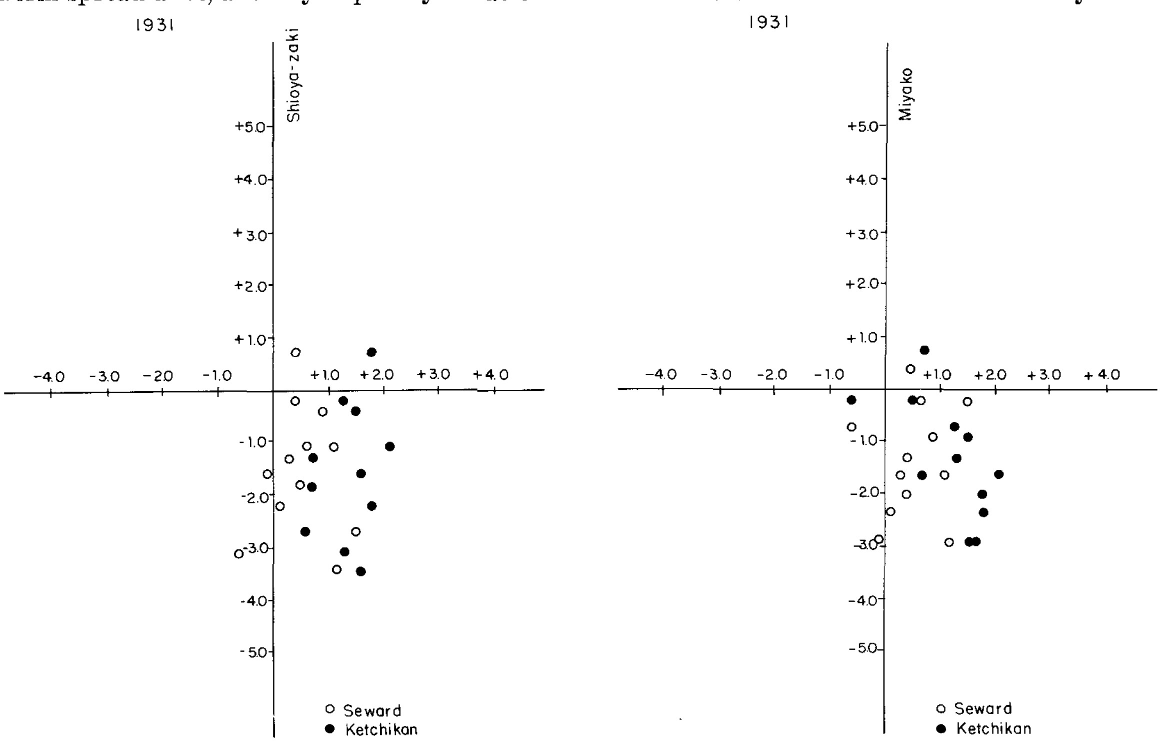

FIGURE 65. The relations between the anomaly of monthly mean coastal water temperature in 1931. (Vertical axis for Western Pacific and horizontal axis for Eastern Pacific.) Anomalies in degrees Fahrenheit.

the big climatic variations this layer always stays so close to the same temperature?

Hubbs: Look at San Diego Bay—it gets up to 23 degrees and then does not vary all summer long, while the outside ocean varies.

Stommel: Really, what we want to find out, is whether or not there was such a uniform layer in the Western Pacific, south of the Kuroshio.

Fleming: Could we get Mr. Namias to correlate what we have just heard about the Japanese area.

Namias: I would like to bring the attention of this group to what I thought was one of the most striking abnormalities in the Pacific as far as the layer of air up to 10,000 feet is concerned (the thickness anomalies). The striking thing in the southwestern portion of the North Pacific is that this entire period has been above normal. For example, figure 6 is for summer 1957. From the Philippines to Southern Japan it is warmer than normal. It is also above normal in the fall, the winter and also this spring (Figs. 6, 7, 8). To me, this long period abnormality is just as striking as some of the other anomalies elsewhere. At the same time at the northern latitudes we have cool Asiatic air. In particular during this past winter (Fig. 8), this Asiatic air was extremely cold and this contrast between the warm and the cold in a way helped the cyclones find their way across the North Pacific and end up as a maximum low. Frequently something of this sort accounts for correlation between temperatures in one area and temperatures in another. Very heavy rains in July were caused by a series of storms in this zone of thermal contrast one after another. Then this winter, part of the great development of the Aleutian low, was associated with these storms that fed on the thermal contrast. Assuming that there is a correlation between sea water temperature and overlying air temperatures, why should that area (southwest North Pacific) have become so warm? I would guess from looking at these things routinely, that there are long period variations in the surface water temperatures over the Southwest Pacific (Northern) as well as in other areas.

Schaefer: In other words, this cooler water off Japan correlated with warmer water in the eastern North Pacific—is this simply circulation around your anomaly?

Namias: Advection is probably associated with it; although I profess to know little about oceanography. I am just drawing parallels with meteorology, but it seems to me from these charts that the dimensions and orientation of these anomalies indicate a large scale advective mechanism. I am reminded of the early days of weather map analysis when it was fashionable to show that local heating and radiation effects explained almost everything. This philosophy ignores the central reason for changes in temperature, namely advection from cold or warm source regions. For example, air from Canada in the winter time usually starts out much colder than the temperature in the States, and so produces most of the temperature change by simple advection. Something of this sort in the free ocean may be the main process of producing changes and anomalies.

Fleming: What I wish to ask, is whether there are possibly two low cells across the North Pacific? We have been looking at anomalies, but what is the dominant atmospheric circulation? There is a characteristic type with a split Aleutian low. It is rather common in shorter period means—particularly monthly and five-day means. When you have these split Aleutian cells, it is another type of circulation. It must have operated part of the time during the winter of 1956-1957. I mention this because Favorite has information on it, and as we see it, cold water came down near Japan and very much warmer water found its way into the Bering Sea. I have a feeling from some of Murphy's data, that at other times maybe this colder water is coming down essentially in mid-Pacific when we again have two cells. In other words, although they may not operate simultaneously, there are two gyres.

Stommel: If you look at Schott's chart of mean temperatures, you can hardly believe that this is so. There is very good indication that there is a single great big gyre.

Stewart: Thinking along the lines of the interrelation of air temperature and sea temperature, I worked up some data from along the Pacific Coast and I think it might be given here, since the matter has been brought up. From four weather stations located near the Coast and Geodetic Survey tide stations, where we also measure coastal water temperature, I worked out the monthly mean air temperature anomaly and compared this with the monthly mean sea water temperature anomaly. The anomalies were, in fact, very nearly parallel for January and February. During the fifty months prior to April 1958, January and February 1958 had the highest sea water temperature anomaly and also the highest air temperature anomaly. However, at the three stations that we were using north of San Francisco, 33 out of 42 station-months had higher sea water temperatures than air temperatures. That was in the Eastern Pacific and along the coast. The air temperatures were considerably higher than the long term mean used as the normal, but not so high as the ocean water temperatures. The ocean temperatures were higher than normal and also higher than the air temperatures, so the warmer waters could not have been caused by warmer air. More probably the reverse is true—the air is warmed by the warmer water. Working with this same sea water temperature and air temperature relationship along the East Coast of Florida, we found that the following periods of colder coastal water, air temperatures were colder with onshore winds than they had been previously. In other words, the water in that case evidently was cooling the air.

Charney: Dr. Takenouti's data on the shifts in the Kuroshio calls to mind a Goldbergian theory that I once dreamt up relating coastal topography to the separation of the western boundary current from the edge of the continental shelf. According to my in-

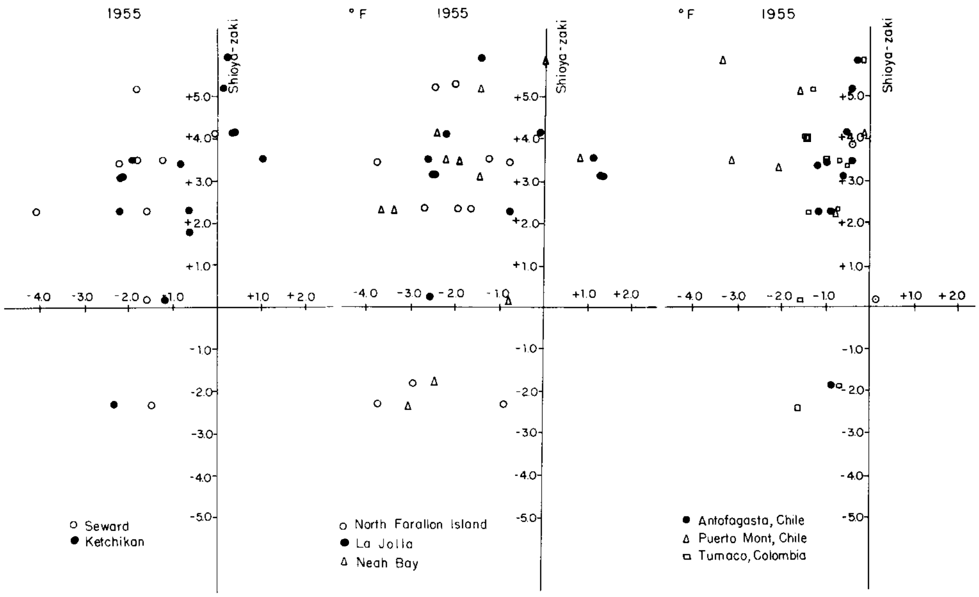

FIGURE 66. The relations between the anomaly of monthly mean coastal water temperature in 1955. (Vertical axis for Western Pacific and horizontal axis for Eastern Pacific.) Anomalies in degrees Fahrenheit.

ertial theory of the Gulf Stream the separation should take place at the latitude of the maximum wind-induced transport. The fact that the separation of the Gulf Stream occurs near Cape Hatteras, where there is a protrusion of land, can be accounted for if one assumes that the coast line south of Cape Hatteras was at one time more or less straight and that the present crescentic contour was produced by the scouring action of the Florida Current. Indeed, when you look at a chart of bottom topography you find that the 1000-fathom contour, which is beneath the swift part of the current, goes straight from the southern tip of Florida to Cape Hatteras, whereas only the surface contours have the indentations. Once the Cape was formed one has to postulate that there was a tendency for the current to "lock in" to the topography. In other words, it is not that the separation takes place because of the existence of Cape Hatteras, but rather Cape Hatteras exists because of the separation.

But the geology of the Japanese Islands and the configuration of the Kuroshio gives the theory a jolt. The volcanic composition of these Islands seems to preclude scouring action south of the latitude where the separation sometimes takes place. Moreover, the separation apparently does not necessarily occur where there is a protrusion of the coast. However, the theory does relate fluctuations in the latitude of separation to long-term fluctuations in the mean wind field over the oceans. I would like to know if it is possible to relate the observed fluctuations in the Kuroshio to long-term changes in the wind field and whether comparable fluctuations are ever observed in the Gulf Stream.

Fuglister: I have a couple of statements. It may seem as though the Kuroshio changes positions more than the Gulf Stream does, but what I worry about is whether it is the Gulf Stream separating from the coast? I think of the coast as separating from the Gulf Stream, because if you look at the globe, you would find that the Gulf Stream generally continues on a great circle course, if you forget its meanders. You can almost consider a great circle course from just north of Florida to the Grand Banks as the mean path of the stream.

Takenouti: In the spring of 1957 we made a study of the western and eastern side of the Ryukyu Islands. We found about half of the Kuroshio Current was flowing to the east of the islands. In general, the Kuroshio flows along the continental shelf from the east side of Formosa. In the spring of 1957 it flowed on both sides of the islands, whereas normally it flows to the east. It then joined together and ran straight again north of these Ryukyu Islands.

Fleming: Charney, I think you started with the right sort of concept here. Somewhere the current is going to have to turn to the right, and if you had a straight coast running north and south, the point where the current would turn away from the coast would fluctuate considerably in location, and there is a topographical control exerted which, in the case of the Gulf Stream, tends to localize the break at Cape Hatteras. I think you can see a number of examples of this. On the West Coast of North America the circulation pattern change is in the vicinity of Point Conception. I think these same events would probably be displaced if Point Conception were 2 or 4 degrees south or north.

Wooster: Could I make a remark? You have been talking about the breaking away of the current from the coast, or the coast from the current, and the same problem comes up in the Peru Current which leaves the coast about 4 degrees south and heads toward the Galapagos Islands. A few years ago, we tried to fix the location of the north boundary of the Peru Current by making many BT observations back and forth in the region off Talara. We found that first of all, this boundary is very clearly marked both by an abrupt change of surface temperature and by a change in the subsurface temperature distribution. We did not find the boundary extending in a more or less straight line out to the Galapagos from the Peruvian coast. Instead it was clearly U-shaped near the coast, and in the course of a week, moved around a great deal. I am sure it moves around in other time scales too. One of the curious things is that while the Peru Current is turning northwestward, the surface winds veer towards the northeast into Panama Bight, so that the current suddenly seems to become oblivious to the winds.

Schaefer: Probably again a matter of scale and measurement over a short piece of ocean. Won't the local currents respond much more rapidly?

Munk: Yes. On operation CABOT the Gulf Stream showed variations with a time scale of a week that were not wind-induced.

Wooster: In this case, I would say there is a quasi-permanent condition. The north boundary of the Peru Current certainly moved around, but it is always in this general area and the winds predominantly go right around the corner into Panama Bight.

Charney: I have the impression that these short-period currents are not directly excited by the wind, and that currents excited by the short period winds would be almost imperceptible. The observed unsteadiness in the Gulf Stream region is due to eddies in the Gulf Stream. These might have been produced by barotropic instabilities associated with horizontal shear or by barotropic instabilities associated with strong horizontal density gradients. In either case the currents would be free, not wind-driven.

Saur: On the West Coast of California you have the upwelling occurring in the spring, and near the end of the season the upwelling ceases. There is some evidence that eddies develop in May. I believe that it was one of Sverdrup's ideas that this was perhaps due to some type of instabilities after the winds slacked off.

Comment: This eddying does not necessarily occur only at the end of windy periods, but exists all the time.

Saur: Current work was done for the city of San Diego during 1956 and 1957. One thing that came out of that, was that following periods of strong offshore winds, Santa Ana winds, there oftentimes were fairly strong currents moving north along the coast. Some other observations indicate this was occurring as far north as Santa Barbara. These would continue as long as two days following the termination of the Santa Ana wind. Just by luck, these were documented very well by scientists who happened to be working in these areas on these days. Therefore, in the days following the Santa Ana winds, we have strong northerly currents moving, in effect, against winds blowing at the same time. Later, offshore the currents were generally from the northwest there, between the islands and the mainland.

Reid: It might be interesting to look into the matter of ocean temperature anomalies over the short periods for which you can make averages, and find out the times when periods of the correlation holds and when it vanishes. Would the net effect of wind become a predominant one?

Munk: Some work has been done on this. First of all, Hela tried to correlate the Gulf Stream with the wind curl over the whole Atlantic. It was a complete flop. Roden has had better luck with somewhat similar studies in the Pacific.

Charney: It may be that we are looking at the wrong data. At one time Stommel and I figured out that the present wind systems would require something like ten years to produce the existing ocean currents. In order to bring about a total balance, one has to transport enormous water masses, a process that is very slow. Just to establish the currents and without the isostatic adjustment, would be very fast, but there must be adjustment. The fluctuations are an appreciable fraction of the whole current. In order to produce them, one would need the integrated effect of the winds over a period of years. If one took the integrated data over five years, one might get the right influence.

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OCEANOGRAPHY OF THE NORTHEASTERN PACIFIC OCEAN DURING THE LAST TEN YEARS[1]

JOSEPH L. REID, JR.

Since 1949 the California Cooperation Oceanic Fisheries Investigations have made hydrographic cruises over the California Current system nearly every month. From these measurements we have been able to construct averages from the first few years' data. We have compared some of the last year's measurements with these longer term averages and the differences are remarkable.

It will be necessary to review briefly the oceanography of the California Current system in order to discuss the recent deviations. I shall draw upon a recent paper (Reid, Roden, and Wyllie, 1958) published in the latest CCOFI Progress Report for this general discussion and for the first five figures, and then proceed to the more recent work.

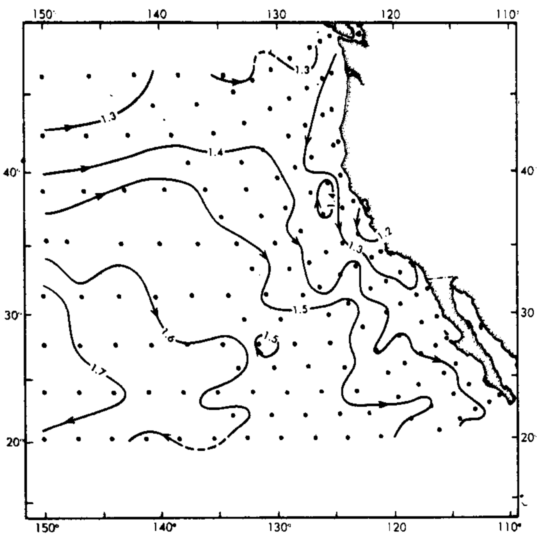


FIGURE 67. Surface current off the western coast of North America in August, 1955. Dynamic height anomalies, 0 over 1000 decibars, in dynamic meters.

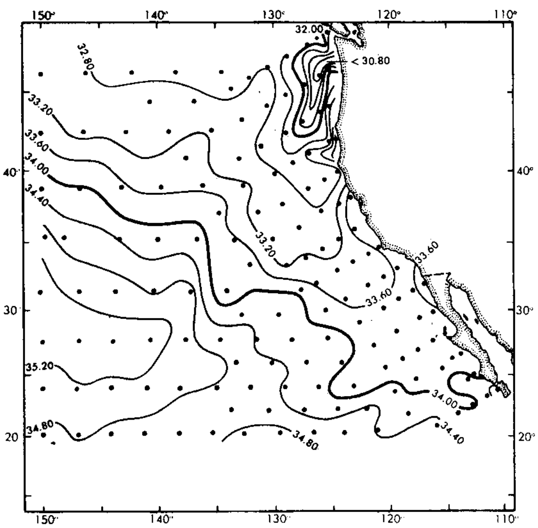


FIGURE 69. Salinity at 10 meters, in parts per mille. August 1955.

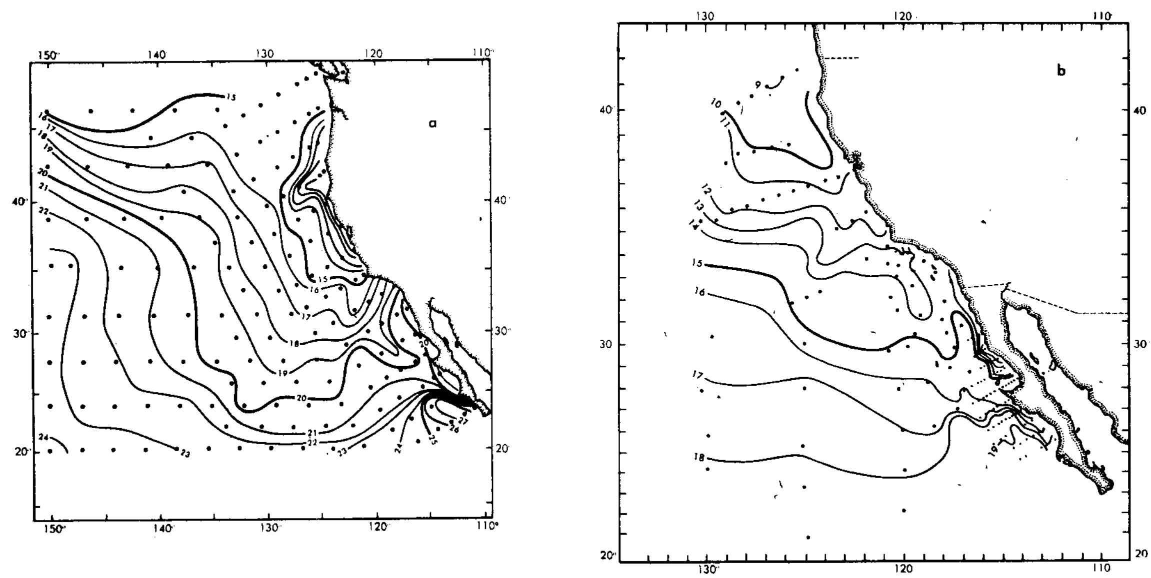


FIGURE 68. Ocean temperatures at 10 meters (degrees Centigrade). (a) August 1955. (b) March (composite).

[1] Contribution from the Scripps Institution of Oceanography.

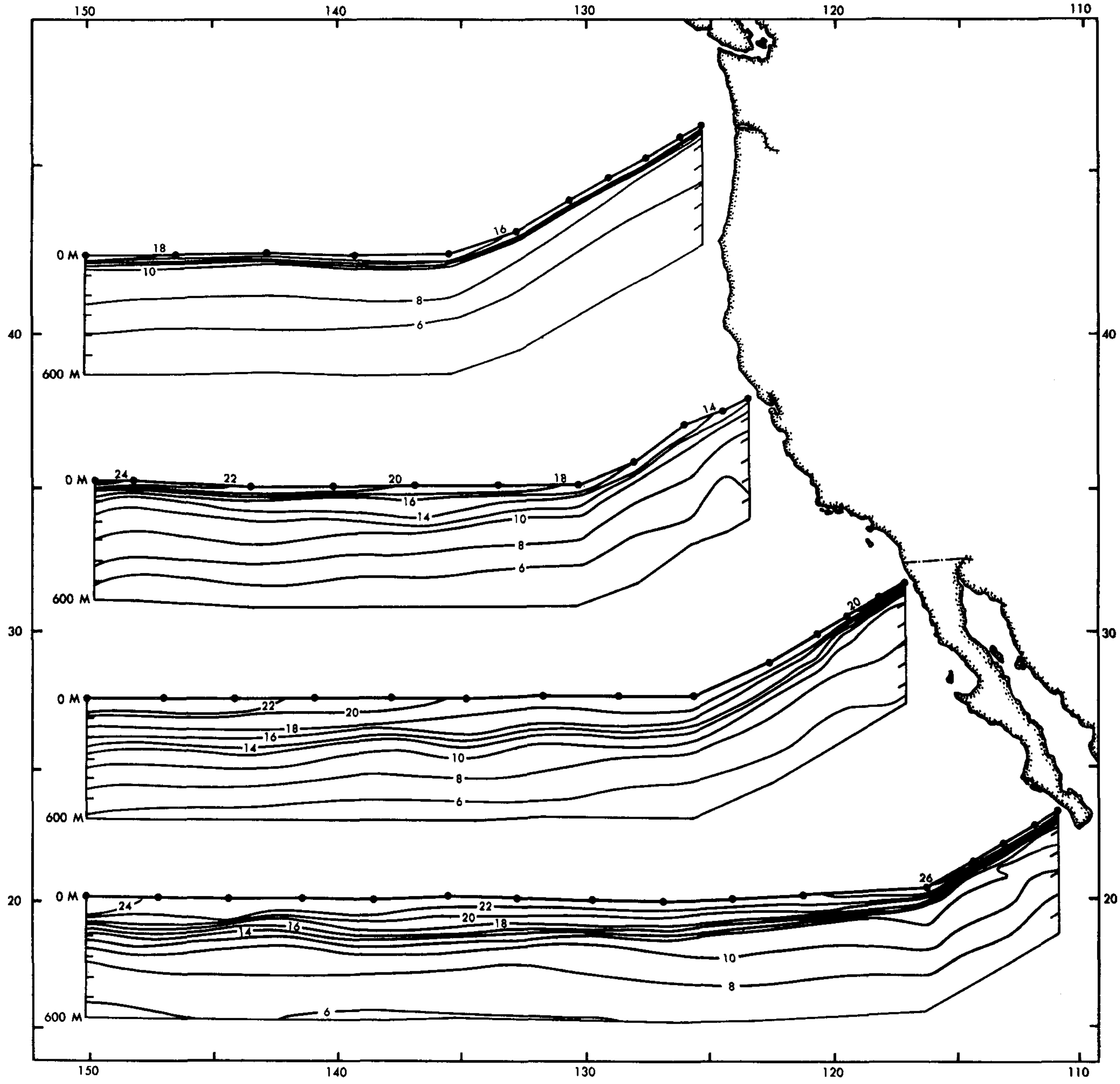


FIGURE 70. Vertical profiles of temperature from the surface to 600 meters, August 1955.

The currents, temperature, salinity and oxygen as they have appeared during most years of the last decade, and the seasonal variation of temperature and salinity are illustrated by the first few figures. The geopotential anomaly in August of 1955 from North America to 150°W and between 20° and 46°N is shown in figure 67. This is the eastern edge of the great oceanic anticyclone which fills most of the temperate zone of the North Pacific. A small eddy is seen south of Point Conception. At 200 meters below the surface a narrow current next to the coast runs counter to the surface flow, and in the winter months the surface waters also run northward near the coast.

The south-flowing current brings water from high latitudes which is both colder and less salty than that farther offshore in the center of the anticyclone. The north-flowing current brings water which is saltier than that offshore. In addition, the deeper waters from the south are very low in dissolved oxygen content.

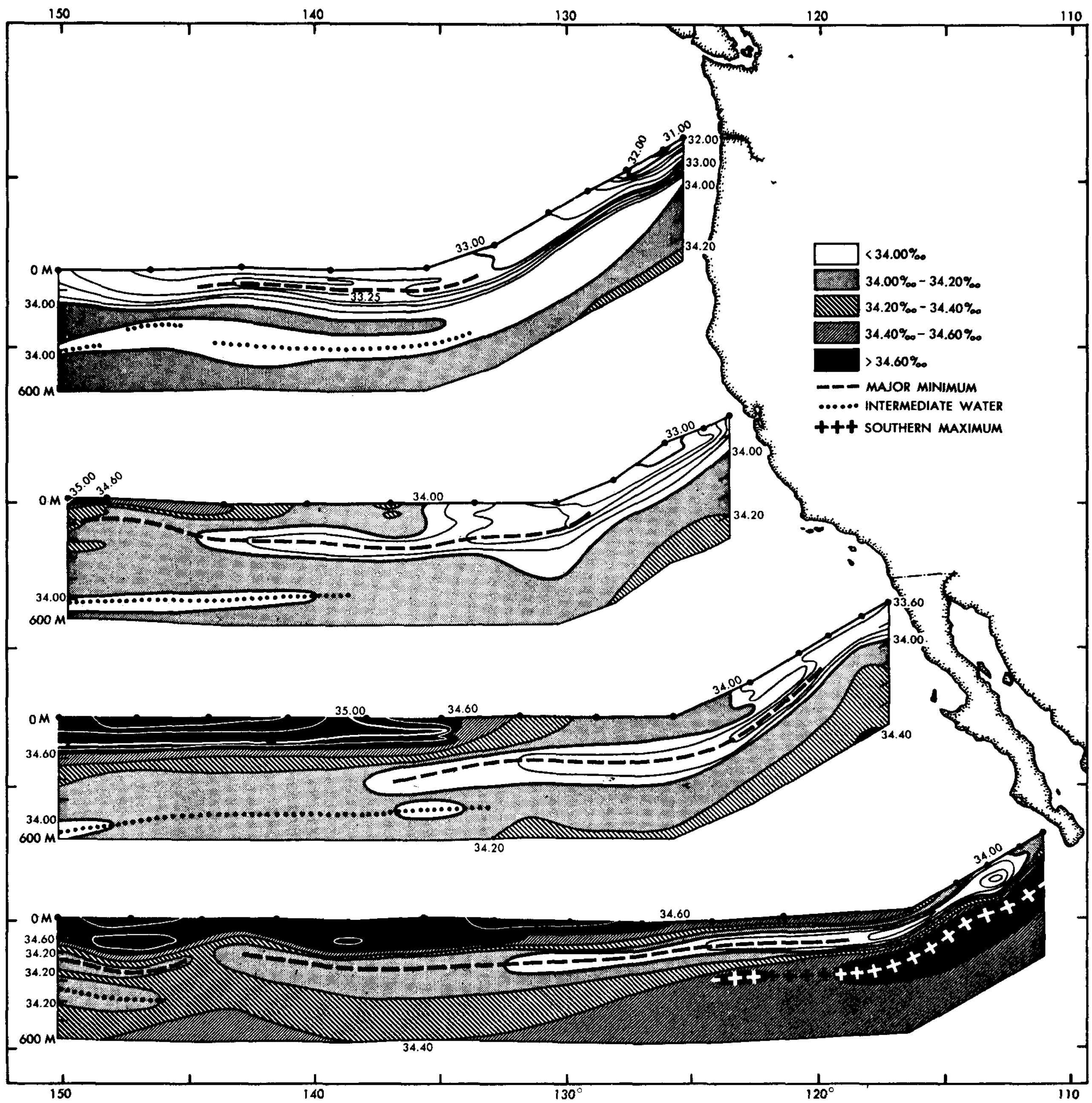


FIGURE 71. Vertical profiles of salinity, parts per mille, from the surface to 600 meters, August 1955.

A remarkable consequence of the strong northwesterly winds of spring and summer is the upwelling which occurs along most of the West Coast. The winds move the surface waters offshore, and they are replaced by the colder and more saline waters from below. Note the enclosed low in temperature from 35° to 46°N in August (Fig. 68a), with a minimum off Cape Mendocino less than 11°C. Note also the strong gradient in the extreme southeast. In March, when there is no upwelling but the seasonal cycle of temperature is at or near its minimum, the temperature off Cape Mendocino is still between 10° and 11°C (Fig. 68b). In the southeast, however, where there is no August upwelling, the seasonal difference in temperature may be as much as 8°C.

The 10 meter salinity in August (Fig. 69), shows the effect of a thin layer of fresh water from the Columbia River as well as the effect of the upwelling off Cape Mendocino. High values of salinity are seen to the west and south.

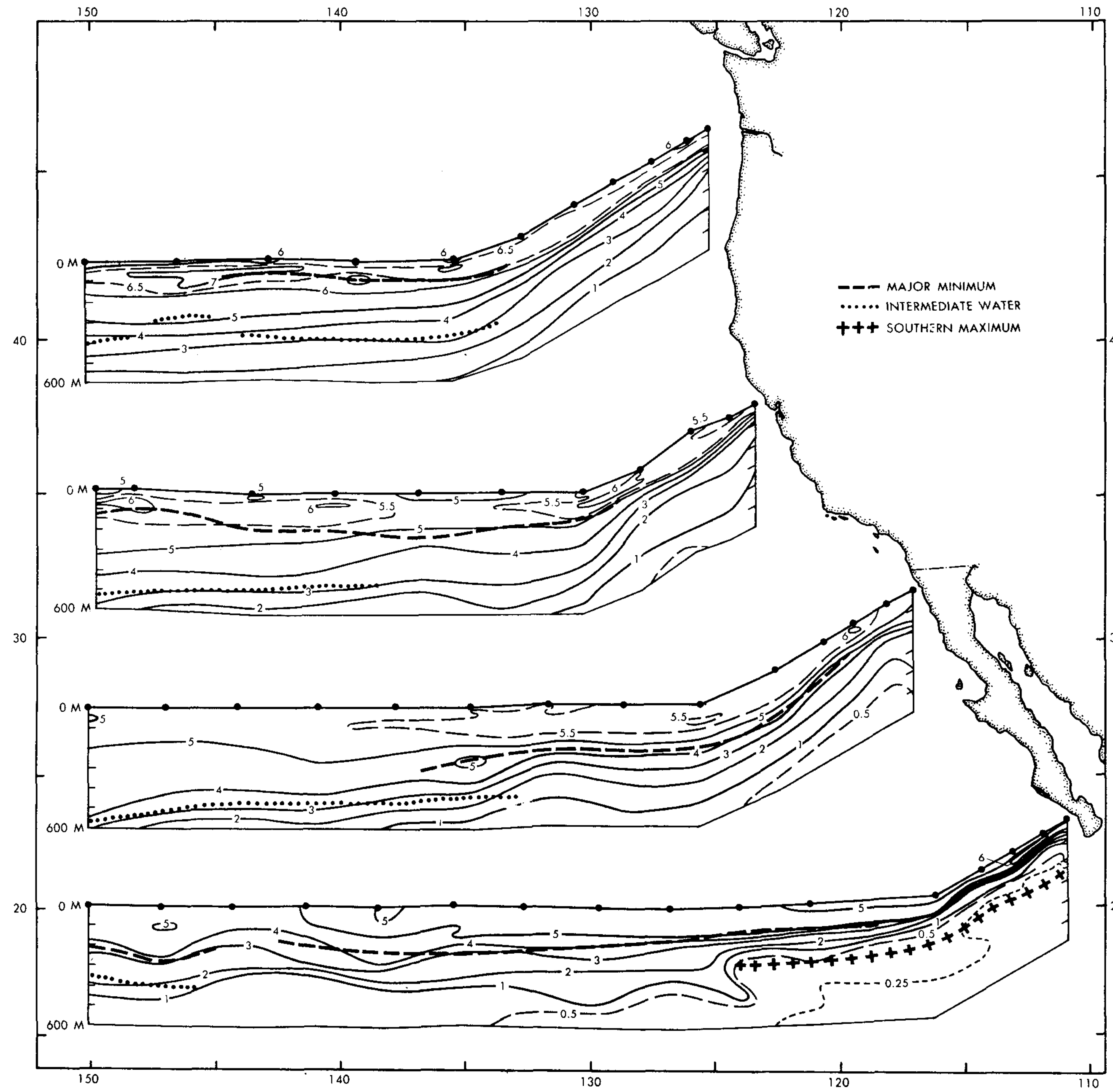
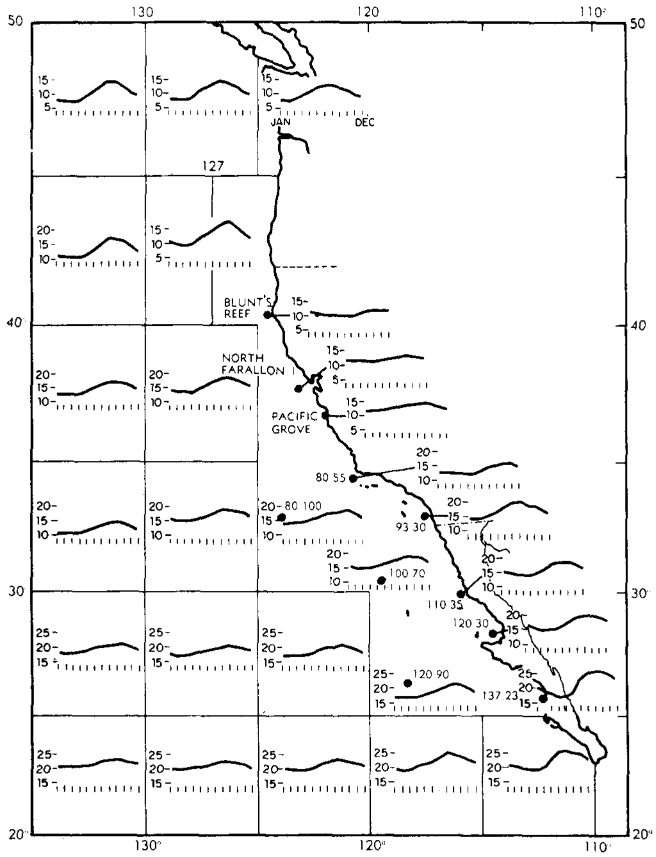


FIGURE 72. Vertical profiles of dissolved oxygen content, milliliters per liter, from the surface to 600 meters, August 1955.

Vertical sections of temperature, salinity, and oxygen in the upper 600 meters in a direction normal to the current are shown in figures 70, 71, 72. Note that at the surface the higher temperatures occur offshore in the north, and in the south they occur at the extreme offshore ends of the section. The vertical sections of salinity show the water of low salinity coming in at the surface in the north. As the water moves south the upper layers are strongly mixed with the more saline water from the west with the result that a subsurface minimum (the dashed line) lies beneath the pycnocline over most of the area. The deeper minimum (the dotted line) is North Pacific Intermediate Water, which is too deep to be of influence in the present problem. In the southeast beneath this subsurface salinity minimum in water from the north lies a salinity maximum in water from the Equatorial regions. The effect of this intrusion of very saline water can be seen to the northward on all of the profiles.



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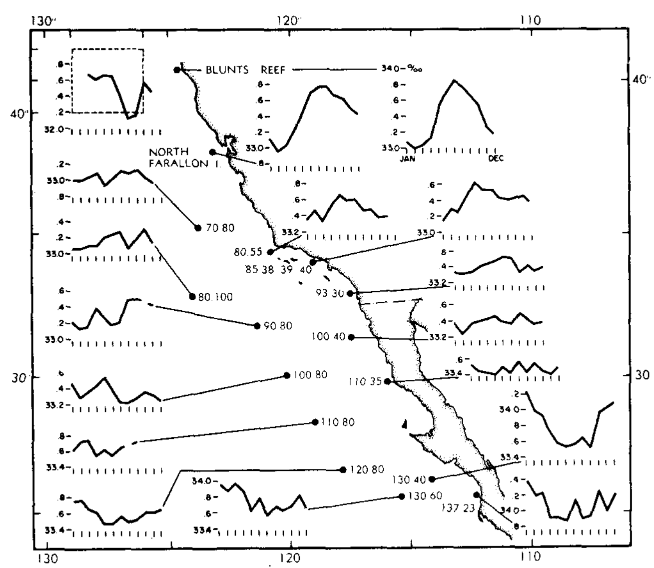

73b

FIGURE 73. Seasonal variation of temperature and salinity at the surface off the western coast of North America.

(a) Temperature in degrees Centigrade. Values over five-degree squares are from Robinson (1957); values at Blunt's Reef, North Farallon Island and Pacific Grove are from U. S. Coast and Geodetic Survey (1956); values at numbered stations are from CCOFI data, 1949-55.

(b) Salinity in parts per mille. Values at Blunt's Reef and North Farallon Island are from U. S. Coast and Geodetic Survey (1954); all other values are from CCOFI data, 1949-55.

The upper waters are saturated with dissolved oxygen or nearly so. The most remarkable feature at depth is the extremely low value found near the coast —especially in the southeast where the water of higher salinity is seen to be of low oxygen content as well.

The extent of seasonal variation of surface temperature over the region is shown in figure 73a. Far offshore the variation, which is principally the result of variation in radiation and exchange with the atmosphere, has a simple pattern with the greatest range in the highest latitude. Near the coast in the region of strong upwelling north of 34°N the seasonal range is reduced and the cool period lengthened by upwelling. Between 28°N and 34°N the upwelling occurs earlier in the year, more nearly at the period of the offshore seasonal minimum, and increases the seasonal range. South of 28°N it is the fall and winter countercurrent which accounts for the high range and delays the low until late spring.

The seasonal variation in surface salinity (Fig. 73b) indicates that the direct effect of evaporation and precipitation is small and, indeed, there is little coherence in the variation of the northern offshore stations. Inshore it is again the processes of upwelling in the north and the countercurrent in the south which dominate the seasonal variation. The effect of the spring and summer upwelling of deeper water to the surface in the north causes a wide range with the maximum value of salinity in summer. In the south the winter countercurrent brings highly saline water northward along the coast giving a maximum in winter. The two effects tend to cancel each other in the middle region between 28°N and 34°N latitude.

The seasonal variation of oxygen is generally in response to the seasonal change in temperature, which causes a change in saturation value.

It has been necessary to give this material as a brief background in order that the variation within the last ten years can be discussed; and again it is necessary to have some idea of the last decade in terms of previous temperatures if we are to think in terms of disturbances of a steady state.

A time-series of temperature anomaly from 1921 to the present and the anomaly of the northerly wind components determined from the pressure difference along 30°N between 110° and 130°W (Fig. 74) indicates that the anomalies of surface temperature along the coast and over the California Current have been remarkably coherent over this period. Certain periods stand out as quite cold and certain other periods as quite warm. The warm periods are seen in 1926, 1931, 1939-40, and 1941. The cold periods are seen in 1924 and 1933. A period of not excessive cold but well below normal values is found from somewhere in the late 1940's through 1956.

The difference is seen more clearly when the mean monthly temperature averaged from 1949-1956 is compared with the 1920-1938 data (Fig. 75). The major differences are seen to occur from March through August. (The gap between 1938 and 1949 is not deliberate. No offshore temperature data were available during this period.)

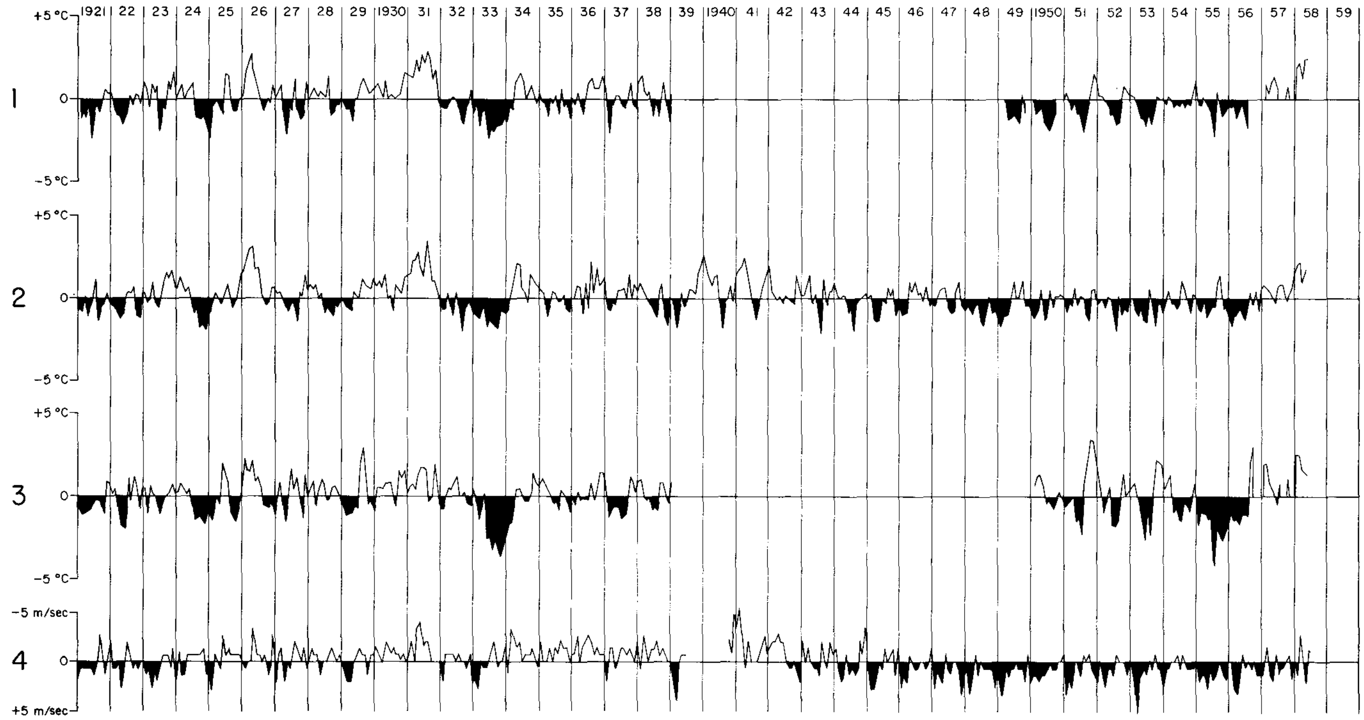

FIGURE 74. Monthly differences from average sea surface temperatures (degrees Centigrade) at (1) 30°-35°N, 115°-120°W, (2) Scripps Pier, and (3) 25°-30°N, 110°-115°W; and (4) monthly differences from average northerly wind component (in meters per second) at 30°N, 110°-130°W. The period 1921-1938 was taken as the average.

However, by early 1957 a substantial difference from the previous eight years had been noted in that the surface waters had become substantially warmer. The recent history of this warming is traced by comparing the weekly averages of temperature for 1957 and 1958 to the present with the longest term means available at each of six positions (Fig. 76). I believe that in the invitations to the Symposium certain anomalies of sea surface temperature over the California Current were presented. I shall refer to those, and more recent ones through May of 1958 (Fig. 77). Let me emphasize that these anomalies are from the mean measured by the CCOFI program period for 1949 through 1954 or 1955, depending upon the stage of data processing. Previous figures (74 and 75) have indicated that this is a period which is different from the long term mean in that February through August temperatures are low and the remainder normal or somewhat high. However, in this series of surface temperature anomalies all of the months since early 1957 have high temperatures relative to the CCOFI mean. It has also been possible to prepare charts of the anomaly of salinity. Here we have selected 10 meters rather than the surface because in the northern regions the outflow from the Columbia River and various other rivers brings into the upper few meters water of low salinity whose values, however, vary from station to station so severely that both an average chart and an anomaly chart are difficult to interpret. By taking the values at 10 meters, rather than at the surface, this incoherence is largely eliminated. These anomaly charts of salinity have also since early 1957 shown consistently high values (Fig. 78).

Another approach is to examine the hydrography of the region with the aim of identifying the source of the anomalous water. Unfortunately when temperature and salinity are both high, suitable sources are found both to the west and to the south. Our ordinary hydrographic casts reach to 600 meters beneath the surface and we can examine the depth of penetration of the anomaly. We find that it reaches to a depth somewhat below the bottom of the mixed layer and is generally not significantly present at depths much below 150 meters. This, however, at first gave some hope of using a third indicator of the source of the anomaly—the dissolved oxygen content. We have prepared vertical sections along three lines extending out from the coast at intervals of 320 miles. These sections show temperature, salinity and oxygen anomalies in the upper 500 meters for July and October of 1957 and for January of 1958.

I shall not present all of the data we have examined but shall discuss briefly the nature of the anomalies in January 1958 (Fig. 79). We find temperature

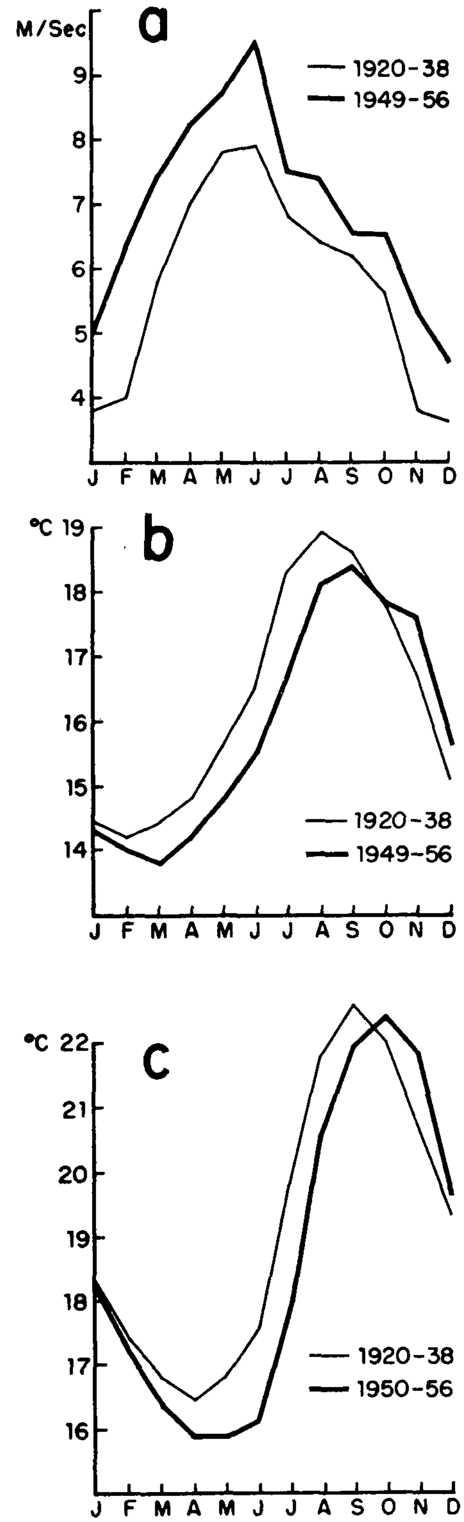

FIGURE 75. Average northerly wind component and temperature in the recent period compared to averages for 1920-38. No data available 1939-48.
(a) Northerly wind component in meters per second at 30°N, 110°-130°W.
(b) Temperature in degrees Centigrade at 30°-35°N, 115°-120°W.
(c) Temperature in degrees Centigrade at 25°-30°N, 110°-115°W.

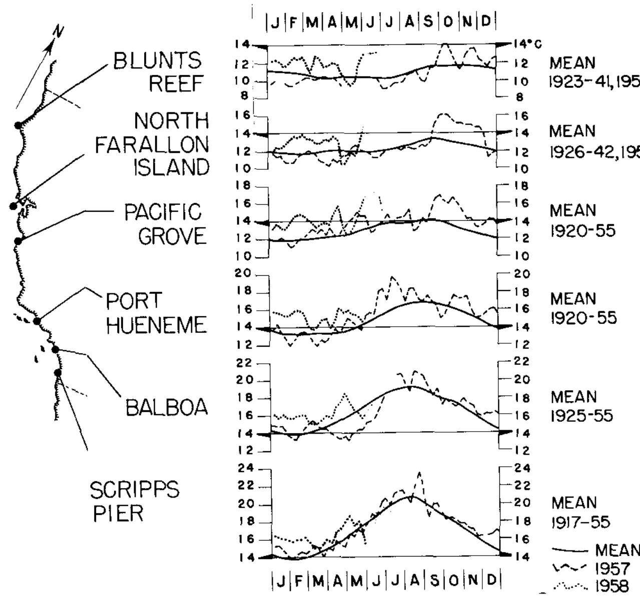

FIGURE 76. Temperature in 1957 and 1958 at six locations along the coast compared to long term means.

anomalies of from 1°C to 3°C at the surface. They generally increase to a high of from 2°C to 4°C in a narrow layer perhaps 10 to 30 meters thick and below the ordinary depth of the thermocline. This depth is from 50 to 100 meters depending upon the distance from the coast, 50 meters being a typical depth inshore and 100 meters more typical at the outer end of the lines. Below this maximum the anomaly dies away to less than 0.5°C nearly everywhere. This would be consonant with a slight warming of the mixed layer and an increase in its depth of 10 to 30 meters.

The salinity anomalies show a somewhat similar picture. The minimum beneath the pycnocline, which we measure at only a few discrete points along the vertical, causes some small confusion, but the general anomaly picture includes a maximum in the pycnocline. The salinity minimum is still present but is somewhat deeper and is higher in salinity.

In the upper layers the change of oxygen should correspond to that of temperature. As the mixed layer temperature has risen, the oxygen should decrease slightly. But if the mixed layer has deepened also, the upper oxygen value should remain constant with depth for another few meters, and the result should be a positive oxygen anomaly in the pycnocline. This situation is indeed found in a great many of the stations.

The deeper oxygen values—those at 200 meters—show something more complex. Both high and low values are found at various stations and at this time no interpretation can be offered.

The data indicate that by January 1958 enough warmer water (or heat) and more saline water have been added to the mixed layer to raise its temperature by about one and one-half degrees and its salinity by about 0.2 parts per mille. The depth of the mixed layer has increased by from 10 to 30 meters. This

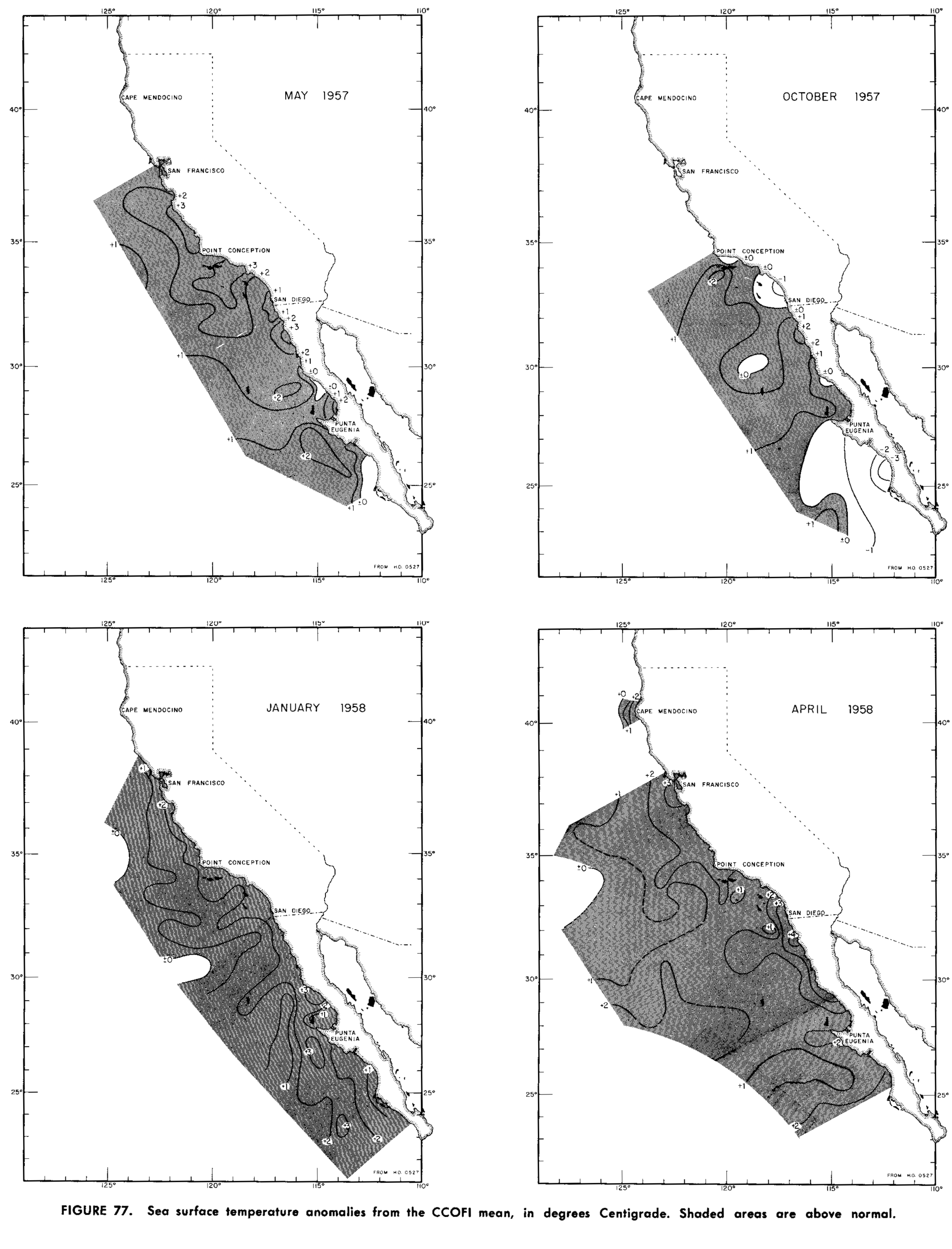

FIGURE 77. Sea surface temperature anomalies from the CCOFI mean, in degrees Centigrade. Shaded areas are above normal.

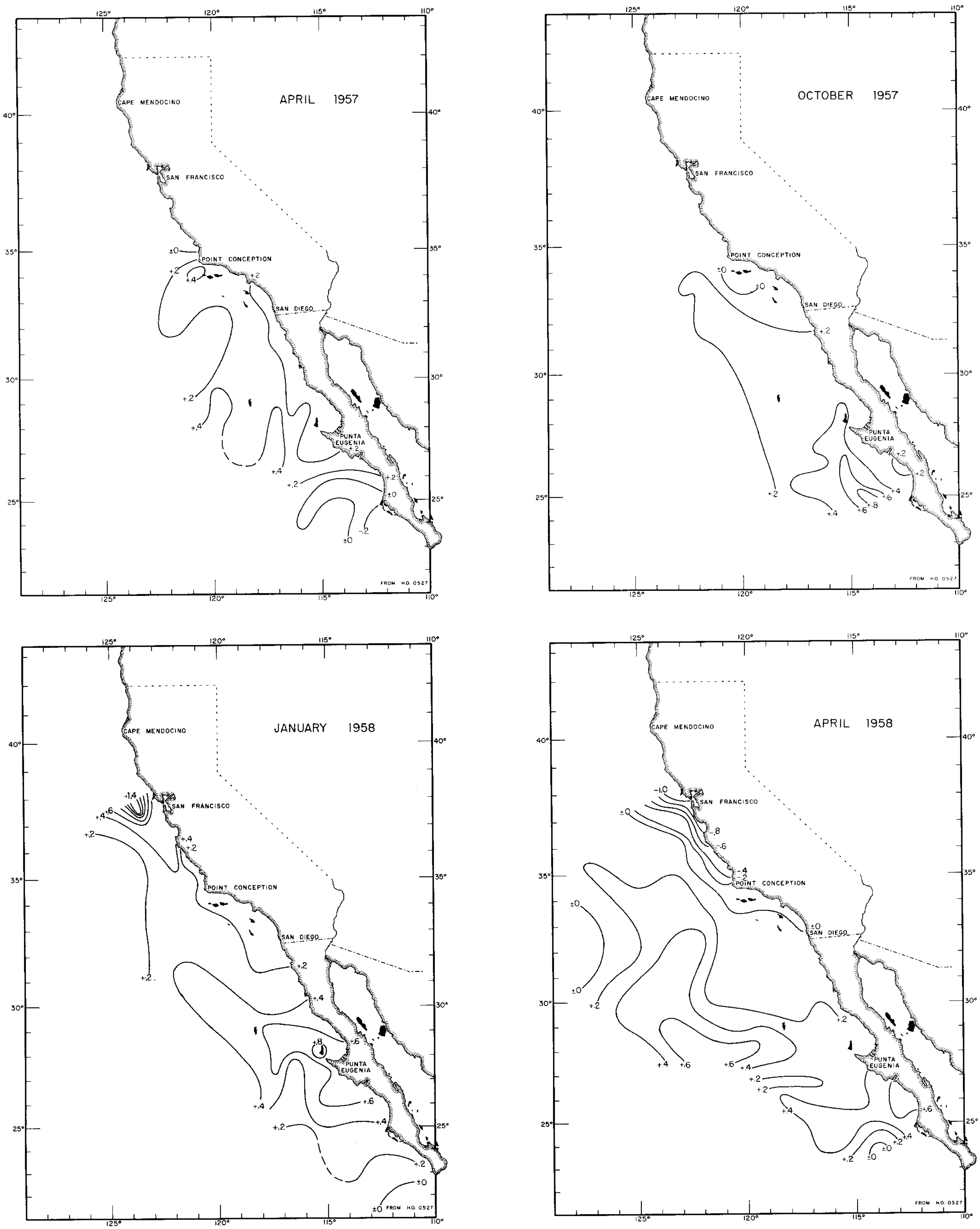

FIGURE 78. Ten-meter salinity anomalies from the CCOFI mean, in parts per mille.

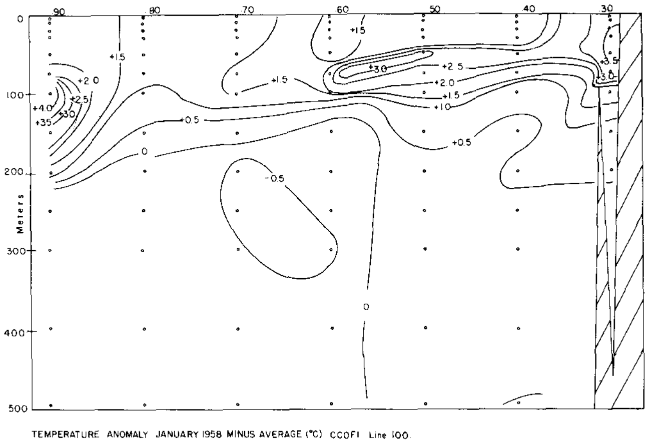

FIGURE 79. Temperature anomaly on a vertical section extending 250 miles offshore. The values are those measured in January 1958 less the CCOFI mean.

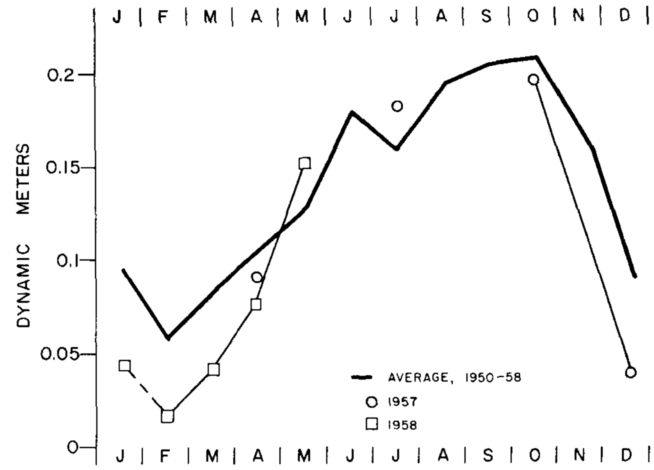

FIGURE 80. Difference in dynamic height (0 over 500 decibars) between a station 140 miles offshore and one 20 miles offshore.

means a net gain of about 10,000 calories per square centimeter and about 1.4 grams of salt. The result of the extra heat is a steric rise of sea level of an average of about 5 cm.

It is not possible at this time to show steric anomalies over the whole area, but a pair of stations, one near the coast and one 140 miles offshore have been compared with each other and with the long-term mean (Fig. 80). Their dynamic heights at the surface relative to 500 decibars are above normal by several centimeters. The difference corresponds to a strong surface flow to the south. The two stations span the region of the countercurrent, with the offshore one so far out as to be always higher than the inshore one. In winter the countercurrent flows north and the difference is reduced. The 1957-58 winter values are about 5 centimeters below the 1950-56 mean. That this really corresponds to a countercurrent is shown by the dynamic topography in January 1958 (Fig. 81) and by the movement of drift bottles to the northward (Fig. 82).

Mr. Roden and I are trying to establish the cause of the anomalous conditions I have described. Our first thought was that in the California Current, coastal upwelling might vary as a result of varying winds. Stronger winds might cause more upwelling, and lower temperatures would result. Under weaker winds upwelling might be weaker and higher temperatures would result. The component of wind from the north along this coast has been higher in the period 1949 through 1956 than the long term mean, and we have seen the sudden change to below-average values from early 1957 to the present. We have also seen the large positive anomalies of temperature and salinity which have occurred at the same time as this change in the wind. Without placing too much confidence at this stage in the hypothesis we have prepared a plot of the north component of wind at 30°N and 110°W-130°W and the surface temperature in the 5° square north of 30°N, east of 120°W and bounded on the north and east by the coast (Fig. 83). We have in-

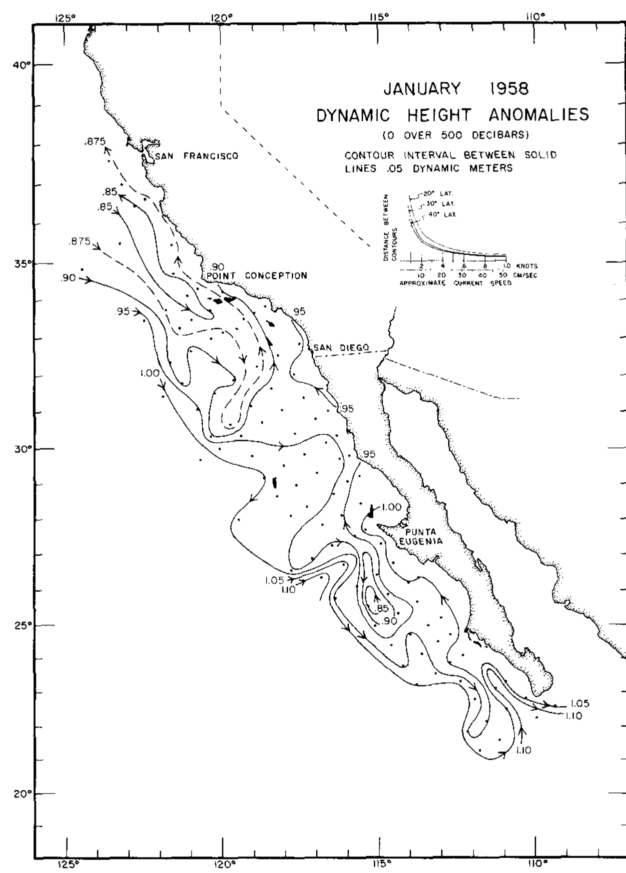

FIGURE 81. Surface currents in January 1958. Dynamic height anomalies 0 over 500 decibars.

cluded all of the available data for the month of May, using CCOFI data for the temperature values from 1949 to the present and values measured by Japanese merchant vessels and averaged and published by the Kobe Imperial Marine Observatory for the years 1916 through 1938. Temperature data are not available in the intervening years and for some of the years pressure data are not available. It would be interesting to have comparable values for 1941 during which period we have good reason from coastal measurements to believe temperatures were high.

Mr. Namias has already described the anomalous wind field of the last year and has presented charts considerably more detailed and accurate than those which Mr. Roden and I have been able to prepare from the data available to us. However, the pressure differences have been so severe that even our limited data have not caused our charts to be substantially different from his.

By looking back into previous years we have found other large anomalies (Fig. 84). January of 1931 was characterized by a severe negative pressure anomaly in the central North Pacific and a temperature anomaly which was negative in the center and positive around the edges. The converse obtained in January of 1933 when the pressure anomaly was positive and large in the center. The temperature anomaly was positive in the center and negative around the edges.

Our own data do not extend far enough offshore to test the effect of the large negative pressure anomalies of the Central Pacific which have obtained during the last year, but by combining them with the ten-day anomaly charts recently prepared by the Pacific Oceanic Fishery Investigations, of which the most recent is for May of 1958, we learn that the anomaly is very similar to that observed in 1931, with low temperatures in the central North Pacific and high temperatures around the edge.

It is thus obvious that even if our first hypothesis of coastal upwelling could account for the temperature variations over the California Current region, there are other and larger area anomalies well offshore for which it could not account. The concurrent large area pressure anomalies suggest that a relation may exist between pressure and temperature anomalies over vast areas of the North Pacific.

The mechanism of the relation could be the intensification or shift of the wind-driven circulation. Over the greater part of the ocean the surface currents are approximately parallel to the winds, yet the isotherms are not everywhere parallel to the currents. They deviate especially on the eastern and western edges of the circulation, along the coasts. Any change in the speed of the current will thus displace the isotherms and result in temperature anomalies. Such an explanation might fit the California Current system as well as that of coastal upwelling, but in itself cannot account for the changes in the central North Pacific temperatures.

Another aspect of the same relation might be a lateral movement of surface waters under increased wind stress, which might account for changes independent of coastal effects. This might be more apt to account for the central North Pacific temperature anomalies.

A third aspect of the relation is the gradual adjustment to a new state of geostrophic equilibrium. If the recent weakened wind circulation over the eastern

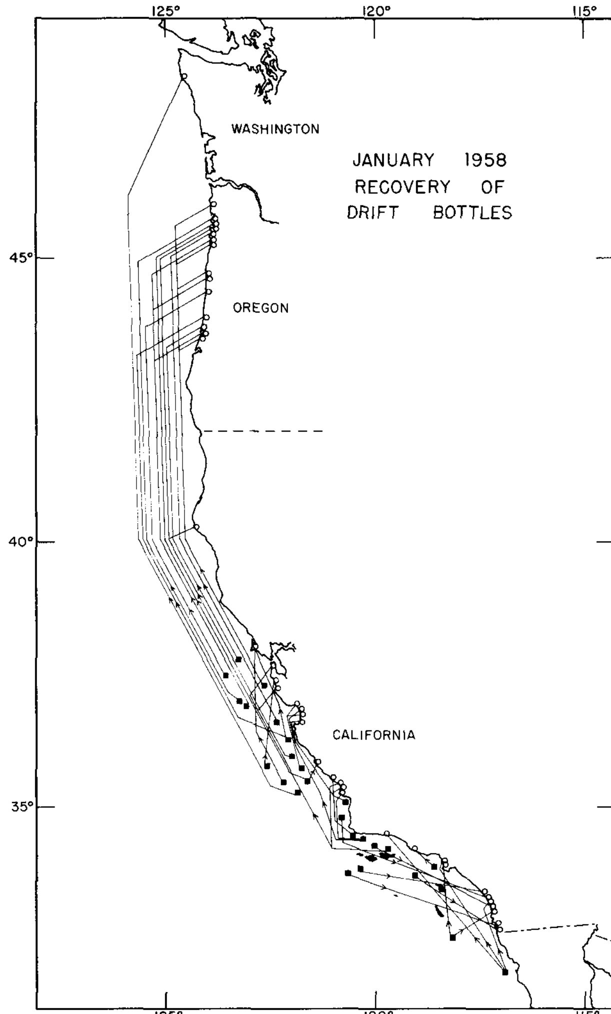

FIGURE 82. Recoveries of some drift bottles released in January 1958. Black squares show the release points, circles show the recovery points.

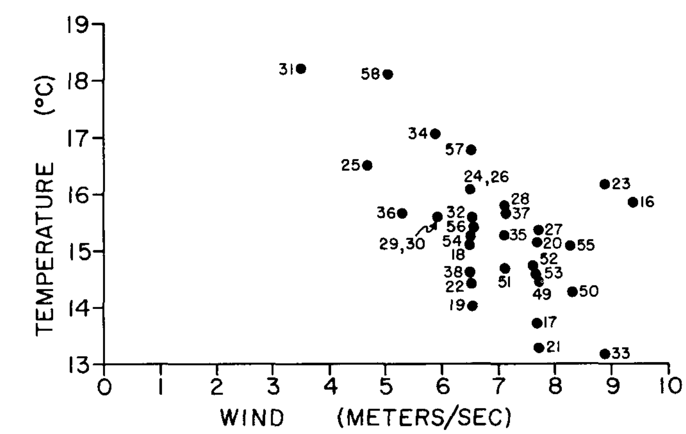

FIGURE 83. May sea surface temperature plotted against May wind for the years 1916-38 and 1949-58. Temperature is measured in the five-degree square 30°-35°N, 115°-120°W. Wind is computed from the pressure difference between 110° and 130°W along 30°N, and represents the component of wind from the north.

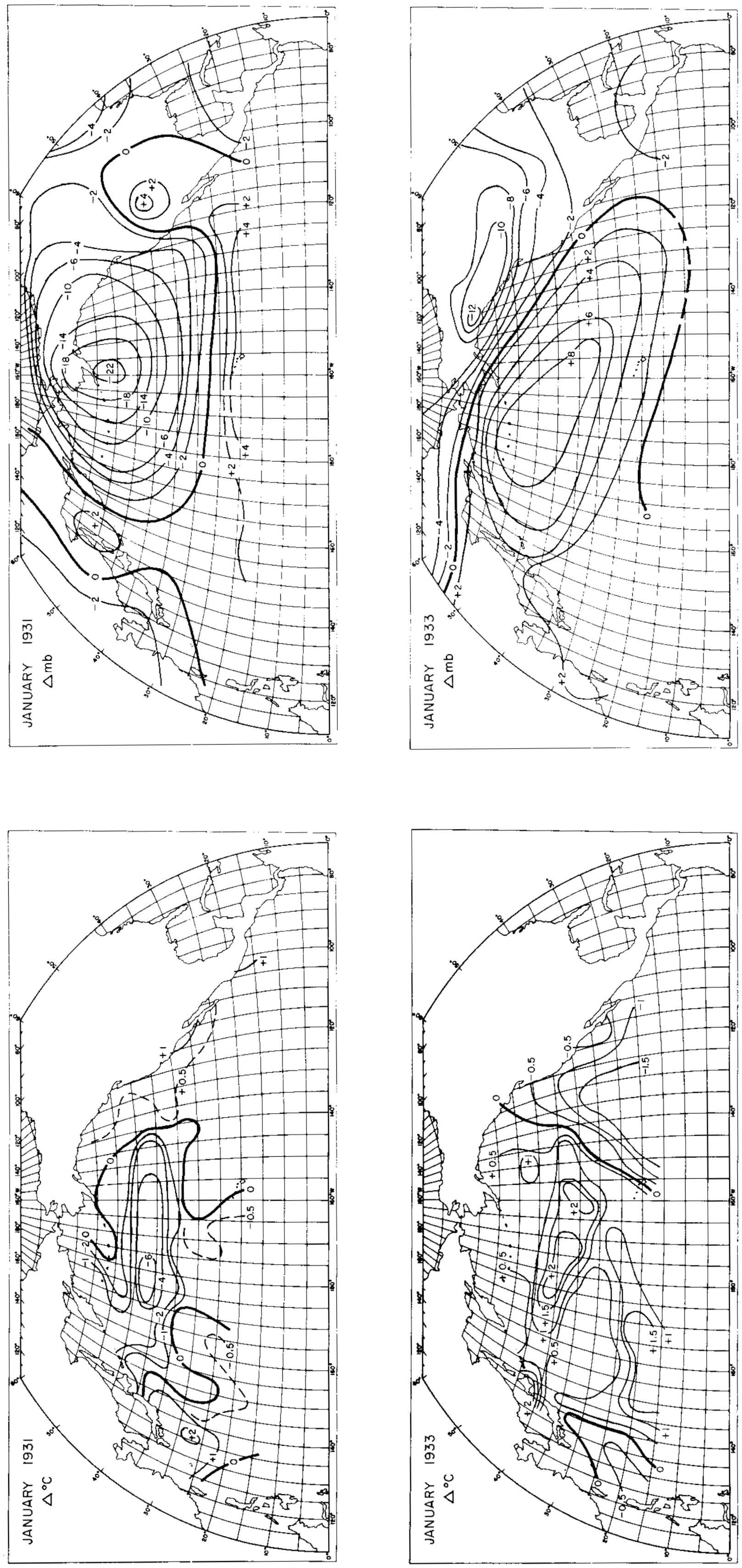

FIGURE 84. Sea surface temperature anomalies and sea level atmosphere pressure anomalies from the long term January means in January of 1931 and January of 1933.

North Pacific has caused a weakened ocean circulation, and a year has passed since the wind pattern changed, some alterations in the height and slope of the sea surface might be expected. We know of at least one location where the slope has decreased (Fig. 80), and the temperature and salinity anomalies have accounted for a steric rise of about five centimeters averaged roughly over the California Current system. The sea level rise at the coast has been about twice the steric rise. Both of these features are consistent with all three of the possible mechanisms I mentioned, that is, with reduced coastal upwelling, reduced lateral flow from the effect of wind stress, and geostrophic readjustment by moving new surface water from the west and south into the region. The latter two, however, might also account for the central North Pacific anomalies.

DISCUSSION

Fleming: Tell us more about your drift bottle program and the countercurrent.

Reid: The drift bottle program began in the fall of 1954. Before January 1958 we released drift bottles from south of Point Conception on the winter cruises. Each time several of the bottles moved northward past Point Conception. In 1958 bottles were dropped north of Point Conception in January for the first time (Fig. 82). Many went northward with velocities as high as 0.5 knot. Some of these bottles traveled over 600 miles northward along the coast.

Isaacs: When you say 0.5 knot, this is only a minimum?

Reid: Yes. This is computed from the time we put them out until the time they are found on the beach. The number of bottles that went past Point Conception was not larger than in the past years. It was principally those we put north of Point Conception that had this tremendous movement to the north.

Wooster: Are you implying that this countercurrent is a double one? And the ones that you put out south of Point Conception did not commonly go north of the Point?

Reid: A few of them did. I have not said that this was a different countercurrent.

Stommel: The deep countercurrent you have mentioned is hard to define.

Reid: You do it essentially by the distribution of the properties. Equatorial Pacific water is identified by certain temperatures, salinities, and a certain temperature-salinity relationship. It extends up from the Equator all along the coast.

Stommel: How deep is it?

Reid: It is found at 200 meters below the surface. The thickness is uncertain since we use as a reference level the 1000 decibar surface, but it is at least 200 or 300 meters thick. Near the coast the deep salinities are higher than those offshore, and at lower latitudes we have a maximum in salinity that shows the northward intrusion of the high salinity water from the Equatorial Pacific.

Question: Low oxygen is a pretty good identifier?

Reid: Yes, it is. The lowest oxygens are along the coast. Offshore the minimum oxygen value is higher and there is a tongue of high salinity and low oxygen water all the way to the Aleutian Islands. Whether it is a movement along the coast or movement upward from deep water, I do not know.

Saur: This is year around?

Reid: Probably, since it is below 200 meters in depth. I have examined it only for July and August.

Fleming: The Davidson Current was well known about ninety years ago, so it is not an original discovery of the Marine Life Research Program. And presumably the knowledge was based in part upon the experience of sailing vessels operating along the coast and from surface temperatures that were available at the time. In other words, it shows up on the surface in the winter when we do not have much northerly wind. We might say that when we have a northerly component of wind it disappears at surface but persists at depth.

Reid: One of the reasons we have had so much trouble obtaining historical data about the Davidson Current is that when Davidson discussed it, he based a good part of his argument on the location of drifting redwood logs. Such information would of course not be available for Southern California since no redwood logs drift into the ocean here, and we cannot draw any information about continuity of northward flow past Point Concepcion from his work.

Schaefer: I think we are talking about three different things: first the Davidson Countercurrent, that is somewhere up the northern coast. The second is this movement of water near the surface from somewhere down off the Mexican-Baja California coast, shown very well by MLR studies. This is near the surface. And the third thing, is the general movement of deeper water off the southern Mexican and Baja California areas. I think they are not quite the same.

Charney: This water has to come from some place —presumably from beneath the thermocline. This would produce vertical motion. And if this is going to be balanced, from considerations of continuity, you have to have northerly flow. This, in a sense, is what you are arguing about.

Reid: This may be perfectly right. I have nothing against your explanation, except that it would have upwelling off California moving water northward from a position 2,000 miles to the south of the upwelling. This may be perfectly right, however.

Fleming: What is your southern boundary on the chart (figure 67)?

Reid: These data were from the NORPAC Expeditions, which extended from 45°N to 20°N.

Wooster: You show the thermocline is getting shallower as it approaches the coast. Some of the profiles also show something I feel is very common on approaching an upwelling coast—a weakening of the thermocline and the presence of a trough in the isopleths. You have shown one profile, and I have seen others across the Peru Current that show this sort of thing, the isopleths separating near the coast. Do you think this trough has anything to do with the countercurrent?

Reid: Certainly. The trough in temperature and salinity means a trough in the density distribution. The integral of the density shows a cyclonic circulation.

Wooster: There are two classical explanations of the trough. One is that you have horizontal advection of water coming in from the south giving these properties, which apparently cause troughs.

Fleming: I do not think you even have to have upwelling. Obviously at the ocean boundaries you have a source of energy for mixing. Temperature structures not unlike what you have drawn on the board might appear in the absence of any permanent flow along the coast, purely by mixing at the ocean boundaries. If you did get this kind of mixing, then you would have some kind of circulation set up. It is a little difficult to understand the characteristics of this circulation that might result from this vertical mixing.

Wooster: Is it not characteristic of upwelling that the deeper isopleths slope downward toward the coast?

Schaefer: You show the countercurrent there. How deep do you put it at its deepest point? What is the greatest depth for this countercurrent off Baja California? Do you put the center of it at 600 meters?

Reid: The basis of computation for these currents is the geopotential above 1,000 meters. The bottom limit of this flow is probably uncertain with the present methods.

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DESCRIPTION OF THE NORTHEASTERN PACIFIC OCEANOGRAPHY

NICHOLAS P. FOFONOFF

Before coming here I went around our building at Nanaimo and gathered a few drawings from people who had worked on various things that had happened in the ocean in 1957, to see if, and how, they differed from previous years. The figures are samples of the type of data that has been collected.

Figure 85 shows the variation of surface temperatures along the coast. Daily observations of temperature and salinity are made at lighthouse stations at various parts of the British Columbia Coast. About fourteen stations are currently sending in these observations. Two of these stations along the outer coast have been selected to contrast temperatures in 1956 and 1957. Both show that 1957 temperatures were higher and that 1956 temperatures were lower than the ten year average.

The surface temperatures at Amphitrite Point on Vancouver Island began to increase in March and at Langara Island, just off the northern end of the Queen Charlotte Islands in April of 1957. Temperatures at both locations continued to be above average throughout the winter of 1957.

The solid curves in figure 85 are monthly mean and the broken curves the mean over the preceding ten years. Figure 86 is based on observations taken at Ocean Station "Papa," located at 50°N, 145°W. Oceanographic observations are taken here in alternate six-week periods throughout the year. These observations, apart from BT's, were started in 1956 and are continuing at present. Twice-daily BT's are available from 1952.

The largest fluctuations of salinity occur in the upper part of the halocline (maximum in autumn, minimum in spring). The salinity has gradually increased in the upper part of the halocline (75-150m) and decreased in the lower part. For example, from August 1956 to January 1958 the salinity at 200 meters decreased by $0.20^{0}/_{00}$. The trend is shown in the T-S curves for the weathership data in figure 86. Each curve is the mean over a six-week period. The seasonal temperature variation is limited to the upper 50 meters. Below this depth, fluctuations are not clearly related to the seasonal cycle. A gradual warming has been observed in the upper part of the halocline. For example, the temperature at 125 meters has increased by 1.5°C. This trend is still continuing. An indication of the temperature change is given in figure 87 in which a comparison is made of the temperature at the surface and 70 meters (200 feet) in

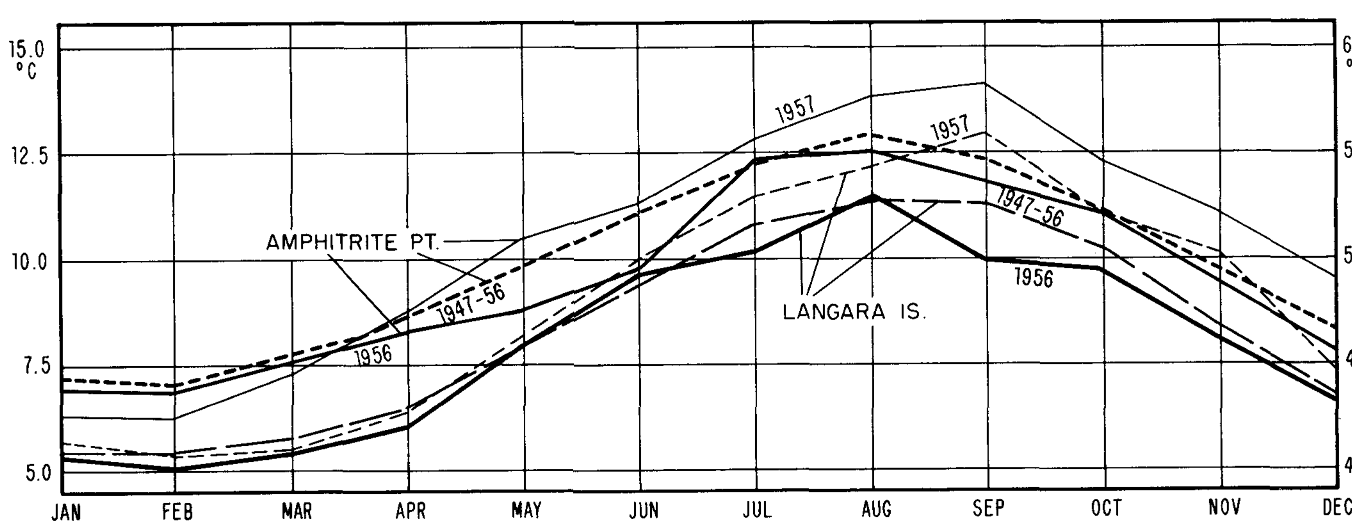

FIGURE 85. Mean monthly seawater temperatures during 1956 and 1957 compared with grand monthly mean at Langara Island and Amphitrite Point. Reference: Observations of seawater temperature and salinity on the Pacific Coast of Canada. MSS Report, *Fish. Res. Bd. Can.* Vol. XVI, 1956, Vol. XVII, 1957.

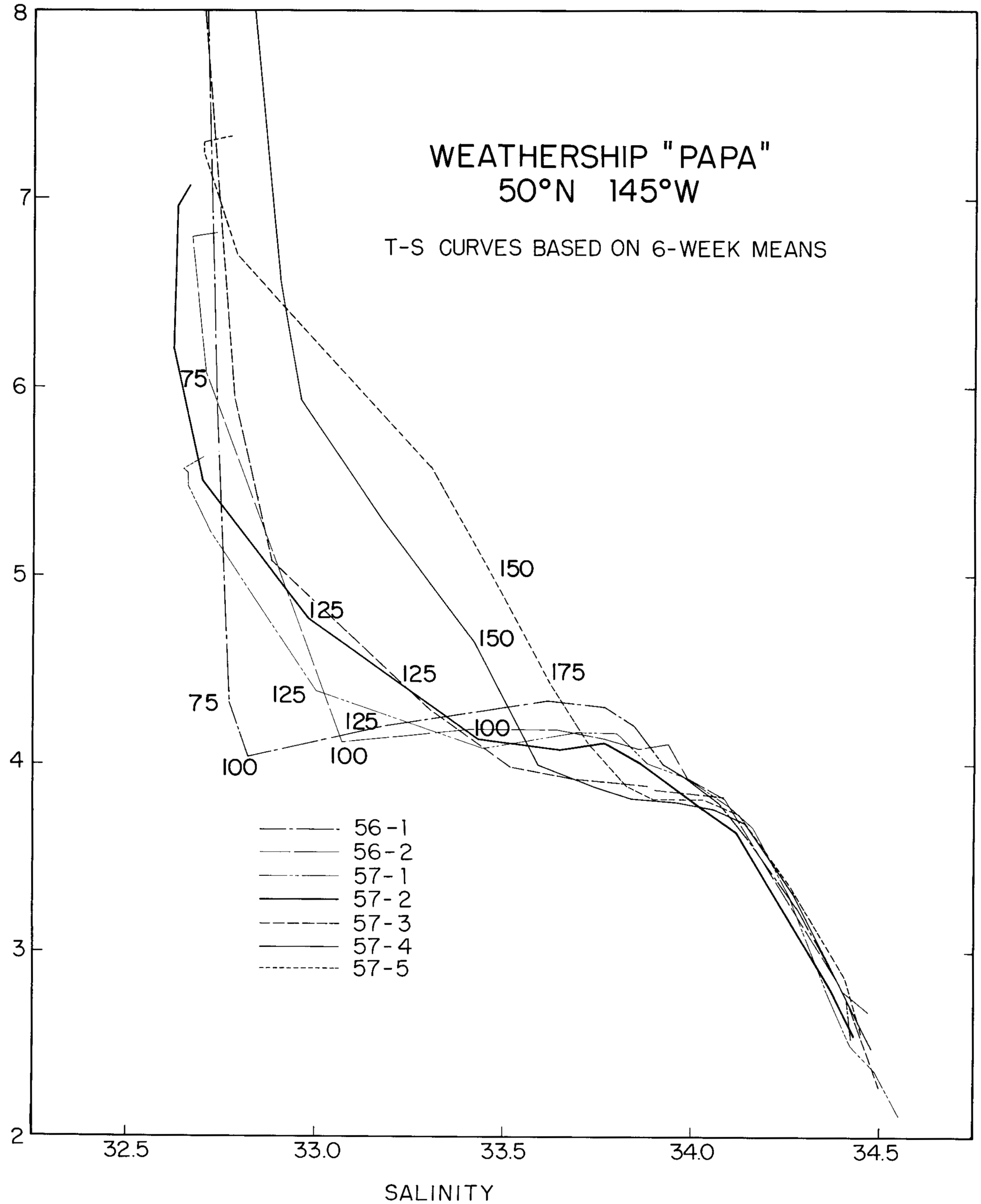

FIGURE 86. Temperature-Salinity relationships based on means of alternate six-week periods at Weathership "Papa" 50°N 145°W, after S. Tabata.

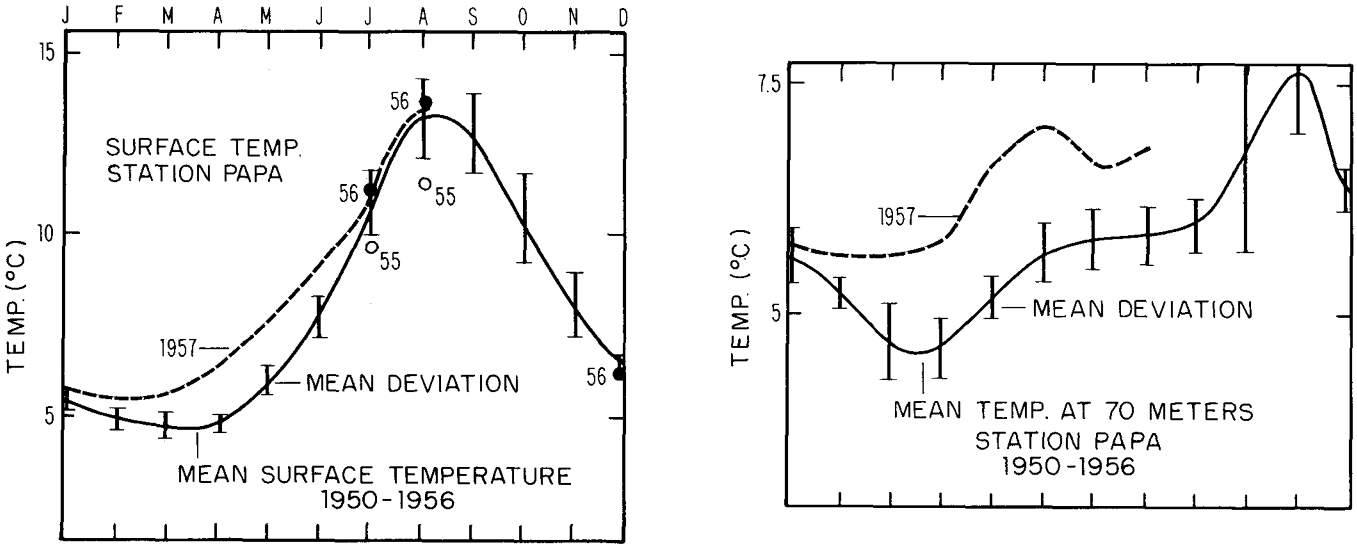

FIGURE 87. Surface and 70 meter temperatures in 1957 compared with the 1950-1956 mean at Weathership "Papa", after S. Tabata.

1957 with the mean for 1950 to 1956. The temperatures in 1957 were substantially higher at 70 meters as compared with the previous seven year period.

The oxygen has been steadily increasing in and below the halocline. An increase of 0.2 mg.at./l has been observed in the depth interval 150 to 300 meters.

Drift bottle studies were initiated in August 1956 and will continue to at least August 1958. Bottles were dropped in lots of 1000 each periodically from the weatherships (Fig. 88) and during NORPAC cruises (Fig. 89). So far, about 25,000 bottles have been released with 816 recoveries to the middle of April 1958. Returns varied from 0 to 16% for individual releases with an overall average of 3.3%. The time at sea varied from four months to one year.

The distribution of returns suggests that shifts in the surface current system occur. The northward component of the current between the weathership and the coast appeared to strengthen after September 1956, weaken in January 1957 and strengthen again in March to July 1957. The stronger northward flow in the summer of 1957 is also suggested by the dynamic topography.

The most southerly releases 40°N to 42°N did not reach the coast (one bottle was picked up on the Hawaiian Islands).

Speeds of drift ranged from 3 to 9 miles per day (6-19 cm/sec) somewhat higher than computed surface geostrophic velocities.

The region near the coast of British Columbia has been surveyed quite intensively in the last two years. The analysis is not completed but there is evidence for marked variations in the northward flow of warm saline water. During 1957 the volume of northward flow appeared to be larger although the temperatures were not significantly higher.

COMMENTS ON THE WIND-DRIVEN OCEAN CIRCULATION

In the interior of the ocean away from the direct influence of coastal boundaries, the steady-state currents are assumed to satisfy a simple set of equations of the form:

$$-\rho f v = -\frac{\partial p}{\partial x} + \frac{\partial \tau_x}{\partial z} \qquad (1)$$

$$\rho f u = -\frac{\partial p}{\partial y} + \frac{\partial \tau_y}{\partial z} \qquad (2)$$

$$\frac{\partial \rho u}{\partial x} = \frac{\partial \rho v}{\partial y} + \frac{\partial \rho w}{\partial z} = 0 \qquad (3)$$

We can split up the velocity vector **V** into two parts so that

$$\mathbf{V} = \mathbf{V}_g + \mathbf{V}_w \qquad (4)$$

where $\mathbf{V}_g$ is geostrophic velocity and $\mathbf{V}_w$ is wind drift. The equations then become

$$\left.\begin{aligned} -f\mathbf{V}_g &= -\frac{\partial p}{\partial x} \\ fu_g &= -\frac{\partial p}{\partial y} \end{aligned}\right\} \text{Geostrophic "observed"} \qquad (5)$$

$$\left.\begin{aligned} -f\mathbf{V}_w &= \frac{\partial \tau_x}{\partial z} \\ fu_w &= \frac{\partial \tau}{\partial z} \end{aligned}\right\} \text{Ekman Spiral Equations "Shallow nongeostrophic currents"} \qquad (6)$$

and

$$div\ \rho\mathbf{V}_g + div\ \rho\mathbf{V}_w + \frac{\partial \rho w}{\partial z} = 0 \qquad (7)$$

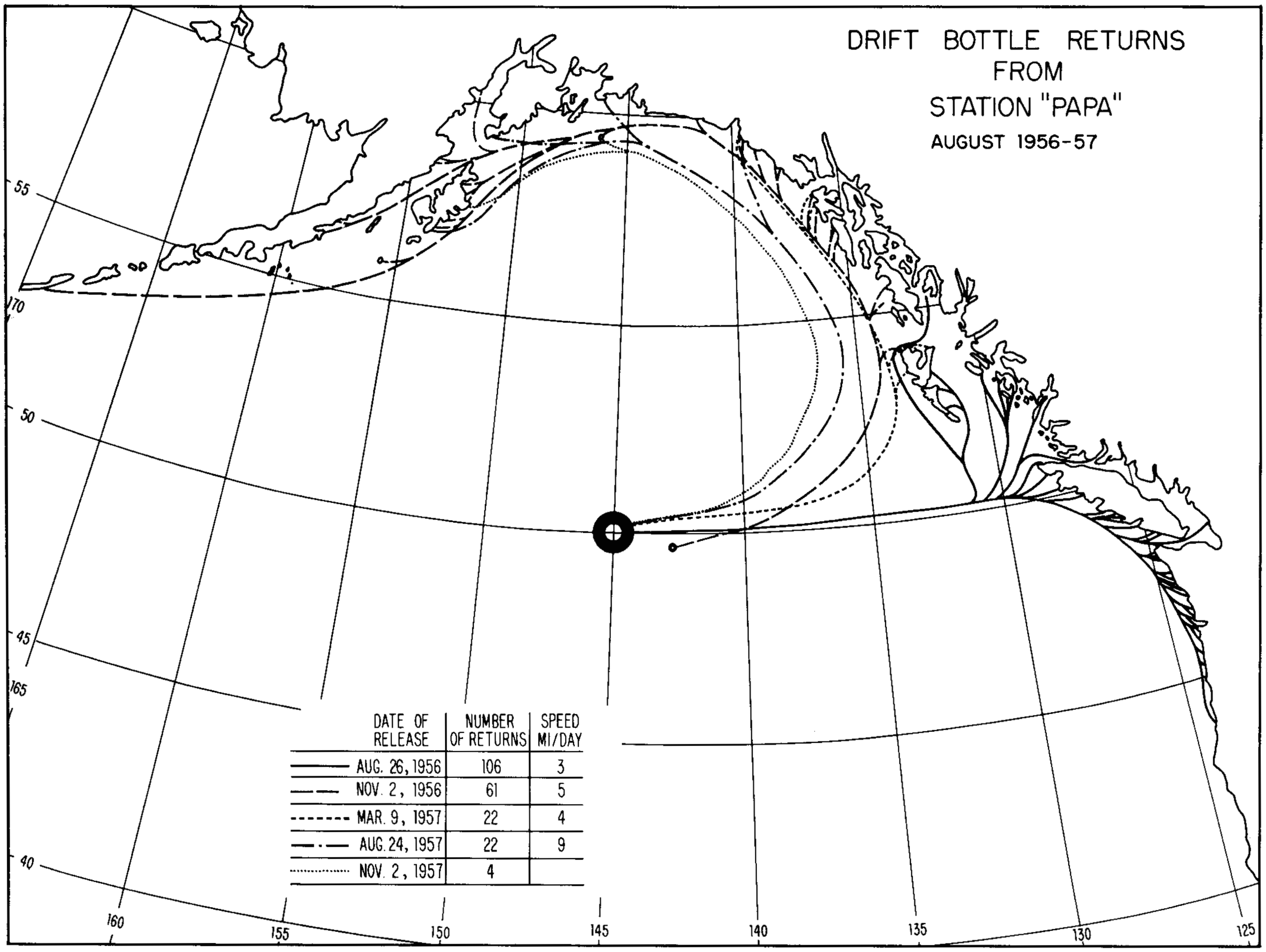

FIGURE 88. Drift bottle returns from Weathership "Papa." Reference: A. J. Dodimead and H. J. Hollister 1958: Progress Report of drift bottles releases in the Northeast Pacific Ocean. *Jour. Fish. Res. Bd. Can.* 15 (5), pp. 851-865.

Where div $\rho\mathbf{V}$ refers to divergence of horizontal components only.

Taking the curl of the momentum equations yields

$$f\ div\ \rho\mathbf{V}_g + \beta\mathbf{V}_g = 0 \qquad (8)$$

$$f\ div\ \rho\mathbf{V}_w + \beta\mathbf{V}_w = \frac{\partial}{\partial z}\ \mathrm{curl}_z\ \tau \qquad (9)$$

The wind drift according to the Ekman Theory is confined to the upper layers ($\sim$ upper 100 meters) whereas the geostrophic currents can extend much deeper. It is therefore evident from (8) that if the geostrophic current has a meridional component there will be a horizontal divergence over most of the depth as required to conserve absolute vorticity. The surface wind stress would add or remove vorticity from the wind drift currents only.

Equations (8) and (9) can be added and integrated vertically to yield the familiar result

$$-f\ (w_s - w_b) + \beta \int v dz + \mathrm{curl}_z\ \tau\ , \qquad (10)$$

which, if we neglect the vertical motion w, is the result obtained by Munk (1950). If the $\mathrm{curl}_z\ \tau$ is zero, the total transport is zero. This is interpreted to mean that the transport of the wind drift is equal and opposite to the geostrophic transport. However, since the geostrophic currents can extend to much greater depths than the wind drift currents, the resulting circulation can be quite effective in transporting heat and salt.

The wind drift is always directed to the right of the wind in the Northern Hemisphere, but the geostrophic

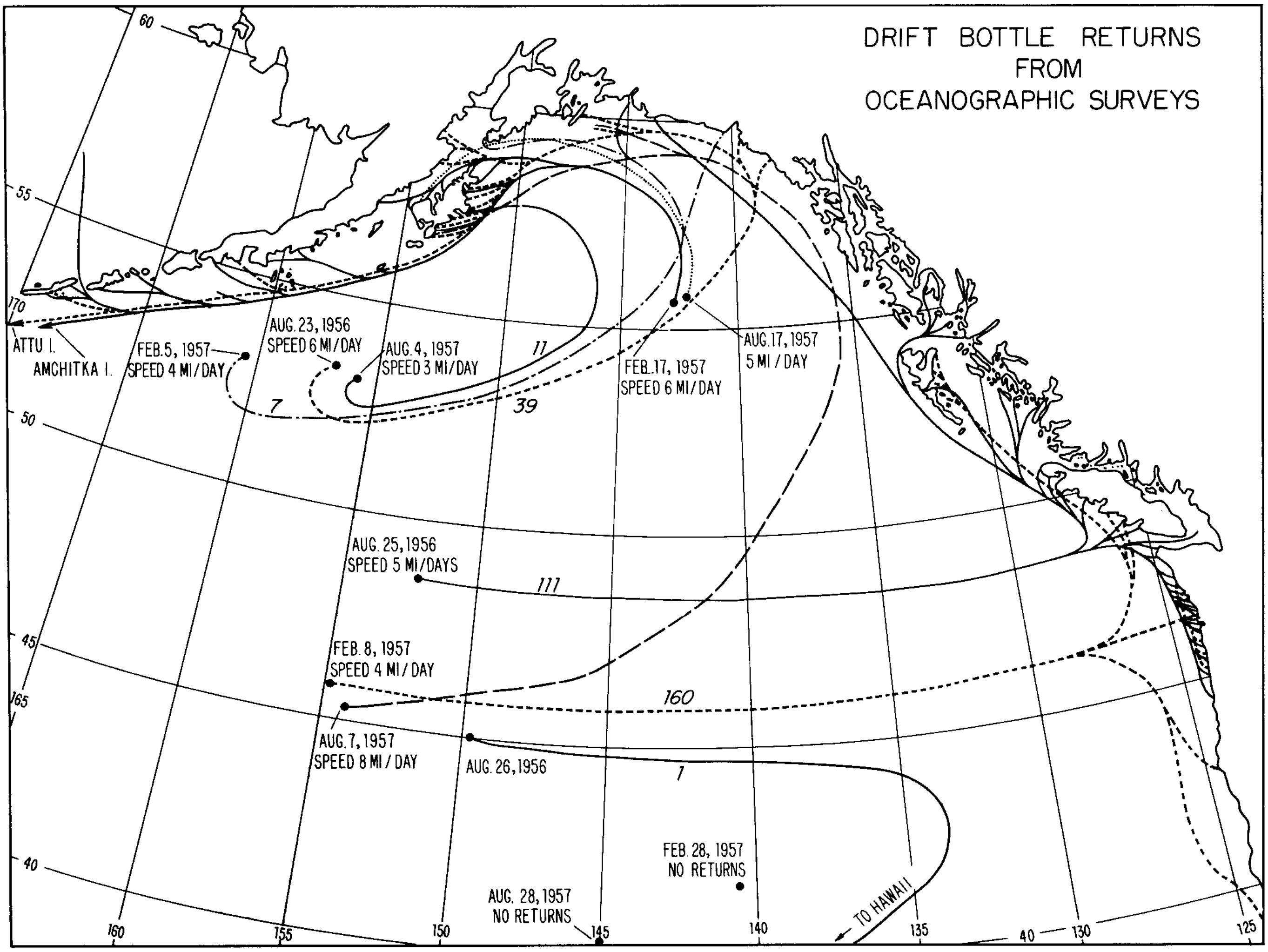

FIGURE 89. Drift bottle returns from NORPAC Cruises. Reference: A. J. Dodimead and H. J. Hollister 1958: Progress Report of drift bottles releases in the Northeast Pacific Ocean. *Jour. Fish. Res. Bd. Can.* 15 (5), pp. 851-865.

current can be to the right, left or against the wind depending on the stress distribution over the surface. Hence, it is often difficult to correlate dynamic topography to the wind system. Conversely, the dynamic topography would not be an entirely reliable guide to surface currents if the geostrophic velocities are of the same magnitude or smaller than the wind drift currents.

DEEP CURRENTS

Munk's analysis of wind-driven ocean circulation depends critically on the assumption of zero pressure gradient in the lower layers. We see from equation (10), neglecting w_s at the surface, that

$$fw_b + \beta \int v dz = \text{curl}_z \tau \qquad (11)$$

If $\text{curl}_z \tau$ is of the order of 10^{-8} dynes/cm^3, a vertical velocity of 10^{-4} cm/sec would be enough to seriously influence the conclusions of Munk's theory. The vertical velocities required to satisfy vorticity conservation are of this order of magnitude.

The divergence in each layer of the geostrophic flow will produce a pressure gradient in the layers beneath it. Consequently, the geostrophic flow cannot be uniform and the current vector must veer to the right or left with depth depending on the motion in the deep water. It is, in fact, possible to construct a deep current system which exactly compensates the divergence of the upper geostrophic currents. Such a current system could then persist for a long time in the absence of surface winds.

COASTAL WATER TEMPERATURE AND SEA LEVEL—CALIFORNIA TO ALASKA

H. B. STEWART, JR.

My attendance at these meetings came up only recently, so the data I brought were quickly grabbed from the files plus a bit we did work up specifically for this meeting. As you know, the Coast and Geodetic Survey for years has been collecting tidal data along the coasts of the United States. The files are full of answers. The problem is now to find the questions for which we have the answers. This question is one to which we may hope to contribute at least part of the answer. Inquiries have for the last several months been coming to the Coast Survey as to why these West Coast temperatures are higher than normal, so we got interested in the story too. First we looked to see if they were warmer; then we looked at sea level, about which we also had had inquiries.

In brief, I will try to summarize the results of studies in which we took the fifteen months prior to April of 1958 and worked out monthly sea level values and monthly temperature values. We picked out ten tide stations that are the most exposed, from La Jolla, California, to Ketchikan, Alaska. The seawater temperature data here are monthly means calculated from daily observations. They are made once each weekday by bucket thermometer at our tide stations. We feel that over a period of time the tidal influence on temperatures is ruled out. These daily values are averaged to give us a monthly mean temperature at each tide station. To obtain monthly sea level value, we use a mean of hourly tidal heights during that month. This is the standard procedure for all these stations. If you look at the charts (Figs. 90-99), the data have been plotted as temperature and sea level anomalies at each of these stations. The anomaly is considered as this: temperature anomaly is the mean temperature during each month, January 1957 through March of 1958, compared with the mean of that month for the period of record. February 1958, for example, is compared with all the Februarys on record. The sea level anomaly is the sea level for the month we are considering compared to the nineteen year mean for that month, usually 1938-1956. For each month we are dealing only with the anomaly; we are not dealing with the monthly values themselves. Essentially we have cleared out the seasonal cycle by using this method.

On this coast during the period averaged, the highest sea level we have is February 1958 at Crescent City at .85 ft. above the long-term mean for that month. The observed tide ran about a foot above the predicted. These values on the graphs are not compared with the predicted but with the mean of the long-term observed data. When the anomalies for all the stations were averaged, we got a picture of the coast as a whole (Fig. 100). The upper graph is the temperature anomaly. The lower one is a sea level anomaly. In summarizing these two curves, one is struck by the parallelism between the temperature and the sea level anomalies. For the first four months of 1957, both the sea water temperature anomaly and the sea level anomaly were generally below normal. Both started to rise in May; were high during June, July, and August. Both dropped off during September, but were still well above normal. They then started to rise in October and November. Both reached the highest of the year in December. January and February of 1958 were both spectacularly high. The average water temperature anomaly in February was plus 3.5°F. The sea level anomaly in the same month averaged about half a foot—a little bit above half a foot actually above the long-term value. The peak for both was in February. Again, this is an average of the ten stations along the length of the coast. The coastal average is made up of data from La Jolla, Los Angeles, Santa Monica, Port Hueneme, Avila, San Francisco, Crescent City, Neah Bay, Ketchikan, and Sitka.

The anomalies I have been discussing have all been listed in tabular form. Some hypothesizing has been done and this is published as a Coast Survey Technical Bulletin. It covers the period from January of 1957 through March 1958. Water temperature and sea level data for these ten stations along the coast together with the anomalies and the long-period means are given in graphical form (Figs. 90-101). One particularly interesting thing that shows up in the average of all stations is a parallelism between the temperature anomaly and the sea level anomaly. You will note on the plots of the individual stations that the correspondence between temperature anomaly and sea level anomaly becomes poorer as we go father north. We automatically looked to the winds, knowing this to be a coast where upwelling existed. We got from Jerome Namias the monthly sea level pressure charts and worked out the geostrophic winds for three positions off the coast, at 35°N, 45°N, and 55°N. I will not go over what the wind pattern shows, as Mr. Namias discussed that in detail yesterday. In working with these geostrophic winds, we found the same thing that he found, that January and February of 1958 were indeed anomalous months. The correspondence of the temperature and sea level anomalies with the winds seems to be pretty good. However, the thing that interested us, was the increasingly poor correspondence between temperature and sea level anomalies as we went farther north. One reason for this undoubtedly is the variation in the wind pattern during January and February 1958 that Namias talked about before. Another reason is that from south to north, there is normally a decrease of the surface

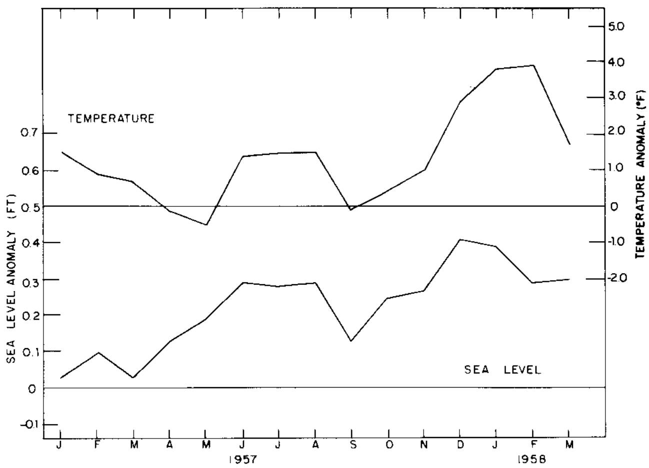

FIGURE 90. La Jolla, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

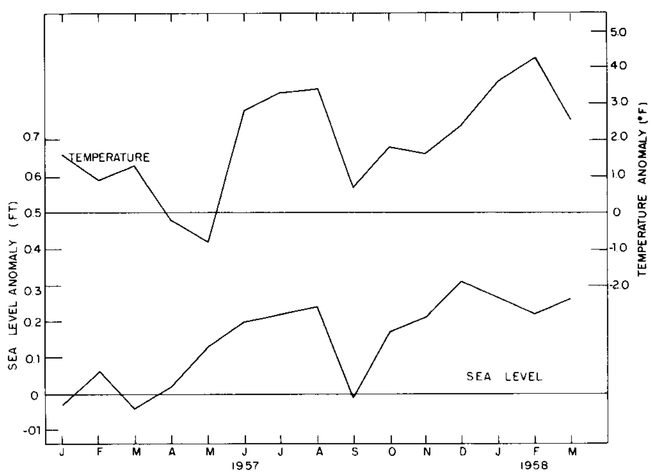

FIGURE 91. Los Angeles, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

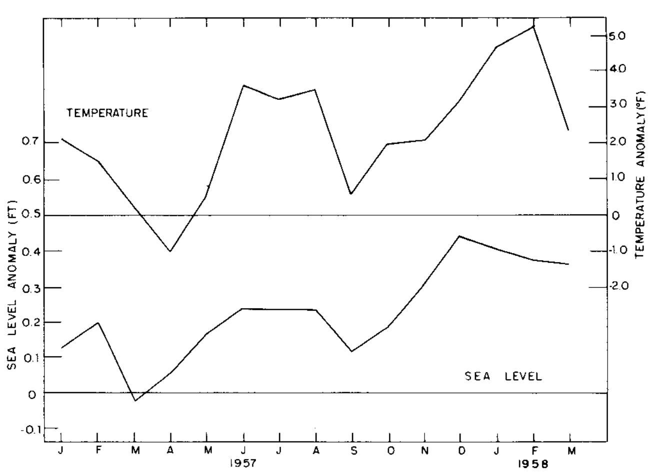

FIGURE 92. Santa Monica, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

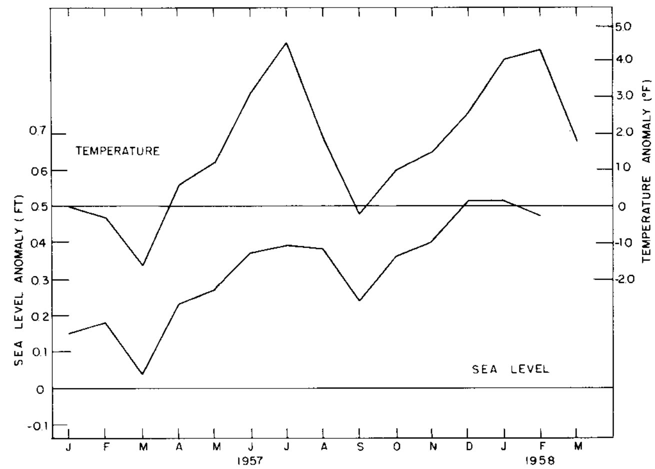

FIGURE 93. Port Hueneme, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

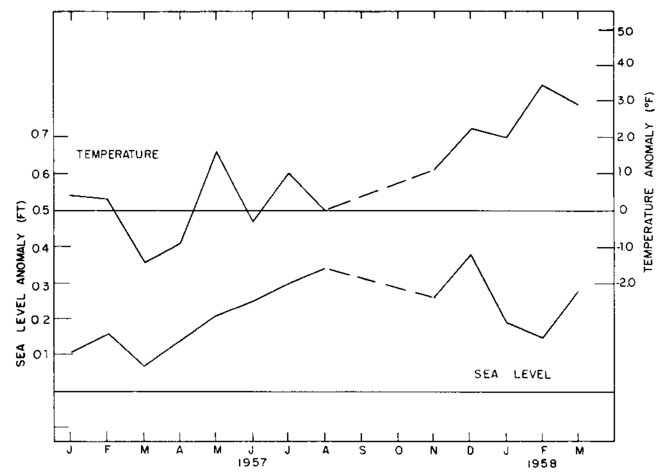

FIGURE 94. Avila Beach, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

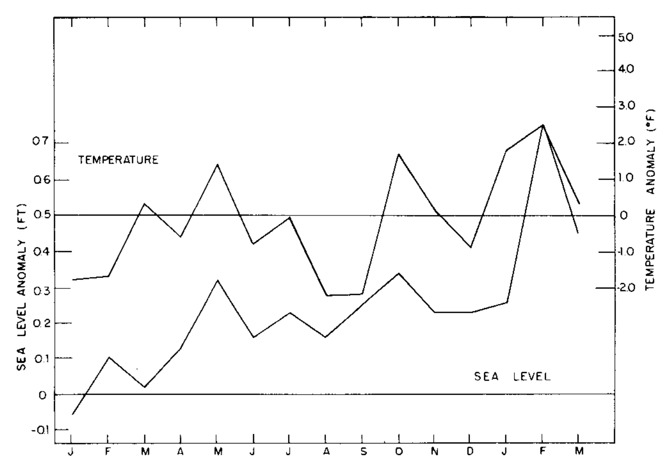

FIGURE 95. San Francisco, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

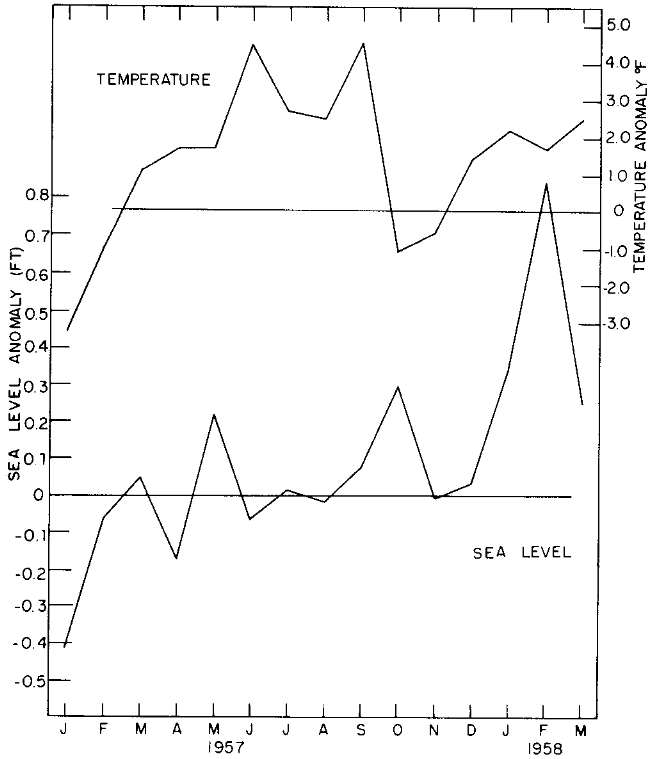

FIGURE 96. Crescent City, California, sea water temperatures and sea level anomalies, January 1957-March 1958.

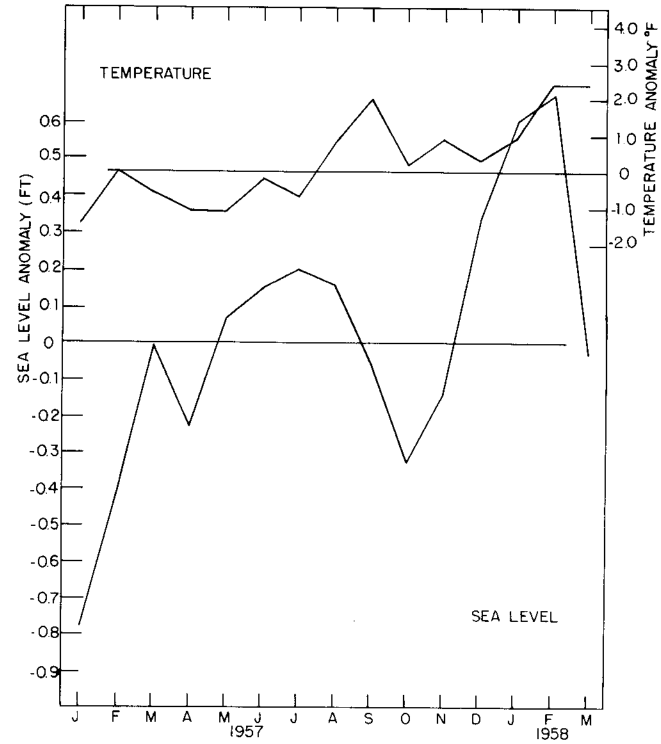

FIGURE 98. Ketchikan, Alaska, sea water temperatures and sea level anomalies, January 1957-March 1958.

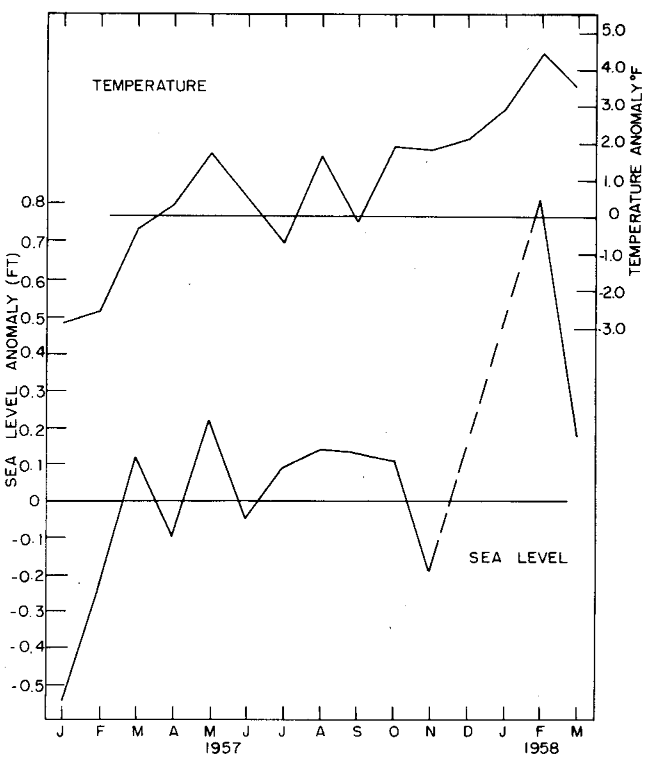

FIGURE 97. Neah Bay, Washington, sea water temperatures and sea level anomalies, January 1957-March 1958.

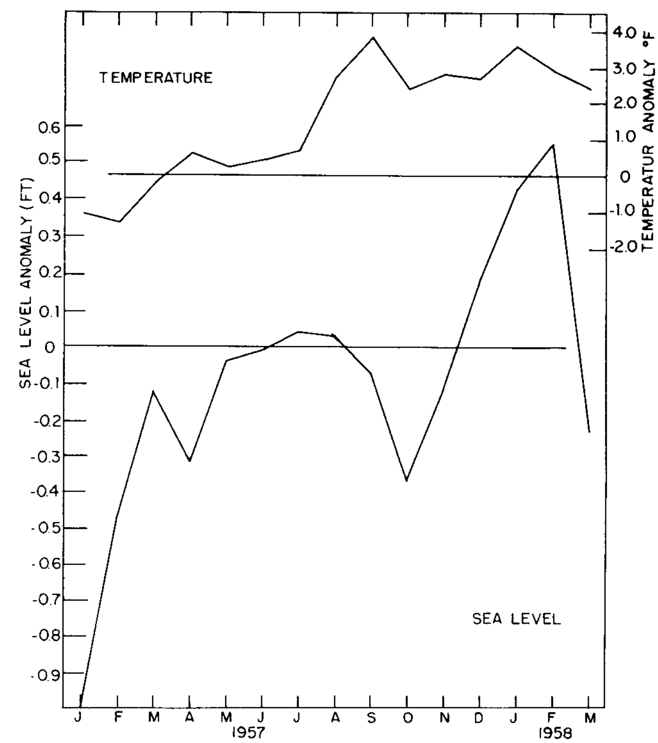

FIGURE 99. Sitka, Alaska, sea water temperatures and sea level anomalies, January 1957-March 1958.

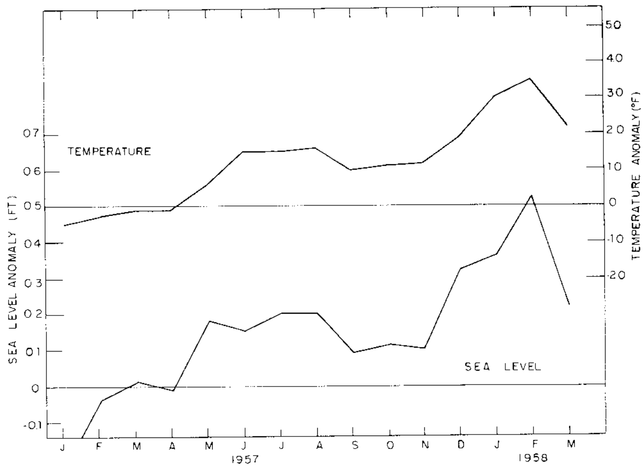

FIGURE 100. Mean coastal water temperature and sea level anomaly, La Jolla to Sitka, January 1957 through March 1958.

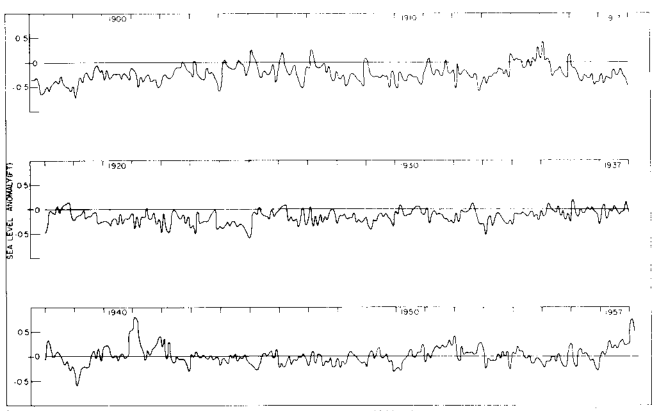

FIGURE 101. San Francisco (Presidio) California, monthly sea level anomaly August 1897 through March 1958 referred to 19-year monthly means 1938-1956.

water temperature from an annual mean of 64°F. at Los Angeles to 49°F. at Sitka. The temperature response to upwelling depends upon the difference between surface water temperature and the temperature of the upwelled water. Assume a constant temperature for water at depth, then the temperature response to upwelling will be less farther north. The range of the temperature anomaly is about the same from south to north, but the range of the sea level anomaly increases going from south to north, so that by the time we get to Sitka, the difference in sea level anomaly between January 1957 and January 1958 is nearly 1.5 feet. I envisage both the temperature and the sea level changes as resulting from the changes in the coastal wind pattern. As Stommel brought out, we are in fact dealing with two different problems—we must think of this facet I am discussing only in terms of the coastal problem for I have only coastal data. We have nothing farther out than the length of our longest pier at our tide stations.

This is strictly the coastal effect, and I think of the winds as producing both the temperature and sea level effect in this manner. The temperature effect is the well-known, well documented, colder water with upwelling. The sea level effect I picture as an equilibrium condition with your strong northerly components keeping the upwelling mechanism operating and keeping sea level at a reduced or lower level. Thus when the northerly components of the winds diminish, the upwelling mechanism slows down so the colder water is not pushed away from the coast and sea level tends to rise to establish a new equilibrium with the wind. There are other things, of course, which will give you a tie-in between sea level and water temperature. One thing, for example, is the atmospheric or barometric tide. If you tend to think of the sea as an inverted barometer, then the same atmospheric pressure changes that change the wind pattern will also affect sea level. When June Pattullo, et al. (1955), did their work, their global treatment—the data off the West Coast, show that when they corrected the recorded sea level values for atmospheric pressure, the change was small, and in addition the corrected values were out of phase with the recorded values. This suggests that at least out here, in what she called the California Current area, atmospheric pressure changes were apparently not important as they were in Bay of Bengal and some other parts of the world. Personally, I feel that this whole problem of the interrelationships of sea level, water temperature, barometric pressure, and winds, needs a lot of good basic research. It is as complex as it is interesting. The IGY island observations will provide some important data on this, and the whole problem must be attacked, I feel, primarily by using the actual observed data, of which we need much more.

DISCUSSION

Munk: We were worrying about the problem of the direct barometric pressure effect last night. If you have a pressure anomaly like the one you investigated, sea level responds as an inverted barometer in a few days. Joe Reid's study indicated the steric height change amounted to about 7 cm. You recorded about 15 cm. The difference of roughly 10 cm. is perhaps due to the inverted barometer effect. This would require a pressure anomaly of minus 10 millibars.

What strikes me is, Stewart mentioned that when you get further north the ratio between surface temperature and sea level was not as large. I think that you would get a better agreement if you actually made a straightforward inverted barometer calculation. The rise in sea level above and beyond the inverted barometer effect may correlate far better with the temperature.

Stewart: I find it hard to believe, Dr. Munk, that this would not be the case, particularly when you have high sea level anomalies with the amazingly high anomalies in pressure. I am sure that there is a bearing there, however at the same time, large pressure anomalies like this will also produce large wind anomalies. I think this is part of it too. We have to be able to sort these things out—which is the inverted barometer effect, and which is direct wind effect?

Munk: I think you are right. I think the pressure is more important than wind. I base this on the fact that for the annual pressure term you get very nearly the same result over enormous regions (like all of Northern Europe), with stations located in different aspects to the prevailing winds.

Stewart: Are you able to remove the annual pressure term from sea levels?

Munk: Yes, but it is relatively unimportant. To the case you have discussed, the yielding under superficial atmospheric pressure may amount to half the recorded change. For the seasonal variation the pressure effect is of the order of 10 percent of the recorded variation.

Stewart: I will be interested to see, as we get into this more, the relationship or relative importance of the inverted barometer effect and the wind effect. I naturally champion wind at this point of the game. The work that Miller did at Atlantic City and DeVeaux's work at Charleston both show a definite sea level response to variations in wind direction and velocity.

Munk: The wind tide is very critically dependent on the slope bottom. For that reason the East Coast with its extremely gradual continental shelf is sensitive to the wind effect, but off the American West Coast this is not so.

Stewart: Do you feel that the temporary response that is observed is due then to the redistribution of mass? Would not this tend to give you, with upwelling, south flowing geostrophic currents coming from this redistribution of mass and bringing cold water down from the north?

Munk: May I introduce a useful nomenclature? Call a barotropic fluctuation one that is due to a variation in water mass in a unit column. The induced pressure changes are uniform from top to bottom, and so are the related horizontal gradients and geostrophic currents. A *baroclinic* variation is due, in part, to variations in specific volume; as an extreme case of baroclinicity we have an *isostatic* condition for which the mass per unit volume remains unchanged. The seasonal variation in low and moderate latitude appears to be isostatic. A pressure recorder at the sea bottom would record no change. Geostrophic currents are then limited to those upper layers in which the specific volume is altered.

Stewart: I would like to see at some time all of our tide stations up and down the coast have associated with them a pressure recorder on the bottom.

Fleming: I think we are oversimplifying a lot of these points. Actually the relationship between sea level and surface temperature are fairly fortuitous. Please do not overemphasize these correlations because you could have the opposite under different physical setups. You could have the inverse relationships.

Stewart: If we consider all the stations, the increase in sea level anomaly average 0.3 foot per degree rise in the temperature anomaly, but this is surface data only.

Fleming: Does this have a physical meaning?

Stewart: I think it possibly does. In terms of the sea level anomaly it might be accounted for sterically, but we have no information on how far down the temperature increase extends. Actually, I do not believe the sea level rise is primarily steric, but the range of the temperature anomaly during these eighteen months appears to be just about the same from south to north, whereas the sea level anomaly increased from south to north.

Namias: There is a high correlation between monthly mean temperatures in the lower troposphere and surface water temperatures over this area the past two years.

Stewart: Apropos of the air temperature, I noticed one thing that is especially interesting. Working with air temperature data from three weather stations near three of our tide stations, we found that 33 out of 42 station months had higher water temperatures than air temperatures. The air temperatures even though anomalously high, could not have been the prime cause for the higher water temperatures. The converse appears more likely.

Fleming: A few cases where we had anomally high sea levels in the Puget Sound we find that primarily this was a barometric response rather than a wind effect. This again is a local situation. The water in the Puget Sound may respond more to barometric pressure changes than that of the open ocean.

Stewart: There is another thing that might produce the higher sea levels in the north. If you look at it on a globe, Sitka and Ketchikan, which show the greatest increase in sea level, are at the head of what might be considered a large embayment where it would be expected that the wind setup would be greater. I do not know the whole problem of what is causing this variation in sea levels. It is something we will have to attack more completely. At some places, like South Carolina, it may be primarily direct wind effect; at other places it may be primarily barometric, and at still other places it may be primarily steric. In some places the three may be so combined that it is impossible to separate them. It is the sort of problem that needs lots of data to work with, and the Coast Survey has long series of good data that can be applied to this.

Fleming: I would like to add the hydrostatic effect. Accumulation of fresh water from the Columbia River for example. This is going to be a factor all the way from the Columbia up into Alaska—fresh water discharged into the ocean.

Stewart: Apropos of exactly that, in connection with our present surveys of New York Harbor, we compared the Geological Survey's river gaging records with our tide gage records trying to see if we got an increase in harbor level with an increase in the river flow. By plotting up the curves for river discharge and harbor level, we found that the curves were very similar. However, peaks in the harbor level came a day or two *before* the same peak in the measured river flow at the Green Island gaging station in the Hudson River. So I think what was happening was that the same storm that dumped the water upon the watershed also caused a wind setup in the harbor so that we got the effect of higher sea level in the harbor before the water actually came down.

Munk: Gordon Groves really did a very complete study of sea level. Day by day values of sea levels for three months were used, with the tides removed by numerical filtering. With each storm the sea level fluctuated more or less according to the inverted barometric rule; perhaps two-thirds of the fluctuations could be accounted for in this simple manner. The foregoing remarks apply to the frequency of major storms: one to two cycles (or cyclones) per week. Miller, Groves and I now have a "sub-diem" project under way to nail down by means of cross-spectral analysis the frequency dependence of the inverted barometric responses in the frequency range from 1 to 200 cycles per year.

Stewart: There is one other facet on this that I feel I should touch on just briefly—the long period aspect. The figure 91 for Los Angeles and figure 92 for Santa Monica, point this up. Los Angeles and Santa Monica are perhaps twenty miles apart, yet the anomaly at Santa Monica both temperature-wise and sea level-wise is greater than at Los Angeles, although the curves are very similar. The reason for this, I believe, is that the Santa Monica anomalies are referred to the period 1947-1956, which was a period of generally lower sea levels and lower water temperatures. Los Angeles on the other hand, has a much longer period to which the recent anomalies are referred. Consequently, the anomaly at Santa Monica appears greater in these warm years (1957-1958) than at Los Angeles where we have a much longer period of record. Figure 101 is a plot of the monthly anomalies of sea level at San Francisco from August, 1897, up through March 1958. Notice that the years 1940 and 1941 which we have mentioned before as being anomalous years temperature-wise, are also anomalous years with respect to sea level. The horizontal line represents the 1938-1956 monthly means, so the deviation of the curve above or below that line represents the deviation each month from the 1938 to 1956 value for that month. The annual cycle has essentially been removed. The reason that they are below the line most of the time is that there has been over the past fifty years an average increase in sea level along this coast at the rate of about .005 foot per year. Note in December of 1940 and in early 1941 the anomaly comes way up to a maximum of nearly a foot. In February of 1941 sea level was 0.8 foot higher than the long-term February mean. It was quite high throughout most of 1941; very comparable to the situation in 1958. 1957 is considerably higher—note how it jumps up in January 1958! This plot is well worth pursuing at your leisure and comparing with some of the long-period data available from the other sources here. I think that with a plot like that and with comparable temperature data and with weather data, it would be well worth while looking for cycles in this thing. Maybe in the end it will be up to the weather people actually to give us the mechanism whereby we can predict when we will have lean years and fat years in the fishery industry.

Munk: Haubrich and I have obtained the power spectrum from mean monthly sea levels for all stations (one dozen) having more than a century of record. For frequencies lower than the annual frequency the spectrum is a typically noisy (or continuous) spectrum, with no significant frequency "lines", not even well developed broad bands. It is then predictable only in the sense the stock market is predictable.

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EFFECTS OF ABNORMAL WIND TORQUE ON THE CIRCULATION OF A BAROTROPIC MODEL OF THE NORTH PACIFIC OCEAN

W. S. von ARX
(Read by Fritz Fuglister)

In running experimental models of the wind driven ocean circulations of the Northern and Southern Hemispheres it was always difficult to adjust conditions so that the zonal wind profiles acting on the models resembled those in nature. Often the wind torques would be too strong or too weak at first or have the subtropical inflection point at something other than the correct latitude. In the course of adjusting these variables toward the normal mean, it was noted that the patterns of water motion in the main sub-tropical gyres changed systematically with the wind speed and distribution to the extent that the correct wind profile could be approximated by a simple inspection of the pattern of motions. It remained then to make the finer adjustments by measuring the winds at a number of latitudes and adjusting the wind speeds so that the Rossby number of the water motion, particularly in the zone of westward intensification, was the same as that in nature.

Unfortunately these preliminary adjustments were not recorded photographically and my impression of the changes of circulation with aberrant winds is drawn almost entirely from memory. But insofar as this fallible faculty may serve to support them, these are the facts:

When the Rossby number of the water motion was too high by a factor near 1.5 or 2 the circulation not only increased but crowded westward in an unrealistic way, particularly in the Pacific compartment of the Northern Hemisphere models. Under these conditions the meandering of the Kuroshio became more congested, the Kuroshio extension migrated southward a few degrees, the West Wind Drift broadened and turned equatorward earlier than normal, following a path parallel to the North American coast but situated about midway between the coast and the Hawaiian Islands. In a sense, this is the same as the California Current having broadened and moved away from the coast (Fig. 102).

In the former place of the California Current, the coastal water between the latitudes of the Gulf of Tehuantepec and the Queen Charlotte Islands moved slowly poleward. Due to the sheltering effects of the model continents in the trade wind zone a poleward component of wind developed and caused some coastal sinking to occur in these latitudes. Where the westerlies struck the rubber model of North America and were deflected southward some upwelling developed. The result of this was to produce a slow cyclonic motion in an elongated triangular path between the coast and the flow in the displaced California Current.

This event caused the normally cyclonic circulation in the Gulf of Alaska to become more rounded and to

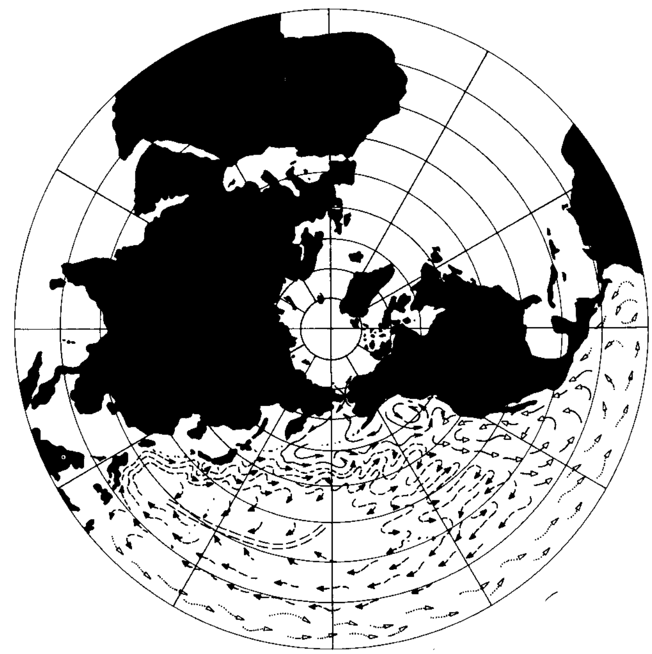

FIGURE 102. A vision of the barotropic motions accompanying high wind torques over a rotating model of the North Pacific.

extend a few degrees farther south. However, the circulation in the Gulf of Panama was practically unchanged, presumably because the Equatorial Countercurrent feeding water into the Gulf of Panama made up the water lost to the reversed flow in the former path of the California Current, and was itself resupplied by a branch of the southerly flow taking place in the displaced California Current.

These changes in the circulation of the North Pacific compartment are peculiar to an abnormally high Rossby number. Reduction of the Rossby number below normal broadens the Kuroshio, reduces its tendency to meander and causes it to turn eastward somewhat farther north than usual, but does not cause a conspicuous change of the circulation in the Gulf of Alaska or along the West Coast of North America.

In considering these experimental effects it should be remembered that they are purely barotropic processes in an Ekman-type ocean. In rotating models of this kind, the time of reorganization of the circulation to an abrupt change of the pattern of wind stresses is about 12 model days. Quite grotesque circulations can result in this short time if the wind pattern is sufficiently garbled by plan or accident. The barotropic readjustment of the model to seasonal changes is so rapid in comparison with a season's duration that unenlightening confusion soon reigns in the pattern of dyes in each of the major water masses. For this reason the experimental winds have been applied without seasonal variations.

These remarks, being made without knowledge of the facts of observation revealed at this Symposium or the sense of the discussion, may be quite useless. But if there is any resemblance between the abnormal circulations I have described from the model experiments and the present pattern of motions in the North Pacific measured by dynamic sections, I think these must result from a prolonged change of climate and winds in order to have time enough to produce a baroclinic reorganization of the water structure in depth. If the resemblance is limited to superficial changes in the water motion and structure the barotropic mode seems more probable.

DISCUSSION

Question: What is a Rossby number?

Stommel: I think it is the ratio of the characteristic water velocity to the velocity of a point on the Equator due to the Earth's rotation.

Sette: Is the baroclinic ocean essentially a two-layer ocean and how does its motion differ from that of a barotropic ocean?

Stommel: There are two kinds of motion. Even though the real ocean is stratified density-wise, you can conceivably set up a motion, like a tidal current, that has a velocity independent of depth. We call this a barotropic motion. Rossby, a long time ago, pointed out that for most short period changes in wind-stress, of the order of a week or a month, the ocean should respond barotropically, and very little change should be observed in the density structure deep in the ocean. Therefore, von Arx's model may be expected to exhibit some features of the real ocean's response to short period transients in applied wind stress. The theories so far, deal with periodic changes. But I do not think von Arx has made any experiments with periodic wind field yet.

Revelle: I was most struck by the fact (maybe I did not understand it), that von Arx actually gets a situation with an intensification of the wind such as we think we have had this last year, with a slackening of the wind. Increased wind torque shoves the California Current offshore, which we would expect, but the countercurrent looks as though it were a phenomena of low winds rather than high winds.

Sette: I think there is a difficulty that arises here of keeping the parallelism between the model and the ocean, because according to my idea of what has been happening, there was a general decrease of current offshore.

Revelle: That is right, and that is why I said that I did not quite understand. In his model he has a wind that is uniform with longitude.

Stommel: von Arx would probably have given quite a different talk if he had been here. Evidently he had the impression that a large part of this meeting might be taken up with the dynamic topography of a large section of the ocean, a mean picture of the circulation on a large scale. So I think his paper was intended as a contribution to that kind of discussion, and it actually turned out that we did not really discuss dynamic topography on such a scale. One of the interesting differences between the circulation revealed in the model, and that according to the Sverdrup theoretical model is that in the latter, the north-south transport, southward in this case, should be distributed evenly along any latitude circle over the entire ocean. However there are considerable indications that most of the north-south transport in the model occurs on the eastern side of the Pacific, and there is a large region to the west in the gyre where there does not seem to be any north-south transport at all. von Arx's model looks more like a chart of the actual Pacific, but rather different from that Sverdrup would have predicted from his model.

Revelle: It is my impression from working at different levels with drogues off the Baja California Coast, that the thermocline is something like a surface of separation. Above it the top layer sloshes around under the actual wind, just the way one would expect water to behave in a bathtub, but once you get beneath the thermocline, there is no immediate effect; everything in and below the thermocline is more stable and conservative. A long time would be required to set up a circulation beneath the thermocline.

Hubbs: von Arx has the main current near the coast going north as the countercurrent. This would be just wonderful from the standpoint of distribution of some tropical animals of which we have obtained only adults in the north. The model would be much more consistent with our observed distribution of fish than is the conventional one, in which the California Current coming south swings to the right and partly constitutes the North Equatorial Current. Instead of swinging westward into the Equatorial Current, the current in von Arx's model swings eastward into Middle America and then back northward. I do not know of any measurements which would show the countercurrent acting quite as beautifully as that, except possibly a deep current. The Davidson Current as recognized, does not cover the area I mention. It is not located in the tropics where we get the young stages of the fishes that are found up north as adults.

Revelle: Do you know if von Arx's system models the longitudinal dependence of the wind system?

Stommel: There is a rather puzzling discrepancy between von Arx's model, the real Pacific, and the explanation by Sverdrup's method, which I would like to mention. In both von Arx's model and in the Pacific, the southward flow is stronger in the eastern half of the North Pacific subtropical gyre than in the western half. In von Arx's model the wind stress is essentially zonal, whereas over the Pacific there is a marked intensification of curl of wind-stress over the eastern half of the ocean. Thus Sverdrup's method does not lead to the circulation that von Arx obtains, whereas it does lead to the observed circulation of the Pacific. One supposes that this means simply that the model of von Arx's does not behave according to Sverdrup's rules as closely as the real Pacific does.

QUATERNARY PALEOCLIMATOLOGY OF THE PACIFIC COAST OF NORTH AMERICA[1]

CARL L. HUBBS

We will now deal in a field where facts are disputed and theories are a dime a dozen. A few points in regard to the climate of the past in western North America, however, are reasonably well established.

As a background for a consideration of recent secular changes in climate we may profitably spend a brief—very brief—time reviewing facts and interpretations of climatic changes in this region during Cenozoic time. The earlier periods will be rushed over; increasing emphasis will be accorded data that are accumulating on the more recent, largely Postglacial events, particularly over the past few millenia; in greatest detail, over the past few centuries.

LONG-TERM TRENDS

In western North America, despite enormous fluctuations, there has been, up to the present time, a fairly general climatic trend, which paleobotanists in particular have worked out in some detail. Through all or much of the Tertiary and Quaternary periods the trend in general has been toward cooler temperatures and greater aridity. Harking back momentarily more than a million years, we note that the trend toward aridity seems to have been rather consistent and extensive through Tertiary time. During late Pliocene time the downward trend toward aridity seems to have been reversed, and during Pleistocene time the surface-water supplies fluctuated widely on four main and various minor occasions. During each ice advance the long-term trend toward cooler weather was accelerated, to be reversed during each of the three Interglacial periods, when, in contrast, the trend toward aridity was resumed. With fluctuations again, this combination of drought and warmth characterizes the Recent period, whether or not we class it as Interglacial.

Throughout these complex trends there seems to have been a tendency toward increased fluctuation, from period to period, from year to year, and from season to season; also from region to region. This circumstance, plus the accentuation of some trends along with the temporary reversal of other gradients during Quaternary time, renders difficult and dubious any definite prediction as to future trends. In view of the past, perhaps the highest plausibility can be accorded the hypothesis that climate will change. My own feeling is that there is a very strong possibility, if not a probability, that the trend toward greater aridity will continue, with fluctuations of course.

The complexity of the climatic trends renders difficult not only future predictions, but also an understanding of the causes of past changes. In broad terms, particularly for Tertiary and Pleistocene history, the trends seems to have been largely worldwide, but, as I shall indicate later, some of the more recent fluctuations seem to have been negatively correlated in different regions of the earth.

The trend toward greater aridity in western North America has commonly been attributed to the elevation of the western mountains, which captured rainfall and produced rain shadows to the eastward, but there is some evidence that this was not the only factor. Interglacial aridity could hardly be so explained. Fluctuations in aridity during Recent time seem to have affected coastal regions and offshore islands, as well as the interior of western North America. And at least some of the changes were simultaneous in the diverse regions. Some factor other than local land elevation seems to have been involved.

Though other and broader factors have doubtless operated, it seems obvious that climatic conditions in the West have been greatly influenced by the massive land elevation that began in the Miocene, gained impetus in the Pliocene, and reached a crescendo in early Pleistocene. An important point of vital climatological bearing that has often been neglected is that not only the mountains but also vast plateaus were greatly elevated. In all probability the Great Plains and the Columbia Plateau were elevated, along with the vast Colorado Plateau. The Range and Basin Province, primarily of the Great Basin, was apparently subject not only to the internal ups and downs of fault blocks, but also to a general uplift of perhaps 2,000 or 3,000 feet. There are reasons for believing that in Pliocene time the Great Basin was essentially part of the coastal plain, standing at less than 1,000 feet elevation and draining to the Pacific. One reason that we have advanced to justify this view is that Pliocene fish fossils in the Great Basin represent coastal-plain types.

Though there are conflicting theories, and some would have the major trends of climate alternating in different regions, especially during the Pleistocene, it seems rather obvious now that in general the major climatic trends of Tertiary and Pleistocene times were essentially simultaneous over the earth, at least over Eurasia and North America. Early Tertiary times seem to have been rather uniformly warm and moist over the world, even toward the poles. There were, apparently, no polar ice caps, and, in seeming response, deep ocean temperatures dropped to only about 10°C, according to Emeliani's data from benthic foraminifera. Low elevation of the continents presumably contributed to the uniformity of conditions, but was probably not the only factor. The free passage of currents through the Panama Strait may have played a part. In any event, the Arcto-Tertiary Geoflora reached far south in the West. The warmth of the North Pacific presumably permitted interchange between temperate faunas on the two sides.

[1] Contribution from the Scripps Institution of Oceanography.

Cooling appears to have continued over the world through late Tertiary, as did desiccation prior to late Pliocene, when there appears to have occurred a reversal that in due time probably led to the first major accumulation of ice. The Panama Strait was closed by the elevation of the isthmus. The Madro-Tertiary Geoflora progressed northward taking over areas previously covered by the Arcto-Tertiary Geoflora.

Enormous fluctuations in precipitation and in temperature, involving the four major glacial periods of the Pleistocene, with their various substages, seem to have been world-wide. Paleontological and paleotemperature studies of bottom sediments confirm the picture in general, but emphasize the multiplicity of substages. Recent paleontological evidence, advanced by Claude W. Hibbard of the University of Michigan, confirms the picture: small fossil mammals of the Great Plains appear to represent all four of the successive cold periods of the Pleistocene, plus, on present evidence, two of the three Interglacial periods. Carbon-14 datings of the recession of the last ice cap about 11,000 years ago, bespeak essential simultaneity over the northern continents and northern oceans. And there is some evidence, as from faunal displacements in the sea, that Wisconsin (Würm) events were simultaneous in the Northern and Southern Hemispheres.

EXTREME COLD IN WISCONSIN TIME?

There are strong indications that the last (Wisconsin) ice stage was one of extreme and far-reaching cold—perhaps emphasizing the long-term trend toward cooler weather. The fact that the continental ice sheet did not advance as far as had the previous glaciers, and the fact that each major ice sheet was less extensive than the preceding one, may well reflect the long-term trend toward greater aridity. The extent of each ice sheet may have depended less on the degree of cold than on the available precipitation. Decreased moisture would have led to less accumulation of ice and less southward flow of the continental ice sheet.

If the Wisconsin was arid, it was dry of course only in comparison with early Tertiary and other ice age periods. Precipitation must have been heavy to have produced the vast ice cap (though less vast than previously) to have formed simultaneously montane glaciers well toward and, some think, almost to the Mexican border; to have produced ponds and marshes in now dry regions on the Great Plains; and to have filled many Western basins with great inland seas. The northern part of the Range and Basin Province had almost as much water as land, and even to the southward at least semipermanent lakes covered the playas. There is much evidence also of increased stream flow, along the coast as well as in the interior. The widespread pimple-mound topography may be interpreted as reflecting greater moisture as well as extreme cold.

Evidence of world-wide coldness during Wisconsin time is rampant. In the East the boreal forest seems to have advanced to the Gulf of Mexico, and the northern biota seems to have been forced into refuges in Florida and Mexico, from which redispersal took place in early Postglacial time. Anadromous fishes were able to move southward through seas now too warm for them: a small salmon became landlocked in Formosa; trout got to Sinaloa, the Atlas Mountains, and the Near East; sticklebacks moved into streams of northwestern Baja California. Disjunct populations of northern freshwater fishes in New Mexico and even in northeastern Mexico almost surely got there in cooler, Wisconsin time. Cool-water mollusks occupied ponds and lakes about which man existed at about the close of the Wisconsin period. The mammoth reached the Valley of Mexico. In New Zealand, I have heard, littoral mollusks dated at about 20,000 years have been dredged, from presumably Pleistocene submarine terraces, farther north than these cool-water species now occur.

It was presumably during Wisconsin time that certain marine plants and animals that now occur in temperate waters to either side crossed the then materially cooled waters of the tropics. One such organism is the giant kelp *(Macrocystis)*, which occurs as the same variants of the same species in Peru and Chile as in California. Another is the Pacific sardine, which is virtually indistinguishable in our waters and in Peru. I have computed that a drop in sea-surface temperature of about 3°C in winter and perhaps 8°C in summer would allow these and distributionally similar species to regain connection off the coast of Middle America, and I am assuming that the seas were cooled about that amount in Wisconsin time. Oxygen-18 temperature estimates from organisms in sea-bottom cores are in essential agreement.

The widespread extinction of large mammals in late Pleistocene time may also be attributed in part at least to extreme cold in Wisconsin time, forcing dispersal far southward into less favorable ranges, and in part to the following period of aridity.

Another line of evidence indicative of the southward extent of Pleistocene cold is furnished by the to-be-sure somewhat controversial pimple-mound or Mima-mound topography. These rather well-aligned mounds cover many thousands of acres in the western half of the United States, from near Canada to the Mexican border, and even, I am told, into the mountains of Sonora. Similar mounds in the far north are attributed to the freezing and thawing of the soil cover overlying permafrost. Farther south, I believe, they were formed by similar seasonal freezing and thawing of waterlogged soil overlying impervious hardpan.

Sea-temperature estimates by the oxygen-18 method from Wisconsin-period mollusks should eventually give us a sounder picture of reduced sea temperatures during that period—and air temperatures were no doubt very closely correlated. But the Wisconsin deposits were laid down when the sea-level was much reduced and, in my belief, the general trend of land elevation (or sea-level depression) has not proceeded far enough since the time of deposit to reveal the fossils of this period. Most of the cold-water fossilmollusk faunas already exposed at low elevations along the coasts of Southern California and Baja California I believe to be of Illinoian age. From mollusks of one such deposit in northwestern Baja California an oxygen-18 temperature estimate of only about 12°C has been obtained, I have heard.

Great secular fluctuations in sea temperatures—and hence of coastwise air temperatures—are indicated by the Pleistocene faunal assemblages in all temperate regions. Some of these assemblages are indicative of colder-than-present temperatures, others of warmer. Recent claims of mixed cool- and warm-water faunas are in part at least, based on erroneous and doubtful data; and the oceanographic explanation offered does not seem to hold water. Recently I have sampled on Guadalupe Island, only 240 miles south of San Diego, a warm-water late Pleistocene fauna, including reef coral, and mollusks that now inhabit tropical waters extending from the Gulf of California to beyond Panama. With modern isotope methods and other more intensive and critical methods, we should eventually be able to seriate and in part date these findings, and get definite temperature estimates.

POSTGLACIAL TRENDS IN TEMPERATURE

About 7,000 years ago (by carbon-14 determination from shell) the Indians of Santa Rosa Island, California, were feeding chiefly on red abalones *(Haliotis rufescens)*, a species that is available inshore only where the coastal water (and air) is cold. Hence a trace of Glacial cold was presumably persisting. Later the warm-water black abalone, *Haliotis crackerodii* was chiefly consumed.

I have obtained oxygen-18 estimates that indicate, in agreement with faunal data, that the sea-surface temperature along the coast of northwestern Baja California was somewhat warmer than that of the present about 4,000, about 2,500, and about 300 years ago. These estimates were obtained on shells of surf-zone mollusks from Indian middens, from which I also have obtained carbon-14 datings from charcoal and/or shell. But between the 2500-year and 300-year B. P.* datings I have evidence of a cooler-than-present period. Oxygen-18 temperature estimates lie below the present growing-season mean, and the dating is about 900 years ago. In this period the Indians over a long stretch of the Baja California coast fed heavily on the giant chiton *Cryptochiton stelleri*, which now, except for extremely rare deepwater strays off Southern California, is confined to the cold-coastal region from near Pt. Conception in California to Japan. No trace of it has been found along the coast from which it was abundantly harvested about a millenium ago.

A point of prime interest in this connection is that at about the same time it is thought that coastal Greenland had become attractive to and was inhabited by Scandinavians—when Greenland was green. This is one of the bits of evidence of negative correlation in regional temperature regimes. Temperature, like money, may be present in adequate amount but not evenly distributed.

* Before present.

Coming closer to the present, we have evidence of a warm period, along the southern half of the California Coast, just about a century ago, after which this region became cooler while arctic amelioration was proceeding in the far north; again, the ambient temperatures of this coast and of the far north seem to have been negatively correlated. I was led into this interpretation of a warm period approximately a century ago by the faunal evidence. The first zoological survey of the West Coast worthy of that designation, conducted by the naturalists of the Pacific Railroad Survey of 1853-57, disclosed at San Diego a fish fauna of a warmer-water affinity than that of the present—about like that of Turtle Bay, and even of Magdalena Bay (near the middle and southern parts of the Baja California peninsula). And at Monterey this early survey found a fauna containing San Diegan elements that do not now occur in the colder waters of central California. Some of these southern forms at each port were sedentary types not very subject to northward dispersal in isolated warm seasons such as we are now discussing. A seahorse, for example, was one of the fishes taken then, but never recently, at San Diego. Other faunal data keeps fitting into the picture.

To check the indications of warm water in the southern half of California about a century ago, I made a short journey into meteorology. First I showed an extremely high correlation between sea and air temperatures near San Diego. Then I analyzed the weather data, which very fortunately began at San Diego in 1849 and very soon afterward at Monterey. There was indeed a definite trend toward cooler temperatures at these localities through the second half of the nineteenth century, notably in the late-spring and early-summer months, which are of most significance to the fishes.

The weather data for San Diego seemed to indicate a continued cooling trend until about 1910, after which the monthly plots seemed to show a slight rise. But this rise may be attributed at least in part to the effect of urbanization, which, as certain meteorologists have shown, yields temperatures in built-up cities that are somewhat too high for the region. The most marked rise toward the end of the years analyzed is definitely attributable to the moving of the weather station to Lindbergh Field, for test runs showed the temperature there to be about 1.0° F. higher than at the Federal Building, where the weather station had previously been located.

POSTGLACIAL TRENDS IN MOISTURE

There is now, of course, no shadow of a doubt that during the Pluvial period, which accompanied the last Glacial period, the rainfall from the Great Plains to the Pacific Coast and from the ice cap to Middle America must have been vastly greater than at present, and the evaporation must have been much less. Together these factors caused rivers to flow full and basins to fill with great lakes. I have reviewed this evidence in some detail and will not repeat it here.

Evidence has been presented to indicate considerable desiccation through the still moist and cool early ("anathermal") portion of Recent time, which was followed by the warm and dry altithermal period (or climatic optimum), around the middle of Postglacial time. A "Little Pluvial" period then is thought to have followed, during which Pluvial lakes and streams were somewhat re-established. Archeological and tree-ring data suggest a severe drought in the thirteenth century.

Obviously there have been marked fluctuations in water supply during Postglacial (Recent) time, but throughout most of the period water was probably much more abundant than it has been over the last few centuries. Archeological and physiographic evidence is replete with indications of more abundant water, at many times during the Postglacial period and at many places from the Pacific Coast to the Great Plains. Indications accumulate of past human populations too large to have been supported by the amount of surface waters presently available. Midden remains are found of fishes and other water-limited animals that could not now exist in the region, and in places the remains of plants indicate vegetation of a more humid period. My own studies keep uncovering confirmatory indications of greater humidity at various dated times in the Postglacial sequence.

In fact, I have for some years thought that the drought of the past three decades has been the most severe of any since the Pluvial period. I have found evidence of the extirpation during this period, as a result of the drought, of wholly isolated fish populations that must have persisted since Pluvial time. Except for isolated introductions by man, fishes occur only in waters that they have at some time reached by normal dispersal.

The "march of the desert" seems to have been continuing right up to the present. In fact I am becoming more and more impressed with the apparent recency of the desert conditions over the southwestern United States. To be sure the desert biota is too distinctive to have suddenly evolved, but the deserts may well have merely been dispersed, with their characteristic biota, northward from some ancient center in Mexico.

Some of the evidence for the recency of the deserts in the Southwest is physiographic. Recent gullying—very striking through the region—is very likely due in part to drought, which has destroyed ground cover. More pointed evidence, in the extreme desert regions of the Colorado Desert and the Colorado Delta, is furnished by the structure of the *bajadas* (the alluvial apron around desert mountains). Typically, sections of *bajada* even against the mountains show fine sediment throughout the greater part of their height. Occasional gravel or cobble streaks tell of flash floods. But the fine sediments are typically capped by desert shingle. Furthermore, secondary fans topping older *bajada* slopes are composed of very coarse rock fragments. The plausible explanation is that over long periods of time until very recently—unfortunately we do not yet have datings—the desert mountains were smooth-topped and largely covered with a mantle of soil, which, from time to time, probably at an accelerating rate, eroded away, to be deposited at the base of the mountains. The abundance of large herbivores until late Pleistocene and early Post-Pleistocene time, and the oncoming drought may have together caused the erosion. Shift of rainfall from a rather even distribution to one of limited but torrential precipitation may have been a major factor. Whatever the cause, the soil on the mountains was largely eroded away, so that subsequent weathering has attacked and broken down the bare rock faces that are now so characteristic of the arid Southwest. I believe that the naked-rock mountain landscape of the West is of much more recent origin than has generally been thought, and that the seas of playa sediments that surround the *Inselbergen* have often accumulated much more rapidly than has generally been assumed. Some radiocarbon dates of buried levels of human habitation in the Southwest confirm this view.

This rapid development of arid landscape may be observed in certain places where the ground cover is being destroyed. Bare rock exposed by landslips in New Zealand mar the mountain slopes that are overgrazed by sheep. The landscape of Santa Rosa Island off California has been transformed over the last two or three decades from one of gentle soil-covered contours to one in which bare rock exposures are becoming more frequent. The soil veneer keeps slipping off to expose these scars. The reason I believe to be a combination of drought plus the foraging and trail-making of many large introduced mammals—elk, deer, and boar, as well as cattle, horses, and sheep. The landscape is beginning to change from one resembling that of the Alleghanies to one beginning to approach that of the desert mountains. What is now happening before our eyes on Santa Rosa Island I believe represents what happened in late Pleistocene or early Recent time over vast areas in the Southwest, very likely from the same causes—drought and over grazing.

The past and present distribution of animals offers much evidence corroborating the view of increasing aridity. For example, the woodchuck over the past few millenia ranged through northern Arizona well south of the range to which it is now limited by reduced moisture and higher temperatures. Fish mummies from caves in Nevada indicate a permanent lake in the Humboldt Sink, in the last few millenia. Now bone-dry canyons in southern Nevada show campsites, pictographs, petroglyphs, and other evidences of human occupation when these water courses contained water. Such evidence abounds throughout the Southwest. My own investigations yield conclusions consistent with the general view.

We are now accumulating evidence of a rather heavy population on the coast of Southern California about 7,000 years ago, in areas where the present water supply would not be adequate. Similar evidence stems from sites dated at about 5,000, 4,000, 3,000, and 2,500 years B. P.

Some of the evidence of more ample water relates to much more recent time. Very extensive middens, predominantly of small Pismo clams, over a long stretch of the Baja California coast, have been dated from a warm period, about 300 years ago, that must also have produced more surface water; carbon-14 dating and oxygen-18 paleotemperature estimates are involved. A rather extensive habitation site on North Coronado Island has been dated at less than 400 years, but there is now not enough fresh water on the islands to supply more than a family or two, and not enough brush to provide fires for any length of time for more than a very few people. But the site carries much evidence of fire, and more rainfall would surely have been needed to grow the wood that was used. Large populations of relatively recent date (to judge from a few radiocarbon tests and from the types of artifacts) existed at points along the Baja California coast, as about San Felipe, Turtle Bay, and San Ignacio Lagoon, where the present available water could not have supported the people.

Evidence of former, much-greater-than-present precipitation in the desert of Southern California was recently obtained on "Fish Creek," in the Fish Creek Mountains on the west slope of the Salton Basin. I went there with archeologists specifically to check on the former fish life of this creek. Some small fish still persisted in water holes in this creek prior to the great flood of 1916, which filled the stream bed with sediment. Since then the rainfall has been insufficient to produce permanent water, in recent years none at all; except for very rare flash floods, the stream bed is bone dry. We travelled up this dry bed by Jeep for about 14 miles, through the defile in Split Mountain, to reach an extensive ancient village site that was already known. On the surface were remains of mountain-sheep horns, suggesting better grazing and more water in former times. Digging in the abundant hearths yielded not only mammal, bird, and lizard bones, but also many fish bones. These represented the humpbacked sucker *(Xyrauchen texanus)* and the bonytail (*Gila robusta elegans*) — species that abounded in Lake LeConte, the vast inland sea that existed until about 300 years ago in the Salton Sink, and that was formed, at least in part, by inflow from a distributary of the Colorado River. These species formed a major—probably the major—part of the diet of the thousands of Indians who lined the shores of the ancient lake. But it was the adults of these large fishes that were caught and eaten about the lake, whereas most of the fish that were eaten at the Fish Creek site far back in the hills were young. The wholly plausible conclusions are that these young fish were caught locally in Fish Creek, and that they had grown from spawn deposited there by big fish that had found sufficient water in the creek to swim up from the ancient lake. To yield such a flow must have required rainfall about equal to that of the coast—say ten inches a year, as contrasted with probably less than five in the same watershed today. The date from charcoal in the hearths is 1,000 B. P.

Surprising as such a rapid decrease in rainfall may appear to be, we may regard the estimate as not at all improbable. The depopulation of the Old World deserts over the past few millenia suggests that the increase in aridity has been very widespread, at least at comparable latitudes.

Other dates of habitations in the desert region over the past few hundred years, about Clark Dry Lake and elsewhere, also suggest that more water was available until very recently, even within the past two to four centuries.

The last few centuries, though almost surely not so excessively arid as the present, have I believe approached the present in desert drought. There has perhaps been no great recovery from the great drought of the thirteenth century (about 700 years B. P.). A bit of physiographic data, for which I am largely indebted to George M. Stanley, bears on this point. The main shore bars of the last stage of Lake LeConte, which I estimate from radiocarbon datings and other evidence to have lasted from at least about 1,000 years B. P. to about 300 years ago, are remarkably unbroken, even where consisting largely of fine sediment in the even sweeps across canyon mouths. Much rainfall during the past 300 years, after the desiccation of the lake, would surely have formed ponds behind the bars, and these ponds on overflowing would have cut much more extensively than they did through these bars (which stand at 45 feet above sea-level). The recession lines representing temporary levels as the lake disappeared also bespeak great aridity about 300 years ago, for they are spaced (or were before the Age of Jeeps) about five feet apart (about half the present rate of evaporation). Earlier Pluvial conditions almost surely yielded enough inflow from the drainage basin to hold the lake levels, without contribution from the Colorado River, but the local rainfall must have been greatly curtailed over the past few centuries.

About five feet higher than these last-formed bars, which give evidence of about three centuries of notable drought, Dr. Stanley found, as remnants only, other bars, representing an earlier lake level. These earlier bars possibly date from a much more ancient, separate lake stage, but my present hypothesis is that they date from an early period during the last lake fill, possibly from about the time Fish Creek provided breeding grounds for the lake fish (1,000 years B. P.). Greater rainfall, in this view, was more responsible than great antiquity for the major destruction of the earlier bars. Archaeological, radiocarbon, and physiographic evidence indicates that in the interval between 1,000 and 300 years ago sedimentary deposits were rapidly formed behind the lake bars, as a result of heavy erosion, before the bars were reworked into the last formation, which has so well resisted erosion during about three centuries of drought.

Historical data confirm the view that the Southwestern deserts have been extremely arid over the past two centuries. Earlier evidence, from the explorers of the coast, dating back more than 400 years, is unfortunately confused. Cabrillo and Vizcaino wrote of forests on Pt. Loma and near Santa Barbara where none have existed more recently, but historians argue over the translations and the meaning. Early Spanish maps of the Central Valley of California show vast expanses of freshwater lakes, ponds, and sloughs, but it is possible that the map maker passed through during a brief flooding. I am inclined to the view, however, that these early observations reflected greater rainfall.

CONSISTENCY IN WEATHER PATTERN

Through all these major fluctuations in climate the general geographic pattern seems to have been maintained throughout the Pacific Coast and Southwestern regions. There have no doubt been north-and-south shifts, as well as general changes in intensity—of rainfall, temperature, wind direction, etc.—but the gradients have been maintained.

The illuminating researches of Axelrod indicate that the pattern was already established in early Tertiary time and persisted throughout that period. Pluvial conditions were graded from north to south in much the same pattern as today, though everywhere the effective precipitation was probably more than twice that of the present. The same pattern almost surely persisted during the greatly reduced rainfall of Postpluvial time.

Along the coast we find evidence of the long duration of the temperature pattern, as indicated by faunal assemblages and some oxygen-18 estimates; and, as I have noted, air temperatures are very strongly correlated with sea temperatures. Though time correlations are questionable, the evidence from Pleistocene deposits strongly indicates a southward increase in temperature with a break near Pt. Conception. Where the coast seems relatively stable, in northwestern Baja California, Pleistocene deposits that I believe will prove to be Illinoian seem to reflect the regions of upwelling that are so striking today; if so, prevailing winds were probably similar to those of the present.

In the middens also we find evidence of the persistence of the temperature pattern. Thus the faunal assemblages of relatively recent middens on the northern Channel Islands show a marked gradient from cold-water types at the western end of San Miguel Island to warm-water types at the southern side of Santa Cruz Island, over a stretch where there is today a gradient of about 10°C in summer sea-surface temperature and a corresponding gradient in the littoral fauna. The region in Baja California where the occurrence of the cold-water *Cryptochiton* in middens indicates, along with isotope determinations, a colder-than-present period about 900 years ago, is precisely the area where upwelling induces today incongruously cold coastal sea-surface temperatures (and cool, moist air). Even within this area the actual points where *Cryptochiton* is found in the middens are those where upwelling is intense (and where ecological conditions were favorable). Other mollusks in the middens confirm the picture. We can feel sure that roughly a millenium ago the winds were predominantly northerly, as now, and that they were, for some reason, sufficiently more intense or persistent to cause even greater upwelling then than now.

Long persistence in available water supplies is indicated by the evidences of especially large aboriginal populations where surface water still remains, or would now exist if the climate turned somewhat more moist. There are many indications through the arid West of ancient and often more or less continuous populations about springs that still flow, or along streams or lakes. Other populations existed, at times as early as 10,000 to 25,000 or more years ago, where streams or lakes are now intermittent, but would contain more or less permanent water if the rainfall were moderately increased. Similar indications can be cited for coastal sites. For instance, radiocarbon dates of approximately 3,500, 3,900, and 7,300 years B. P. have been obtained from camp or village sites along Batiquitos Lagoon in San Diego County, California, right beside a present-day cattail marsh. Along the northern Baja California coast ancient habitation signs dated at about 900 and 2,500 to 3,000 years B. P. tend to be concentrated near the mouths of streams that are still more or less permanent.

POSSIBLE PREDICTIONS

It is obvious that we can venture predictions of future climate only with great uncertainty. Past changes, however, lead us to believe that the climate will almost surely fluctuate widely, perhaps rather abruptly. It is almost certain that the fluctuations, however great or abrupt, will be superimposed on a geographic pattern much like that of the present.

As to temperature, I see no clear suggestion of what we may expect. We have some indication of negative correlation between temperatures along the Baja California coast and in the far north about 900 years ago, and rather definite indication of the cooling of the southern half of California during the recent arctic amelioration. There are suggestions that cool periods as well as warm periods over the past few thousand years were more moist than the present. The general trend since Wisconsin time has been toward warmer weather, but with great fluctuations, and this trend is contrary to the longer-term trend toward cooler conditions. The apparent recency of the evolution of desert conditions in the American Southwest suggests at least continued heat in this area.

As to aridity I feel rather strongly that the long-term trend toward dryness has been continuing, perhaps with acceleration. If so, the trend may well continue into the future. At least, we ought to be prepared, on the existing incidence, to meet the very strong possibility, if not probability, of even increased aridity in the Southwestern regions, including Southern California and Baja California. It would be most unwise to plan otherwise.

DISCUSSION

Isaacs: As I remember, in his book *Two Years Before the Mast,* Dana told about trees on Point Loma.

Schaefer: Dana and some of the others were left on the beach to cure hides and they had to go out to cut the wood. He described it as brush or small trees.

Isaacs: I always thought the storms he described were exaggerated. He was in a small ship and I have always thought that they were probably ordinary storms as we now experience, but some time ago I picked up an edition of *"Two Years Before the Mast"* that I had never seen before. An epilogue in it records a conversation that Dana had with a ship's captain in San Francisco in 1859. Dana came back to this coast in 1859, and in this conversation, the ship's captain said that the storms off Point Concepcion had stopped some time ago. They had not seen anything like them since. I now feel that it is quite possible that these were storms of some significance. I have often wondered about the storms because he described them in quite vigorous terms, as being much more severe than the storms around Cape Horn.

Hubbs: I will have to look into this historical evidence.

One of the things I particularly want to do is to get some botanist to identify the charcoal fragments from ancient hearths so that we can reconstruct the vegetation of various regions at the determined times. There is promise, I think, of our being able to reconstruct the past oceanography, climate, and human occupation.

Namias: Do you have any ideas as to what produces weather regime?

Hubbs: Are you asking me?

Namias: You made a prediction that it would be drier.

Hubbs: Well, over enough millenia, it will be.

Charney: The changes of which you speak are probably part of a word-wide pattern. It would be interesting to see if one could find the same thing, say, on the west coast of Africa.

Hubbs: I have not looked into this, but when Ahlmann was at Scripps, he mentioned that there is some evidence that in Portugal there was cooling during the period of Arctic amelioration.

Charney: The reduction of temperature contrast in the past would probably have weakened the westerlies. This would have slowed down the ocean circulation.

Hubbs: The general pattern has probably long remained the same, because the locations of upwelling are such that if we had had south winds instead of northwest winds, the pattern would have been reversed, so that it would be cold where it is now warm, and warm where it is now cold. However, the evidence from remains of fauna in kitchen maidens is such that reversals could not have taken place along the coast of California and western Baja California since late Pleistocene time.

Charney: What is hard for me to imagine, is how the small change in the same weather pattern could produce enough additional rainfall in the arid regions to enable you to measure it.

Namias: In the climatic pattern of this last winter, we have had storm activity spreading into the southwest with surprising frequency. The smallest disturbances would trigger rains in Arizona and Mexico, for instance. This condition was associated with very warm air in the Labrador area (Fig. 8). The normal temperature contrast between the subtropics and the polar regions was reduced. This increase in precipitation at lower latitudes probably extended over the southern North Atlantic and possibly into the British Isles, Spain and Portugal.

Fleming: Hubbs, what do you mean by this aridity?

Hubbs: Less water for people to drink is the main thing.

Fleming: Do you actually mean to imply that there would be less evaporation from the oceans, too?

Hubbs: That of course goes right with it, and the change has been both in the evaporation and the rainfall.

Fleming: I cannot imagine that there is much change. I would assume that the long-term average evaporation over the ocean must remain relatively constant. Although, obviously, we have had a short term change in the last two years.

Namias: Why should it remain constant?

Fleming: Evaporation amounts to just about a meter of water per year. The thing that brought this to mind, first of all, was this statement made that during the Pleistocene there must have been tremendous changes in the amount of precipitation. Of the estimates I have been able to get out of my geological friends, the total amount of ice accumulated during the glacial period would result in about a millimeter of water evaporated from the ocean per year. This, incidentally, is about the same rate the sea level has risen in the last fifty years. This great change of the evaporation precipitation balance, which gave rise to the glaciers and their subsequent melting therefore resulted from an imbalance of only about 1/1000 of the mean annual evaporation. It reflects a very small annual change in the distribution of water between land and ocean. The change over millenia is, of course, conspicuous, and is essentially a very sensitive thing. I question whether you have to invoke this idea of large changes in precipitation patterns. Certainly you can, but can it be world wide?

Charney: These temperature changes we are speaking of, 2 or 3 degrees decrease in the south and 2 to 3 degrees increase in the north, would produce 10 to 15 percent changes in the wind pattern. Such changes in the mean westerly winds would in turn produce large shifts in the semi-permanent centers of action, and probably even larger changes in the wind-driven ocean circulation patterns.

Hubbs: What would this circulation change then do to north-south temperature gradient?

Fleming: Would it increase it in terms of the ocean as an equalizer?

Charney: I don't know the answer to that. A decrease of wind would perhaps reduce the intensity of the ocean circulation and hence the heat transported northward by the oceans. This change in transport would have to be compatible with the reduced north-south temperature gradient in the atmosphere. The oceanic heat transport across latitudes is usually considered to be small compared with that of the atmosphere, but do we actually know this?

Saur: It is on the order of 10 percent at mid latitudes, according to Jung.

Charney: I have heard this; but does anyone believe it? The oceans certainly transport heat from equatorial to polar regions. The question is: how much do they transport?

Saur: How much? Jung's figures are fairly good, I believe.

Charney: How did Jung arrive at them?

Saur: He took the Atlantic stations and used, I think, latitudinal sections of stations across the Atlantic, balanced each one for the current, and constructed a pattern of currents for the whole Atlantic on the basis of a geostrophic circulation. He also used salt transports in establishing the pattern.

Charney: But how did he know what the deep currents were?

Saur: He took a section of stations and adjusted his levels of no motion till there was essentially no net mass transfer through the section.

Comment: Isn't it so that there is an infinite number of solutions to this problem? Any current assumed will give you the surface topography that will also satisfy continuity, so nobody really knows what the deep currents are unless you measure them. Is he trying to get something for nothing here?

Fleming: But one thing that you do know, is that the heat transfer of that part of the ocean is negligible.

Charney: But do we know this?

Fleming: You can at least set certain limits on it in terms of the heat budget. You can consider how much water sinks from the surface in the higher latitudes and rises at the Equator, and you can set upper limits. We might have violent circulation in the deep ocean, but this will not give you net flux of heat. This water may be circulating very rapidly in the deep water, but it will not give you any net flux across latitudes. Thus the deep currents are not important in the heat budget.

Saur: I agree to your argument that although we do not know accurately how fast the currents are going in the deep ocean, that would not be very important here. We can set some sort of limits.

Charney: Well, put it this way: we know that the atmosphere plus the oceans receive a certain amount of excessive heat in the southern equatorial regions, and deficient heat in the polar regions, therefore this heat has to be transported either by the atmosphere or the ocean. There is no other way. What part of this transport is carried by the oceans? If we had an accurate idea, for example, of how much the atmosphere transferred, exactly how much was coming in and going out, we could then ascribe the difference to the oceans, but we do not have that idea. I think Jung's figure does not contradict anything we know. The oceanic transport could be twice as great as the atmosphere or half as great.

Saur: His figure for oceanic heat transport is twice that given in *The Oceans,* but he still attributes more heat transport to the atmosphere than to the oceans.

Charney: There is probably no point in trying to speculate because in a few years we will be able to get numbers. A difficulty, of course, is, or will be, that a proper climatic theory must consider the atmosphere and the oceans as a coupled dynamical system. A change that takes place in the atmospheric circulation will produce a change in the ocean surface temperatures and thereby produce changes in the atmosphere.

Hubbs: Sometime it would help to have paleotemperature data from Japan at the same periods we are getting it over here. Are there any, Takenouti?

Takenouti: No.

Hubbs: That would be a very interesting thing to do in Japan. If we obtained the same data 1000 years ago here, and 1000 years ago there, it would be extremely interesting comparing them.

Fleming: Three hundred miles off Washington Coast we get into a band of non-calcareous sediments. What is interesting in making a time reference is that we can find this band of non-calcareous sediments, which is an indication of warmer water conditions, extending all the way up the Gulf of Alaska, where there it is about a foot or 18 inches of non-calcareous sediment material. Considering a reasonable rate of sedimentation, this appears to represent a few thousand years. Definitely there must have been some change in the character of the circulation and of the temperature from roughly 50° to 55° or 56° N. We are going to try to get some carbon-14 dates from that layer.

Hubbs: Oxygen-18 determinations too would be tremendously significant. The deposit might be interglacial.

Fleming: Another thing we have that makes a good marker, is the ash fall, ash layers that occurred about 6000 years ago. They constitute a very useful sort of marker.

Hubbs: Apparently the volcano that went off with a big bang was Mount Mazama. There were Alaskan volcanoes too. The ash falls certainly could help.

SOLAR EVENTS AND EFFECTS ON TERRESTRIAL METEOROLOGY

R. G. ATHAY

I feel a little uneasy about speaking on the subject I have been asked to—"Solar Events and Effects on Terrestrial Meteorology." This subject involves two branches of science that are somewhat sophisticated by themselves, but, when brought together, they unfortunately have somewhat of an unsavory reputation. Much of this has come, I think, from a great desire to improve weather forecasting by any means available. Admittedly, much of the effort to improve forecasting by this particular means has been of questionable merit. I suspect that one reason sunspots were used is that their remoteness makes them a little difficult to check up on. There are those who are determined to blame everything that varies cyclicly on sunspots. For example, you can find in the literature claims for strong correlations between sunspots and such events on the earth as birth of babies, growth of wheat, and stock market fluctuations. There is no doubt but what these claims have contributed quite a bit to the somewhat poor reputation of the field of solar weather relationships. If I felt that the subject merited this reputation, however, I would not be speaking here. We are beginning to make some progress and I shall spend my time this evening outlining the basis of this progress.

While sitting in the audience the last few days, I have learned a lot and enjoyed the meeting very much, especially the informal atmosphere, and I hope it will be preserved tonight. I have also cultivated a sympathy for those sitting in the audience who do not know what the speaker is talking about. I would like to spend most of my time trying to explain to you what I am talking about. Toward the end we can get around to the relationships between the sun and weather.

In starting I would like to say that in looking for a relationship between the variable sun and the weather, we do not have in mind explaining the general atmospheric circulation. I do not think anyone seriously maintains this idea. What we do have in mind are some particular anomalies of the circulation that appear to be related to events on the sun.

In making the step from the sun or events on the sun to anomalies in atmospheric circulation, we are taking a step that most meteorologists would prefer not to take at this time. I would not advocate that we make the additional step of attempting to relate the anomalous features of the sun to oceanographic anomalies, which in effect is making two steps beyond our knowledge and which would not be justified.

I came here with the impression that it is very fortuitous that the period of time we have covered in our studies coincides with the period that you are interested in. After listening to the discussion, I almost wish that it had occurred at some other time, and I am a little afraid it might detract from the central issues here. I sympathize very much with Charney in the attitudes that one has first to consider the relationships between atmosphere and the oceans. I hope that anything I say will not detract from this main idea.

The whole field of solar weather relationships is very complex at both ends of the problem. The atmosphere is a very complex medium. The circulation of the atmosphere depends on many factors, all of which must be taken into account before we can fully explain the circulation. The Sun, at the other end of the problem, is at least as complex, if not more so. As in the study of oceanography and meteorology, we cannot really control the experiments. We take what we observe and interpret. We have the apparent handicap of being further away, but, after listening to your discussions, I am not sure it is a real handicap.

I would like to summarize some of the evidence for variations on the sun. I will talk specifically on only those we are interested in tonight. There are a great variety of features observed on the sun that are variable in time and that may result in important perturbation in the terrestrial atmosphere. There are two in particular which I would like to talk about, both basically related to sunspots, with which I am sure you are familiar.

Sunspots as you know, come and go in an eleven year cycle, and are currently at a maximum. Actually, we are not certain whether they are past or approaching the maximum. The evidence seems to be that they are very near the maximum at this time. The last year, 1957, and the beginning of this year, represent the greatest sunspot activity that has been observed in the last 2½ centuries. The last two years, therefore, have been of extreme importance to us, as well as to you. Other solar activity associated with sunspots is also apparently at an all time high. The rise to maximum activity has not been monotonic. It has fluctuated a good deal, which is also typical of other sunspot cycles.

The visible part of the sun we refer to as the photosphere. We may think of the photosphere as a surface, but it is actually the visible layers of the radiating solar gases. Above the photosphere there is a chromosphere and corona, which we can observe at the time of a total solar eclipse. Or, we can observe them with a coronagraph, which is a special telescope for making an artificial eclipse of the sun. We use coronagraphs at high altitude mountain observatories, where the air is exceptionally clear. The chromosphere lying just above the photosphere, and the corona overlying the chromosphere are anomalous features of the sun, as I will now illustrate.

The surface temperature of the sun is of the order of 6,000°C. The corona, however, has a temperature of above 1,000,000°C. We have not yet accounted for this spectacular increase in temperature. It is in itself a departure from the normal idea of a sun that can be represented by a blackbody at 6,000°C. As a consequence of the high temperature in the corona, there are strong solar radiations beyond the visual part of the spectrum, some of which have been observed and some of which have not been observed.

The corona varies markedly during the sunspot cycle. Generally over sunspot groups we see regions of the corona that are very active and have abnormally high density and high temperatures. These regions carry the implication of a strong excess of ultraviolet and X-ray radiation. How strong and how much in excess, we cannot really say. I will mention in a minute the sort of estimates we can make. First, however, I should remark that the corona itself does not vary rapidly in short periods of time. If we pick out a particular active region, the life time is of the order of weeks to perhaps months, and we do not expect really rapid variations.

However, there are features apparently associated with the corona that exhibit rapid variations. The most important of these is the solar flare. Flares usually lie near the upper chromosphere or lower corona and are sometimes called chromospheric flares. The flare has a life time on the order of half an hour. They usually take something less than 10 minutes to reach maximum brightness and then fade out, in something like a half-hour's time. We customarily observe these flares by their increased brightness in the cores of the strong Fraunhofer absorption lines in the visible spectrum. Normally, we cannot observe them in the white, undispersed light of the solar spectrum. Three or four have been observed in this way, but they are the exception rather than the rule. Therefore, flares have no appreciable effect on the visible radiation from the sun. In the cores of the strong Fraunhofer lines and in the ultraviolet and X-ray spectrum however, flares have a pronounced effect.

Let us consider the specific changes that are known to occur in the solar spectrum. The solar constant has been measured over a period of several years by workers at the Smithsonian Institution. These data seem to indicate that there is no significant variation in the radiation from the sun in the visible part of the spectrum. There have been reports in the literature of significant periodic variations in the solar constant. In fact, several periods have been ascribed to these data by the questionable technique of somewhat arbitrarily selecting a period and amplitude, subtracting this periodic component from the raw data, then picking other periods and amplitudes from the remaining data until a reasonable representation is obtained. One can always find periodic components by doing this, but, in a power spectrum analysis of the same data, no significant periodic components are evident. Summarizing the data for the visible part of the spectrum, one can say that there are no variations larger than .1 of 1 percent. This is about the limit of the observational accuracy, and is an upper limit to any possible variations.

On the other hand, if we go the extreme limits of the spectrum, we observe very violent variations in the spectrum. In the radio end, which I will mention just briefly, there are variations of many orders of magnitude in the intensity of the radiation. So far as we know, these variations have absolutely no effect on the terrestrial atmosphere. They carry practically no energy and do not seem to be of any particular interest to us as far as terrestrial effects are concerned. Going in the other direction, however, we observe large variations in the ultraviolet and X-ray part of the spectra, and we suspect that there are still larger variations which have so far escaped detection. We have observations beyond the visible range down to about 1,000 angstroms. The spectral region beyond 1,000 angstroms is unobserved down to 100 angstroms, but below 100 we again have observations. This is because of the absorption spectrum of the earth's atmosphere, which will not let the radiation between 900 and 100 angstroms penetrate far enough for us to get rockets and other observing equipment into the proper altitude, so we simply have not observed it. In the parts we have observed, we detect variations that are at least of the order of 100 percent. However, we have every reason to suspect that we have come nowhere near to observing the extreme variations.

You can imagine the difficulty we have trying to observe the radiation from a flare. The radiation reaches maximum intensity in about 10 minutes time. Flares are totally unpredictable insofar as our present stages of sophistication are concerned. In order to observe ultraviolet and X-ray radiation from a flare, we must constantly watch the sun; and when a flare begins, we must somehow get instruments into the upper atmosphere to a height of at least 70 kilometers to observe the effect we are looking for. This must be accomplished in about ten minutes time at the most. There have been deliberate attempts to do this by the Naval Research Group, in the summer of 1956, I believe it was. A ship was stationed off the west coast of Central America with several pre-equipped rockets ready to fire. The rockets were carried aloft in balloons to get them to altitude faster and make them a little more efficient. The rocket crews on shipboard were in radio contact with our observatory, and they had flare detectors of their own. Because of mechanical failures on shipboard the flare detectors did not work. When flares occur the excess radiation often disturbs the ionosphere, disrupting radio communications. Thus, to some extent, this entire operation was plagued with difficulties to start with. One flare occurred that did not disturb the ionosphere too much, and a radio message got through. The rocket was fired and reached altitude during the waning stages of the flare. The rocket instruments showed a rather large increase in X-ray radiation, but in the wave lengths around the resonance line of hydrogen where we expected to observe variations, no variation was evident. However, the visible flare was practically gone and we cannot really state conclusively that the resonance line of hydrogen did not change in intensity. In fact, we know that it has to change since the lines of hydrogen in the visible spectrum change rather remarkably, and with any reasonable model the resonance lines have to change by a much larger factor. Another attempt to observe the ultraviolet and X-ray radiation from flares was made from San Nicolas Island, with similar failures. One rocket reached altitude during late stages of a flare and again indicated a strong increase in X-ray radiation. However, the ultraviolet equipment did not operate properly. Our real hope for observing the ultraviolet and X-ray radiation from flares lies in artificial satellites. This is why there has been a vigorous effort to have satellites in orbit during IGY. We must have an observing station that is up there a long time so we can catch flares during the initial stages and observe them throughout their life history.

Returning now to the corona, I should point out that coronal radiations are more or less steady on a time scale of hours and days. However, they do vary over periods of weeks to months. Just to give you a picture of what is happening on the sun, I have prepared a chart (Fig. 103) showing the variation in monthly means of coronal radiation and flare activity. The time in years is plotted along the bottom. For comparison with the sunspot cycle I have also plotted the sunspot numbers. The sunspot curve rises rather irregularly from 1954 on and varies considerably from month to month and even more so from week to week. In 1957 you see a very high intensity of coronal radiations, which we have integrated to provide a global value. The observed coronal radiation is in the visible part of the spectrum. We have reason to think that the ultraviolet radiation will be somewhat proportionate. We are not certain in exactly what way, but certainly the two vary in the same direction. You can also see an overall increase in the coronal radiation along with the sunspot cycle. In fact, there is a quite good correlation between the two curves. On the bottom of the chart, the dashed line, indicates the flare activity. This is an index that involves the number of flares. In some ways this curve is misleading. I could multiply the index by a factor and get something like the curve for the coronal radiation. I plotted the curve this way in order to get the very large peak during 1957 on the chart, but it has made the rest of the graph very misleading. In general flare activity increases along with sunspot activity in rather direct proportion to it. Late in 1957, September and October, there was a very large rise of the order of a factor of five to six in the flare activity index. As far as I know, this was the greatest flare activity that has ever been observed on the sun.

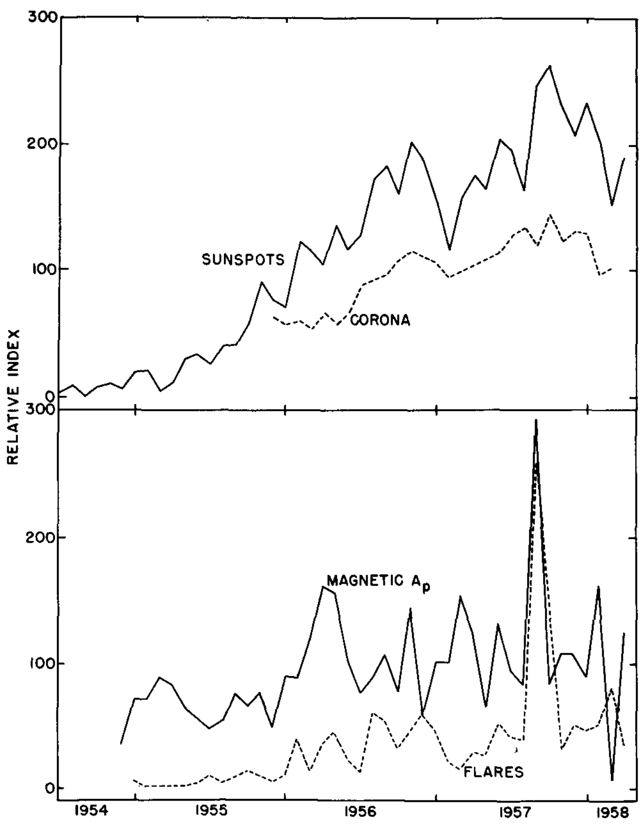

FIGURE 103. Monthly means of sunspot number, coronal radiation, geomagnetic A_p index and flare activity.

Essentially this summarizes what we know about the ultraviolet ray and X-ray radiation from the sun. I could say more about it, but it is not the particular feature I want to talk about tonight. There have been attempts to correlate some of these features to weather on the earth, for example, the work by Clarence Palmer. I think it rather difficult to evaluate what success he has had, so I will say no more about it.

There is another feature of solar radiation which we cannot observe but which seems to be extremely important, and it shows up in a variety of phenomena on the earth. This is what we call corpuscular radiation, actual radiation of matter from the sun. We have never observed a single case of matter actually leaving the sun and impinging on the earth. We have observed solar events that are suggestive of matter leaving the sun, and a variety of terrestrial events that imply that matter has left the sun and bombarded the earth, but it is a problem in which we must rely on indirect evidence, circumstantial evidence in a sense, because of the very nature of the solar atmosphere.

Let me first tell you about the evidence from the events in the sun's atmosphere. If we look at the sun in the light emitted by hydrogen, we observe huge clouds of material often shooting toward outer space from the sun. In many cases the matter is not decelerating as it would if it were moving only under the force of the gravitational field of the sun, and as far out as we can observe it we have every reason to assume that it will continue to escape the sun and fly off into space. We see a variety of such features in association with flares. Surge-type prominences, which are characteristically associated with flares, usually decelerate and fall back into the sun. Many, however, shoot up at constant velocity and gradually fade from view. The implication is that they shoot out entirely away from the sun. There are other times when comparatively small globules of material (and when I say small, I mean small in the sense that they are 90 million miles away and they look small) shoot out at high velocity from the sun with no deceleration. There are still other prominence types, which you have undoubtedly seen pictures of, that move out from the sun with rather high velocity.

Of particular interest is the phenomenon associated with the chromosphere that we call spicules. The whole surface of the sun is literally covered with spicules, which number of the order of 10,000 at any one time. As near as we can tell, they move upward with constant velocity and one can stretch his imagination a little bit and say that they continue to move on at this rate right out into space. They are only visible through a path extending about 10,000 kilometers into the outer atmosphere. We are not certain why they fade from view, but the general indication is that they simply become too hot to radiate in the visible part of the spectrum where we can see them. As far as we can tell, they do not decelerate so we can therefore infer that they may escape from the sun.

With radio telescopes we have observed events on the sun which indicate that material is moving outward at high velocity by the characteristics of their spectrum. These disturbances generate random noise at radio wave lengths and the frequency of maximum noise decreases with time. This implies that the disturbance is moving outward through the atmosphere. If we interpret this in terms of our conventional pictures of the solar atmosphere, we find that some types of disturbances move outward at a velocity of the order of 1,000 kilometers per second. In others, the velocity is very nearly the velocity of light.

This pretty well summarizes what we know from the sun itself about matter moving outward, with one exception. When we look at the spectral lines emitted by flares, they have a depression on the violet side. We can account for this if we assume that there is absorbing material between us and the flare that is moving toward the earth. The velocities of this material are consistent with the other velocities we have mentioned.

When we come to consider the evidence of solar corpuscles impinging on the earth, the evidence gets somewhat better. The very first flare observed was followed by a strong storm in the earth's magnetic field. It was also followed by an aurora or the so-called Northern Lights. The second flare observed, which was some thirteen years later in 1872, was also observed to have a magnetic storm following it with an associated auroral display. In a sense these were very unusual flares. They had to be in order to be observed with the crude observing techniques that were being used. However, since that time there has become a rather well-known relationship between sunspot activity and geomagnetic disturbances and auroral displays. That solid curve in the lower part of figure 103 indicates the variations of the magnetic Planetary A index through the sunspot cycle. The Planetary A index is essentially a linear measure of the variations in the earth's magnetic field weighted over several observatories throughout the world. When we look at the general sunspot cycle, there is a very pronounced correlation between variation in the earth's magnetic field and sunspot activity. Eleven year cycles go hand in hand and over long periods of time, so there can be no doubt that the two are very closely related. You certainly cannot claim a detailed correlation however, as there is really no clear-cut well-established association on a short time scale. Some flares seem to cause violent magnetic storms. Other flares of equally great size seem to have absolutely no effect on the earth's magnetism. Similarly some sunspots seem to produce magnetic disturbances, while other sunspots do not. This does not say that there is no relationship; it might simply imply a complicated relationship.

I hope you realize that when we talk about features of the sun, we are talking about symptoms of a disturbance on the sun and not necessarily the main disturbance. It is not even obvious that we have observed the main disturbance. All the phenomena we observe are correlated with each other. They all occur on the same areas on the sun and are all pretty much symptoms of a common disturbance. Just because we find a correlation of one particular solar event with a terrestrial event does not mean that that particular solar feature is causing the terrestrial event that we are observing. This seems to be the case with flares and solar corpuscular emission. We have to consider flares as an indicator of some more basic disturbance, which, in turn, is perhaps what has caused the magnetic storm and the aurora. Aurorae show pretty much the same correlations with solar activity that the magnetic storms do. On a long time scale they correlate very well. However, if we look very carefully at the data on a short time scale, say on a daily or weekly basis, there is no clear correlation, all of which indicates a complex relationship.

If you see an auroral display in Southern California, it is almost a foredrawn conclusion that there has been a flare on the sun. These low latitude auroral only come with, or after, rather large flares on the sun, and you observe them only near the maxima on the sunspot cycles.

The theory of geomagnetic storms and aurorae was really the first indication we had of the sun shooting matter into outer space. In both these events we have to pick something associated with the sun that interacts with the earth's magnetic field. The only logical choice is electrically charged particles that move from sun to earth. Now we can in a somewhat satisfactory manner present arguments to show that protons and electrons are shot out from the sun in electrically neutral streams. With a somewhat reasonable interpretation of the effects of these streams on the earth, we can account for many of the features of magnetic storm as well as the auroral features. Incidentally, the auroral zone seems to be very narrow, but, even so, we can predict it in the right place.

In the case of the aurorae, we have a little more direct evidence to go on. Since the streams of solar particles are interacting with the earth's magnetic field, we know that they are charged particles, probably protons. If protons come into the earth's atmosphere, they will capture electrons, and emit the spectral lines of hydrogen atoms. When we examine the light of the aurorae looking along the magnetic zenith, we do indeed see radiation emitted by hydrogen atoms that are moving toward us at rather high speeds of up to 3,000 kilometers per second. Now it takes roughly two or three days following the flare before an aural display occurs. If you interpret this in terms of the time it takes for the particles to come from the sun to the earth, they traveled something around 1,000 kilometers per second, which is consistent with the sort of velocity we observe for these incoming hydrogen atoms. We also observe, perhaps in the most direct way, radiation from the sun in the cosmic ray spectrum. At the time of large flares, there have been observed on several occasions large increases in cosmic ray flux. The largest increase occurred in February 1956, when the cosmic ray intensity in the lower energy part of the spectrum increased by several hundred percent and came from the direction of the sun. Cosmic rays traveling almost at the speed of light seem to come with almost all large flares. This is the most direct evidence we have of the sun emitting streams of particles.

Another piece of evidence comes from a somewhat unexpected source, the study of comet tails. When we look at the tails of comets, we observe matter moving out from the head of the comet away from the sun. In some cases this matter accelerates away from the sun. Initially, we thought this was caused by ultraviolet radiation from the sun, simply as a result of radiation pressure, but, having observed the part of the spectrum where we expected this to occur, we now know that the radiation is not of sufficient order of magnitude. The only other way we can account for this phenomenon is to assume that there is matter coming out of the sun that is pushing matter out from the comet. There have been attempts to correlate the motions in comet tails with geomagnetic storms with enough success to suggest that there is a relationship between the two events.

So all in all, the assumption that streams of particles leave the sun makes a reasonable picture. However, I would like to put in a word of caution. That is simply that we have no direct evidence of these particles coming from the sun to the earth, with the single exception of cosmic rays.

Now then, in looking for specific association with weather, we have taken a somewhat different philosophy from that which is usually taken; the usual one being an attempt to correlate some weather parameter averaged over a large area of the earth and over a large period of time, say a year, with sunspot activity. No real positive correlation has been demonstrated by this approach. Since the particles coming from the sun are funneled into a narrow belt in the polar regions, it seems to us to be a logical approach not to take space averages, but to look for specific events, specific occurrences, of the weather, and particularly for things that vary in some areas but not in others. Therefore, we will not consider space or time averages, but we will look for specific events.

All of the solar variations that I have mentioned are observed in the high atmosphere on the earth—at seventy kilometers or above where they are absorbed. The pressure at this height is about one quarter of a millibar compared to 1,000 at sea level. Our normal weather observations are restricted to the atmosphere below 100 millibars or so. Therefore, any attempt we make to correlate events on the sun with events on the earth has to leave a large gap in the atmosphere. In a sense this makes the study difficult. Even if we know what the solar event is that we are looking for, it is difficult to say what the resultant atmospheric events are going to be, because there must be some intermediate mechanism connecting the high and the low atmosphere.

Many arguments can be advanced against any relationship between the sun and terrestrial weather mainly on the basis that all of the known changes in solar radiation are absorbed in the high atmosphere and that they make up a small fraction of the total solar energy. The first objection is certainly true. If there is to be a solar weather effect, it has to be a large effect in the outer atmosphere in order for this tenuous tail to wag the big dog down below in the lower atmosphere, which has a great deal more mass.

When it comes to the question of just how much of solar energy changes, we find it difficult to give exact numbers. In fact, we have become suspicious of limitations that have been placed in the past on these variations. We can make some guesses as to how much it changes by extrapolating from those events which we have observed. In general, these estimates lead to the conclusion that the energy changes are small. The solar energy comes into the earth's atmosphere at the rate of about 10^6 ergs per sq. cm. per second. We observe variations on the sun, which suggest variations at the earth of the order of a few ergs per sq. cm. per second, probably about 10^{-5} of the total solar energy. But, this does not rule out the possibility of much larger variations. One recent suggestion that this is really the case comes from Professor Wenkler of Wisconsin, who has been using high altitude balloons to study high energy radiation and has picked up, quite by accident, very strong X-ray radiation at an altitude of 70,000 feet. If one extrapolates this energy back to the top of the atmosphere, the implication is that the X-rays are generated by high speed electrons coming into the atmosphere with an energy of something like 100 Mev. There are enough of these electrons to be equivalent to about 3 ergs per sq. cm. per second. From all indications about the nature of the streams of particles coming from the sun, protons come along with electrons at the same velocity. They, therefore have about two thousand times the energy that the electrons do. If we accept this, then these streams feed in energy at the rate of about 10^4 ergs per sq. cm. per second. Admittedly, this is in the realm of speculation. However, other ways of estimating particle density and the energy carried by solar corpuscular streams lead to similar results. To be perfectly honest, we can not restrict the sort of variations that occur in the solar energy received locally at the top of the earth's atmosphere to anything less than 100 percent of the solar constant itself.

Another question that I would like to raise is simply how much do we have to change the energy we put into the atmosphere before there is a noticeable effect on atmospheric circulation? I am not certain this is a question that can be answered. If it can, then I would like to have the answer. I do not think we really know whether it is one percent of the energy which normally comes in, or 10 percent, or 1 part in 10,000.

We have adopted the attitude of simply looking for a possible connection between the solar particles coming into an atmosphere and some atmospheric response. The work I would like to report on tonight stems from work done earlier by Shapiro of the Air Force Cambridge Research Center, in which he made a study of the persistence of atmospheric circulation. What he did was to lay out a grid of latitude and longitude over the United States and then study the time correlation in the heights of the constant pressure surfaces at the grid points. What he found in doing this sort of correlation was that the heights of the constant pressure surfaces were very persistent in time for periods of several days, or, in other words, the height contour pattern at a particular time correlated very strongly with the contour pattern a few days later. However, he also found that there were specific periods in which the persistence or correlation was not as good as it was at other times. These periods of breakdown in the persistence of atmospheric circulation were apparently correlated with geomagnetic storms. Since then, he has extended his studies to Europe using surface weather maps that cover a period of forty to fifty years. Some features of the initial correlation show up in all of his studies; some do not. Those that show up commonly in the studies indicate a very significant relationship between atmospheric circulation and geomagnetic storms.

The implications from Shapiro's work are that magnetic storms are followed a few days later by changes in atmospheric circulation. The nature of the change is such as to indicate that the primary change is in the long wave hemispheric circulation.

In our study, we have deliberately started with this point in mind, that is, we started with the hypothesis that there is an association between wave disturbances in the atmosphere and geomagnetic storms. To carry out the study we have used data supplied by the Department of Air Transport of Canada because we wanted data at the highest altitude where a sufficient area of the Northern Hemisphere was covered. The charts furnished by the Canadians were for the 300 mb level.

For purposes of this study, we chose the region from zero to 180 degrees west longitude. As one index we used the length of the contour line defining the position of the 30,400 ft height of the 300 mb surface between the extreme longitudes. This is a convenient index to use since it is quantitative and objective. We also defined another index intended as a measure of trough development. For each trough, we picked the points of inflection in a fixed contour. We then defined a trough index based on the ratio of the distance between the inflection points to the distance measured along the trough line from the point of maximum cyclonic curvature to the line connecting the inflection points. This is one way to get a measurement of the development of a trough and its intensity. There is, of course, considerable uncertainty and arbitrariness in such an index. The only saving point in this case is that the amount by which you can force the index for any particular trough is considerably smaller than the range you observe from trough to trough. One could always re-draw the chart in a somewhat different way, of course, but even by doing this you cannot force the index for any particular trough nearly as far as you can the indices for different troughs. In that sense, it has some useful characteristics. For the geomagnetic index, we took the Cheltenham "A" values simply because they were the most readily available. (All magnetic indices correlate very well.)

Since we were looking for a specific feature of the circulation as I have already indicated, we decided to look for trough development. Somewhat arbitrarily we chose the region between 180° and 120°W longitude and north of 40°N latitude as the test area. Actually, we picked this region with the thought in mind that it is a region well-known for being a more-or-less semi-permanent area for generation of troughs, and because it is the western boundary of our charts.

Using the magnetic data, we picked out certain key days on which the magnetic index was greater than 23, and on which the increase in the index from the preceding day was greater than, or equal to, twelve. In other words, on key days the Cheltenham A index, A_{ch}, had to be greater than 23 and on the preceding day it had to be less than eleven. The reason for this selection is that there are long-term variations in the magnetic indices characterized by a slow rise in intensity with subsequent decline, which need to be distinguished from the variations of a distinctly different type that represent a sudden onset of magnetic activity. It was the latter we wished to use. There were nineteen geomagnetic key days during the period of study running from October 1956 to March 1957.

We also found it desirable to define key days in terms of troughs. We did this by picking those troughs that were first observed in the test area three or four days after an A_{ch} key day. The day on which the trough was first observed then became the key day.

The average trough index for the 54 troughs studied is about 0.6. If we pick large troughs for which the trough index reaches a maximum value greater than 0.7, which is somewhat above the average value, and do a superposed epoch analysis of the number of troughs first appearing in the test area versus magnetic key days, we find the results shown in figure 104. Evidently there is a significantly larger number of these troughs that first appear in the test area three to four days following the magnetic key days than on other days. If we take all troughs, we do not find any such relationship. There is still a peak three to four days following the magnetic key days, but there are other peaks equally as large.

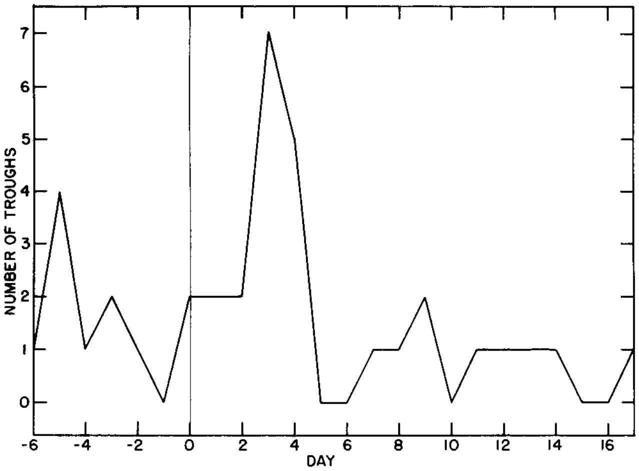

FIGURE 104. Number of large troughs appearing in the Alaska-Aleutian area before (—) and after (+) days of geomagnetic disturbances.

The next step in the analysis is to look at the development of the key troughs following their appearance in the test area. Figure 105A shows the trough index plotted against time for days following the time when the trough first appeared. The solid curve at the top is for the key troughs. In this case there were sixteen key troughs. That is, of the nineteen magnetic key days, sixteen were followed three to four days later by the appearance of a trough in the test area. The trough index, $\overline{I_t}$ increases to a much higher value

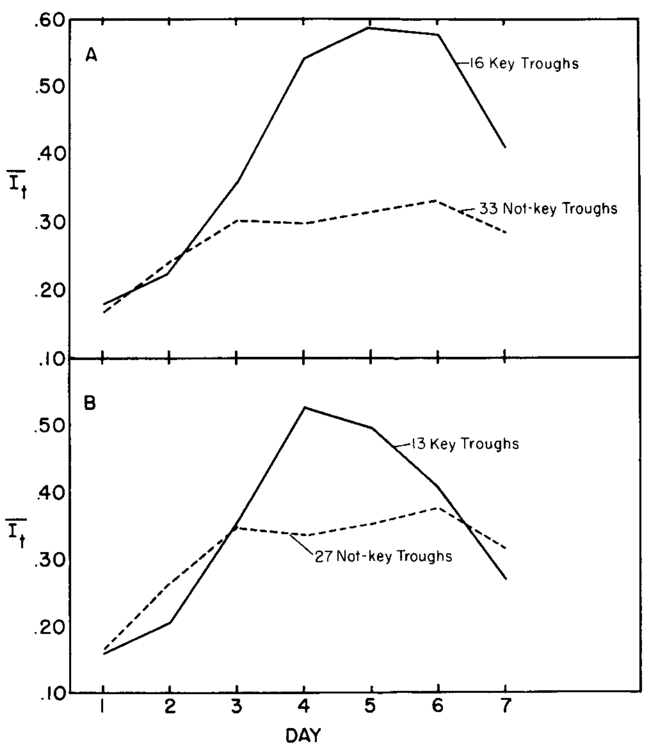

FIGURE 105. Average values of the trough index ($\overline{I_t}$): A. For 16 "key troughs" that followed magnetic disturbances and 33 non-key troughs. B. With three largest key troughs and six smallest non-key troughs removed.

for key troughs than for the rest of the troughs. The trough index is essentially a measure of cyclonic curvature of the trough, and does not necessarily measure the amplitude of the trough. Figure 105B illustrates the same plot with the three largest and six smallest troughs removed. This is used as a test to see if the result is determined by just a few extreme troughs. Again the same effect shows up. If we consider the fact that the troughs first appear three to four days after the magnetic storms and then take something like five days to develop, we expect to see an effect in the circulation something like a total of eight to nine days after the magnetic storm. I should point out that not all the troughs that show up three to four days after the storm get to be large troughs, and not all of those that follow at other times in the test area are small troughs. There are large troughs that are not key troughs, as I will point out later, but figure 105 definitely shows that those troughs that appear three to four days after a magnetic storm tend to become troughs of large cyclonic curvature.

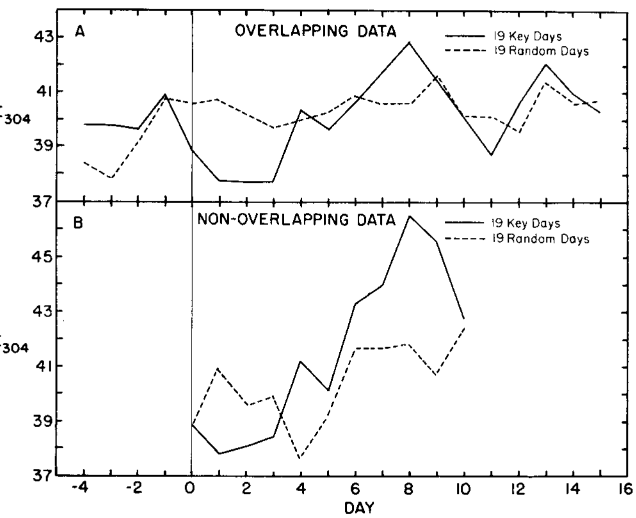

FIGURE 106. Average value of the counter index (I_{304}) for days before (—) and days after (+) magnetically selected key days. The broken curve represents means using randomly selected key days.

The next step in this study was to look at the length of the 30,400 ft. contour versus the magnetic key days. Figure 106 exhibits a superposed epoch analysis using the eighteen key days for the solid curve and nineteen random days for the dashed curve. The top set of curves in figure 106A use the data just as it was collected. The average period between key days of about nine days is less than the period of the analysis, so in many cases the same data were entered twice. In other words, the magnetic storm will occur, a trough comes into the picture and before it has time to develop another trough has occurred. Since this will tend to smooth out any correlation that is present, we separated the cases in which there was this overlap and plotted the curve for the non-overlapping days in figure 106B. Unfortunately, when you do this you cannot really define an average so it is difficult to test significance. However, the peak at about eight days is still very much evident in the non-overlapping data.

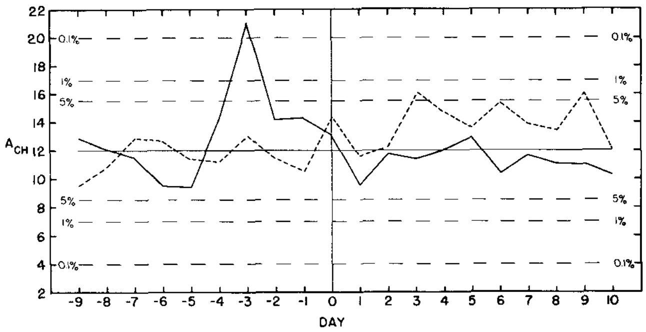

FIGURE 107. Mean value of the geomagnetic index (A_{CH}) for days before (—) and days after (+) the 25 days when trough type A or B (solid line) and the 28 days when type C (broken line) first appeared in the test area.

At least it is suggested rather strongly that this eight-day lag between the magnetic event and the development of the trough is really there.

In inspecting the synoptic maps it is quite evident that the trough that developed into a larger trough exhibited tendencies to have a preferred position with respect to the surface of the earth at maximum development. These preferred positions seemed to be over the West Coast and East Coast of the United States. Therefore, we deliberately divided the troughs into three categories. Those troughs that develop to maximum intensity over the West Coast of the United States were called type A troughs; East Coast B; all others C. Type C again will include some of the large troughs. We are not throwing all the large ones into A and B when we make this separation.

If we then pick out key days for troughs defined in terms of troughs first appearing in the test area three to four days after the magnetic key day, the A and B troughs exhibit the behavior illustrated by the solid line in figure 107. The peak comes at minus three days, as you would expect it to. For the type C troughs, which are the group that do not reach maximum intensity over the East and West coasts, there is no such relationship. Alternatively, we may illustrate the difference between A and B and C troughs by means of a contingency table.

TROUGH TYPE	A_{CH}	NO A_{CH}	TOTAL
A & B	14	11	25
C	4	25	29
TOTAL	18	36	54

CHI-SQUARE TEST P = .001

The key troughs, which are those that first appear in the test area three to four days after the magnetic key day, are placed in the column labelled "A_{CH}". All other troughs are placed in the column labelled "No A_{CH}". The first row contains the A and B troughs lumped together. From a total of 25 A and B troughs, fourteen first appear three to four days following the magnetic key days. There are eleven that appear in the test area at some period other than three to four days. I might point out that we have nineteen key days. We are taking two days for each key day so there are 38 days out of six months, and we get

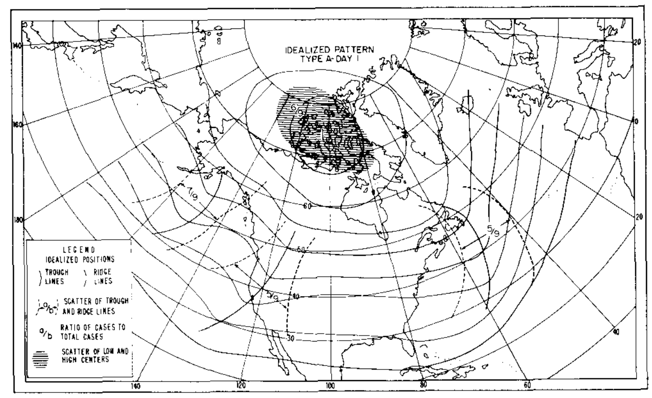

FIGURE 108. Idealized 300 mb chart, Type A, Day 1.

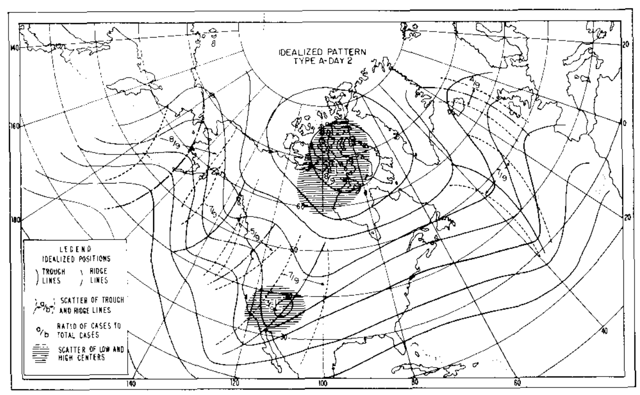

FIGURE 109. Idealized 300 mb chart, Type A, Day 2.

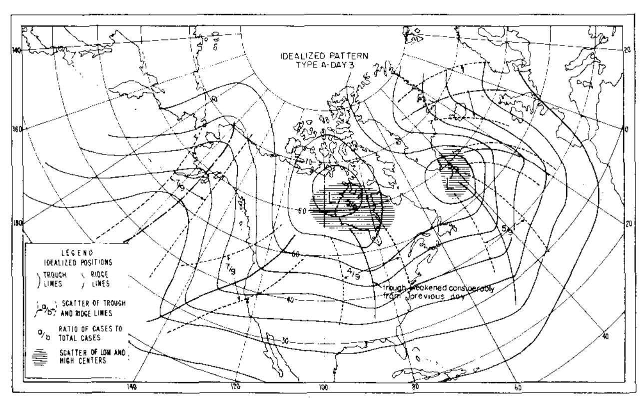

FIGURE 110. Idealized 300 mb chart, Type A, Day 3.

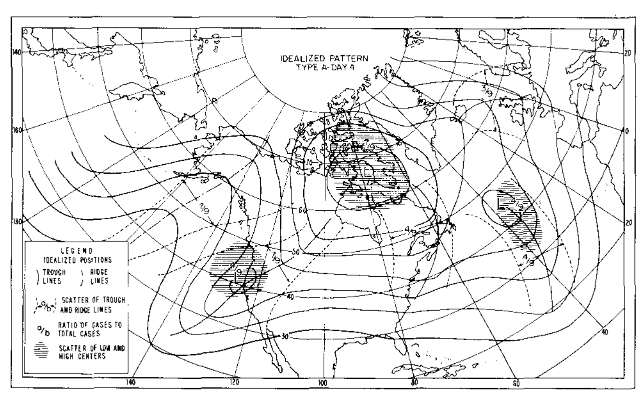

FIGURE 111. Idealized 300 mb chart, Type A, Day 4.

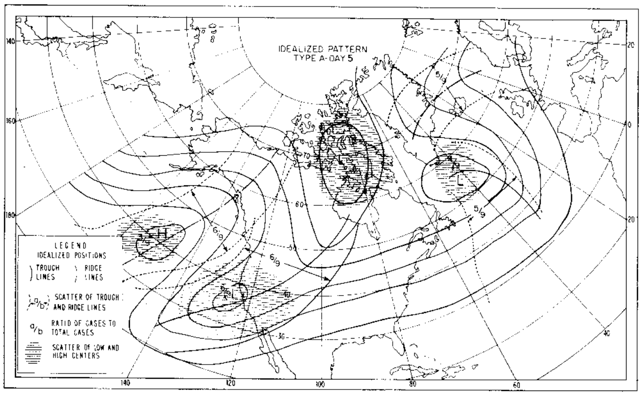

FIGURE 112. Idealized 300 mb chart, Type A, Day 5.

something over half of those troughs showing up in the test area within those 38 days. For the type C troughs, the corresponding numbers are 4 and 25. The probability of such an arrangement of numbers is something like 1 part in 1000, which is significantly different from a random relationship. We probably have not proven anything conclusively by these arguments. I, myself, am not a statistician, and I am in general quite skeptical of statistics because of my lack of understanding. However, I think it is at least very suggestive from this analysis that, following an abrupt magnetic storm, the troughs at the 300 mb level that appear in the Aleutian Island area three to four days after the magnetic storm, tend to develop into large troughs, become major perturbations in the circulation and come preferably over the West and East Coasts.

We have attempted in the series of idealized charts shown in figures 108-112 to show a typical development of a type A trough. These charts were drawn by superimposing the actual synoptic charts. A total of nine cases of type A troughs were chosen and the fractions entered in figures 108-112 give the number of cases in which the trough and ridge lines fell within the indicated boundaries. These charts presumably show the typical development of any one of these troughs. If you focus attention on the two troughs over the West Coast and Gulf of Alaska on day one, you will notice that the trough over the West Coast deepens some as it moves eastward on day two but subsequently fills. The trough over the Gulf of Alaska, however, deepens steadily into a cut-off low. The first trough is considerably east of the test area, and the latter is in the test area on day one. The troughs that are in the test area on day one deepen markedly as the statistical analysis suggested. We have constructed similar charts for the type B troughs and these show similar developments over the East Coast rather than the West Coast of the United States. As I have said before, we have not really proven anything, but the consistency of the results when looked at from several standpoints, at least suggests that the relationship is real. I think it is further supported by the fact that we started out expecting to find such a relationship. We deliberately chose the magnetic index and the circulation indices in order to test the hypothesis that following magnetic storms there is a marked perturbation in atmospheric circulation. This is just what we found, which in some ways adds confidence to the result.

DISCUSSION

Saur: Did you look back to see whether these troughs were unrelated when the storms actually reached the earth?

Athay: Our maps do not extend far enough to show this. There is one thing I should point out, however. We have picked troughs in a particular area, and this is not to be interpreted as necessarily meaning that the development of these troughs is the major effect. As Namias pointed out, this could be a result of developments someplace upstream or downstream. If indeed we are detecting anything, it would be going too far to say that we have actually found a cause and effect relationship.

Schaefer: In regard to indices, did you inspect this particular series of data before you made these indices, or did you make the indices first from prior data?

Athay: Before I answer I should say I did not do the work I am reporting on. It was done by someone else, and it has been checked independently by other people. The particular indices used were chosen after a preliminary inspection of the first three months of data.

Namias: Is it possible, if your results are statistically significant, that you are getting an evolution via the connection between magnetic activity and circulation features of the atmosphere? The magnetic index is not a unique characteristic of the solar activity as I understand it.

Athay: This is always a possibility. All I can say here, is that Wolf's theory has not been generally accepted by geomagneticians. Furthermore, the large geomagnetic storms are indisputably of solar origin, and the general solar control is evidenced by the correlation with the sunspot cycle. It may be a backwards sort of correlation, but the fact that we are finding atmospheric effects *following* magnetic storms implies that the relationship is the other way around is the reverse of what Wolf suggested. Whatever is causing the magnetic storms produces changes in the circulation, rather than the reverse. If it were as Wolf suggested, we should expect to find changes in circulation coming before the storm instead of behind the storms.

Revelle: Not necessarily.

Athay: Perhaps, but it is difficult to visualize any mechanism that would lead to such a relationship.

Charney: Do you ascribe any unusual significance to a three to four day lag, or is this the lag that gives you the best correlation?

Athay: This lag may be caused by the test area, and it is not necessarily a significant result. It may be that the event is occurring down or upstream.

Revelle: I am surprised at the conservative figures of the radiation flux.

Athay: I was conservative deliberately. Frankly, the estimates range through several orders of magnitude. It is not obvious now just where you should place the order of magnitude. One can give arguments, as Chapman did, that the flux is of the order of one-tenth of the solar constant. Others may raise the estimate up to the solar constant or lower it several orders of magnitude. We hope that the satellites will be able to furnish a conclusive answer, but it has not been done yet.

Namias: There must be a test of this hypothesis. This type of trough development into the southwest, is frequent enough so that it might be worthwhile going back in the past records for special cases. There are certain months when it has a tendency to occur, and other months when it does not occur at all. By separating these months, perhaps you could clarify the relationship with solar activity.

Athay: Would there be data available? We have done the same thing for the past winter '57 and '58 and we generally find the same result. There are some differences in detail, but the same general results are still found.

Namias: In that location, or did you change the location? How many troughs occurred?

Athay: The same test area was used but I do not know how many such troughs there were. I have not followed the later part of the analysis.

Namias: At M.I.T. a student tried to correlate the success or failure of forecasts with solar events, but I do not believe anything conclusive has come of it. Of course there are times when a trough of this nature develops which is not forecast so there is plenty of room for another element not presently considered to come into the picture. On the other hand, many successful predictions of such trough developments are made routinely.

Athay: Especially on a time scale of the order of the one we are considering.

Namias: Just before I left, I had the occasion to see some work prepared by Teweles of the Weather Bureau, in which some rather extensive maps have been constructed up to the 25 millibar level. Some of these involve cases where there are major changes in the high levels of the atmosphere including cases where there was very rapid warming. As Charney indicated, I think that the coupling between whatever goes on below and with events at high altitudes is very important. Up to now this has been more or less neglected because of lack of data.

Isaacs: The index of magnetic change is purely a terrestrial thing, isn't it?

Athay: It is a terrestrial effect, but it is related to solar activity both empirically and theoretically. We are using it as a measure of solar corpuscular radiation.

Isaacs: How does this correlate going back in time—the solar activity that you consider in something like cycles if you consider the sunspots?

Athay: It correlates very well on a long time scale. If you look at short term correlations, it is not nearly as good, however, except for the large storms.

Isaacs: But how can you go back to this kind of weather condition?

Athay: The charts that are available for this elevation do not go very far back.

Isaacs: But can you recognize these troughs from sea level data?

Athay: Presumably there is an association between the 300 mb trough and features on surface charts. Shapiro's work went back some forty years using surface charts and 700 mb charts. He was not looking for particular features; he was looking for any change in circulation. He found that if he deleted the years around sunspot maxima the correlation between the persistence of the atmospheric circulation and geomagnetic storms was enhanced.

Namias: It is rather difficult to relate simply sunspots to the sea level pressure patterns. As Athay indicated, all sorts of studies have been made not revealing very much.

Charney: What is the chance that disturbances in the lower atmosphere can produce magnetic storms?

Athay: One reason it is not believed is that many of the storms of the particular type we are considering occur simultaneously over the earth, which suggests that they are caused by something a long way away from the earth. The theory of solar particles interacting with the earth's magnetic field indicates that the initial disturbance occurs at about five earth radii out. This is one reason. Another reason is that many geomagnetic storms are so closely associated with solar phenomena as to immediately dismiss any alternative hypothesis.

Fleming: In your study criteria, Shapiro selected these events which are related to magnetic storms. What sort of events would lead up to these situations? This is not the beginning. There is nothing very difficult to explain about the indices. These are something to merely measure. What might be concurrent with your magnetic storm and increase the indices by eight days?

Athay: I cannot answer that.

Revelle: The underlying physical assumption is that when the magnetic storm occurs, something also happens that initiates the wiggles in the contours. The question is, if they are not due to magnetic storms, what other types of event could happen eight days before this happening?

Schaefer: Revelle, if it is not the magnetic storm, what could it be?

Revelle: The magnetic storm can only act at the time it exists. It can not act at some other time.

Fleming: I am thinking of something that leads up to these meteorological features. This is not the beginning. What could precede these features?

Revelle: How does a magnetic storm act? Either both the magnetic storm and this event are caused by something else, or the existence of a magnetic storm brings about this event.

Namias: During certain months the monthly mean planetary wave patterns favor a strong ridge over the eastern North Pacific and an associated strong trough over the southwestern United States. In such cases the type of development indicated by Dr. Athay will be frequent. Each daily trough and accompanying cyclone will plunge into the Great Basin, deepening as it moves. In other months such trough developments may be absent. If this is a solar effect, it should be shown perhaps most strongly by a study of monthly means.

Revelle: There is no observed immediate relation between magnetic storms and the visible radiation penetrating in the lower atmosphere. Wolf has claimed that there is, but no one else has been able to find it. We can correlate visible radiation with magnetic storms that occur two or three days later. It is not the radiation that produces it but something else. All we can observe is something we see visually. What Wolf did was try to correlate the geomagnetic storms with the occurrence of an active region just coming around the sun. He claimed to find such a relationship but no one else has been able to verify it and it is generally discounted as a theory for storms. There are recurrent storms—magnetic storms which occur over a 27-day period that have defied any attempt to correlate them with any visual features. It might very well be that you get a greater or lesser absorption of a visible radiation in the high atmosphere. I am bothered about the machinery, the means of getting the energy from the high atmosphere to the low atmosphere. This has been one of the arguments against such a relationship since no one knows the height of the atmosphere. Winkler suggests one possible mechanism which you can perhaps settle by radiation.

The presence of noctilucent clouds may indicate the same high altitude process of some nature.

Athay: The first column of the contingency table contains essentially the key troughs which determine the three to four days lag after magnetic lag days. The other column is simply the remainder. The point is that most of the A and B troughs come after magnetic key days, but very few of the C type do. We have not said anything about energy mechanisms, we just assume that particles are radiating from the sun, and we look for some effect on atmospheric circulation.

SECTION II

THE BIOLOGICAL EVIDENCE

CHAIRMAN'S STATEMENT

OSCAR E. SETTE

From the evidence that has been put before us from the fields of meteorology and oceanography, it appears clear that there has occurred during the past year, a substantial modification of ocean conditions, at least in the eastern part of the Pacific. We have been looking at this physical evidence, trying to understand the nature of the physical changes, their extent and their possible causes.

We are now about to look at the biological evidence, to try to find out whether the organisms that live in the sea reacted to these changes, and if so, in what way,—how they, rather than we, looked at the physical changes, if you please.

The able organizer of this meeting, Professor Isaacs, has programmed a series of papers well covering the biological field. He also, at the opening of the meeting, introduced the speakers. For biology they bring evidence from the phytoplankton, from the zooplankton, and from the fishes, both as to their distribution as adults, and as to their most important life process, that of reproduction.

I hope the speakers will not mind if we continue in the informal style, inviting discussion from participants whenever a point needs clarification, or whenever additional evidence or an additional thought can be offered.

Unfortunately our first contributor could not be here, but fortunately, Dr. Haxo, who is thoroughly familiar with the material, will relate Dr. Balech's findings to us.

THE CHANGES IN THE PHYTOPLANKTON POPULATION OFF THE CALIFORNIA COAST[1]

ENRIQUE BALECH
(Read by Francis T. Haxo)

Since the studies of the Scandinavian planktologists, particularly those of Cleve at the turn of the century, the microplankton have been recognized as potential indicators of the origin and characteristics of water masses. In the intervening years, the taxonomy of the more important phytoplankton constituents has been advanced and a number of studies have been published in which the distribution of phytoplankton has been carefully correlated with analyses of the chemical and physical features of the surrounding water masses.

Partcularly noteworthy are the studies of Hardy in the South Atlantic and Antarctic (Discovery Expedition), of Steeman Nielsen in the Pacific (Dana Expedition), of Peters and Käsler in the Atlantic Ocean (Meteor Expedition), of Gaarder in the North Atlantic (Cruises of the Michael Sars), and of Graham and Bronikowsky in both the Pacific and Atlantic Oceans (VII Carnegie Cruise). These have provided a great deal of carefully analysed data, especially concerning the dinoflagellates of the genus *Ceratium*. To these we can add my own studies, which have covered the following areas: the south of South America, the Antarctic seas, the Mediterranean, and the Atlantic littoral regions of France and the English Channel.

From these studies it is apparent that, within limits, the broad temperature realms of the oceans have a characteristic flora. While some species are fairly tolerant of wide ranges in temperature, others are restricted in their temperature distribution and are limited, for example, to tropical or to cold waters. Thus the species composition of a given water mass will reflect something of its present and/or previous temperature history. This is particularly valid when assemblages of indicator species are enumerated in characterizing a given water mass.

As background information on the area of present concern, the coastal waters of California, we have among others the studies of Allen extending over a period of more than twenty years, the studies on diatoms by Cupp, the investigation of Sargent and Walker on the relationships between diatom populations and water eddies, and the unpublished studies by Holmes on the phytoplankton of the North and Eastern Pacific. There are also the detailed studies by Kofoid, primarily of a taxonomic nature, on the dinoflagellates and tintinnids, a group of ciliate protozoans.

[1] Contribution from the Scripps Institution of Oceanography.

COMPARISON OF THE QUALITATIVE COMPOSITION OF THE PHYTOPLANKTON IN LA JOLLA DURING THE YEARS 1938-1939 (ALLEN'S COLLECTIONS) WITH 1957-1958

When in December 1957 it became evident that the planktonic populations in La Jolla were of an unexpected warm water, even tropical character, I began a comparative study of recent collections with Allen's samples from the nine month period August 26, 1938 through May 1939, when the water temperature averaged below the long-term mean with the corresponding recent collections in 1957-1958. For this study I have re-examined Allen's collections so that differences in species identification by different observers could be eliminated. Both collections were made from the end of the pier at the Scripps Institution (La Jolla).

Before presenting the results, some mention should be made of the differences in sampling methods employed. Allen was primarily interested in quantitative studies. For that reason he collected small volumes of sea water and the plankton were concentrated by settling. For my studies, the collections were made with a 62μ mesh net. The volume sampled was many times that of Allen for any given day. Because of these differences in methods and in volumes sampled, we can expect that the larger forms would be better represented in my collections. The smaller forms not retained by the net would be better represented in the plankton concentrated by Allen's settling technique. However, because of the greater frequency of Allen's collections (daily), the qualitative differences due to sampling methods might be expected to disappear when collections of a whole month are considered. Notwithstanding the difference in methodology, the differences in composition of the plankton described below were considered to be of real significance.

The results are presented in a somewhat simplified way in figures 113 and 114, in which the more important indicator species of dinoflagellates, diatoms and tintinnidae are listed according to their appearance by months in the two periods under consideration. Such a simplified presentation can give only a rough approximation of the character of the plankton, since stress was placed here on the more persistent species and abundances are not indicated. Considera-

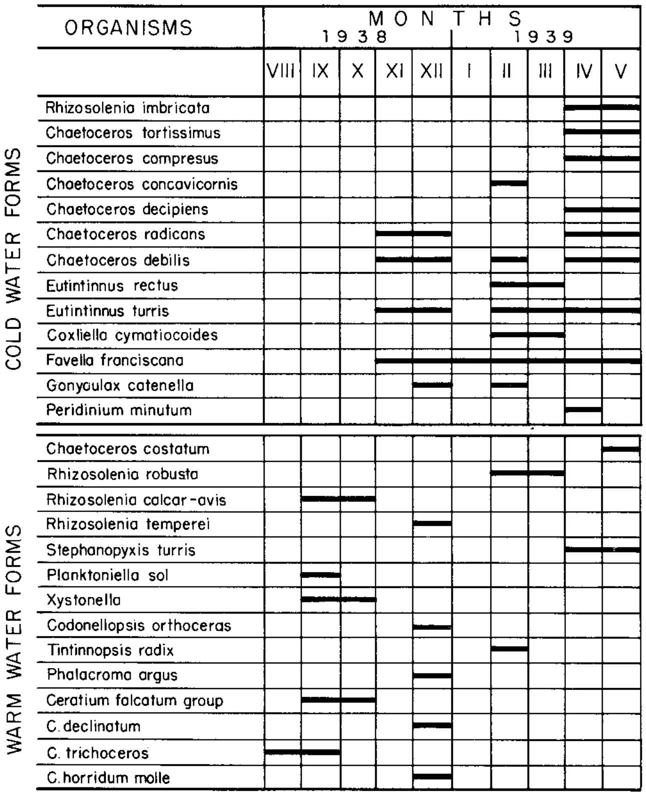

FIGURE 113. Occurrence during 1938-1939 of dinoflagellates, diatoms and tintinnidae in the plankton at La Jolla.

tion of the sporadically occurring species only sharpens the contrast between the two years. The figures do not bring out, for example, the significant differences observed in the planktonic populations of January through the first half of February 1958 and those of March 1958. While the former were relatively homogeneous, the latter showed considerable variability in composition.

The most important general conclusion is that the phytoplankton populations of 1957-1958 were atypical in that, on the one hand, warm water and tropical phytoplankton were abundantly represented (largely absent in 1937-1938) and, on the other hand, the appearance of cold water forms was delayed and restricted. No doubt this is a reflection of the warm water prevailing during the past year.

Looking at the two periods in greater detail, we note that Allen's samples collected during the summer of 1938 contained some warm water species but none of the more rare and tropical species, for example, *Ceratium carriense, C. lunula,* and *C. ranipes,* which are well represented in my pier samples of the past year. By late September of 1938 all but one quite tolerant tropical species of *Ceratium* had disappeared. By contrast, in 1957, many tropical species persisted throughout the first part of autumn. Furthermore, in December of 1957 there was an unexpected development. The planktonic population took on a most tropical character in spite of a relatively low water temperature. This was especially striking for the week of December 8-14, when two tropical species of *Ceratium* were collected for the first time. It is noteworthy that during the period December 19-22 of 1938 there was a slight indication of the same phenomenon in Allen's samples, but very much less marked and involving species less truly tropical in nature.

In the early weeks of 1958, as in 1939, the phytoplankton was not very abundant. In the 1958 samples, however, warm water plankton continued to dominate until the middle of February, at which time some changes were noted. We still found some tropical species, but many noted previously had disappeared. On the whole, the plankton showed a relative abundance of diatoms, including some that thrive in cold

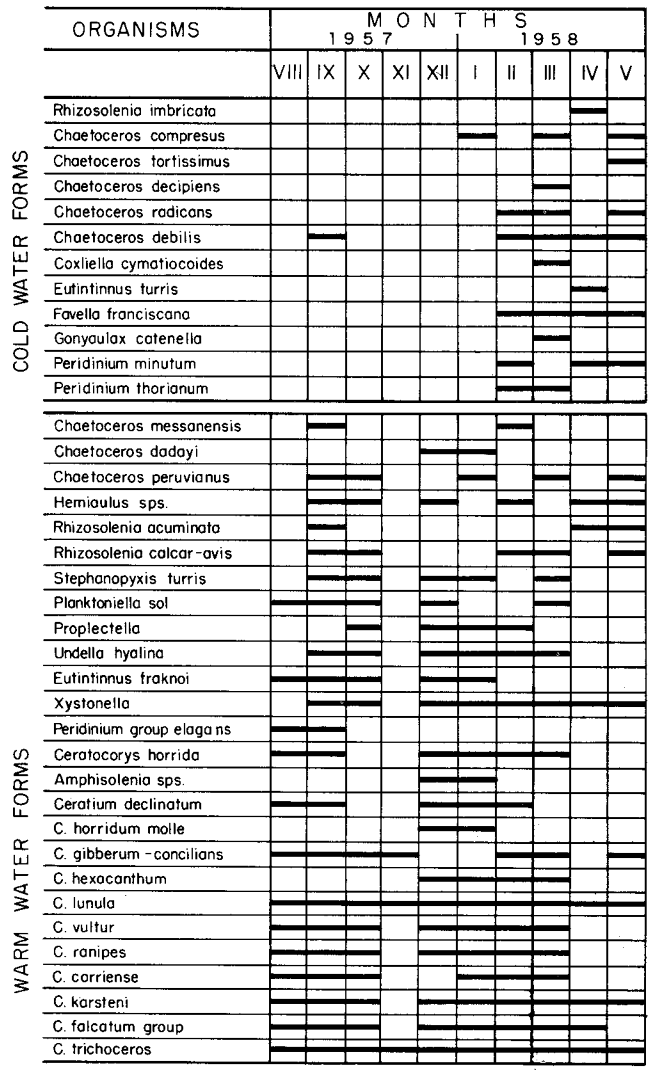

FIGURE 114. Occurrence during 1957-1958 of dinoflagellates, diatoms and tintinnidae in the plankton at La Jolla.

water. Present also were several other protists (*Peridinium* and tintinnidae) that seem to prefer cold and temperate waters. During the last part of the month there were noticeable fluctuations in the character of the plankton from a population typical of warm water to a mixed one, with several northern species. By contrast, in 1939, warm water species were not found in January and the plankton of February was dominated by tintinnidae, generally cold and temperate or cosmopolitan species, a situation which continued through April of that year.

Of particular interest is the characteristic presence in the February 1939 collections, as well as those of December 1938, of chains of *Gonyaulax catenella* and its presence in only a single sample of the 1957-1958 collections (a single chain of two in a sample of March 13). From laboratory experience it is known that this dinoflagellate has markedly lower temperature preferences for growth than other *Gonyaulax* species commonly found in the La Jolla area.

In March of 1958 the collections for the first time showed many common features with those of 1938. In general, the plankton were of a mixed and fluctuating character, dominated by zooplankton, especially larvae, with several of the same cold water species of tintinnidae, diatoms and dinoflagellates.

April 1958 showed a regular increase of temperature but not a very great change in the composition of the plankton. However, some typical warm water species were again present, but these were in general poorly represented.

May 1958 showed significant changes by the middle of the month, with a dominance of dinoflagellates, especially *Gonyaulax polyedra* and *Diplopeltopsis minor,* a truly cosmopolitan species. There were several cold or temperate species of diatoms, but the most conspicuous elements of the plankton are widespread species. Also present was a typical warm water diatom, *Hemiaulus membranacea*. By contrast, May 1939 showed plankton with little changes from that of the preceding month. The last half of May 1939 was dominated by *Prorocentrum micans.*

From all this we gain a strong impression of marked difference in phytoplankton composition in the years under comparison. The difference attenuated after 15th February but did not disappear, as shown by the persistence in 1939 of the typical cold water forms and the almost total absence of warm water species so much in evidence in 1958. The greatest anomaly embraced the period, December to the middle of February.

An interesting feature of the plankton studies is that an invasion of warm water in the San Diego region seems normal for December. This feature was also observed in plankton samples for December 1953, available in the Marine Botany Laboratory at the Scripps Institution of Oceanography. These were largely dominated by *Prorocentrum micans,* but samples of December 17 and 18 contained some typical warm water species. On the whole, the plankton composition was very similar to that of December 1938 and quite different from that of December 1957.

GENERAL SURVEY OF THE CALIFORNIA PLANKTON IN APRIL 1958

Although it was not possible to extend observations during the period of greatest anomaly at La Jolla, opportunity was presented during April of this year to incorporate microplankton collections into the CCOFI cruise program. These were made by vertical and horizontal tows with a 62μ net at stations on the MLR grid extending from Baja to Northern California. Very rough weather restricted the planned coverage, particularly north of San Francisco.

Based upon preliminary examination of the collections, the general character of the plankton distribution is shown in figure 115, that of selected warm water dinoflagellates in figure 116, and that of the genus *Ceratium* in particular, in figure 117. It was indeed found that tropical to warm water forms were commonly present as far north as the 15°C isotherm and in some cases extended to the 14°C isotherm. According to previous investigations, species included

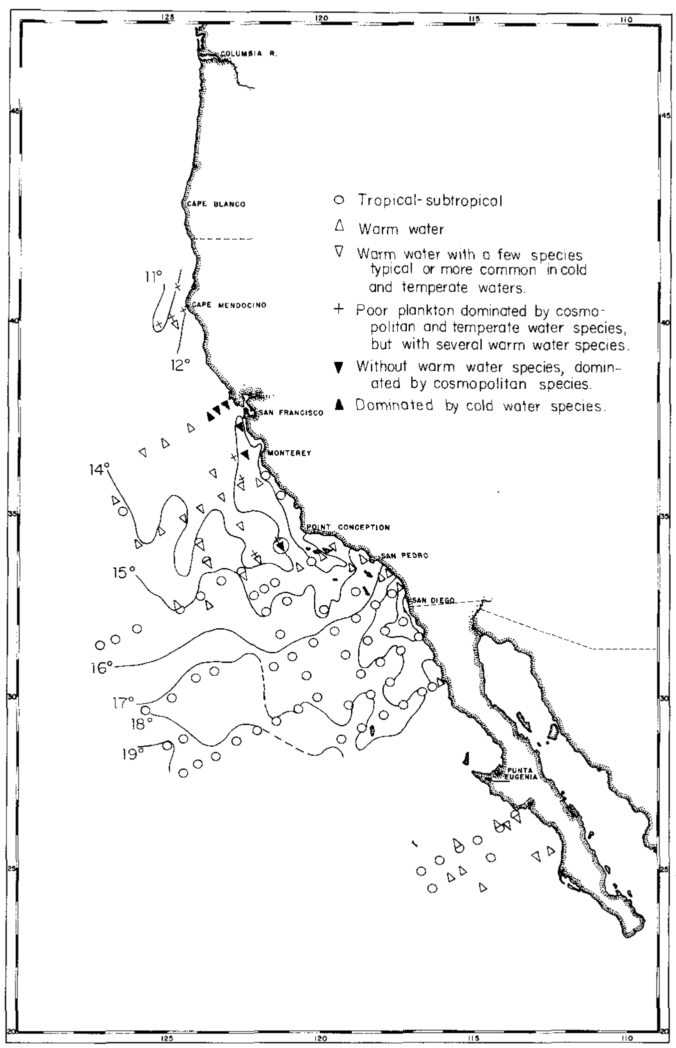

FIGURE 115. Distribution of micro-nannoplankton, March 29-April 28, 1958 (CCOFI Cruise 5804).

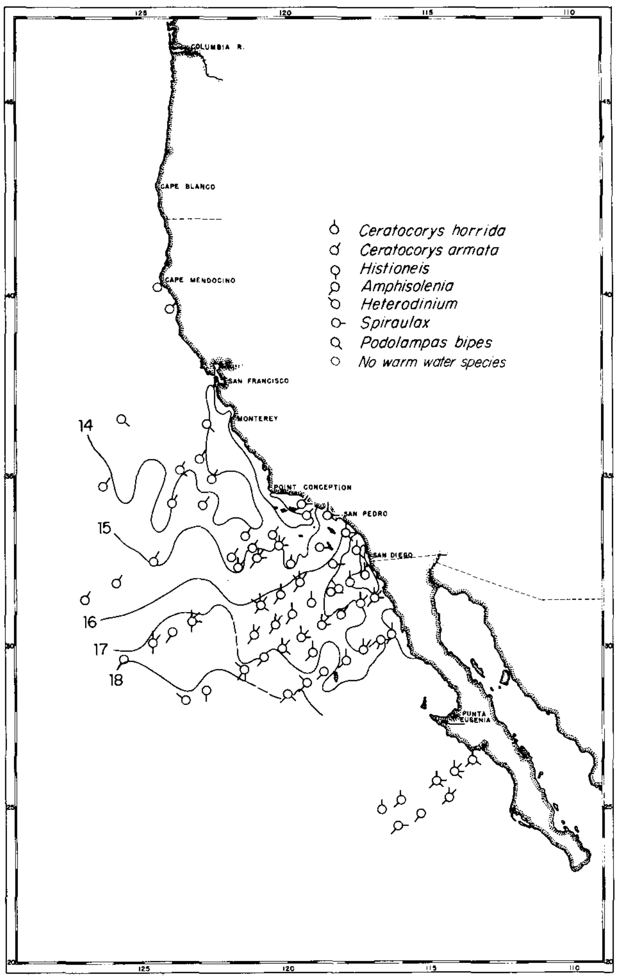

FIGURE 116. Distribution of some warm water dinoflagellates, March 29-April 28, 1958 (CCOFI Cruise 5804).

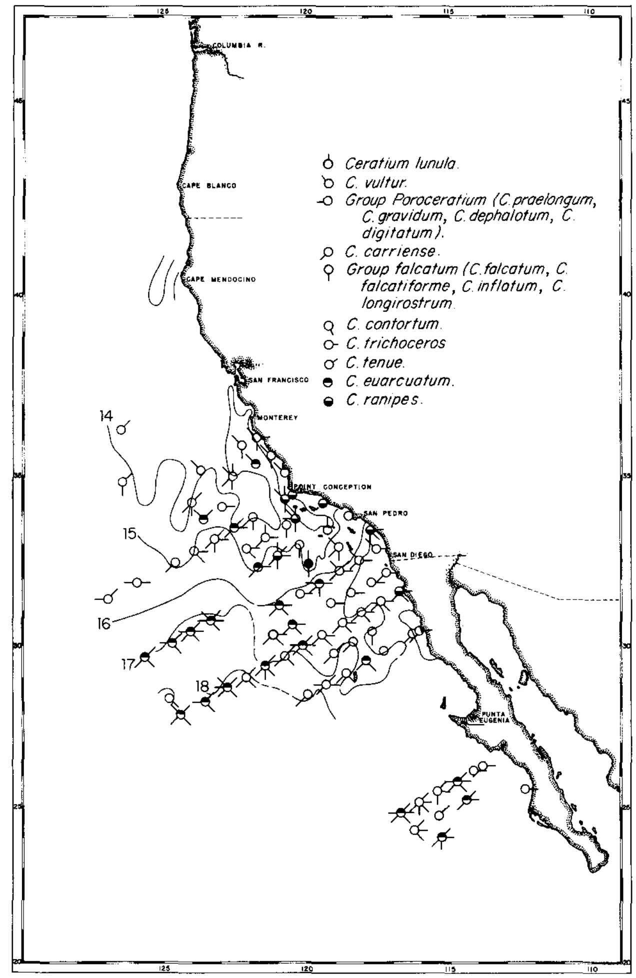

FIGURE 117. Distribution of Ceratia, March 29-April 28, 1958, (CCOFI Cruise 5804).

in this designation are not normally found in waters cooler than 18-19°C.

Looking to the north off San Francisco and Monterey, we see a different situation. Here the populations are dominated by cold water species; tropical or warm water species were not observed. The collections made farther north off Cape Mendocino, where the temperature was between 11° and 12°, present a surprising picture. Here we note the occurrence of several warm water species of *Ceratium*. Although all of these are tolerant species, they have never previously been found in such cold waters. Perhaps the drift bottle data may shed some light on this anomaly.

In the intermediate area largely delimited to the south by the 14° isotherm, the planktonic populations were of a mixed character. This is probably a reflection of the strong mixing of the water masses.

In conclusion, one is impressed with the striking change in the character of the planktonic populations from south to north and the far northward extension of typically warm water forms. Considering the normal temperature tolerance of these warm water species, it is tempting to ascribe this abnormal distribution to an active transportation to the north and a strong mixing of the waters.

DISCUSSION

Reid: Can you tell if the warm water species are from the south or from the west?

Haxo: I have raised the same question myself, but I believe that Dr. Balech's data do not permit distinguishing between these two alternatives.

Johnson: I am glad that he did not say that this actually established a transport. You are dealing with biological groups which respond to temperature conditions, and they reproduce in large numbers when the factors are favorable. You will notice throughout his paper he indicates the presence of warm water forms in cold water areas, or vice versa, which means that the seed material is really there, and where he speaks of temperature tolerance of a species, this does not necessarily mean that the species cannot get outside those temperature tolerances, but instead, these are the temperature tolerances within which it reproduces in large numbers. I believe we have to think of this when we interpret these things. If the water warmed up sufficiently off Cape Mendocino, we would have many more of these tropical and subtropical forms than are showing up there now. Not because they were carried in, but simply because the conditions became favorable for rapid reproduction; as you know, these organisms reproduce at a geometrical rate, so in a very short time propagate a considerable population. This does not mean that they have not been carried in, but that you have to think of the two things at the same time.

Haxo: Yes, one cannot look at the phytoplankton as simple drift bottles, which they certainly are not. All you need is a basic seed population, and if conditions are suitable for growth the population will increase in size. However, the source of the seed population, whether resident or transported, might be difficult to determine on the basis of present qualitative data. Perhaps the answer will be forthcoming when the collections have been more fully analyzed.

Hubbs: You did mention a few species that have not been found here before, such as tropical species of *Ceratium*. These are examples of temperature restricted distributions, in this case to warm water. The opposite situation is seen in the case of *Gonyaulax catenella,* a cold water form which actually cannot survive in warm water.

Haxo: This is certainly true. Unfortunately, information on temperature restrictions or tolerances is still largerly subjective. It would be very helpful if we had more extensive laboratory information on temperature tolerances of California coastal phytoplankton.

Sette: When you speak of temperature tolerance, do you mean mere survival, or, in addition, the ability to reproduce and maintain a population?

Haxo: Dr. Balech's approach has been that of a phytogeographer. His temperature designations are derived from information obtained at sea from other cruises from which it becomes known that certain species are to be found only in certain temperature realms. Therefore, based on this kind of information, they are thought to have certain temperature requirements and tolerances, presumably for survival and growth. This will remain an uncertain point until more exacting information is available. It is of interest that Dr. Balech has already encountered a number of dinoflagellates four or five degrees colder than previously reported.

Sette: In other words, these are tolerances as they are found to exist in the field.

Haxo: Yes, the tolerances are based upon observed distributions correlated with temperature.

Johnson: In a very few laboratories, cultures have been subjected to experiments to elucidate much of this kind of problem. There has been some work in Braarud's laboratory in which they studied a fair number of dinoflagellates for temperature tolerances. Some were also found to have different salinity tolerances, which may sometimes tend to have a reciprocal influence.

Haxo: Yes, these important studies have provided interesting examples of the complexities in the interaction of environmental factors on the growth of marine organisms and further indicate the desirability of extending efforts to cultivate phytoplankton under controlled laboratory conditions.

Hubbs: A suggestion (I think it is probably true of many of the invertebrates) is that when we have a seeming incursion of the southern forms, (we seem to be certain that it is an actual incursion) they will come up in a warm period and then tend to stay here even if the conditions are so bad that they die during the winter. Some of these that may have come up from the south are now living in water that they would not normally have reached simply because it has turned colder. The second point is a question regarding the 1939 data.

Haxo: The two periods were compared from late August through May. The temperatures of the 1938-1939 series were markedly lower (—0.85°C to —1.89°C) than those of the long term means (1917-1955) only during the months of October, November, February and March.

Johnson: I might mention here I had not realized to what extent Balech was looking at some of Allen's old samples which were concentrated by settling techniques. I have collections made with a No. 20 net which he might look over for some of these rare forms which he did not see before. I am quite sure that Allen did not pay much attention to rare forms, for his opinion was that it was the dominant forms that were important biologically, but they may not be the ones most important from the point of indicating changes in the oceanographic conditions.

Brinton: The phytoplankton reflects rather shallow environment compared to zooplankton forms, so that you can probably define depthwise the environments of these organisms better than you can many zooplankton samples. I seem to recall that Balech did find some forms relatively near to shore which he felt fairly certain were related to tropical waters. How did this compare to offshore?

Haxo: The general distribution of warm water dinoflagellates is brought out by figure 116. *Ceratocorys horrida* is an especially good indicator of tropical conditions and in April was found inshore as far north as San Pedro and far offshore from San Diego at the outermost station made along the 17°C isotherm.

Could I re-emphasize one point? Dr. Balech has dealt in all cases with assemblages of species, single members of which might be absent from adjacent stations. Assemblages of a half dozen or more species were used to indicate a given oceanographic condition.

ADDENDUM BY DR. BALECH

(Prepared following his reading of the transcript of the discussion)

We do not know actually how many species of marine dinoflagellates form cysts, although there is a general belief—far from proven—that most can do so. However, I should stress that these "seed" forms would give active forms with the approach of the conditions most suitable for each species. Thus, for warm water organisms, the main (evidently not the only) factor would be the increase of temperature. In this connection, it is well to remember that there exists what I call "ecological inertia" which delays the development of a population even after the onset of favorable conditions and the disappearance of a population after conditions become unfavorable.

Let us apply what is stated above to the situation at La Jolla during the past year. The theory of the appearance of the warm water population by reproduction (excystment and growth) *in situ* of a seed population requires a very noticeable and protracted increase of temperature, which had not happened. The water temperature at the surface at La Jolla was about 19-20°C. in September. In October it oscillated between 17.5°-18°. In November it was between 16-17°C. and by then the impoverishment of the warm water plankton was plainly noticeable, *i.e., Ceratium vultur, C. ranipes, C. carriense, C. karsteni, C. horridum molle* and the species of the falcatum group were not found in this month. It was the same for some of the most conspicuous warm water tintinnidae. But in December, with the temperature *still decreasing,* all these species plus some others such as *C. hexacanthum,* appeared again and were found consistently from the middle of December to March.

All this supports the conclusion that the populations observed at La Jolla did not arise locally but were transported from a center of subtropical or even tropical water. Whether this center was situated to the south or to the west of La Jolla, we cannot tell.

We tried to get some supplementary information from the samples of the April 1958 cruise (CCOFI Cruise 5804). From the distribution charts we cannot make any clear distinction between a possible southerly or westerly origin of the plankton. However, the distribution of *C. lunula, C. trichoceros* and *Spiraulaux jollifei,* species also found at that time in La Jolla, seems to give some support to a possible west northwest origin. It seems that these species were pushed against the southern coast and then, caught by a current from the south, carried northward, along with forms coming from the south. This explanation seems to fit especially such species as *C. lunula, C. hexacanthum, C. platycorne,* that prefer oceanic oligotropic waters, as shown by distribution data (Steemann Nielsen, Graham and Bronikowsky, and Peters).

The same mechanism, *i.e.,* transportation to the north, may account for the presence of several warm water species near Cape Mendocino, notwithstanding the low temperature of that region. When these species were found in cold waters, they were never far removed from currents having temperatures of at least 16° C.

All these species found near Cape Mendocino are tolerant. By this is meant that under unfavorable conditions, they are able to maintain at least an impoverished population. The species found near Cape Mendocino seem unable to maintain a population if the temperature is below 15°C. These data are derived from the collection studied all over the world. As pointed out by both Dr. Haxo and Dr. Johnson, we have very few data from laboratory experiments. All attempts to cultivate these warm water species have failed as far as I know.

Unfortunately quantitative data on the plankton distribution are not available. However, populations were designated as consisting of warm water plankton only if *many* warm water species were present. The somewhat simplified presentation in the graphs and charts of distribution do not bring forth the full weight of the evidence. For instance, in most of the samples taken at La Jolla in December through February, no less than 25 very definite warm water species were recorded including very intolerant forms such as *Ceratocorys horrida.* Dr. Haxo pointed out the interesting information derived from the distribution of this species.

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UNUSUAL FEATURES IN THE DISTRIBUTION OF PELAGIC TUNICATES IN 1957 AND 1958[1]

LEO D. BERNER

Of the zooplankton animals I have studied, two species of the pelagic tunicate group, *Dolioletta gegenbauri* and *Doliolum denticulatum*, are interesting and pertinent to the subject of this meeting. It appears to me that a discussion of only two species rather than a broad group may be a better approach to our problem.

These two species have served as good indicators of cool (California Current or sub-Arctic water) and warm waters (central or sub-tropical water). During several cruises, especially in 1949 and 1950 they were found at adjacent stations but very seldom at the same station.

Both species live almost exclusively in the upper 100 meters of the water column, with their major populations in the upper 50 meters. This is important because, with a vertical distribution such as this, they can be used to interpret happenings in the surface layers where temperature and salinity relationships are confused by annual variations and local modifications.

I have summarized the distributional data for the two species during March, June and September 1949-1952 in figures 118 and 119. These are based on the collections of the oblique meter-net hauls used in the regular fish egg and larvae surveys of the California Cooperative Oceanic Fishery Investigations.

The hauls sampled the upper 70 meter stratum in 1949 and 1950 and the upper 140 meter stratum in subsequent years.*

These figures indicate the percent of successful hauls for the species at any one station and are intended only to give a general idea of where the two animals live. Seasonal changes in distribution have not been taken into account and the shading represents only percentage occurrence, regardless of numbers taken in the hauls. In many cases the distributions of numbers would be quite similar. The percentages south of line 130 (Fig. 1), which extends seaward from Pt. Asuncion, Baja California, are based on only one or two net hauls. To the north most percentages are based on five or more hauls, many on ten or eleven. Although more cruises are included figure 136 gives a good idea of the intensity of sampling over the region.

Figure 118 shows the distribution of the cool water species of the pair, *Dolioletta gegenbauri*, along our coast. It may be seen that its distribution extends rather far south, especially near the coast. In the period since October 1950, when the fishing depth of

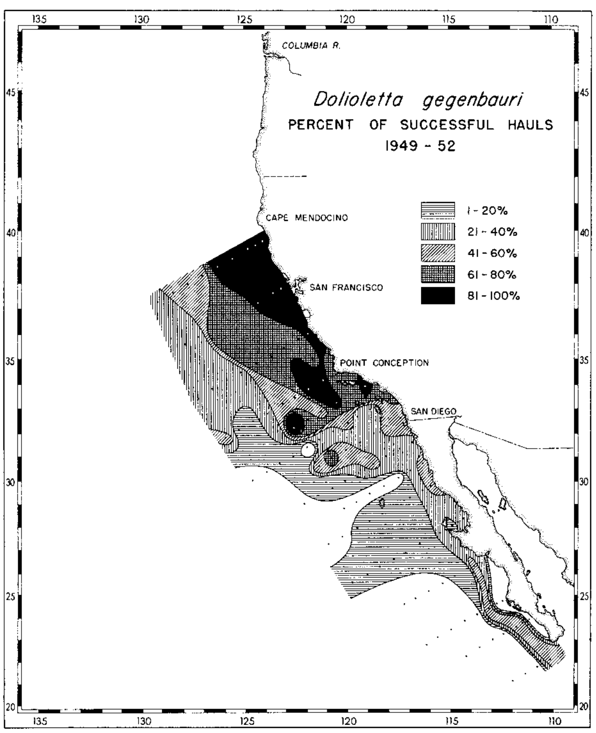

FIGURE 118. Per cent of successful hauls for *Dolioletta gegenbauri* during March, June and September, 1949-1952.

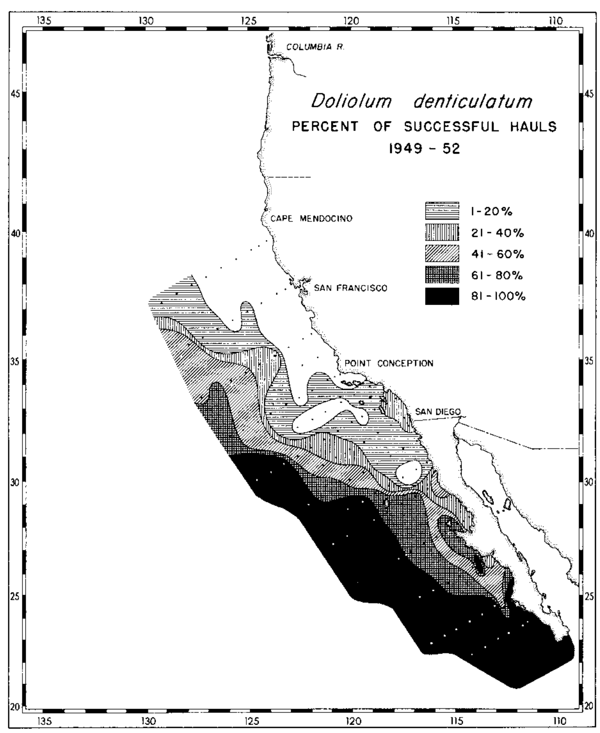

FIGURE 119. Per cent of successful hauls for *Doliolum denticulatum* during March, June and September, 1949-1952.

[1] Contribution from the Scripps Institution of Oceanography.

* For a description of the nets and method of hauling see Ahlstrom, E. H., 1954.

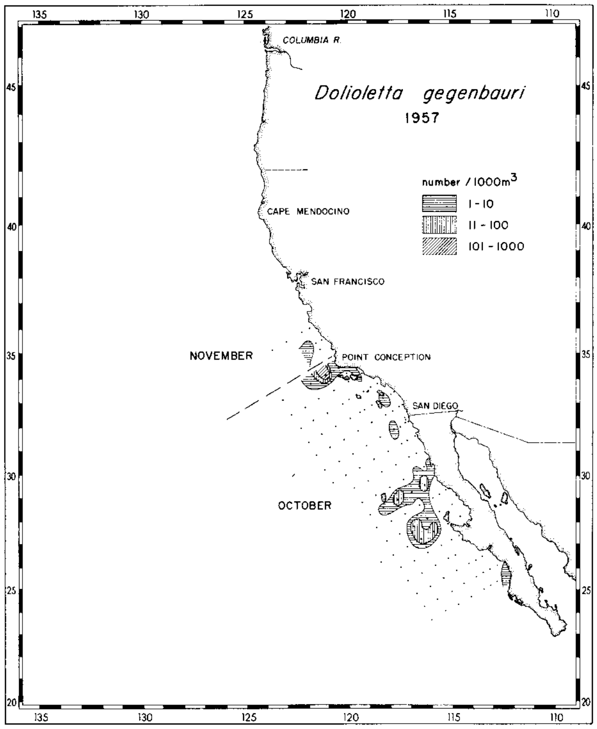

FIGURE 120. Distribution of *Dolioletta gegenbauri* during October 4 to November 8, 1957 (CCOFI Cruise 5710).

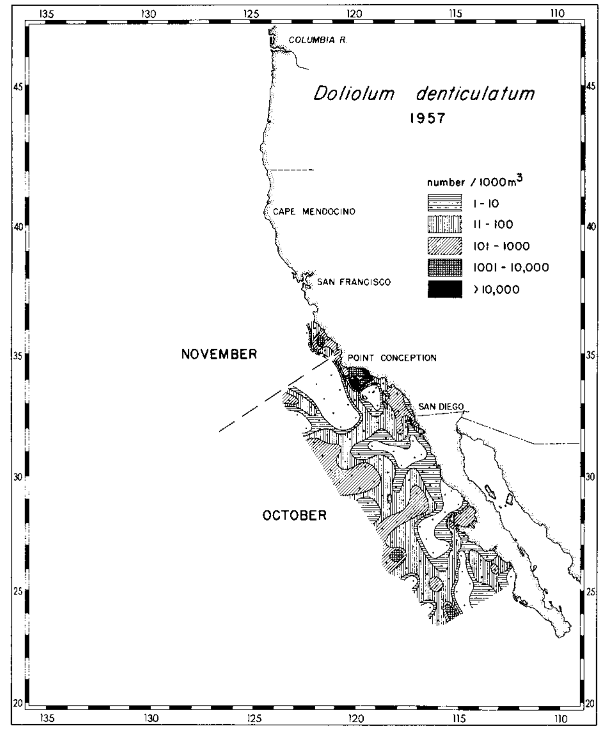

FIGURE 121. Distribution of *Doliolum denticulatum* during October 4 to November 8, 1957 (CCOFI Cruise 5710).

our net hauls was deepened from 70 to 140 meters, this has been a rather typical distribution.

The distribution of the other species, *Doliolum denticulatum,* is shown in figure 119. It may be seen that this species is the warm water analogue of *D. gegenbauri.* During the period 1949-1952, *D. denticulatum* did not occur near shore to the north of the Santa Barbara group of Channel Islands and only rarely north of San Diego.

In October 1957, *D. gegenbauri* appears spottily over the survey area with a rather large group east and south of Guadalupe Island. This might be called a more or less "typical" distribution (Fig. 120). An examination of the distribution of *D. denticulatum* (Fig. 121) however shows a rather marked difference from the "typical" distribution (Fig. 119). It occurred nearshore and north as far as the sampling extended, line 80 off Pt. Conception. In addition to mere occurrence, the maximum numbers were found in the nearshore area around and north of the Santa Barbara group of Channel Islands. The October cruise unfortunately did not sample north of Pt. Conception so I have taken the liberty of superimposing some stations from the November cruise to the north. The fit between the two is rather good at line 80 (Fig. 121). Before leaving the discussion of these two charts it may be of some interest to point out that *D. denticulatum* is missing from several of the stations where *D. gegenbaura* occurred far to the south.

The distributions of these two species during April 1958 again are unusual. *D. denticulatum* is found north almost to Monterey in the nearshore area (Fig. 122). A population center is found in the Santa Monica Bay area where usually at this time of year the species is not found. The offshore distribution is not considered unusual for this time of year. *D. gegenbauri* during April 1958 shows a very patchy distribution, which, to a lesser extent, is seen early in other years (Fig. 123). Samples that have been examined subsequent to the time this chart was prepared indicate the species was also found in patches toward the south.

It is my opinion, on the basis of the biological data, that during 1957, sometime after July, the flow of the California Current for some reason was reduced. During this time upwelling must have also been reduced. At the same time these two processes supplying cool water to the area were reduced, a nearshore countercurrent, as indicated by the distribution of *D. denticulatum,* developed carrying warm water and its contained populations to the north.

DISCUSSION

Isaacs: As a general rule *D. denticulatum* is never in the area north of Pt. Conception near shore. However, it does occur offshore as shown in the October 1957 chart.

Berner: In October 1957 the offshore distribution may have been somewhat closer to shore but was not markedly unusual. The nearshore extension to the north was unusual however. It extended even further in April 1958 while the offshore distribution remained quite "normal." In the same period there were unusually low numbers of *D. gegenbauri* in the area north of Pt. Conception; so it is my feeling that during the latter part of 1957 and early 1958 there was a definite transport northward of *denticulatum* near shore. The individuals were large well-preserved specimens.

Isaacs: Is that not a short period of time for them to grow large?

Berner: We do not know much about growth rates, possibly they could grow this large in a short time. Another interesting thing about these animals is that there are two forms that are readily identifiable. There are animals produced from eggs but which in turn do not produce more animals from eggs. The sexual form and asexual form are usually found together. The egg producing or sexual form of *denticulatum* did not appear in the area. It is my feeling these animals may not have been reproducing. There is another point of interest; at the outer end of line 133 (Fig. 121) a tropical salp, which had not previously occurred in the survey area, was found during the October 1957 cruise.

Marr: Is it an unusual occurrence, having that tropical salp at line 133 in October 1957?

Berner: This is a hard thing to answer. I did not find the species in the NORPAC samples. It has been reported extensively in the POFI material south of Hawaii. From what little can be learned from the literature I would regard it as a warm water form.

Wooster: Are there any specimens in the Shellback Expedition material?

Reid: Is this the first time it has been found north of 20°?

Berner: Yes to Wooster, and yes, as far as I know, to Reid.

Isaacs: Did you look at the samples from those few stations off Mendocino?

Berner: Yes, I did. They look much like those off Monterey—there are no warm water salps or doliolids in them.

Isaacs: It appeared from Balech's data, given by Haxo, that there might have been some remnant of the southern population.

Berner: The animals found north of Pt. Conception looked healthy. One learns to tell when they do not look healthy—the muscle bands are broken down, etc. Some of the cool water forms taken far south were not well preserved while other organisms in the sample were. Another thing I should point out, though not well versed on it, is the distribution of the pelagic crab *Pleuroncodes*. Carl Boyd, who is studying these animals in our laboratory finds that the larvae do not normally occur north of San Diego, certainly not as far north as Pt. Conception. During April 1958, however, they occurred near shore as far north as Monterey.

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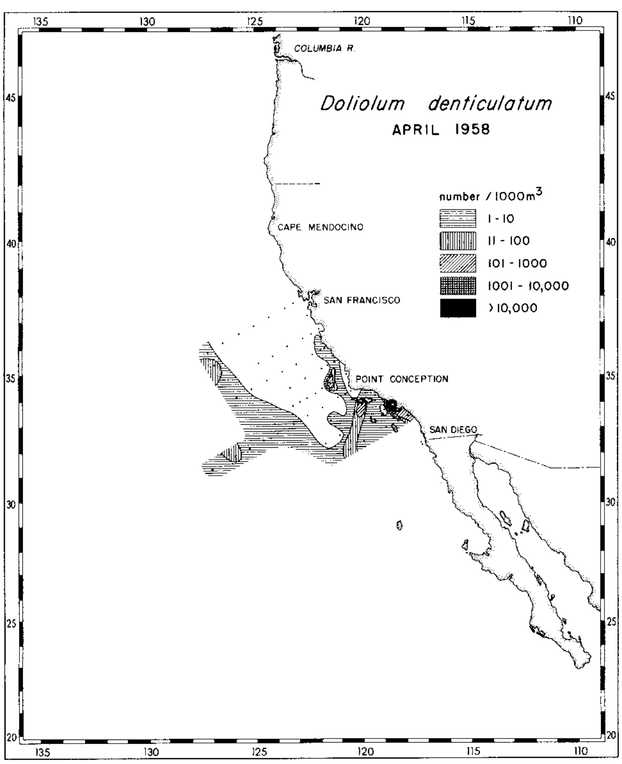

FIGURE 122. Distribution of *Doliolum denticulatum* during March 29 to April 28, 1958 (CCOFI Cruise 5804).

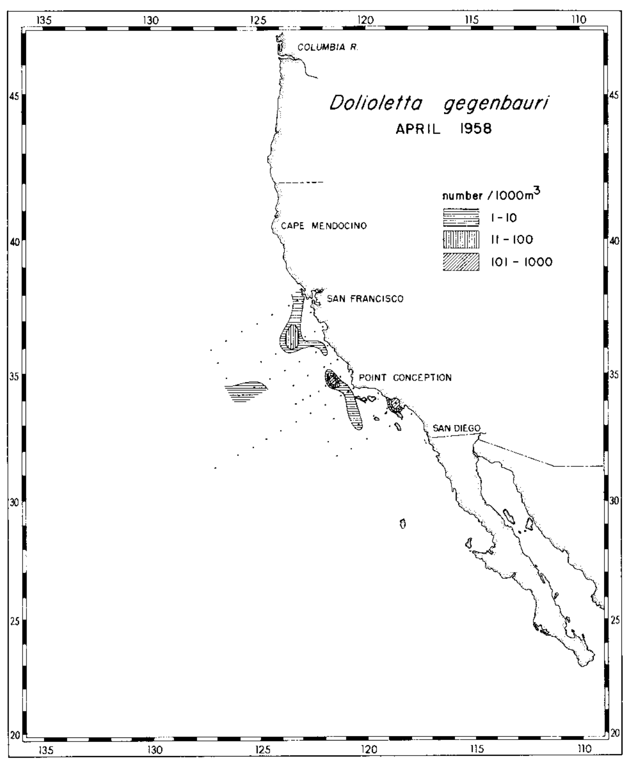

FIGURE 123. Distribution of *Dolioletta gegenbauri* during March 29 to April 28, 1958 (CCOFI Cruise 5804).

CHANGES IN THE DISTRIBUTION OF EUPHAUSIID CRUSTACEANS IN THE REGION OF THE CALIFORNIA CURRENT[1]

EDWARD BRINTON

Euphausiids are large zooplankton organisms of shrimp-like appearance that make up a high crustacean Order, the Euphausiacea. All species are marine and pelagic; most are oceanic in distribution and appear to have little dependence upon coastal regions. Some euphausiids, for example *Nyctiphanes* and *Meganyctiphanes*, are frequently most abundant in coastal regions, but it is not yet known whether it is essential for any species or assemblage of species to have access to neritic or coastal waters during its life history,—except as such coastal conditions as upwelling may influence the temperature and fertility of the local pelagic environment.

There are approximately 85 euphausiid species distributed over the oceans. Some Pacific species have very wide ranges: fifteen have cosmopolitan ranges from 40°N to 40°S, and six equatorial and twelve antitropical species occupy east-west oceanwide belts in the tropics and mid-latitudes respectively. Twenty-five of these species are numerically important in the California Cooperative Oceanic Fishery Investigation survey area, and an additional eight are occasional indicators of intruding environments. Eight deep-living cosmopolitan species are sometimes sampled by tows reaching deeper than the standard oblique tow from 0-140 meters.

To use a word that is commonly applied to parameters that are the antithesis of biological activity, the euphausiids might be called *conservative* organisms. They are long lived compared to the majority of plankton forms. It is believed that many species live on the order of a year. Some, notably *Euphausia superba* and *Thysanopoda acutifrons* live two to three years (Bargmann, 1945; Einarsson, 1955), but a year may be a reasonable approximation for the euphausiid life span in temperate waters. Species that live in extreme northern or southern regions, where the seasonal effect upon the waters is conspicuous, may be analyzed in respect to their growth rates and life spans. Warm water species, the populations of which may include breeding individuals and larvae at all times, are more difficult to study from the standpoint of the ages of populations sampled by plankton collections.

Euphausiids are deep-living animals relative to the other major components of the zooplankton in the upper layers of the ocean. Many species perform extraordinary diurnal vertical migrations, rising near the surface of the sea at night and descending to depths of 500 to 700 meters during daylight hours. Thus within à 24 hour period, a migrating population may pass through a range of temperature of as much as 16°C.

[1] Contribution from Scripps Institution of Oceanography.

If one looks at the distributions of euphausiids on a wide geographical scale, it is evident that many species' boundaries can be compared with environmental factors measured by standard oceanographic sampling techniques. Correlations, particularly with temperature and oxygen, appear to be part of the ecological definition of some of the species distributions (Reid, Roden, and Wyllie, 1958). An environmental factor may act at a particular depth to govern or limit the distribution of a vertically migrating species. A temperature-depth value may limit an essential metabolic activity, such as digestion of food at depth or ability to feed at the surface at night as Moore (1952) has suggested. Enough is not yet known about euphausiid distribution to specify limiting factors.

A summary of the typical patterns of distribution found in the Pacific can provide a framework in which to consider the incursion or the occurrence of euphausiids in more local areas such as the CCOFI survey area. These have been derived from collections made during oceanic expeditions carried out by the Scripps Institution, 1949-56 and are described by Brinton (1957). They are essential to a consideration of the California Current as a region of faunal convergence. Northern (subarctic) species are not consistently carried far to the south in the cool current, but appear sporadically along the coasts of California and Baja California. Offshore (central) forms sometimes are found near shore. *Nyctiphanes simplex* and *Thysanoessa spinifera* are adapted to the shoreward part of the California Current, extending variable distances to the north and south, and may be carried to the west in tongues of distribution. (*Nyctiphanes* is the shallowest living local euphausiid, living above 100 meters at night but descending to near that depth in the daytime). Species adapted to an oceanographic transition zone (Sverdrup, Johnson and Fleming, 1942) are the dominant euphausiids in the California Current, while equatorial species are present off Baja California.

Oceanic species distributions have areas of occurrence that approximate the positions of major temperature-salinity water masses. Certain of the factors that operate to maintain species in an area (current systems, intensity of incident radiation) are the same influences that give integrity to large masses of water.

SUBARCTIC DISTRIBUTIONS

Thysanoessa longipipes and *Tessarabrachion oculatus* occur north of about 42°N in the mid-Pacific and sometimes penetrate southward to the latitude of San Francisco (37-38°N) in the California Current. *Euphausia pacifica* has a distribution in the mid-oceanic area which extends south of the two previous species,

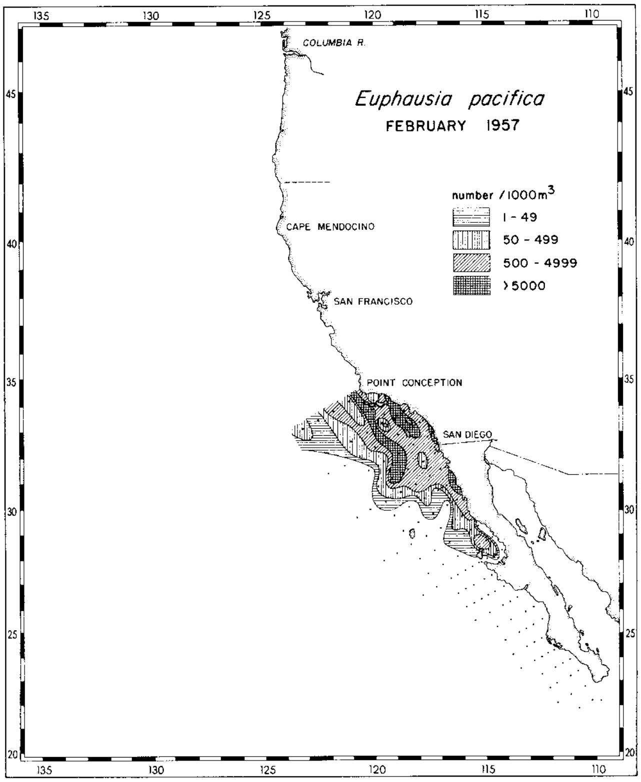

FIGURE 124. Distribution and abundance of the euphausiid *Euphausia pacifica* during February 6 to 20, 1957 (CCOFI Cruise 5702).

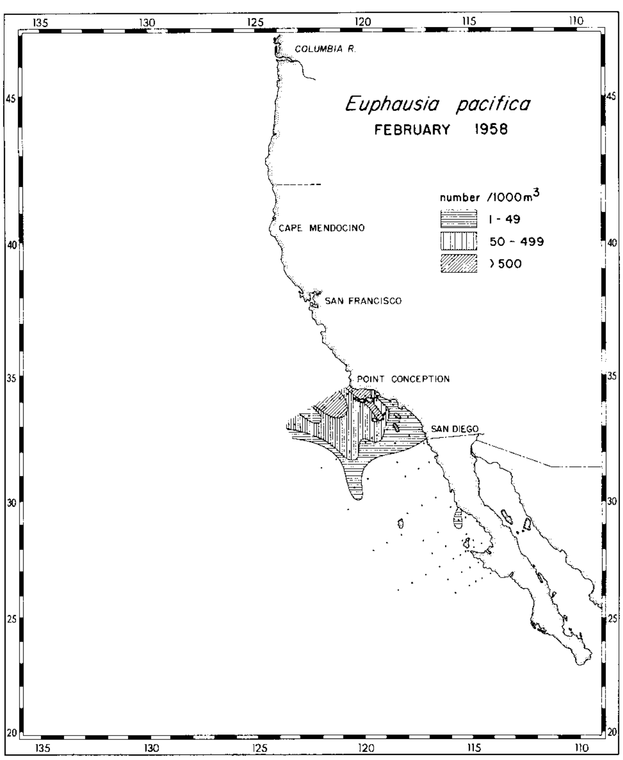

FIGURE 125. Distribution and abundance of *Euphausia pacifica* during February 6 to 24, 1958 (CCOFI Cruise 5802).

to near 40°N. It ranges southward in the California Current, terminating off Southern California or Baja California (Figs. 124, 125, 126). *E. pacifica* is frequently abundant, and its distribution may be used as a measure of the southward extension of the cold-water fauna.

TRANSITION ZONE DISTRIBUTIONS

Nematoscelis difficilis and *Thysanoessa gregaria* are present along the boundary between the subarctic and central regions, overlapping both. In the California Current region their distributions bend southward toward equatorial waters. They are dominent off Central California. If plankton sampling were limited to the CCOFI area, these species would be present at nearly all stations, suggesting a wide range. Actually they are present in a narrow (35-44°N) east-west oceanic belt with a southward extension in that part of the California Current which contains semi-permament eddies of Southern California and mid-Baja California.

CENTRAL DISTRIBUTIONS

Euphausia brevis and *E. hemigibba* represent an assemblage that is dominant south of 40°N and north of about 20°S. extending eastward to the offshore waters of central and southern California and Baja California. In these latter waters their presence is regarded as an indication of encroachment of the offshore "central" environment upon the coastal waters (Figs. 127, 128, 129). In order to consider assemblages one must lump together those species that have similar zoogeographical affinities, even though no two species have exactly the same range.

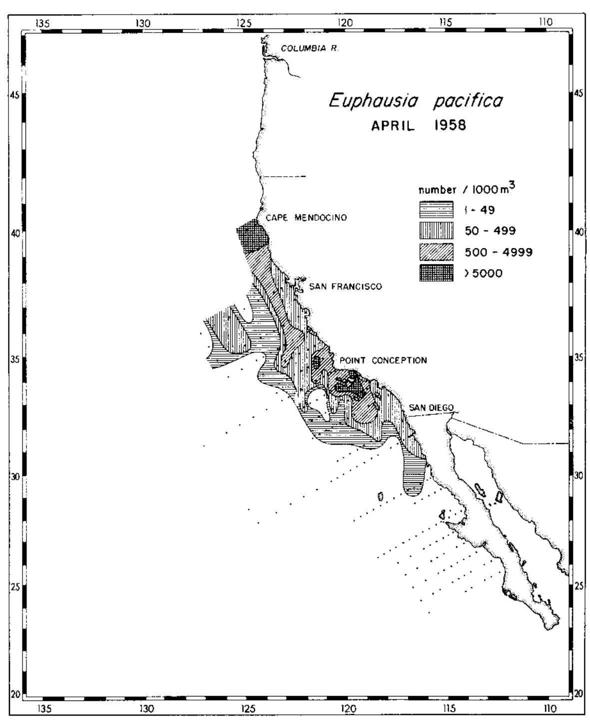

FIGURE 126. Distribution and abundance of *Euphausia pacifica* during March 30 to April 27, 1958 (CCOFI Cruise 5804).

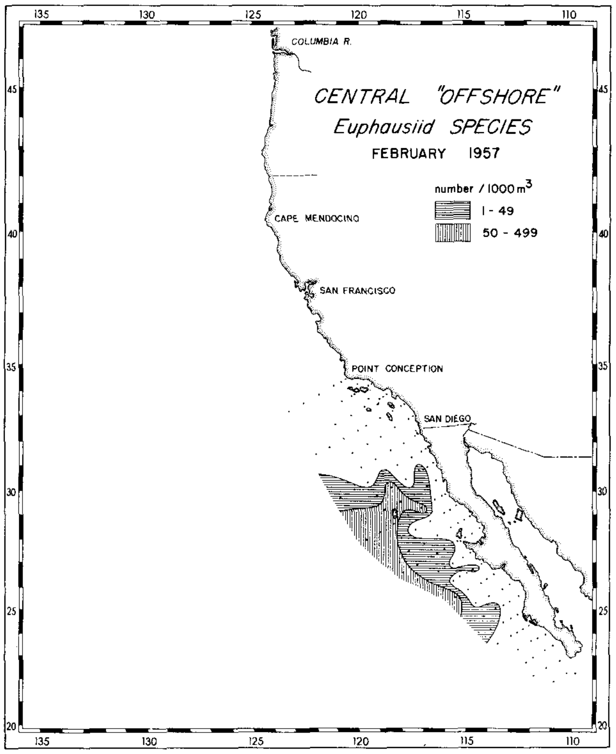

FIGURE 127. Distribution and abundance of central offshore euphausiid species during February 6 to 20, 1957 (CCOFI Cruise 5702).

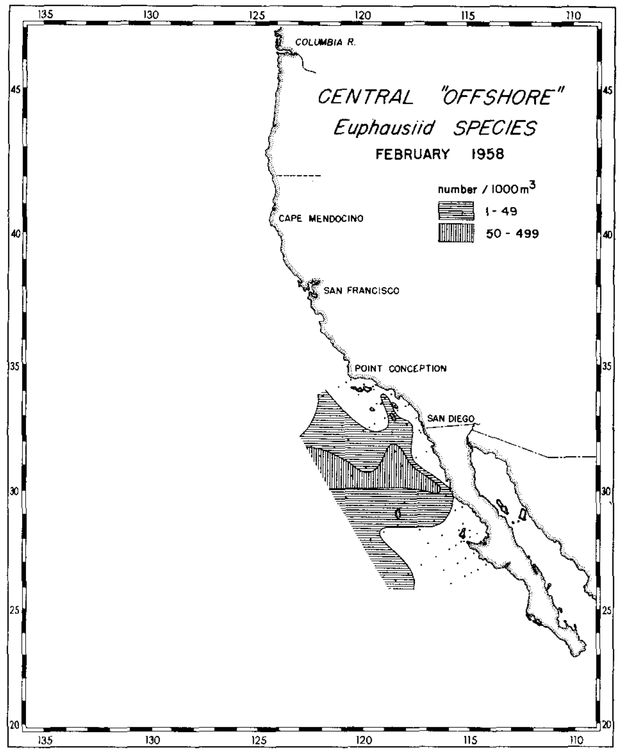

FIGURE 128. Distribution and abundance of central offshore euphausiid species during February 6 to 24, 1958 (CCOFI Cruise 5802).

EQUATORIAL DISTRIBUTIONS

Euphausia eximia (Figs. 130, 131) is a species which is confined to the Eastern Equatorial Pacific. It has a more restricted type of equatorial distribution than *E. diomediae,* which extends all the way across the Pacific between approximately 20°N and 20°S. *E. eximia* is most numerous in two regions: 1) where California Current waters merge with the equatorial current system, and 2) where the Peru Current contributes to the fertility of an environment near the beginning of the South Equatorial Current. The distribution boundaries of equatorial species undergo changes in their terminal region off Southern California and Baja California. The equatorial assemblage is dominant south of 20-23°N.

We have reason to think that *E. eximia* is deeper than, for example, *Euphausia pacifica* and *Nyctiphanes. E. eximia's* vertical distribution indicates that it migrates to a depth of about 300-700 meters in the daytime; adults approach the surface at night, but have not been found at the surface. Off Southern California *Nyctiphanes* migrates within the 0-150 meter layer.

Compared to *E. eximia, E. distinguenda* is an eastern equatorial species that is consistently present only south of the latitude of Cape San Lucas (23°N). When it occurs in the California Current off Baja California it is rarely numerous but may indicate northward transport of water. However, in late 1957 *E. distinguenda* was more numerous off Baja California south of Pta. Eugenia than at any previous

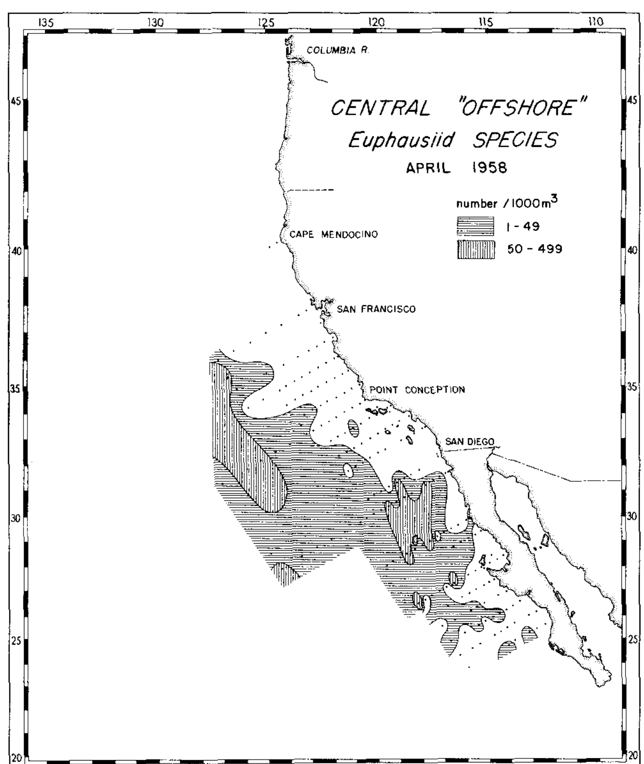

FIGURE 129. Distribution and abundance of central offshore euphausiid species during March 30 to April 27, 1958 (CCOFI Cruise 5804).

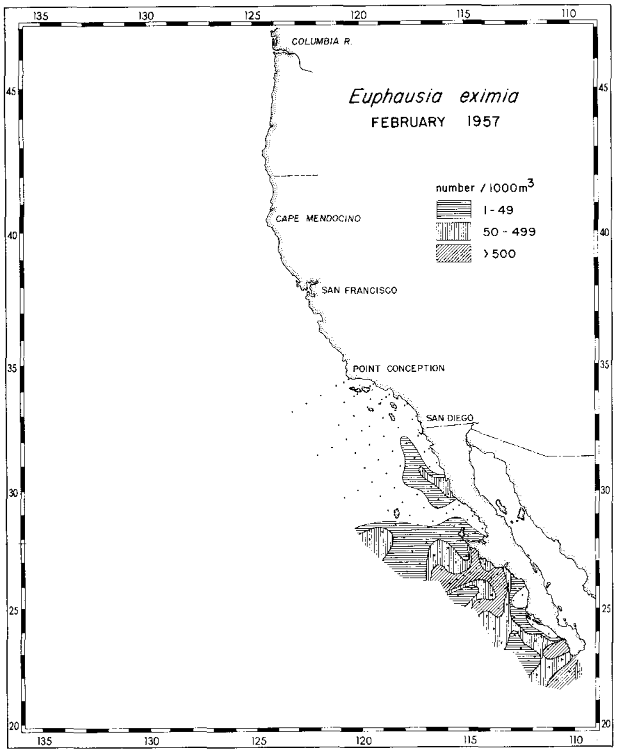

FIGURE 130. Distribution and abundance of the euphausiid *Euphausia eximia* during February 6 to 20, 1957 (CCOFI Cruise 5702).

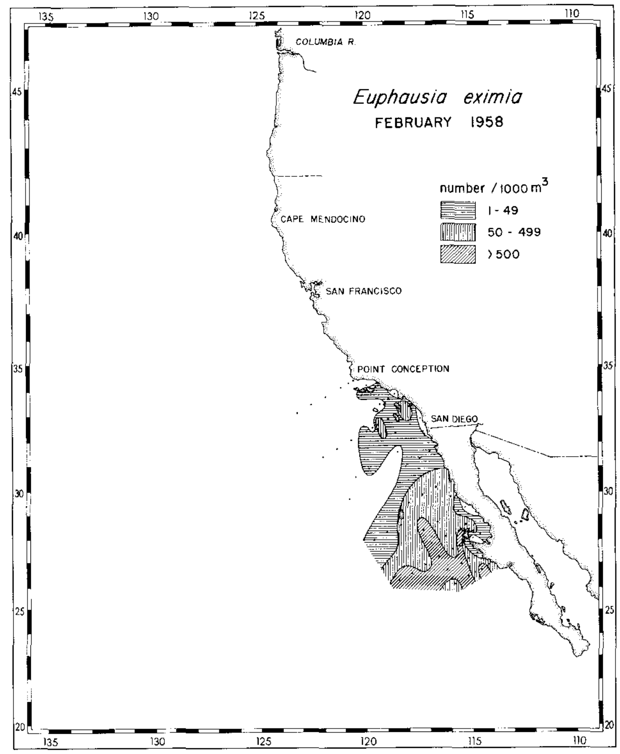

FIGURE 131. Distribution and abundance of *Euphausia eximia* during February 6 to 24, 1958 (CCOFI Cruise 5802).

time, 1949-1956. The population contained many developmental stages, suggesting that an equatorial environment had developed to the north enabling individuals to reproduce there. In addition to being carried north from its eastern equatorial habitat, this species was found to have been carried as far west as 175°W in the latitude of the North Equatorial Current (Equapac Expedition, Sept. 1956). This is one of the dominant species in the Gulf of California; populations live and develop there. But the occurrence of *E. distinguenda* in the waters west of Baja California is exceptional.

The standard oblique plankton tow made in the course of CCOFI sampling is to a depth of 140 meters, using a net one meter in mouth diameter, of 0.6-0.7 mm mesh width. Most euphausiids counted are immature. Tows made below 140 meters indicate that there the adults are dominant, particularly during the daytime. Thus the counts for the CCOFI data are weighted toward small specimens.

If the tows upon which the distributions are based had been made to a depth of 300 meters (as were the tows made by POFI and the Scripps Expedition) some differences in species boundaries would be expected, but differences would be in details. For instance, during the 1953 Transpac Expedition tows were made to at least two different depths at each station. In some cases the 0-700 and 0-1,000 meter strata were sampled. A change in the southern boundary of northern cold-water species was found at only a few stations by deepening the stratum sampled. A good measure of the importance of stratum thickness was derived from sampling done during the Scripps Institution's 1939 cruise off the coast of California, where the series of station lines was similar to the present CCOFI pattern. Changes in apparent distribution brought about by deepening the stratum sampled from 0-70 meters to 0-140 meters or to 0-180 meters were few, qualitatively. The three depths were sampled on the 1939 cruise. If the species was present in—or was absent in—the shoaler tow, the chances that a different record would be shown by the deeper tow were about two or three in fifty. Multiple tows were also made on NORPAC cruise, 1955. The deepest (0-700 meter) tow was not made at every station, but considered on the basis of tows through 0-140 meters and 0-280 meters, species' distribution boundaries were rarely altered by taking the deeper sample into consideration.

Some species (e.g. *Thysanoessa gregaria*) descend to a limited extent as warm waters are approached. There are also warm water mesopelagic species (e.g. *Nematobrachion boopis*), which ascend as their coolwater distribution boundaries are approached. These are local phenomena and do not greatly alter the broad scale distributions derived from 0-140 or 0-280 meter sampling.

Animals that do not have unlimited depth tolerance might be restricted horizontally by superficial isotherms.

Certain isotherm lines nearly coincide with species distribution boundaries indicating that there are physical properties that have the same type of distribution. For example the 12°C. isotherm at 100 meters bounds the southern limit of *Euphausia pacifica*. Certain species relate best to surface or shallow temperatures. Others relate to deeper isotherms, and inasmuch as the vertical ranges of most species are considerable, it is not easy to say how the isotherms could be limiting for each species.

It may be useful, before considering 1957-58 changes, to review the three main zoogeographic features of the coast of California and Baja California—features that may fluctuate from season to season or year to year.

SOUTHWARD EXTENSION OF THE SUBARCTIC AND TRANSITION ZONE DISTRIBUTIONS

Cold water species were consistently present in large numbers off Central California and in small numbers off Punta Eugenia, Baja California, in the years of the CCOFI program 1949-1954. Populations of these species changed quantitatively off Punta Eugenia in 1954-55. *Euphausia pacifica* and *Thysanoessa spinifera* became numerous and widespread in this region in April of 1954, and 1955, related to cooled waters there and an extensive eddy west of mid-Baja California, near the southern limits of the ranges of the species. In April-June of the years 1949-1952 the cold-water populations in this southern region were inconspicuous.

EASTWARD INCURSION OF OFFSHORE "CENTRAL" SPECIES INTO THE COASTAL AREA

This group has a distribution complementary to that shown by the westward tending tongues of cold-water. The central euphausiids may appear close to shore, usually off northern Baja California and Southern California, apparently in relation to the cyclonic circulation there. They are carried eastward as they are caught up in the southern half of this gyre, and sometimes enter the coastal region of Viscaino Bay, overlapping the western boundaries of the cold-water animals.

NORTHWARD EXTENSIONS OF EQUATORIAL DISTRIBUTIONS IN THE COASTAL REGION

Euphausia exima has been consistently abundant off mid-Baja California, 1949-1955. Other species with equatorial affinities *(Euphausia distinguenda, E. tenera, E. diomediae)* sometimes have been present in small numbers south of 27° north; there have been very few records from waters north of Viscaino Bay (28° N).

1957-1958 OBSERVATIONS

February and October, 1957, cruises and February and April, 1958 cruises have been examined for euphausiids. The distribution of *Euphausia pacifica* in February 1957 (Fig. 124) was normal for that month both qualitatively and quantitatively. In contrast, February 1958 (Fig. 125) found this species distribution extremely retracted from the southern part of the coastal area. This is normal for October-December, but not for February. By December *E. pacifica* has usually (1949-55) withdrawn northward to near the latitude of San Diego (33° N).

During April *E. pacifica* was characteristically (1949-55) present in the oceanic area from Guadalupe Island to Pta. Eugenia, sometimes occurring even farther to the south. In contrast, in April 1958 (Fig. 126) this species was sparse in this region and its distribution off Southern California was more inshore than had been previously found for this month of the year. Nevertheless, it must be noted that this cold-water species was *still present* in substantial numbers off Southern California in 1958.

E. pacifica probably lives somewhat deeper than the next species, *Nyctiphanes simplex*, which in 1949-57 occupied the coastal region, usually south of Point Conception. In February 1957 (Fig. 132) *Nyctiphanes* was present in low concentration south of the Point. This was like previous years. In October 1957 all developmental stages of this species were more numerous than usual at the northern limit of its range in the region of the Southern California Channel Islands. I interpret this as meaning that the environment in the upper water layers off Southern California was modified in late 1957, permitting *N. simplex* to reproduce in a relatively northern area. It has subsequently (Figs. 133, 134) reproduced heavily in the areas immediately south and just north of Point Conception, where it had heretofore been rare or absent in April. The April 1958 cruise (Fig. 134) shows that *Nyctiphanes* occurred all the way to Cape Mendocino (40°N) though in very small numbers; but off San Francisco (38°N) concentrations were greater than 50 per 1,000 cubic meters at two stations. I do not think we can regard this northward distribution as necessarily a manifestation of a persistent countercurrent. Drogue measurements made in March showed that the countercurrent was not present at the surface at that time. The large populations of *Nyctiphanes* that were off Northern California in April 1958 must have been residual there, if continuing transport from the south had not been maintained through the winter. These southern animals have, during this 1957-1958 period of change, extended far north of their "normal" range.

The offshore occurrence of warm water animals of the central type seems to have undergone little change in the critical region off California. This is the region off San Diego and south of Point Conception. The warm water animals are few here, and while their encroachment toward the east is somewhat greater in February and April 1958 (Figs. 128, 129) than in February 1957 (Fig. 127) or February 1949-55, interpretation in terms of water movements must be speculative. This warm water environment has moved northward and toward shore, but the area *dominated* by the central species is still far offshore.

Another species, *Euphausia eximia* (Fig. 130) is an equatorial type, which occurs in the southern part of the CCOFI survey region and was present north of its usual range, close to shore in 1957-58. It was more numerous to the north, off Southern California in February 1958 (Fig. 131) than during 1949-55. This might be regarded as another indication of the northerly extension of the warm-water environment provided by the current system that distributes the animals.

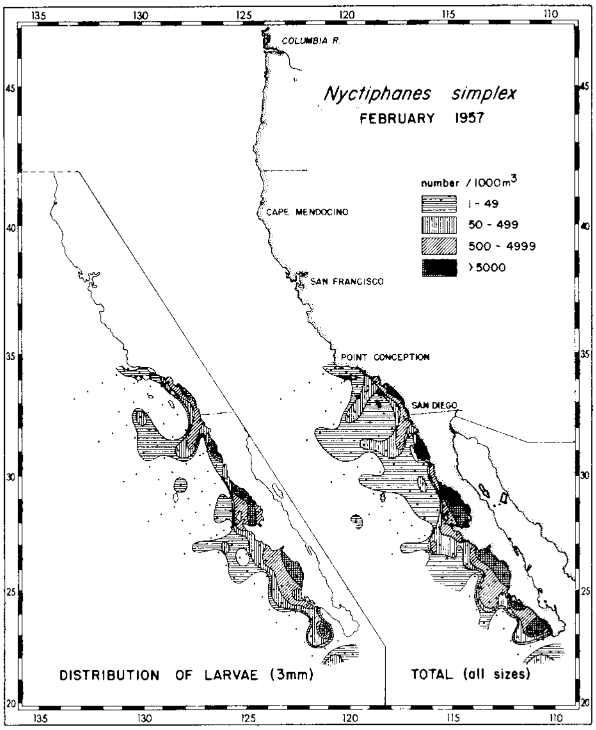

FIGURE 132. Distribution and abundance of the euphausiid *Nyctiphanes simplex* during February 6 to 20, 1957 (CCOFI Cruise 5702).

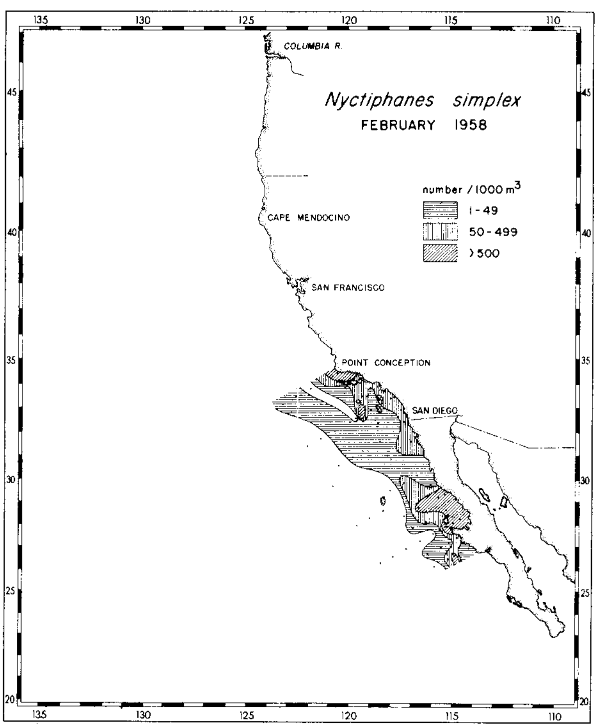

FIGURE 133. Distribution and abundance of *Nyctiphanes simplex* during February 6 to 24, 1958 (CCOFI Cruise 5802).

DISCUSSION

Revelle: Does *E. eximia* occur in the same area where you have *Nyctiphanes simplex?*

Brinton: No, *E. eximia* is in a slightly more offshore region and behaves differently from *Nyctiphanes* because it is a deeper living animal. If there were a very shallow northerly current, you might expect *Nyctiphanes simplex* to be transported farthest, because it would be in the surface layer more continuously than *E. eximia,* which rises into the superficial layers only at night.

Isaacs: Was *E. eximia* found north of Point Conception in April 1958?

Brinton: Yes, it was present at one station off Monterey Bay, and at two other stations north of Point Conception. *E. eximia* had never been previously found north of Point Conception.

Fleming: Practically all your figures show that euphausiids are mostly concentrated near the coast —an example of the Fuglister principle?

Brinton: That is the trouble with our area, I think, rather than with the animals. A feature of our coast is that regions of fertility occur just south of Point Conception, off Punta Eugenia and very close to shore in the intermediate region 28-33°N. All are cool regions as far as the northern species are concerned. The animals might be basically oceanic and still be numerous near-shore because of nutrient renewal expected in the coastal regions.

These animals appear to hug the coast in these regions. I still think they may be regarded as oceanic animals. Euphausiids are primarily phytoplankton-filtering animals that are not found near the surf zone so to speak, but offshore. These small-scale maps are somewhat misleading, particularly where you have heavy concentrations against the shore. Here high euphausiid concentrations are adapted to the cool oceanic waters in coastal regions.

Berner: I think this is true of many species.

Reid: Our region is relatively barren offshore. There are more animals living in the inshore regions which are seasonally rather stable compared to the offshore areas. These animals take advantage of the small seasonal changes in temperature and the cold water masses which are found year round somewhere within the range of 200 miles—a characteristic of this transitional region.

Hubbs: Do you have any collections from the previous warm years, say like 1926, '31 or '41 from Monterey or from anywhere up that way?

Brinton: No. I had intended to make reference to the 1939 cruise. It extended from the Columbia River to south of Punta Eugenia. This is significant from the standpoint of our survey program because the cruise was carried out at a time when many of the subtropical fish were taken off Central California, and when the sardine was spawning there.

Hubbs: What part of 1939?

Brinton: May through July. That is before it got very warm off Southern California. The southern line

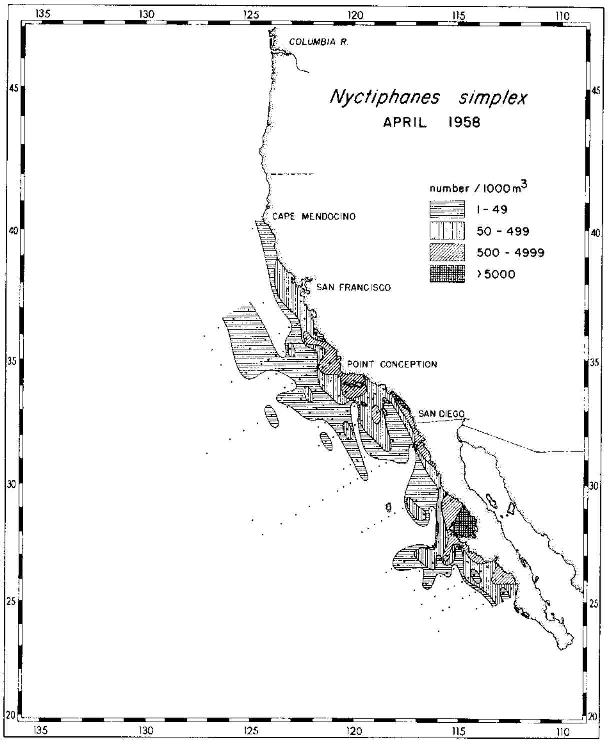

FIGURE 134. Distribution and abundance of *Nyctiphanes simplex* during March 30 to April 27, 1958 (CCOFI Cruise 5804).

of stations, extending westward from Viscaino Bay, was occupied in July.

Hubbs: Then we missed that warm water north of Point Conception that year.

Brinton: Yes. According to the chart of *Nyctiphanes* for 1939, one feature that I think is remarkable, and unique for 1939, is the occurrence of a cool water population off Point Eugenia as late as July, including large numbers of *Nyctiphanes* and *E. pacifica.* Cold eddies in this region in 1954 and 1955 were not observed later than April, I believe. In 1939 they persisted at least until early July. There was a very cold spring that year. The cruise was too late to record what was going on in the winter time, of course. In general the 1939 May through July distributions looked typical, with the exception of this particular area off Point Eugenia.

Sette: As I recall, February was almost a record cool month.

Isaacs: One of the coldest springs on record occurred in 1939, and one of the hottest falls.

Brinton: In February 1957 *Nyctiphanes* (Fig. 132) ranged north of Point Conception where it was present in small numbers near shore. This was not strikingly different from previous Februarys. The countercurrent, which seems to have been conspicuous in April 1958, was already pronounced in the winter of 1957-58 when *Nyctiphanes* was abundant as far north as the survey went (Pt. Conception) and *E. eximia* was well north of its distribution of February 1957 (Fig. 130). Where we have stations to the north of Pt. Conception it is evident that *Nyctiphanes* extended as far north as Cape Mendocino (Fig. 134). The central species had moved nearer than usual to the shore off Southern California in April 1958. This change is not great. Nevertheless we have to look for small changes. Only the northward extension of *Nyctiphanes* was conspicuously different in 1957-58.

Hubbs: The distribution of *Nyctiphanes* in April 1958 (Fig. 134) is the only one that shows numbers offshore at all, north of Pt. Conception.

Brinton: In the waters off Baja California there is an example of a change, which is evident in February 1957, however slight. This change was the northern occurrence of other equatorial species that normally do not occur north of Cape San Lucas except for occasional stragglers. They were not present at all stations, but in February 1957 there were more stations where equatorial animals occurred than is usual off Baja California. Again, these equatorial species were not being swept into the area, but were present in larger numbers at the southern stations in early 1957 than in the first three months of any of the years 1949-55.

Berner: The animals that apparently live in the upper layers show the changes more than those that live throughout deeper layers. Apparently *Nyctiphanes* and *D. denticulatum* are more inclined to the upper layer, and show this northward movement more than the other animals that live at a greater depth.

Brinton: Yes, I would say that of the species I have looked at, the one that shows the strongest northward shift in distribution seems to be restricted to the waters above 100 meters. Some of the deeper ones are slightly farther north than is normal.

Johnson: What are the youngest stages included in your counts?

Brinton: The youngest stages are about 3mm in length, sometimes smaller. Some of the small stages are lost through the net mesh. These larvae are probably two to four weeks old by the time the net retains them.

Johnson: So the count indicates a smaller population than is actually present.

Brinton: Yes, and larvae of *Nyctiphanes* made up some of the population north of Point Conception. The general distribution of larvae is not as extensive as that of adults, as is evident for February 1957 (Fig. 132). Spawning occurs close to shore; the adult population diffuses somewhat seaward. In February 1954 spawning occurred to the northern limit of the adult range. There is always a possibility that only the population of larvae near the surface, may be swept into an area, but, more often, the general population is carried together.

Reid: One thing that seems to stand out is the change in the distribution of the southern types rather than in that of the central types. Is the central type documented well enough to say that it did not move in?

Brinton: It is not documented well enough to be definitive. The central types that I have plotted for California waters in February and April 1958 (Figs.

128, 129) suggest that they all move slightly toward shore. But I feel that this change is not as conspicuous as the northward movement of *Nyctiphanes* and *E. eximia*. The presence of unusually warm water in the California Current could lead to intrusion of both assemblages.

Question: The results might show some on-shore encroachment although they do not swim in but are moved in by the water.

Brinton: They probably do not swim in very actively. Encroachment in the course of a month or two months may be helped by random diffusion of the population in mixed areas. The animals swim up and down—spending most of their lives in swimming up and down. They probably move at random laterally, some of the time, and this could be enough to extend their range into an adjacent region that might be tolerable to them.

Johnson: Directive forces drive them up and down, so they swim straight up or straight down, but the thermal gradients to the right or left are less. Perhaps they do respond to smaller gradients than we think.

Murphy: Do all euphausiids come to the surface during a 24 hour period?

Brinton: No, not all species. I have lumped the species into three bathymetric groups. One group of species lives above 700 meters; these migrate near to the surface at night. A second group lives between 500 and 2,000 meters. These never get to the surface. The third group is bathypelagic—only the larvae live in the upper layers while adults live below 1,500 meters.

If you take the depth distribution of a species which lives in the 0-700 meter environment the youngest vertically migrating stages of development are nearest the surface. The adults would be the deepest. Many of the larvae which are one to two months old live within 20-25 meters of the surface.

Question: Do you suppose sampling of plankton should be switched to the night stations because more adults are found then?

Brinton: Night hauls frequently contain more adults than day hauls. Numbers of larvae and immatures may or may not vary in this way. Most of the animals that are immature seem to be well sampled by the relatively shallow 0-140 meter tows. However, the picture would be much more reliable with a standard time of sampling.

Reid: Your pictures are very coherent.

Brinton: Considering this type of difficulty there still seems to be coherence to the distributions.

Wooster: I would like to borrow Dick Fleming's iconoclastic role for a moment, if I may. We have said before this session started that in many ways the animals might be better oceanographers than the oceanographers, and I am beginning to think that, like many a good oceanographer, they are very difficult to understand. It seems to me the use of zoogeography as an indication of oceanographic circulation has to be calibrated with physical or chemical oceanographic parameters. If species distribution agrees with distribution of temperatures, it is good that this all fits together. Actually, you have not learned anything about circulation; you have learned something about the temperature tolerance or affinity of the organisms. If distribution does not agree with the particular physical parameters with which it is compared, then you are sort of perplexed. One of two things happened: either you chose the wrong physical parameter, such as surface temperature (often it must be that, I think), or something else has happened that you do not really understand. It seems to me that looking at this in general, the comparison of distribution of organisms to those of physical and chemical parameters is likely to show more about the beast than about the ocean.

Johnson: It brings out some points where an oceanographer should look for more information than he has.

Wooster: What he is saying is that where distribution of the physical parameters and the distribution of the beast do not agree, then the oceanographers may have the responsibility of explaining this. It may be something in the biology of the beast, or it might be something not being looked at properly in oceanography.

Isaacs: Euphausiids migrate vertically through a range of 10 to 12 degrees Centigrade, and yet for years they had a distribution that turned out to be consistently limited within 0.2°C by a certain 10 meter isotherm and this all in a moving current. Does anyone think that the temperature of this isotherm was restricting this distribution?

Brinton: It is hard to say. So little is known about the actual physiological requirements of the animals. One temperature might be limiting for feeding, another for reproduction, another for a metabolic function. So it is probably an extremely complex business. One isotherm might be limiting in one area and a different isotherm in another.

Johnson: I think probably while we are on the particular question of temperature requirements and also in the matter of deposition of eggs near the surface, I might point out the case of *Thysanoessa spinifera* washed up on the beach at La Jolla in large numbers. They all turned out to be spent females. What actually happened may have been that they had risen unusually close to the surface to spawn and had been caught in inshore currents and washed up on the shore. Surface temperature might be exceedingly important to spawning.

Ewing: There is one feature that perhaps we are missing or have overlooked, that is the biological edge effect, which we all know, does not affect the extent of distribution, but the density of populations. This is very dependent on boundaries between currents or water masses. On land the transition conditions between dissimilar environments very much increase the carrying capacity of the environment, cutting it up into smaller areas so that the animals have a longer boundary environment. There are cases where this seems to be true in the ocean as well, such as at a front. It is not so much the average temperatures of the water mass itself that determines the carrying capacity of the water as it is the amount of boundary. The albacore is quite sensitive to this. It likes to stay in fairly warm water and feed in cool inshore water. It likes to feed in one environment and live in another. One reason why many so-called warm water animals are at times most abundant along the edge of the California Current, or at least along the cold water, is that they can stay within their temperature tolerance and have access to cold waters which are in general more nutritious. They get as close to their food as they can and still stay in their own temperature range. As an example, the boundary off Cape San Lucas is very nutritious and always full of marine life. Very high gradients are found there. So it is very easy for an animal to pick an optimum area. Many animals must choose environments adjacent to boundaries where there are gradients rather than those areas where optimum conditions prevail.

Ahlstrom: These temperature ranges that I believe we are mentioning, none of these concerns both adults and the larvae. The larvae could have quite different and much smaller temperature ranges, and I think for each stage this has to be established. I think it is a mistake to take the range of an adult and apply it to larvae.

Brinton: Yes, I think there is danger in that too. However, the thing that is remarkable to me is that more often than not, you find very many stages of development together. But of course this is sampling through a long vertical column. In this extreme northern extension of *Nyctiphanes* (Fig. 134) occurring off Mendocino all were adults however. At the stations off San Francisco there were some larvae present. Surface temperatures were 12°C. off Cape Mendocino. I am certain that this species could not survive long there.

Isaacs: What are the comparative ranges of temperature of the distributions of *Nyctiphanes simplex* in the two hemispheres?

Brinton: The temperatures circumscribing the distribution of *simplex* in the Peru Current on Shellback Expedition were very similar to the temperatures found in our CCOFI region: the 20°C. isotherm, normally, I think. But in 1958 it was in colder water at Cape Mendocino, 8 degrees cooler than the usual limiting temperature.

Revelle: What interests the biologists, what interests the organism, what interests the physical oceanographer? The physical oceanographer can measure the temperature very easily and is not interested in using organisms as a thermometer. But he cannot always follow the water masses around by temperature alone; he likes to use organisms as drift bottles. In order to use the drift bottle you have to know where you release it and where you pick it up. These animals are very easy to identify as to where they are picked up, but not as to where they were released. The second difficulty is of course that they apparently migrate vertically as well as horizontally and therefore they are drift bottles that can be used in a kind of hazy way for water at different depths. Looking at this from the standpoint of the organism, the biologist or the physical oceanographer, you have three different things you are concerned about—can the organism swim? How fast can he swim? How far does he swim horizontally? Really, they are no good as drift bottles because they can migrate from that deep scattering layer to the surface in half an hour. This is 400 meters which means they can swim roughly a kilometer an hour or about 20 kilometers a day. They could move extensively in a rather short time.

Brinton: There is no question that they could move into an area that is tolerable as to temperature. It would be nice to know the extent to which they do move laterally, but the observations that have been made on their swimming behavior are only on their vertical movements. They are usually in a sort of spiral path.

Question: What would happen in a boundary region where an animal might be expected to extend his range by random swimming?

Brinton: I do not know what would happen. I do not think there is much likelihood that it would purposefully swim in any given direction horizontally.

Revelle: If you invoke random swimming, though, you would not have as sharp a boundary as you actually have. Look at these February distributions of *Euphausia eximia* (Figs. 130 and 131). A great many are in a certain region, then they stop abruptly. That is true in almost all cases. Your distribution has quite definite boundaries.

Isaacs: This is a strong argument against random dispersion—they do not randomly swim as individuals but as swarms.

Revelle: But it will still cause a dispersion.

Murphy: The map says they are not randomly distributed.

Wooster: Why can not these organisms be spreading, diffusing, or swimming horizontally in a random fashion? In fact, they probably are. They are exerting pressure on the limits of their distribution at all times. Due to physical circumstances the population is tending to spread all the time for one reason or another, and when it goes into an environment that is not favorable, it is dying off or somehow being wiped out. When it spreads out and finds a suitable environment, then it prospers and would develop a tongue by diffusion. On the other hand, a distribution might be reduced around the edge by attrition. A group of parameters rather than a single one, limit its distribution.

Sette: I think we are all agreed that the animals are reacting to something essentially other than temperatures. Nonetheless we use temperature as a marker of water masses or circulation systems. An animal or a group of animals may be an indication of many things—just as temperature is, by the way. The observation of events would not be confined to the physical parameters if other things could as easily be measured.

Murphy: I agree with Warren Wooster; you just do not know the life requirements of these animals, so that finding an animal in a particular place does not say very much about the reasons it was there. We need a better history of it. As to temperature, as it was pointed out before, if you want to know the temperature, you might as well go out and measure it. We do get some data on living things without sending an expedition. Fish kills and unusual distributions of fish are reported by fishermen.

Revelle: I am not arguing; I do not object to the collection of animals. I am simply saying that in the process it is also very easy to measure temperature.

Radovich: Regarding the question of whether or not euphausiids can swim purposefully, I recall that during one of our sardine conferences Dan Miller of the Department of Fish and Game mentioned that schools of gravid *E. pacifica* were seen in Monterey Bay from an airplane. These schools were subsequently sampled from a boat—they were moving similarly to fish schools. I also observed what I thought was a school of euphausiids off Anacapa Island several years ago. I tried to observe the way they were moving.

Question: With the current?

Radovich: No, they were swimming. As we came upon them the school would separate and move away from the boat.

Murphy: This means they do have capacity for coherent motion.

Brinton: Their spawning behavior shows a local peculiarity. For example, these schools have never been observed in the open temperate ocean to my knowledge.

Berner: These schools of *Euphausia pacifica* have been observed at one time or another off central California between Point Conception and Cape Mendocino.

Johnson: Where you have shading on the map—it does not mean that every station yielded animals, does it? There would be zero values in a number of cases.

Brinton: Where the relative numbers of animals belonging to the different assemblages are plotted, for example as the "central offshore" species are plotted (Figs. 127, 128, 129) the zero values for the assemblage would be shown as clear places. There is continuity within these species distributions. Within the distribution there might be smaller dense patches. Often whales actively seek out swarms of euphausiids; that is the only way they can get as many as they do. A plotted record is, of course, a reflection of the situation at a moment, perhaps a week before a species did occur at stations outside of its indicated range. A southern group extending northward as a tongue may be really receding, having had earlier a more extensive distribution.

Isaacs: Some areas of apparently low concentration result from day samples when the euphausiids are at deeper levels as compared with night samples when more of them are in the upper layers.

Brinton: Possibly. I have sometimes in the past tried to correct for day-night differences in apparent concentration. Sometimes numbers are consistently higher in night hauls, sometimes not. If you omit all the day stations on the periphery of a range, it sometimes makes a much improved contour—a smoother one. In other instances there seems to be no reason for concentration irregularity. It would be nice to have all night stations, then you would know that the animals are within the sampling limits.

Revelle: Is it possible to interpret these in terms of onshore movements?

Brinton: Not with certainty. It is impossible to say that there is movement of water in any particular direction. However, I have tried to speak in terms of the environment of the animals being extended in one direction or another. This may be due to local changes or it may not, but I do see examples from time to time where I feel that there are movements of the water.

Revelle: I am talking about the change that we are discussing during this Symposium, that is, whether the water moves from the center of the ocean to the edge, or whether it is actually motion in a north or south direction. I wonder if any light can be thrown on this from the various kinds of critters. I gather the impression that, except for small features and except for coastal species found to the north, the distribution may very well be accounted for by the major movement of the warm offshore waters toward shore, plus the ability of these critters to seek out environments that they like.

Sette: The evidence does not clearly settle the question of whether or not the euphausiids were drifted, or whether they moved by their own effort or a combination of both.

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THE OFFSHORE DRIFT OF LARVAE OF THE CALIFORNIA SPINY LOBSTER *PANULIRUS INTERRUPTUS*[1]

MARTIN W. JOHNSON

The information that I have to offer regarding the drift of the larvae of the California spiny lobster *Panulirus interruptus* (Randall) will perhaps not contribute much directly toward indicating the relative strength of the northward components of water flowing along our coast during the past year. It will, however, point up some interesting questions regarding water currents and recruitment of lobster stock. I will be able to show you in a way that has not been done before, what happens to a floating population of larvae originating near the coast of Southern and Baja California.

For the discussion to follow it is important to note that the adult *Panulirus interruptus* is reported to range from slightly north of Point Conception southward to Manzanillo, Mexico. From the present study it appears that the main centers of concentration are in the regions of the Channel Islands and of Cedros Island off Baja California.

For those of you who are not familiar with the life history of the spiny lobster, it should be said here that the adult female carries its eggs attached to the swimming feet. The larvae, which hatch from the eggs, are thoroughly transparent, flat and thin as a bit of paper, and are known as "phyllosoma." The first stage is about $1\frac{1}{2}$ mm long and the last stage is about 30 to 32 mm long. Between each stage, of which there are eleven, there is a shedding of the old skin to allow for increased growth.

When released in the water, these larvae float about with prevailing currents like so many tiny drift bottles. The last phyllosoma stage metamorphoses into a "purulus" stage which, though still transparent, resembles the adult and soon seeks the bottom to assume the adult habit.

With this brief background we can now discuss in a summary way, when and where these larvae first appear in the plankton as Stage I, and when and where they are found in subsequent stages. A much more complete analysis will be published in the Bulletin, Scripps Institution of Oceanography, University of California, 1960. Here, it will suffice to give only a few typical examples selected from seven years of study based on the monthly plankton collections made by the California Cooperative Oceanic Fisheries Investigation. Involved are a great number of stations extending along the coast from above Cape Mendocino to well below Cape San Lucas, and seaward to distances up to 200 to 300 or more miles (Fig. 1). The collections were made with a one-meter net towed obliquely usually from 70-0 meters or 140-0 meters.

Figure 135 shows the periods of the year in which each of the phyllosoma stages I to XI were found during each year of the seven-year study period. The first stage occurs only from about mid-June to mid-November (once in early December). A line drawn through the mid-period of occurrence of the successive stages, indicates that the total larval life, Stages I to XI, requires about $7\frac{3}{4}$ months. Hence, for this long period the larvae are presumably drifted about at the mercy of prevailing water currents.

In a summary (Fig. 136) of many samples, it can be seen that the source of larvae is at the immediate coast or in the vicinity of islands. This is, of course, in keeping with the known distribution of the adults.

The later larval stages occur in diminishing numbers and usually at greater distances from the coast (Fig. 137). In general, there is a drift of larvae to the south and southwest. This is to be expected in view of the prevailing southward flow of the California Current.

Rarely are larvae found to the north of Point Conception. There is a notable exception shown in figure 138. A Stage X larva was caught in May 1954 about 200 miles at sea in the latitude of Monterey Bay. It is difficult to account for this specimen on the basis of the calculated prevailing currents. It was found in water characterized as Southern or Central Pacific by the presence of only one variety, (i.e. *californicus)* of the copepod *Eucalanus bungii,* in contrast to the inshore tongue of colder water where the northern variety *Eucalanus bungii bungii* constituted up to 28 percent of this species. There is also a record of a Stage I larva taken August 1954 near the coast just north of Monterey Bay in 14°C water in which only *Eucalanus bungii californicus* was found.

Evidence of flushing of larvae from a restricted area is shown in figures 139 and 140. The area around the Channel Islands was surveyed by a cruise, the second half of which immediately resampled the stations visited during the first half. During the first sampling 41 percent of the stations located inshore of the dashed line shown in the figures yielded Stage I larva, whereas none were found there during the second sampling.

Despite these instances of larval dispersal, it is amazing that when the whole area is considered, there is so little direct evidence of larvae being flushed wholesale from the area. Evidently there prevail along the coast countercurrents, long back swirls, and eddies that effectually retain a good number of larvae up through the later stages within or near the area of adult distribution even for so long a period as $7\frac{3}{4}$ months. The calculated dynamic anomalies, which

[1] Contribution from the Scripps Institution of Oceanography.

were here kindly provided by the Hydrographic Section, describe patterns of flow tending to support this view. The charts shown in figures 141 to 147 illustrate such back currents as are at times detected from hydrographic studies. Some of these charts also show sections as having currents which, if continuous for long periods, would flush all surface living larvae from the area.

Doubtless many, if not most, of the later stage larvae which we have caught at stations distant from the shore or south of the heavy dashed line shown in figures 136 and 137 are on their way out on currents that will sweep them into uninhabitable areas. What portion of the larval population this loss may represent we cannot estimate with any certainty without further extension of our collecting methods to include more sampling at or near the bottom in both shallow and deep water. For it is not entirely clear just what hydrographic mechanisms combined with larval behavior make possible a recruitment of lobsters sufficient to support the rather stable lobster fisheries that we enjoy.

DISCUSSION

Sette: Dr. Johnson, I am going to ask the first question. Do you have some data on larvae for the last year or two?

Johnson: We have not sorted all of the 1957 material as yet but we have made certain spot tests to give some idea of what is taking place with regards to larval distribution in 1957. At least some significant data seem to be emerging. In the first place, the larvae appear not to have been swept in detectable numbers to the north of Point Conception, or if they are swept to the north they quickly succumb to changing conditions. But as shown in figure 147 for the July 1957 cruise, there is an unusually large number of early-stage larvae in the region of the Channel Islands. The number of Stage I larvae taken in that area was far above average for the area for past years, and furthermore these larvae were present in numbers greater than ever before during the month of July, which appears to indicate that hatching had begun about a month earlier than usual.

Another unexpected feature of this cruise is the almost complete absence of larvae from the collections south of Punta Eugenia as if there had been an intrusion of phyllosoma-free water from offshore. The dynamic height anomalies seem also to bear this out. It can also be said that more late stage larvae were taken between February and July than for that period in any previous year. Thus, while the data do not show more drift out of the area, there appears to have been some earlier hatching in the northern part of the range and probably a better survival to later stages or better retention, especially in the Baja California area.

Isaacs: Would you say that most of the population has shifted to the north?

Johnson: No, larvae are still abundant in the central Baja California area, but there might have been some shift northward from below Punta Eugenia during July. However, for other months, especially October 1957, this is not borne out.

Berner: Could the larger number of Stage I larvae in the Channel Islands area be explained by a water movement along the coast carrying larger numbers from the south into the Channel Islands area?

Johnson: Yes, this could be so, since the larvae apparently remain in Stage I for a matter of two to three weeks. However, the presence of so many as 76 larvae at one station argues against the idea that there had been much opportunity for dispersal prior to the catch. But still it is probable that the lobsters in that area are largely restocked by settlement of larvae that have drifted in from the south. The extent of adult migration into the area is not known.

Radovich: Regarding the possibility of the lobsters getting back to the Channel Islands area, I talked with a gentleman from the cannery at San Quentin who mentioned a rather interesting phenomenon. He had observed a school of full-sized, 12-16-inch lobsters swimming at the surface. He had never seen this before but in talking with other lobster fishermen in the area, they told him they had witnessed this phenomenon some few months before. One fisherman has used scoop nets to fill this boat with lobsters. This sounded peculiar to me and I mentioned it to Mr. W. L. Scofield at the California State Fisheries Laboratory. He said that about 20 years ago he interviewed a fisherman who had observed the same phenomenon.

Johnson: On the whole, I have considerable respect for observations made by fishermen, but sometimes they do make mistakes.

Davies: This is similar to a report from one of our South African lobster fishermen, who witnessed thousands of lobsters swimming all in one direction.

Marr: There is a record of marked spiny lobsters at Bermuda having been released offshore at the surface over deep water and fifty miles from the island. Subsequently, they were recovered at approximately the same place from which they were taken originally. The supposition was that they swam back instead of sinking.

Johnson: There have been tagging experiments on our local lobster which show some short migrations but mostly random movements so far as we know.

Sette: I suppose the question before us is whether or not the drift of larvae differed in 1957 from previous years. According to conclusions you have drawn from your charts, they drift with no swimming effort and should reflect changing conditions. We will look forward to a more complete story when more data have become available.

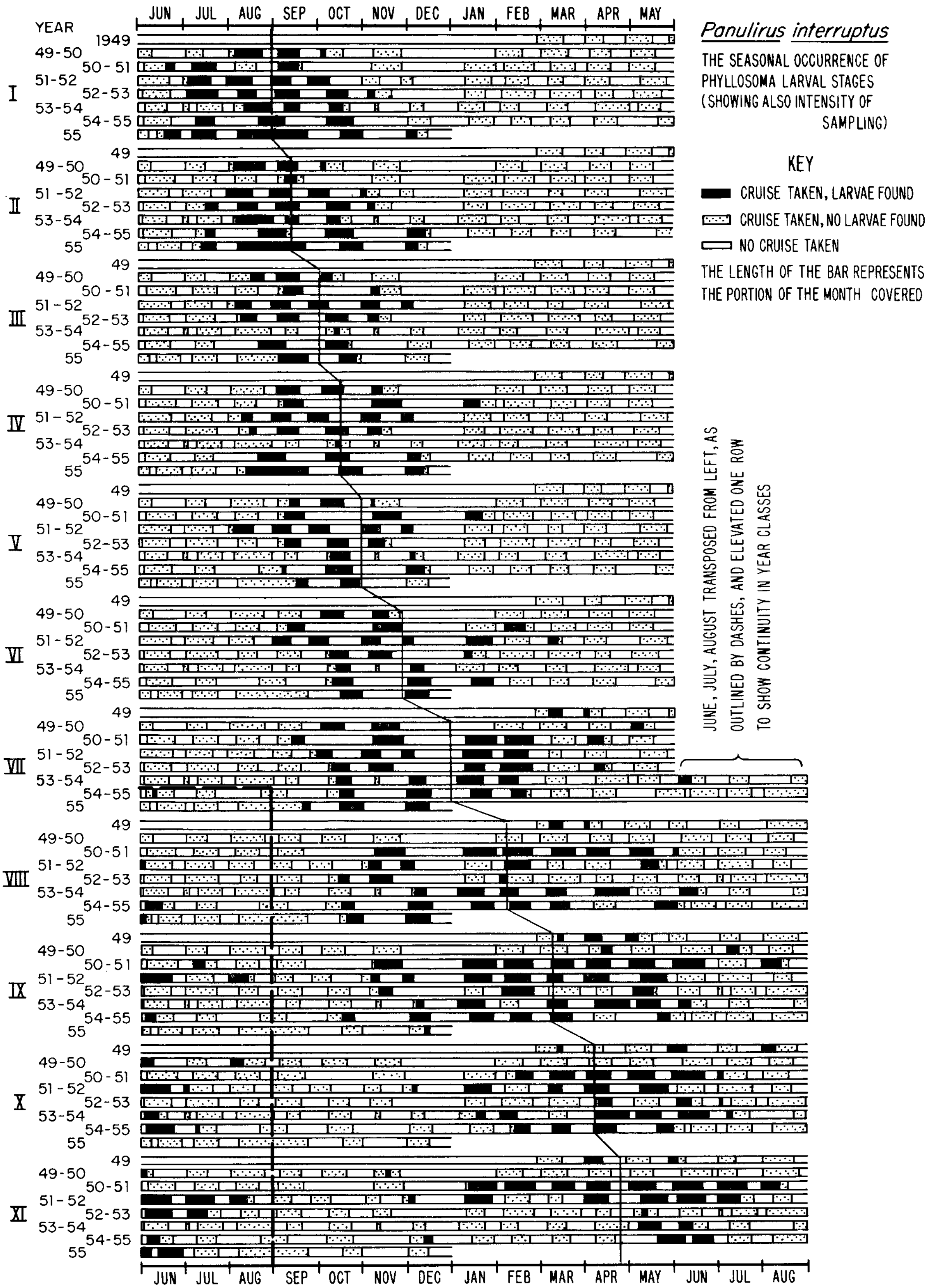

FIGURE 135. Seasonal occurrence and duration of larval stages of *Panulirus interruptus*, with indication of the intensity of sampling.

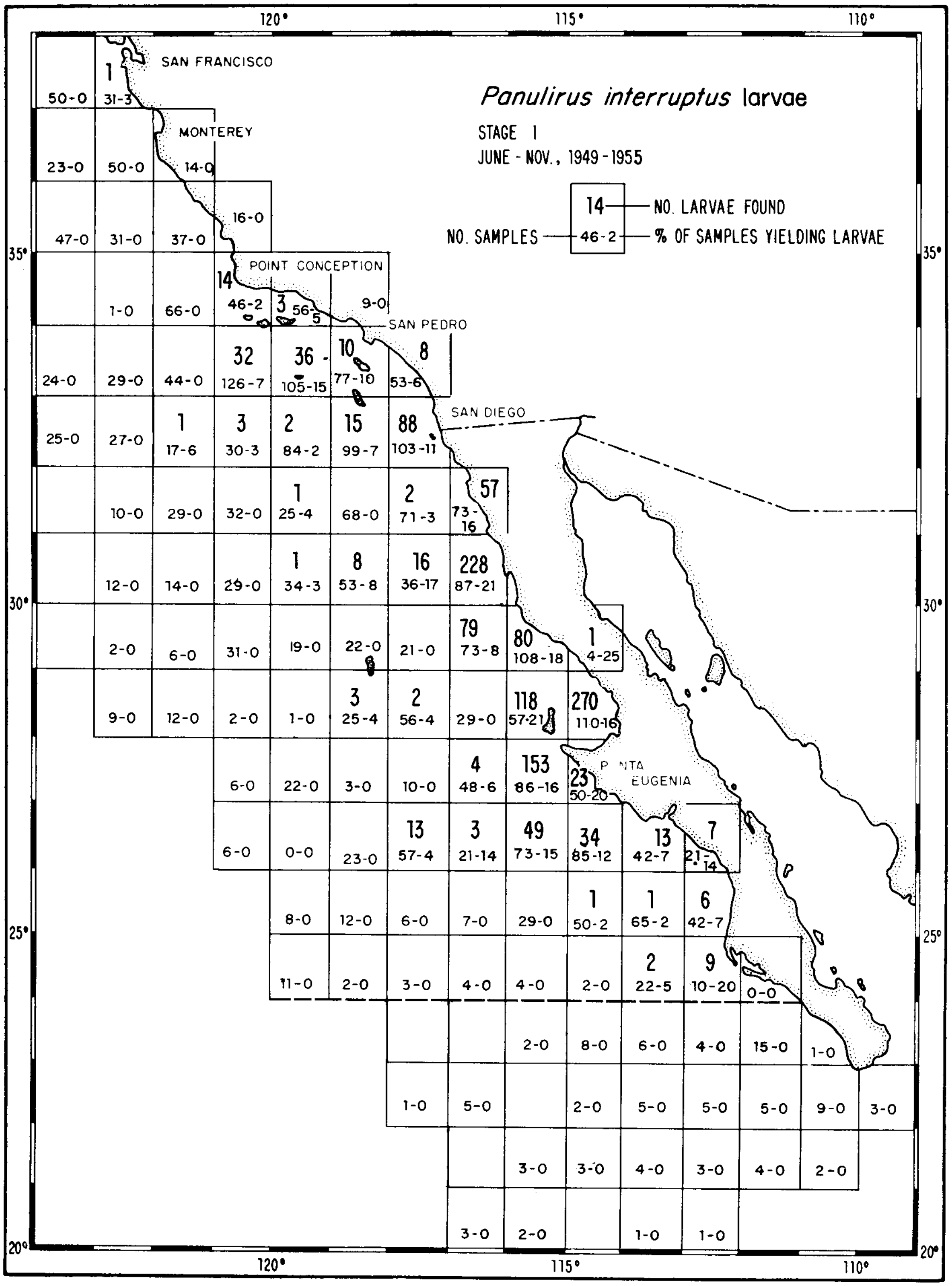

FIGURE 136. Summary of geographic distribution of Stage I phyllosoma larvae of *Panulirus interruptus* for the hatching periods June-November of 1949-1955 inclusive. The number of larvae caught, the number of samples taken, and the percentage of samples yielding larvae are shown for each one-degree square.

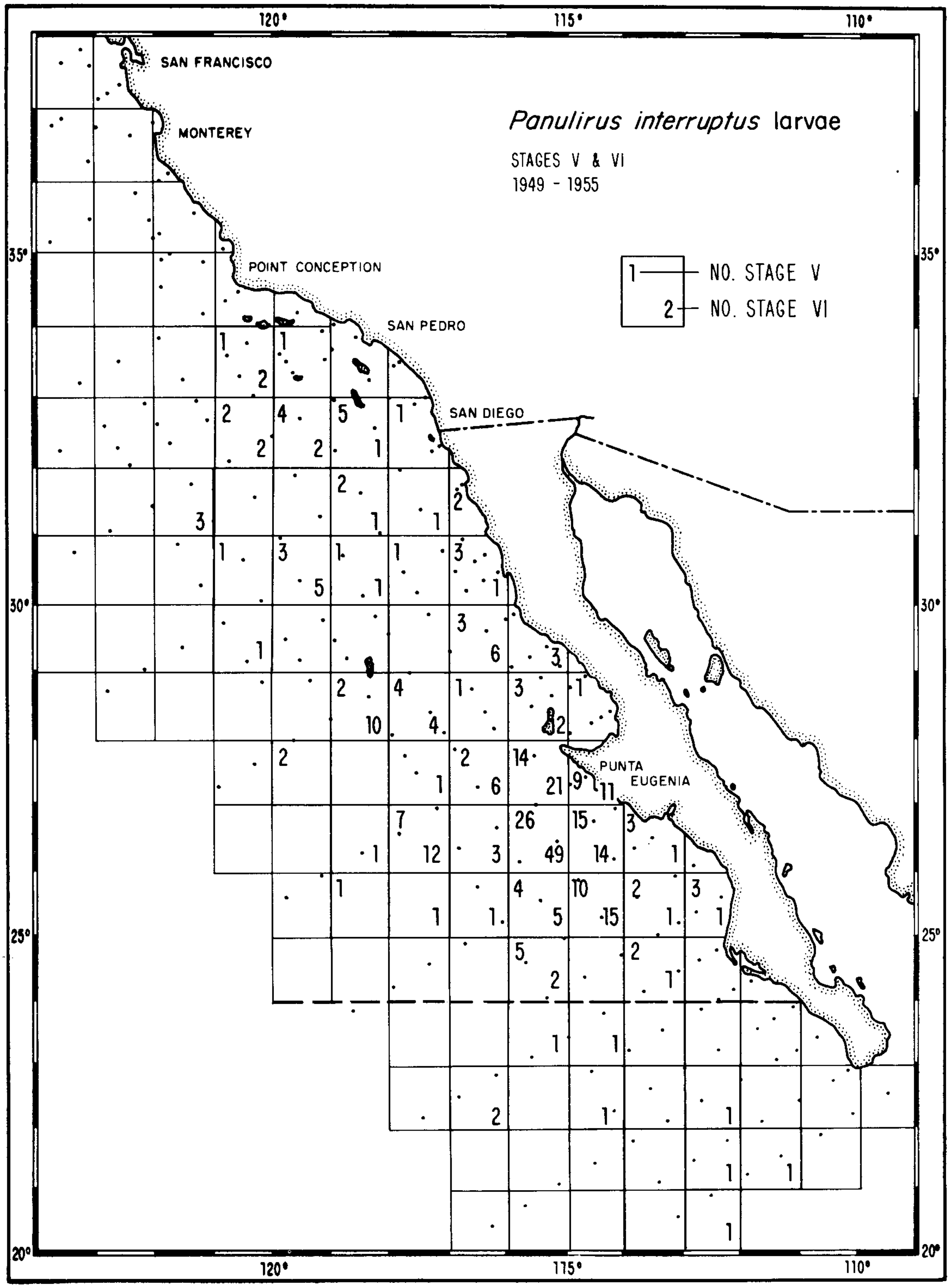

FIGURE 137. Summary of geographic distribution of Stages V and VI larvae of *Panulirus interruptus*, 1949-1955 inclusive. The numbers of larvae caught are shown for each one-degree square.

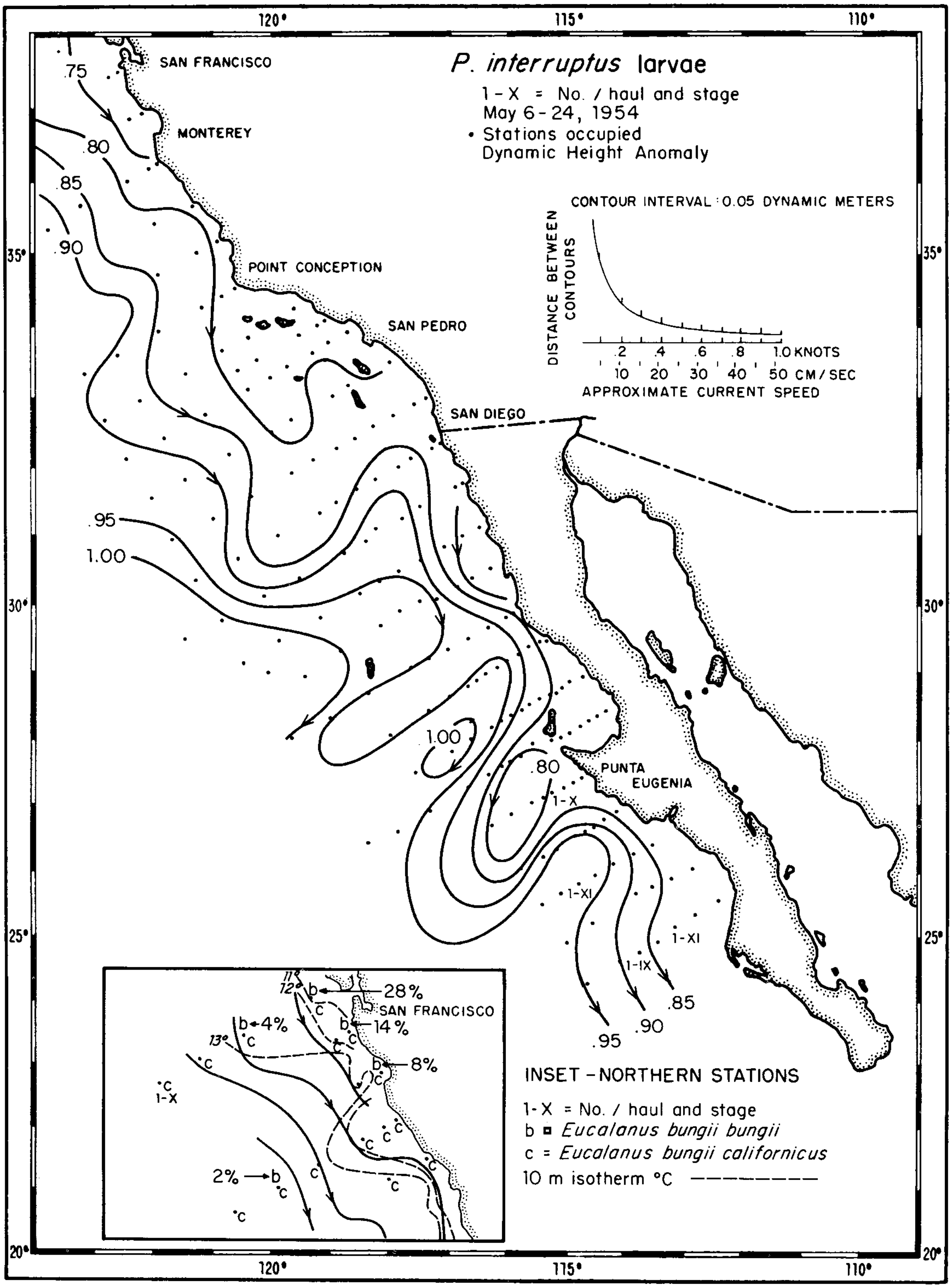

FIGURE 138. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 500 decibars) during May 6-24, 1954 (CCOFI Cruise 5405).

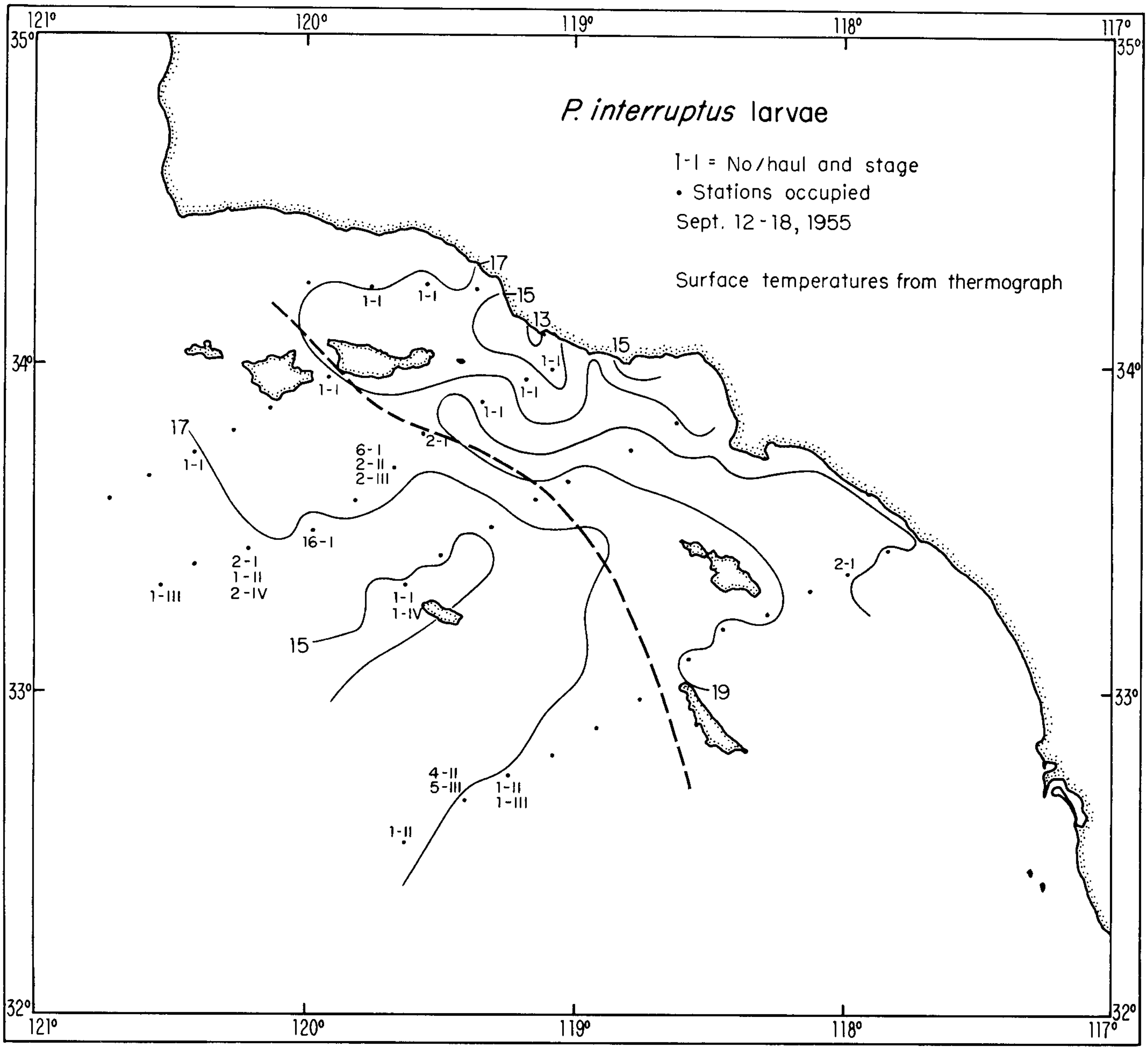

FIGURE 139. Locality records for *Panulirus interruptus* larvae and surface isotherms in the Channel Islands area during September 12-18, 1955 (CCOFI Cruise 5509). Inner and outer areas separated by dashed line.

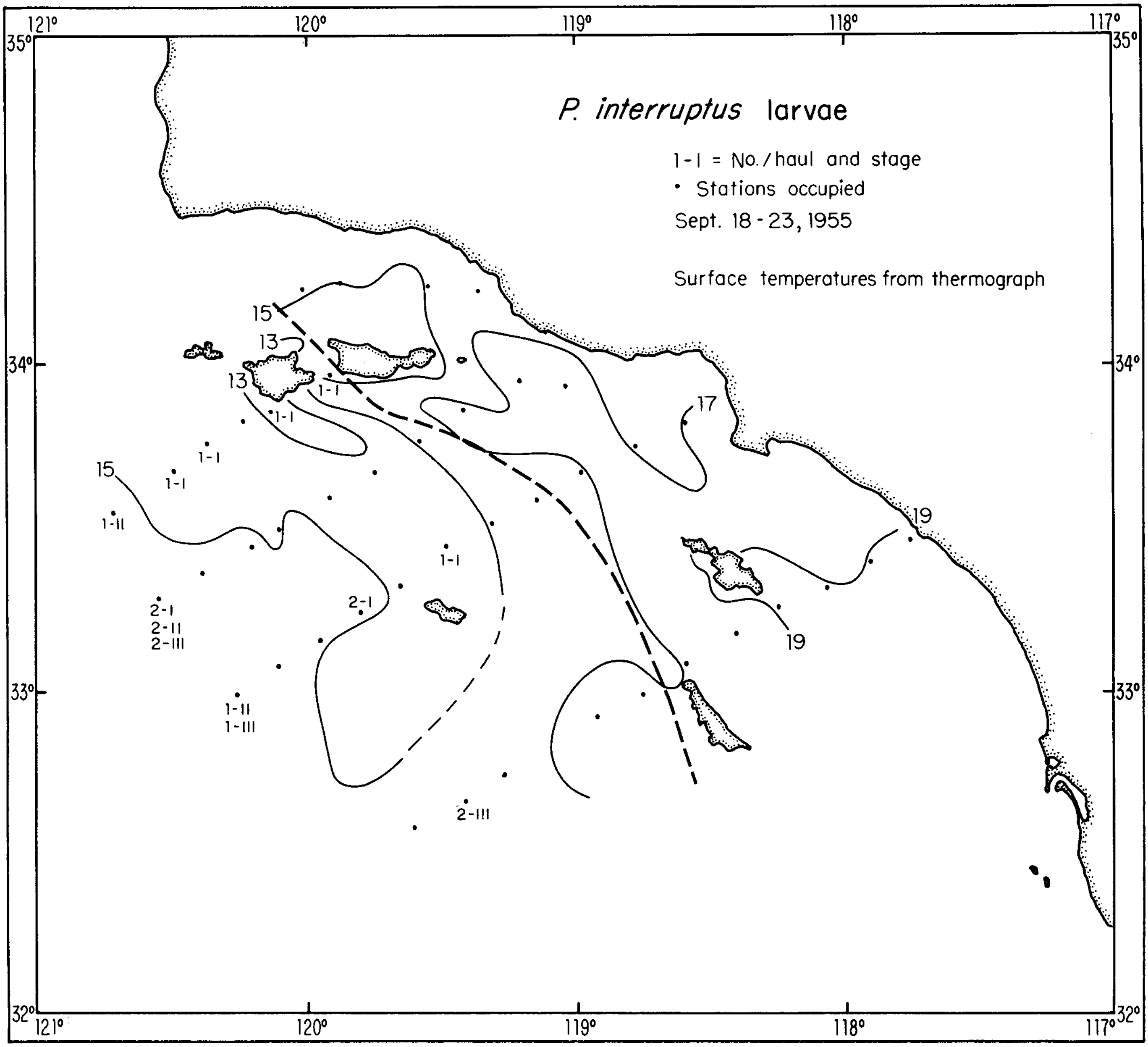

FIGURE 140. Locality records for *Panulirus interruptus* larvae and surface isotherms in the Channel Islands area during September 18-23, 1955 (CCOFI Cruise 5509).

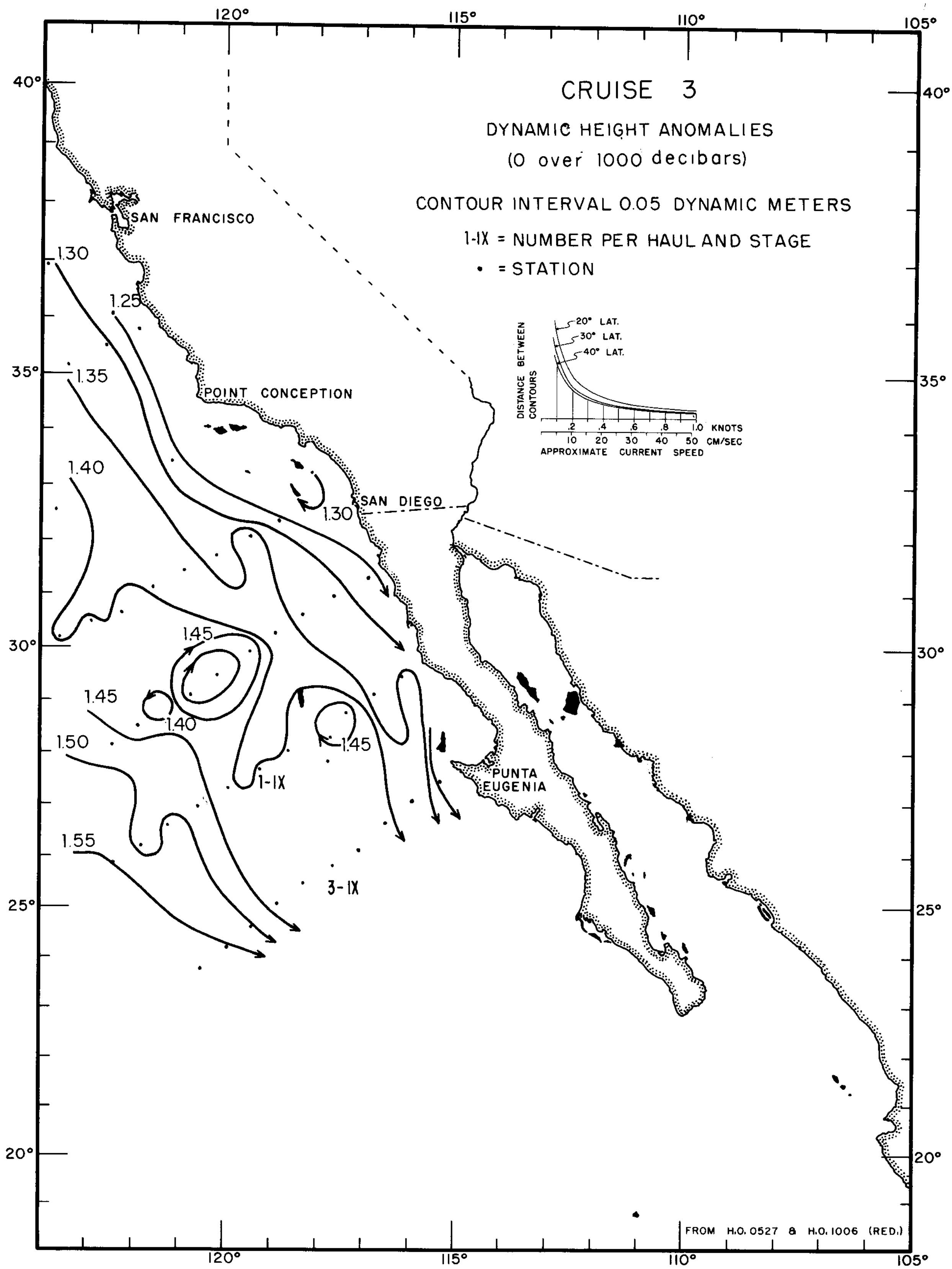

FIGURE 141. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 1000 decibars) during April 28 to May 14, 1949 (MLR Cruise 3).

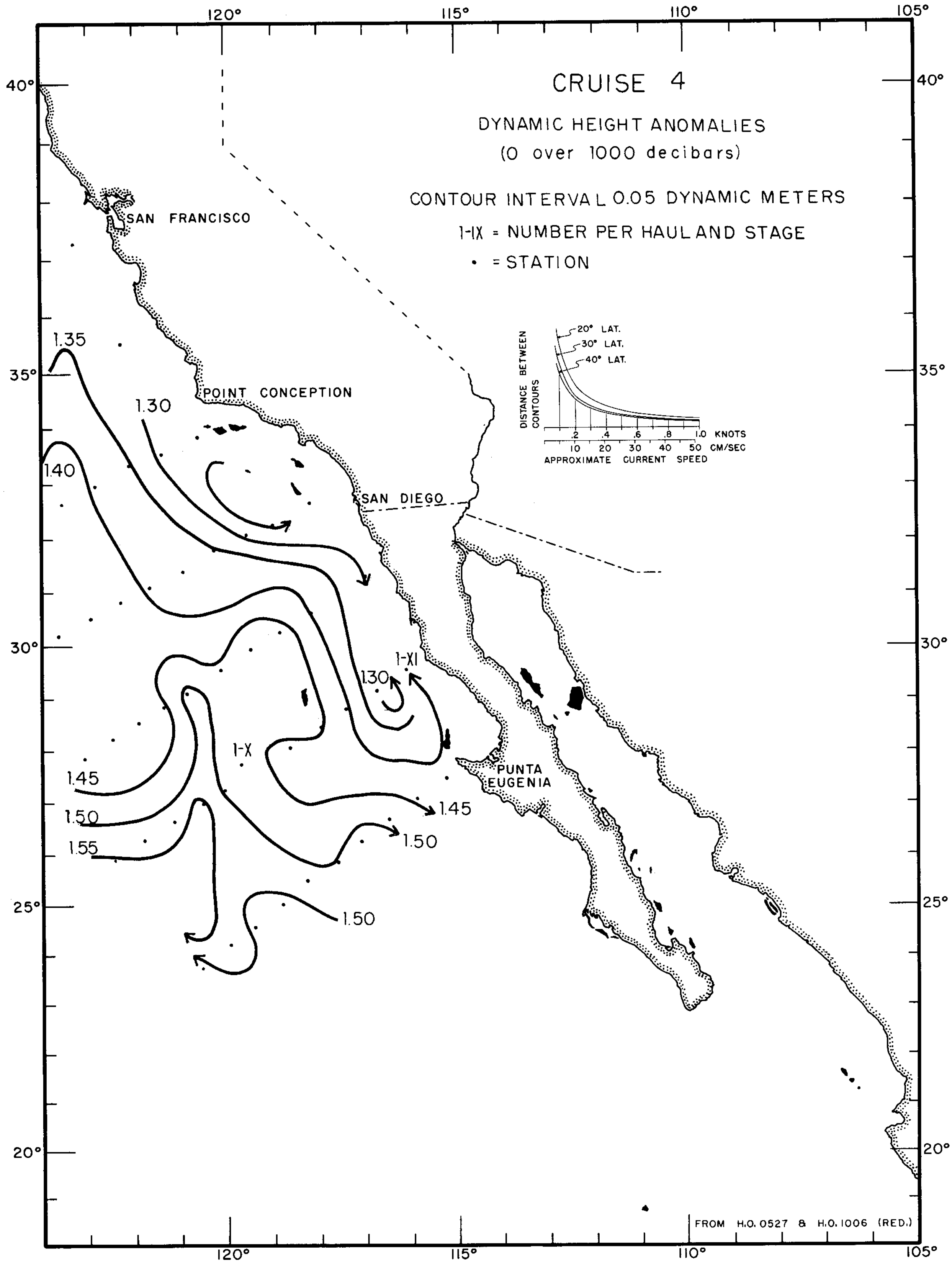

FIGURE 142. Locality records for *Panulirus interruptus larvae* and dynamic height anomaly (0 over 1000 decibars) during May 28 to June 9, 1949 (MLR Cruise 4).

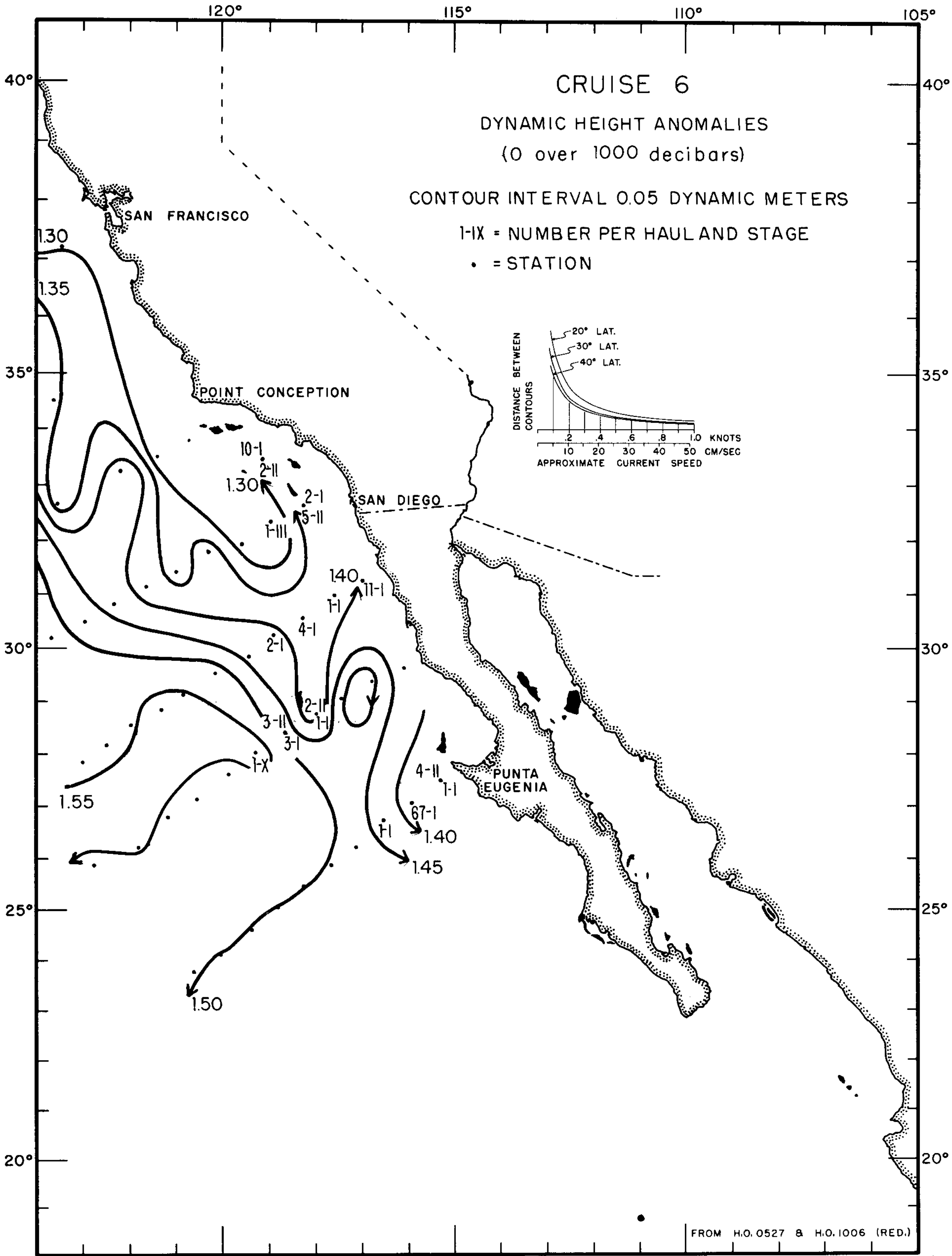

FIGURE 143. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 1000 decibars) during August 2 to 22, 1949 (MLR Cruise 6).

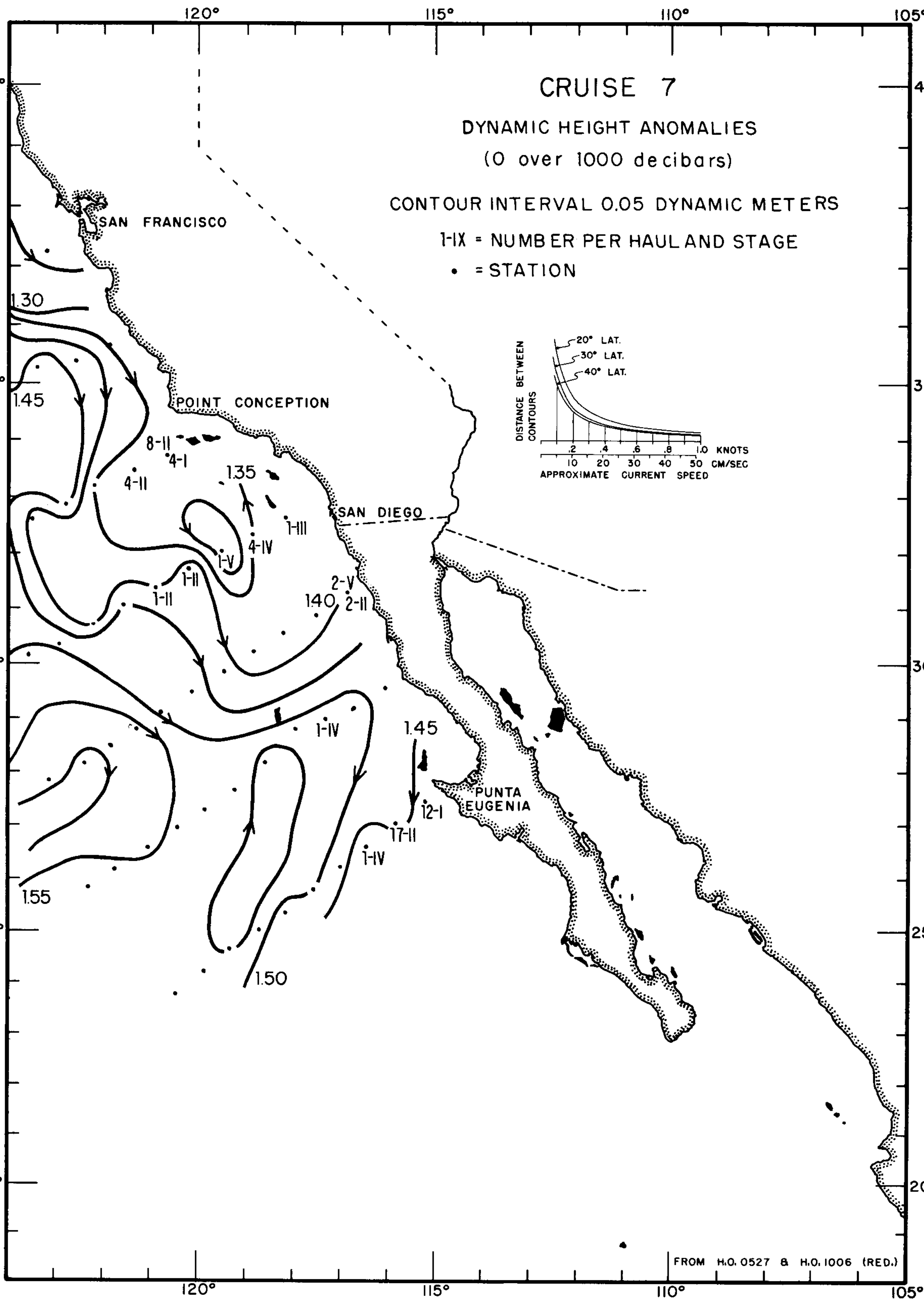

FIGURE 144. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 1000 decibars) during September 4 to 18, 1949 (MLR Cruise 7).

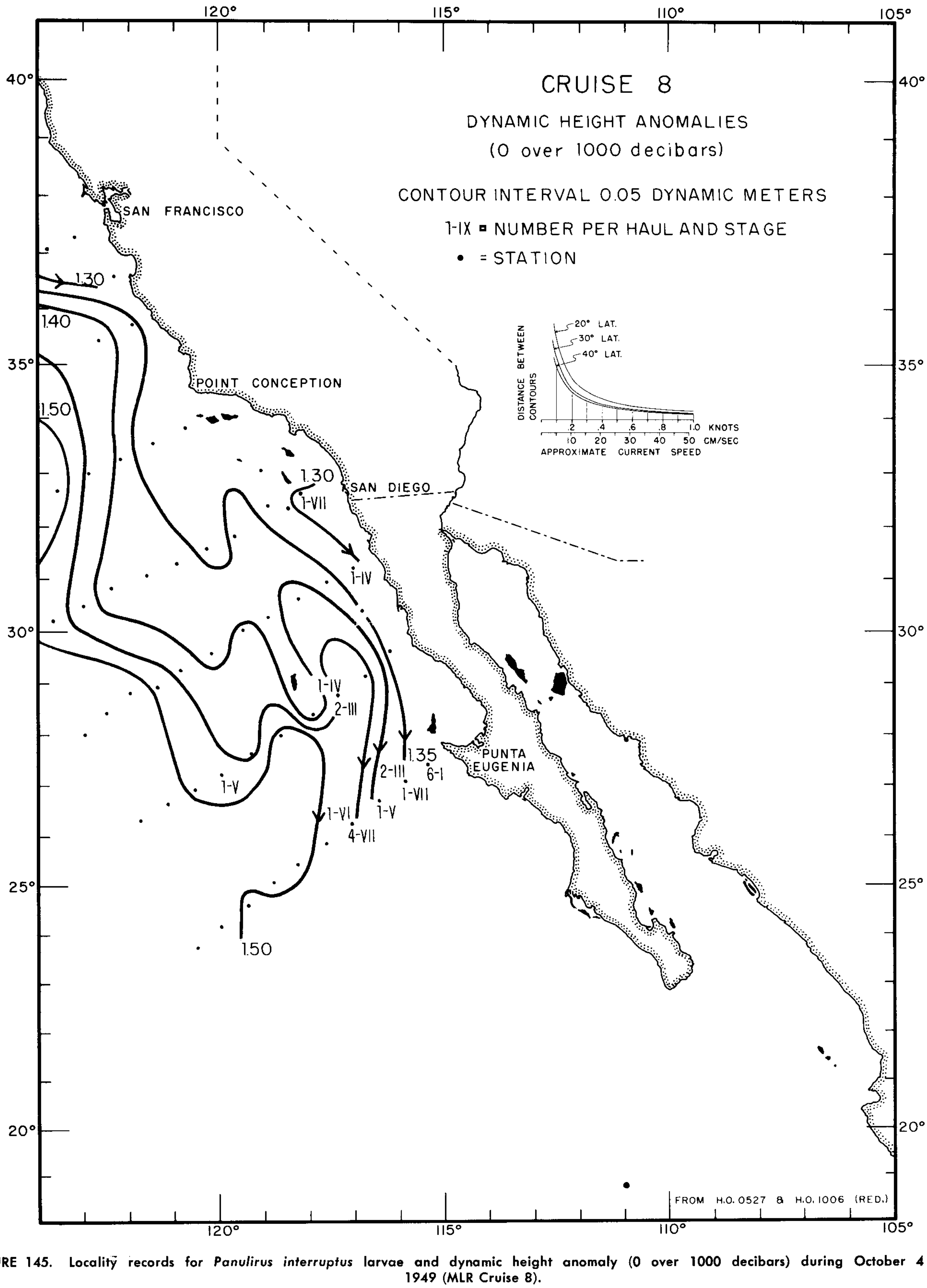

FIGURE 145. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 1000 decibars) during October 4 to 19, 1949 (MLR Cruise 8).

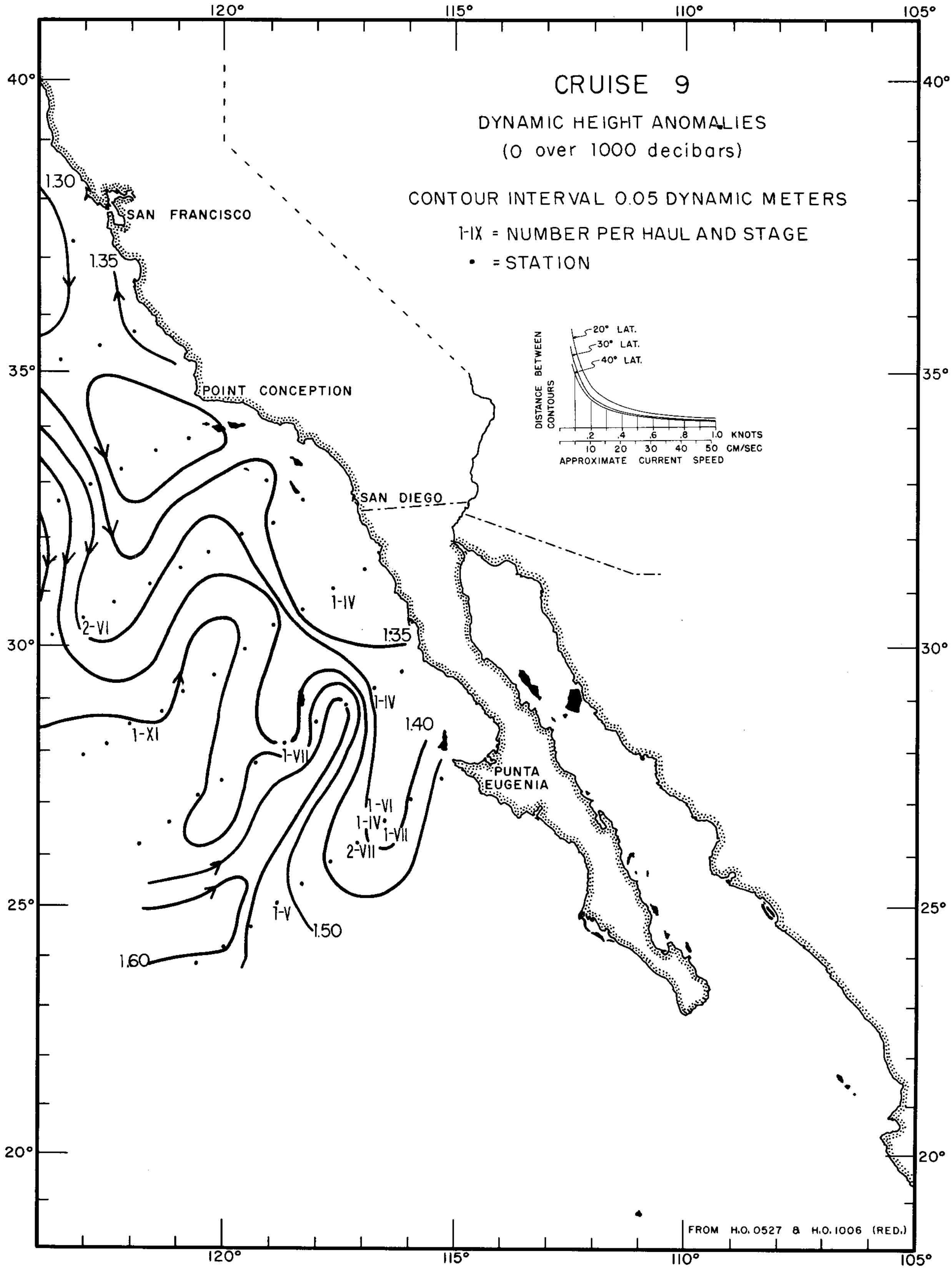

FIGURE 146. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 1000 decibars) during November 8 to 25, 1949 (MLR Cruise 9).

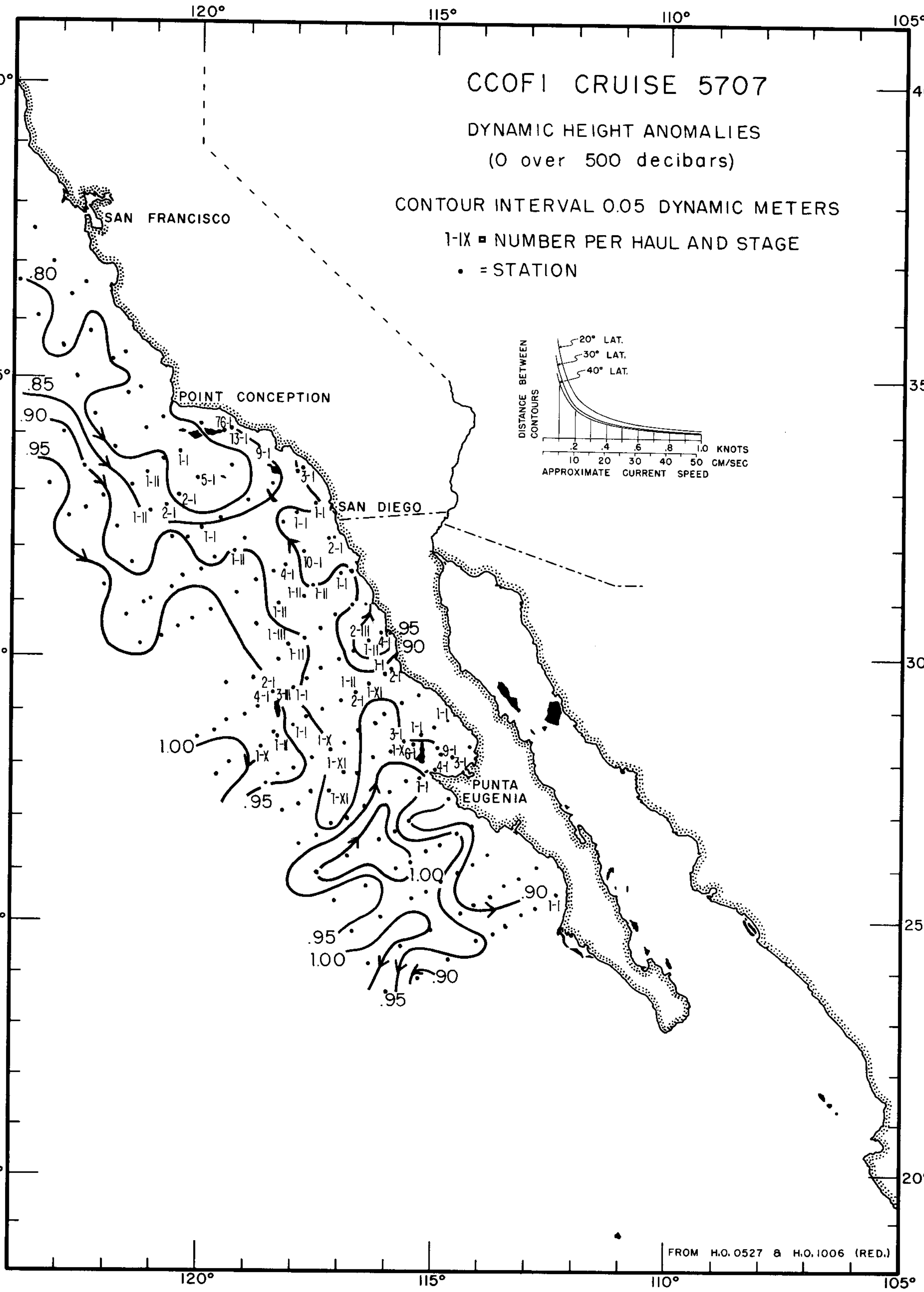

FIGURE 147. Locality records for *Panulirus interruptus* larvae and dynamic height anomaly (0 over 500 decibars) during July 8 to August 3, 1957 (CCOFI Cruise 5707).

REDISTRIBUTION OF FISHES IN THE EASTERN NORTH PACIFIC OCEAN IN 1957 AND 1958

JOHN RADOVICH

Anomalies of fish distribution may be regarded as an effect of variations in ocean climate. During the extremely warm years of 1926 and 1931 there was a heavy influx of southern species into the waters of California (Hubbs and Schultz 1929 and Walford 1931), and again in 1957 and 1958, there has been a mass northward movement of southern species. Examples of the northward distribution of southern fish have been listed in Table 1.

TABLE 1

Some of the Warm Water Fish Species Collected in California Waters During 1957 and Early 1958

No. taken	Common name	Scientific name	Years formerly reported	Location of capture in 1957
1	Bullet mackerel	*Auxis* sp.	1918 1919 1935	Coronado Island
2	Sharpchin flying fish	*Fodiator acutus*	1931	Long Beach
1	Tai or Porgy	*Calamus brachysomus*	1953?	Oceanside
1	Shortnose spearfish	*Tetrapturus anguistirostris*	(Never previously taken off California)	60 mile bank
1	Spiny trunkfish	*Lactoria diaphanus*	1932 1933 1949 1951*	Santa Monica Bay
2	Pilotfish	*Naucrates ductor*	1926 1936 1945	San Clemente Island
5	Triggerfish	*Verrunculus polylepis*	1924 1931 1946 1950 1951 1956*	Santa Monica Bay, Laguna Beach and San Diego Paradise Cove Dana Point
1	Monterey spanish mackerel	*Scomberomorous concolor*	1931 1937 1939 1944 1947 1948 1949 1951 1952 1953 1954 1956*	Santa Barbara
1	Green jack	*Caranx caballus*	1858 1924 1945 1953 1955*	Belmont Shore
1	Razorback scabbardfish	*Assurger anzac*	1951	Coronado Island
1	Thread herring	*Opisthonema libertate*	1947 1948 1954*	Belmont Shore

* Probably other years, also.

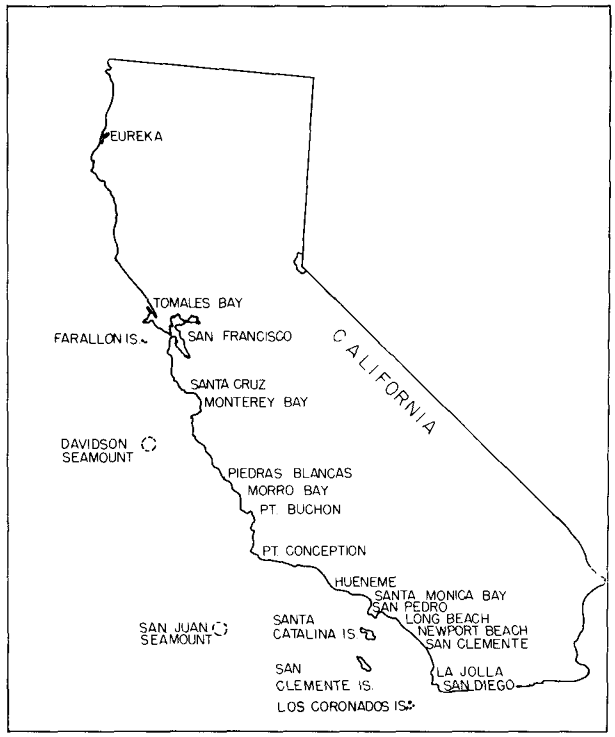

FIGURE 148. Location chart of the coast of California.

Since preparation of this table reports of southern species of fish have continued to come in to the California State Fisheries Laboratory. Another pilot fish *(Naucrates ductor)* was collected at San Clemente Island (Fig. 148) during August of 1957. A razorback scabbord fish, *(Assurger anzac)* was caught off North Coronados Island in May. This species had not been taken locally since 1950 when one was collected from Santa Monica Bay. Incidentally, some of these occurrences are of rare species and do not necessarily represent northward extensions. A yellow snake-eel *(Ophicthus zophochir)* was collected at Newport Beach, California, in December 1957. Another trigger fish *(Verunculus polylepis)* was collected in March of 1958 off Dana Point (north of San Clemente City). A thread herring *(Opisthonema libertate)* was caught off Belmont pier (south of Long Beach) on May 29, 1958. In August of 1957 an ocean white fish *(Caulolatilus princeps)* was caught off the Farallon Islands.

Among the conspicuous invertebrates, *Velella* Sp. have been abundant from San Francisco to Alaska. The pelagic red crab *(Pleuroncodes planipes)* was abundant in December of 1957 from La Jolla to San Pedro. Its last occurrence in real abundance was in April of 1941, another year of high water temperatures. Green sea turtles were seen by albacore fishermen, during 1957, as far north as Oregon and Wash-

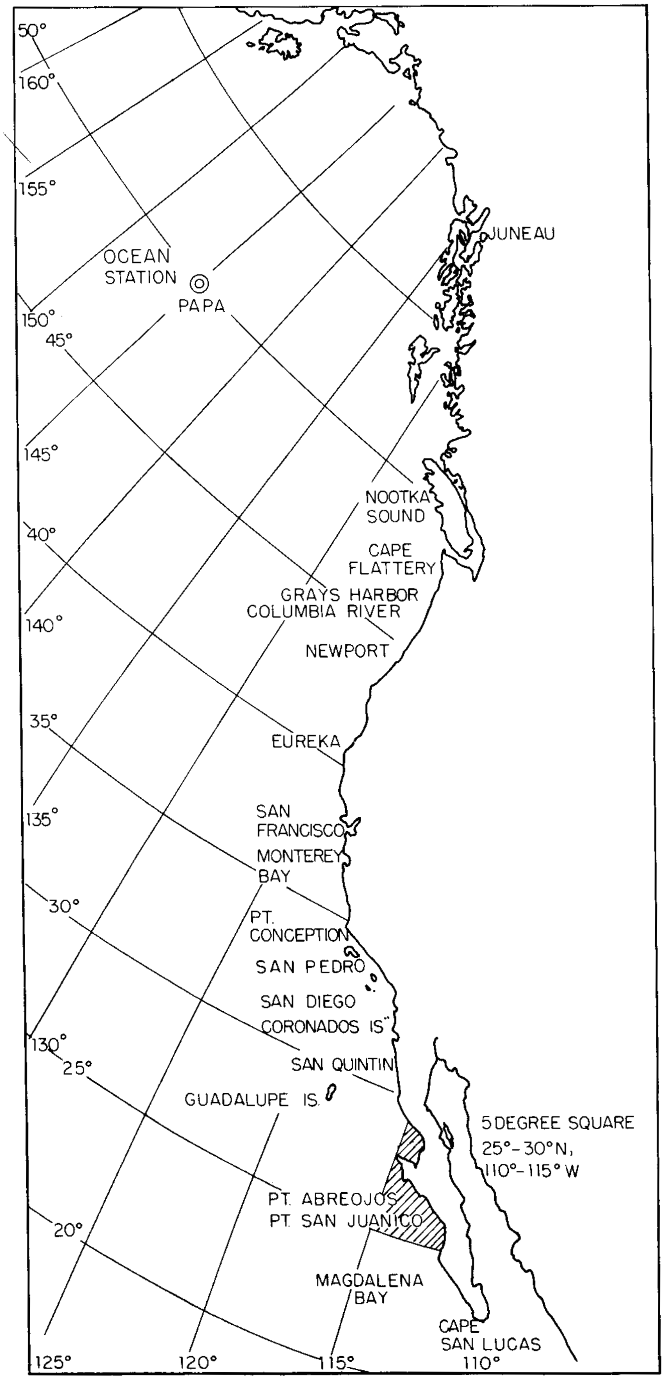

FIGURE 149. Location chart of the Pacific Coast of Alaska, Canada, United States, and Baja California, Mexico.

ington and off British Columbia as far as Nootka Sound in April and May of 1958.

In addition to these species, Dr. Hubbs has called to my attention the fact that a large number of tropical sea birds were seen far north of their usual range, including southern skuas; frigate birds; and black, ashy, and leach petrels. Also, I received a note from British Columbia indicating that at ocean station "PAPA" at 50°N. Lat. and 145°W. Long. (Fig. 149), black-footed albatross were less abundant than usual during late summer of 1957, while Laysan albatross were more numerous; becoming even more common than the black-footed albatross.

One of the first unusual events that was called to our attention was a die-off of sea gulls in May of 1957. Although we were able to confirm the fact that there was a widespread die-off, we did not find out why the gulls were dying, however, concurrently red water, which usually occurs during warmer months, was noticed off the coast. During the same month, sea otters were reported to have been observed in an area just north of Piedras Blancas, for the first time in the last three decades. They were eating quantities of abalone. The California Department of Fish and Game abalone diving crew investigated the reports and found many broken abalone shells showing typical sea otter breakage patterns.

Probably the first real clue the California Department of Fish and Game had of the changed ocean climate came from the popular sportfish, the yellowtail, *(Seriola dorsalis)*. Yellowtail catches at Los Coronados Islands were exceptionally good in March. By the end of April, when we became alerted to the fact that certain events were unusual in 1957, the yellowtail sport catch had exceeded the total of the previous year. In May, more yellowtail schools were observed during the Department's aerial surveys than on any during the previous three years. Some schools were seen as far north as Catalina Island. Through May and June sports fishing continued good and by the end of July the yellowtail catch had exceeded 100,000 fish. By the end of August, the catch was more than double the next best recorded year, 1947. In September, sport fishing continued good throughout Southern California and the October catch of yellowtail exceeded all other species taken on party boats. Some yellowtail were caught in the Los Angeles-Long Beach area during November and December, while the catches off Los Coronados Islands remained good. From January to April of 1958, yellowtail continued to be caught off Southern California and on May 4th 1958 several small yellowtail between two and three pounds were caught outside the Long Beach breakwater. They were mixed with a school of bonito. Another small yellowtail was also caught on May 4th from kelp off Long Beach. It was twelve inches long and weighed only 16 oz. These were the first juvenile

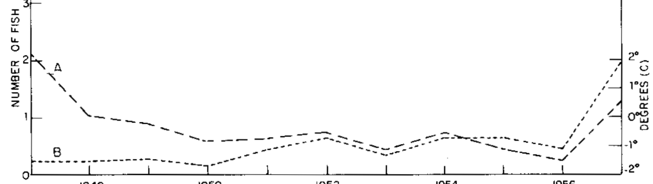

FIGURE 150. Party boat catch per angler day (1947-1957) of (A) barracuda off Southern California, and (B) yellowtail off the Los Coronados Islands, Baja California.

yellowtail recorded from Southern California by the California Department of Fish and Game.

Barracuda *(Spyraenia argentia)* fishing was also phenomenal, but it did not start until a little later. On figure 150 you will note that the catch per angler day was high in 1957 for both the yellowtail and the barracuda. In addition, the manner in which the sport catch of these two species has varied since 1947 is similar, indicating that sportfishing for both species might be related to the same oceanic conditions.

To examine this more closely, let us look at what is known about the distribution of these two species. In general, the Southern California area represents the northern limit of the yellowtail population, which extends southward along the Baja California coast and into the Gulf of California. While the yellowtail is, perhaps, the most popular marine sport fish in Southern California, most of its population is far south of the sport fishery. On the other hand, the barracuda population does not extend nearly as far southward as does the yellowtail. Its biological range extends only to Magdalena Bay. South of Pt. Abrejos, the population tapers off rapidly and barracuda are encountered only occasionally south of Pt. San Jaunico. In the past six or eight years, barracuda have been plentiful in the area just north of Pt. Abrejos. Although their abundance drops off abruptly at the southern end of their range, they extend northward a considerable distance and gradually diminish in numbers. This suggests that they prefer the warmer waters of their range, but have a broad tolerance for cold water. Apparently they do not tolerate the warmer water south of Magdalena Bay but are most abundant near the warmest part of their range.

The barracuda fishery usually develops in the late spring or summer off Southern California. Yellowtail show up during the early spring. Both are caught every summer by sportfishermen and neither remain all winter except perhaps, during years of unusually warm water. Therefore, Southern California is a fringe fishing area for both species.

Since both species begin to spawn around July, a preference for a narrow range of temperatures during their spawning period could influence their movement northward.

The barracuda catch per angler day for all anglers fishing on party boats off Southern California, and the yellowtail catch per angler day for all anglers fishing on party boats at Los Coronados Islands, for each year since 1947, are shown on Figure 150. Since these are the preferred species, I am assuming that whenever party boats fished, they were fishing for the two species. When large numbers of other fish are caught, it is usually because the two preferred species are less available. Prior to 1957 the yellowtail sport-catch was predominantly from the Los Coronados Islands area, but in 1957-1958, yellowtail were caught throughout Southern California waters. The graphs of catch per angler day for the two species are similar. In fact, allowing for the difference in trends, the variations correspond closely. If barracuda favor the warm waters of Baja California, and abruptly cease to

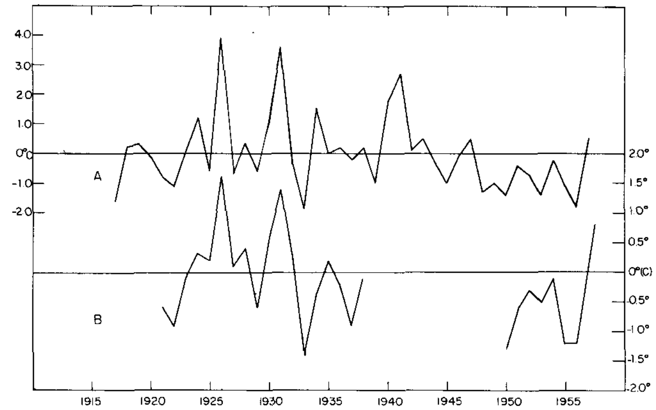

FIGURE 151. Average monthly deviations of sea surface temperatures from January to June at (A) La Jolla, and (B) 5 degree square 25°-30°N latitude, 110°-115°W longitude.

exist at the southern end of their range, then a rise of water temperature in the south may thrust the population northward. Since the fish seem to be more tolerant of colder water, they may not be pushed southward as rapidly by a drop in temperature. Eventually, they may return to the warmer water they prefer, but over a longer period of time.

Figure 151 shows the La Jolla monthly surface temperature deviations from the 1917-1955 monthly means, averaged for January through June of each year, and the January to June average of monthly deviations of surface temperatures from the 1921-38 means, of the five degree square 25°-30° N. Lat., 110°-115° W. Long. The five degree square (Fig. 149) encompasses the southern limit of the barracuda population, and the center of the yellowtail distribution. Unfortunately there are no temperature data for this area from 1939 through 1949. However, since the La Jolla temperature deviations so closely resemble those of the five-degree square, they may be used to indicate changes which occurred during this time. You will note that the warmest January through June temperatures occurred during 1926, 1931 and 1941. The coldest temperature occurred during 1933 and the water temperatures since 1947 have been below the 1917-1955 mean, until 1957.

In figure 152, the yellowtail and barracuda catch per angler day may be seen to vary closely with the January to June average temperature deviations. Therefore, it appears that even during the "monoton-

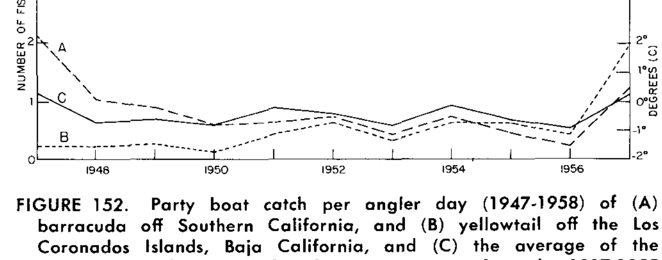

FIGURE 152. Party boat catch per angler day (1947-1958) of (A) barracuda off Southern California, and (B) yellowtail off the Los Coronados Islands, Baja California, and (C) the average of the January-June deviations of surface temperatures from the 1917-1955 mean at La Jolla.

ous, cool'' years since 1947, variations in the environment have elicited a response by barracuda and yellowtail.

To carry the barracuda extension still further, in 1958, for the first time in several years barracuda were caught in Monterey Bay. During April of 1958, many were caught as far north as Santa Cruz by sportfishermen and some were reported from the Farallon Islands. We have considered that Southern California is a fringe fishing area for barracuda, but the waters off central California are in even more of a fringe area. California Department of Fish and Game commercial catch records reveal that; in 1926, about 67,000 pounds of barracuda were delivered to fish markets in the Monterey Bay area; in 1927, about 2600 pounds were delivered; and in 1928, only about 1000 pounds. Water temperatures were high during 1926, yet in 1927 and 1928, when temperatures were not particularly high, some fish still remained in the Monterey area. Apparently, they were lingering in the northern waters for a couple of years after they had been pushed up by the warmer waters. No barracuda were caught, commercially, in Monterey Bay during 1929 and 1930; but in 1931, when the water temperatures rose markedly, 140,000 pounds were caught in Monterey Bay. Although the water cooled in 1932, 2000 pounds were caught; and in 1933, the year of the coldest water temperatures of the series (1917-1955) 29 pounds of barracuda were caught in Monterey Bay. Following two years which were colder than normal, a catch of 58 pounds in 1934 indicated that some barracuda were still present in the Monterey Bay area.

From 1935 to 1940 no barracuda were caught in this area, but in 1941 the water temperatures rose again, and 1550 pounds of barracuda were landed. Since

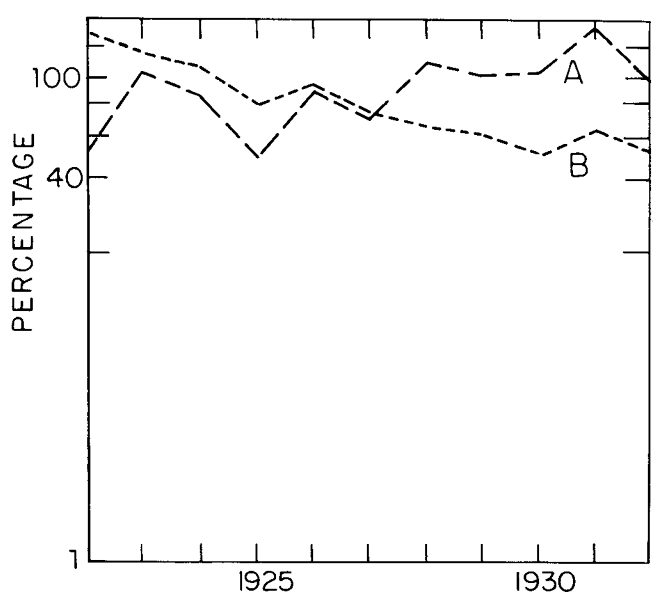

FIGURE 153. Graph showing a comparison of the relative average daily boat catches of (A) barracuda, (B) yellowtail. Reproduced from Whitehead, S.S., Condition of the Yellowtail Fishery, vol. 19, no. 2, *California Fish and Game*, where it appears as Fig. 66.

1941, no barracuda have been delivered at Monterey, however in April of 1958, following high winter surface temperatures, barracuda again appeared in the Bay. In a paper by S. S. Whitehead (1933), a graph appears which is here reproduced as figure 153. From this graph, it is apparent that the yellowtail catch per effort, of the small commercial boats fishing in the vicinity of San Diego and Los Coronados Islands, declined steadily during the period 1922 through 1932. Only during two years of this period was there an increase in catch per effort for yellowtail. These were the two warm water years of 1926 and 1931.

Although the primary effort of these boats was for yellowtail, and their barracuda catches probably were strongly influenced by the availability of yellowtail, the trend of the barracuda catch per effort is upward while the trend for yellowtail is downward. This is just the reverse of the situation during the period from 1947 through 1957 when the trend for yellowtail is upward and for barracuda, downward (Fig. 150).

Unfortunately, no data appeared in Whitehead's paper, nor was there a clear explanation of how the daily boat catch was calculated, however, the basic data from which his calculations were made can still be evaluated, since they have been kept in the files of the California State Fisheries Laboratory.

Isaacs: Could the reverse trends for the two species be the complementary effects of a preference by the fishermen for one over the other?

Radovich: It could during the earlier period, but I feel the chart of the recent period reflects the local abundance of both species.

Isaacs: One interesting thing is the reported occurrence of yellowtail in Monterey in the years of 1915, 1916, 1917, and 1918. Were they from yellowtail present in 1914 and 1915 which persisted until 1918?

Radovich: Yellowtail may have been present in those years, however, water temperatures were not high in 1917 or 1918. I do not know whether the water temperatures were warm or cold prior to 1917. Although barracuda tend to remain in Monterey Bay for a couple of years following a warm year, I do not know if this holds for yellowtail. When it gets cold, they may go south immediately.

Isaacs: Such incidental reports, going back in the past might enable us to reconstruct the occurrences of the kinds of anomalous years that we recognize in the more immediate past. This is a very valuable thing to have.

Hubbs: Do you find it difficult to explain why the barracuda were abundant in 1947? This is one year the barracuda were abundant when there was not much reason for it, except that perhaps they had not been fished down from former abundance.

Fleming: This is just following the war. Was not all fishing stopped during the war?

Radovich: It was slowed down. We do not have any party boat records during the war. The discrepancy in 1947 indicates that the picture is not perfect, but in general, I think the relationship of the local abundance of both species with water temperatures is pretty good.

The barracuda data for 1947 and 1948 are not as reliable as for the following years. There was a tendency for some of the party boat operators to exaggerate their catches of certain species, particularly barracuda, but this condition was improved considerably.

The collection of party boat records began in 1936 and stopped in 1941. Mr. Parke Young, who is in charge of the Department's sportfish project (responsible for the maintenance of party boat records), believes that the data collected between 1936 and 1940 are unreliable. They may not be comparable to data collected since 1947. Figure 154 shows the catch per angler day for yellowtail around Los Coronados Islands, and for barracuda off Southern California, and the January to June average surface temperature deviations from the long term mean at La Jolla, during the period 1936-1940. Even considering that the data are not reliable, I have no explanation, at the present time, for the poor correlation of barracuda abundance with temperature deviations during this period.

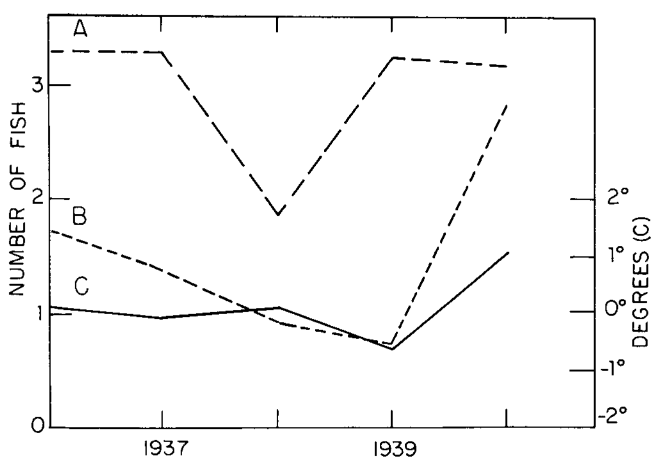

FIGURE 154. Party boat catch per angler day (1936-1940) of (A) barracuda off Southern California and (B) yellowtail off the Los Coronados Islands, and (C) the averages of the January to June deviations of sea surface temperatures at La Jolla from the 1917-55 mean.

Revelle: Would not this be affected by the number of anglers on a boat regardless of how many fish there are?

Radovich: There are variations here, of course, but in general, between years, the number of anglers per boat is fairly constant. Boats were filled to capacity then as well as now.

Isaacs: In which year does the yellowtail catch per angler reach its lowest point while the barracuda catch per angler goes up?

Radovich: That was in 1939, which was an unusual year. There was an extreme range of water temperatures during the year.

Isaacs: There was an extremely cold spring, which may have affected the yellowtail, and an extremely hot fall, which might have affected the barracuda.

Radovich: Looking at the average of the annual temperatures, one does not really get a very good picture. Since both species come into the fishery during the spring or early summer, the January to June average temperatures should be more important.

Murphy: In terms of cause and effect of these relationships, this picture might be more convincing if you had some data on some other species of fish that were repelled from here by warm water—something other than stragglers and something that was fished here during the years when it was cold—some fish that presumably belonged in the north and would go north to get away from the warm water.

Radovich: I have data on several other species. The white sea bass *(Cynoscion nobilis)* is another fish whose range extends into California waters from the south. It lives in the Gulf of California and it is also found along the outer coast of Baja California and off Southern California. In former years it was abundant off San Francisco, but in recent years, except for a small and erratic fishery in Tomales Bay and an occasional fish caught in the southern part of Monterey Bay, it is rare north of Point Conception.

In 1957 white sea bass began moving northward. It is not a species which party boat anglers primarily fish for, but it is a desirable fish and many are caught. During September of 1957 many white sea bass were caught off Morro Bay by sportfishermen, and in Monterey Bay and off San Francisco by sport and commercial salmon trollers, and gill net fishermen. Six white sea bass weighing an average of about forty pounds were caught five miles north of the Columbia River by salmon trollers and one twenty-eight pound fish by a dragger; several weighing up to forty-five pounds were caught off Gray's Harbor, Washington; and two white sea bass weighing between thirty and forty pounds were caught off Juneau, Alaska.

Let us consider a species with a different distribution, albacore *(Thunnus germo)*. The albacore range across the Pacific from the waters of Japan and Hawaii to the North American continent. They generally appear along this coast in June about sixty miles southwest of Guadalupe Island, and off northern Baja California. In general, the fishery begins in this area, and as the season progresses, it moves northward. When the fish are closer to shore, their movement northward is slower. When the fishery develops farther offshore, the northward movement is more rapid. The sport fishery depends on fish close to shore, and the offshore fast moving schools are less available to the angler. The commercial fishery is also influenced by the proximity of albacore to the coast and the speed of the northerly progression of the fish. About 65 percent of the albacore caught off this coast are caught from water between 60 and 64 degrees Fahrenheit.

In June of 1957, the California Department of Fish and Game research vessel, N. B. Scofield, made an exploratory fishing cruise in an attempt to intercept the incoming albacore schools well offshore. As the vessel sailed southward, unseasonably warm water was encountered south of Guadalupe Island and the cruise was shifted to the north. On June 9, the first albacore of the season were caught by the N. B. Scofield about 80 miles WNW of Guadalupe Island in 62°F. water The albacore fishery, subsequently began about forty miles off the coast, between San Quentin and San Diego. The season's first albacore landings were made during June and sport catches were good out of San Diego.

In July, the fishery ranged farther north than during the Julys of the past six years when they had been taken primarily south of Catalina Island. A few catches were made north of Monterey. On July 15, some were taken 140 miles off the Oregon Coast. Fish as large as fifty pounds were taken inshore off Baja California.

By the end of August, a major fishing area had developed between San Juan and Davidson Seamounts, and the catch was about one million pounds behind the 1956 catch for the same period. Up to this time, the fish appeared inshore farther north than usual. They moved closer to shore off northern Baja California, and then suddenly shifted offshore and northward. It looked as if the 1957-58 season's catch would be a poor one. However, the albacore again did the unexpected—they remained in the area between Point Conception and San Francisco through the entire month of September. They were extremely available to the fishery during September, and by the end of the month, the catch was about three million pounds ahead of the 1956 catch through September.

In October and November, fishing dropped off in the central California area as the fish moved northward. Some sport catches were made off Avila and Morro Bay.

During the first week of December, fair catches were made about 300 miles west of San Diego. By the end of the month, most fishermen had returned to port.

Murphy: May I comment on the albacore? They are a case in point.With respect to temperature, albacore are found in water of 62° and 65°F here. In Japan, it is known from tags that they have the same population of fish, the same fish, in fact. There, the bulk of the catch is taken between 68° and 72°F, quite a bit over the optimum temperatures here. In our POFI surveys north of Hawaii, also fishing on the same population, we got our best catches at 58°F by gill netting. To be sure the Japanese catches are by live bait and yours are by trolling. Therefore, with our present state of knowledge I do not see how temperature alone can be used as an index—it seems more complicated. There may or may not be some relation to extreme temperatures.

Isaacs: When they get to central California do they usually head north?

Murphy: That is not quite correct. Your tagging pattern suggests that normally they go north from Southern California to waters off San Francisco. I think they turn off at about San Francisco because the Oregon fish are a different group according to tags.

Isaacs: As Giff Ewing pointed out, the junctions between currents and boundaries between water masses are waters in which organisms tend to concentrate. Their location would be influenced by the currents. If these discontinuities are evident from isotherms, there would be a correspondence between temperature and albacore, though the absolute value of the temperatures might differ in different regions of the ocean.

Radovich: Albacore fishermen reported running into 70°F water from the Davidson Seamount to the Farallon Islands. This was the area in which the fishery picked up albacore inshore, and skipjack and dolphinfish, also.

Murphy: They do get pretty nice catches up to San Francisco, then they do not get much up beyond that.

Stewart: Not implying a cause and effect relationship, I could not help but be struck by the parallel in your albacore data and the water temperature anomaly as recorded, for example, at Los Angeles. You said, as I recall, the albacore stopped dead at the end of August and then stayed there through September and then started off in October, November, December, which parallels exactly the water temperature anomaly of Los Angeles (Fig. 91) or even the mean temperature anomaly for all stations along the coast (Fig. 100).

Sette: In other words, the points where the travel of the albacore was checked, correspond with a change from the warm temperature anomaly for August toward temperatures more nearly normal in September.

Radovich: Aside from the close relationship of barracuda and yellowtail to temperature, most of the material I am presenting represents anomalies of fish distribution that may or may not be correlated with temperature anomalies. They should be critically evaluated.

Another example of an anomalous distribution is bluefin tuna, which usually is caught only south of Pt. Conception. Nothing seemed unusual as the bluefin season began in June, but later they were caught off Monterey. Between July and October, bluefin tuna were caught from 80 to 100 miles north of Cape Flattery, off Eureka, and off Newport, Oregon.

In addition to bluefin, there were a number of tropical oceanic fish, such as skipjack, yellowfin tuna, and bonito that were caught far north of their usual range. On July 24, albacore fishermen began catching skipjack and dolphinfish off San Diego. In August many skipjack, dolphinfish, and yellowfin tuna were caught off Southern California. A fifteen pound yellowfin tuna was caught on August 18th off Pt. Buchon (above Point Conception). In September, swordfish were commonly observed around the Davidson Seamount and some were caught off Monterey. Skipjack were frequently encountered off Point Arguello. On September 25 a yellowfin tuna weighing 75 pounds was caught off Davidson Seamount. This probably is a northern record. The fishery for bonito, barracuda and yellowtail, lasted all winter, and is still continuing.

Among the small pelagic fishes, sardines, anchovies, and Pacific and jack mackerel, nothing dramatic happened until July. In July there were a number of large sardines taken by bait netters. About twenty tons of sardines were caught for bait off Monterey.

In August sardines were very abundant in the bait from Hueneme to the Mexican Border and young sardines began appearing in the bait catches. Anchovies were difficult to catch in Southern California and at Monterey there was a large number of very young anchovies. In September more young sardines that were spawned late in 1956 or early 1957 appeared and some of them showed up in Monterey Bay. Adult sardines were observed as far north as Eureka, California, for the first time in about six or seven years. The commercial sardine fishing season was hampered by a strike during the first part of the season. Only about 20,000 tons were landed during the season making it one of the poorest on record. In March of this year some six-inch sardines were caught at Monterey. At the present time, the 1957 year class of sardines is still in evidence off Monterey.

That concludes the chronology of the anomalous events related to Southern California species.

Off San Francisco the king salmon troll fishery had one of its poorest years. Another bad year for salmon was 1939. It stands out as the poorest in a forty-year period. The same year, 1939, was outstanding for sardines. In fact the 1939 year class of sardines is the largest one on record. I do not know what is significant about 1939. The average annual temperatures do not seem unusual, but a closer scrutiny at the different periods within the year might reveal anomalies significant to both salmon and sardines.

Isaacs: From the standpoint of what?

Radovich: The temperatures in 1939 were quite low early in the year, after that it was very warm. I do not know how outstanding this was.

Isaacs: It was very outstanding—the warmest fall on record.

Stewart: The lowest sea level was recorded in 1939, too.

Radovich: Salmon and sardines behaved abnormally, but very different from each other in 1939. The salmon troll fishery failed and sardines produced their largest year class.

Returning to 1957 and going farther north to British Columbia; pink salmon on their spawning migrations apparently came in from a more northerly route than usual in 1957. Actually, the 1957 pink salmon run in British Columbia was the best since 1930.

DISCUSSION

Isaacs: As far as the salmon are concerned, they are more abundant in the northern areas, whereas California is at the southern fringe of their distribution.

Radovich: This is right. We have mainly the king salmon in California.

Hubbs: This is explained by Brooks as a shift northward of the warmer temperature in the environment after the ice age.

Radovich: The spawning run in the Sacramento River system was very poor in 1957. This suggests that it was not only the availability that changed. Salmon find their way back to their own streams. Although a few sometimes stray into streams other than their own, there has never been evidence of mass straying. In some manner they are capable of detecting differences between the various streams and find the right one. It is possible that if ocean conditions changed significantly in the general area of the parent stream they might not recognize their parent stream.

Isaacs: Apropos of 1939 being inexplicable as to two cases: yellowtail and barracuda in one case, and salmon in the other, I think if you closely studied the water temperatures, you would find the lowest February temperatures in many years and the highest November temperatures. February temperatures were about 4°F below normal and those of November, about 4°F above normal.

Stewart: If you assume that sea level is a reflection of sea water temperature, San Francisco had the lowest sea level anomaly in February on record since 1897 and this would suggest an anomalously low temperature.

Radovich: There was a significant difference in temperatures, fish distribution and survival in 1939, but there are so many variables involved that I do not believe you can explain it as simply as this. For example, in 1939 there was a high survival of young sardines north of San Francisco to as far north as British Columbia. Conditions prevailing above San Francisco allowed the fish to go north; and the fact that spawning was successful in that northern area is important. However, in other years, when one considers both, the distribution and the survival of young fish in specific areas, the picture becomes a little too complex to be explained simply by temperatures.

Fleming: I think that we are jumping a long way when we try to relate distribution of very mobile fish such as you have been describing to us—yellowtail, barracuda, salmon, and so forth—with such a thing as surface temperatures. These fish can obviously move around rapidly. I feel in these cases that a more rational approach to this problem would be thorough consideration of the availability of food for some of these forms. I do not know the main food of barracuda and yellowtail for example.

Radovich: For barracuda, anchovies probably constitute one of the important constituents of their diet. This is based on the fact it is the primary bait for barracuda and that it is the most common species found in barracuda stomachs. They have large teeth and prey on small fish, usually small ones. Yellowtail eat a large variety of animals including sardines and anchovies.

Fleming: What I would like to see is some relationship between the view you have presented here with the talks earlier today, which were on planktonic forms and so on—forms that are more subject to the movement of the water and the physical chemical environment, and then see if there may be a better explanation of the distribution of some of these predators.

Radovich: There have been food studies conducted on a number of species. I do not have much faith in them, myself, for the simple reason that, in general, most fish seem to eat anything of the proper size that happens to be present.

Fleming: The albacore might have hung around, in the way you described because of the availability of food.

Radovich: That is a possibility. They might have run into an abundance of food in that area. However, I do not visualize the migrations of yellowtail and barracuda (or albacore) as a chase after specific food items. They might remain awhile in a spot if an abundance of food were present, but this is not the same as moving great distances because of a change in distribution of one or more food organisms.

Hubbs: We find white sea bass where food is abundant. Show me where the white sea bass spawn in a particular time and I can predict small boats will be out catching white sea bass within about a week. You (Fleming) say we cannot ascribe the movements of these larger free-swimming fish to temperature changes? I think this view is contradicted by a lot of the evidence. If you break down the data within a year, nearly every year back for a number of years there has been a winter of low temperatures. In the spring we get northern fish suddenly appearing. We get a high peak of temperature in July or August. This high peak lasts only two or three weeks, at that time or right after it, we get southern fish suddenly popping up. These do not seem to be enormously sensitive to the temperature in their optimum area but in the fringe area they respond to it very sharply.

Radovich: Frankly, I consider that during the series of years 1947-1957 there is a good correlation. Two species (barracuda and yellowtail) respond in the same way. I feel that the correlation between the temperature and distribution of these species is real.

Murphy: Other things change in this environment when the temperature changes.

Radovich: There may be changes in oxygen, salinity, or in a combination of several things. If species, such as barracuda and yellowtail, were looking for any specific foods, it would be difficult to explain this close a correlation with temperature. Of their food items I know none which vary the same way over this period.

Fleming: One of the hypotheses is that the survival of the year class of sardine depends on availability of the proper food of the larvae. You can start way back there.

Radovich: This has been expressed, and may be a possibility of course. But it is rather a loose hypothesis which we cannot test until we know a little more. The hypothesis refers to survival of young larvae, but adult fish, particularly predators such as barracuda and yellowtail are capable of moving great distances.

Fleming: Do you admit these fish are spread over a wide band of latitude? For the barracuda and the yellowtail, I think food response is a perfectly valid hypothesis in that in this range there is concentrated a great abundance of food.

Murphy: There is additional complication in respect to tuna. Their availability to certain fishing techniques varies according to what they are feeding on. Off Japan apparently where the albacore is apparently feeding on squid, it is available to the commercial fishery. As the boat catches change, in response, I suspect, to the food supply, you might expect a change in the availabilty.

Radovich: This does not appear to be true for barracuda. They seem to be eating mostly anchovies and sardines along the coast. If there is any food preference here, it should be for those particular species, but I haven't been able to find that the catch of barracuda corresponds to abundance of anchovies and sardines.

Schaefer: May I contribute a nickel's worth? I think the data we have on the tuna in the Eastern Pacific, (that is in the tropical region), indicate that the fish respond both to food concentrations and to temperatures. We find at the two edges of range, for instance off Baja California, that tuna concentrations vary approximately with the migration of the 20 degree isotherm, yet there is practically no change in the standing crop of food. Along Baja California there is food in concentrations the year around, but the tuna come seasonally. Similarly at the southern end, off Peru, when water warms up the tuna moves south toward Chimbode. In the warm water of the intervening zone between Cape San Lucas and Cape Blanca in Peru, where the year-round temperatures are generally favorable, the tuna tends to be concentrated where the food is most concentrated, provided water is warm enough. So there are seasonal migrations at both ends of the range but no pronounced seasonal movement in the middle.

Radovich: This is the kind of relationship I would expect, rather than a close relationship to a particular type of food. During unusual years when warm water extends far north, tuna may extend far north also. For instance, last year there was a considerable amount of skipjack caught off San Diego.

Murphy: We tend to build up ideas of what the various species of tuna consider to be good temperatures. But if we go to some other place in the world, we could find the "good temperatures" were quite different. If I remember correctly, the skipjack off Australia are caught in the low 60's, which is quite strange to us.

Schaefer: Off Australia they may be fishing at the edge of a concentration. This is quite analogous to the fishing off Southern California.

Murphy: As to the albacore, we have a certain set of temperatures where albacore are found along this coast—but they are never seen at these temperatures in the area south of the Aleutians or off Japan, except in rare instances.

Radovich: I do not mean to imply that temperature by itself is the only factor that motivates the albacore. During 1957, they certainly did make their appearance along this coast well north of their usual location. The temperatures where the fishery usually begins (Guadalupe Island) were very warm. During previous years, the catches were distributed as one might expect if they were related to temperatures. With a superficial examination, I did not see a direct positive relationship between warm water and the amount of fish that was caught. What was observed, and what I have presented, was an anomalous distribution and what seemed to be an anomalous behavior during the warm year, 1957.

The albacore appeared farther north and moved in closer to shore at the beginning of the season. Then instead of moving northward slowly, as they usually do when they are close to shore, they apparently moved offshore, rapidly, went northward and stopped. They stayed north of Pt. Conception for about a month and a half, and the catch was exceptionally good for that month and a half. They could have been stopping for food rather than anything else, or they might have run into a temperature barrier, or some other kind of barrier. They behaved essentially as I have described it—whatever the explanation.

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FISH SPAWNING IN 1957 AND 1958

ELBERT H. AHLSTROM

The title on the program suggests that I should report on the spawning of various species of fish in 1957 and 1958, but I will have to confine my remarks mainly to sardines, because the identification and counts of eggs and larvae of other species from our plankton surveys has not been completed beyond the July 1957 cruise. However, the sardine material has been studied through April 1958 and shows striking changes under the warm conditions during the 1958 spawning season.

Before discussing this, we need to have the background of sardine spawning during previous years as it is known from the eggs taken in plankton nets during our systematic surveys. Figures 155 and 156 are charts giving yearly distribution of sardine eggs from 1951 to 1958. In our surveys the fish eggs and larvae are obtained in oblique plankton hauls made from a depth of approximately 140 meters to the surface. The net employed is 1-meter in diameter at the mouth by approximately 5 meters in length. It is constructed of No. 30 xxx silk grit gauze (mesh openings 0.55 mm.). The amount of water strained during each haul is recorded by a current meter, fastened in the mouth of the net. Based on the amount of water passing the current meter the number of eggs per haul was standardized to represent the numbers per 10 square meters of sea surface. Stations in the spawning area were occupied at approximately monthly intervals during the spawning season. The distribution charts (Figs. 155, 156, 157), which I will show you, were drawn from the sums of the standardized egg numbers taken at each station during each season. Sardine spawning that occurs in the Gulf of California is not taken into account in the charts; it has been investigated, however, particularly in 1956 and 1957.

Outside of the Gulf, sardine spawning in recent years has occurred mainly between Point Conception, California, and Point San Juanico, Baja California. For convenience I have divided the sardine spawning area into four subareas which are numbered in figure 157. Area two is Southern California, from Point Conception to San Diego. Area three is northern Baja California, extending down to about San Quentin Bay. Central Baja California has been divided into two areas: upper central Baja California, extending between Punta Baja and Point San Eugenio, and lower central Baja California, between Turtle Bay and San Juanico. Areas two and three together comprise the "northern spawning center," and the two central Baja California areas (areas four and five) the "southern spawning center." In comparing the series of years 1951 to 1958, shown in figures 155, 156, I would note that the most spectacular change that occurred during the last eight years did not take place in 1957 or 1958, but between 1953 and 1954. You will note from figure 155, that in 1952 and 1953 the center of spawning was in the southern spawning area off central Baja California. In fact, in 1952 only three percent of the sardine eggs were taken in the northern spawning center, and in 1953 only one percent. The percentage in the northern center in 1951 was six percent. The decline was progressive for these three years.

It is difficult to imagine a greater contrast between two seasons than occurred between 1953 and 1954. Sardine eggs were obtained in only 20 hauls in 1953 in the northern center; they were found in 149 hauls in 1954. In only two instances were sardine eggs taken at adjacent stations in 1953. In 1954 they spread continuously over most of the northern spawning area. We estimate that in 1953, 4 x 10^{12} eggs were spawned in the northern center, while 137 x 10^{12} eggs were spawned in 1954. Literally interpreting these figures, there was 35 times as much spawning in the northern center in 1954 as in 1953.

Furthermore, this difference in egg distribution was directly reflected in adult fish distribution as shown by the California commercial catch. In the 1952-53 and the 1953-54 seasons (the sardine fishing season, by law, began in November and continued through February 1st in these years), less than 5,000 tons of sardines were landed. When spawning became widespread in the northern spawning center in 1954, the catch immediately jumped to 67,000 tons. In years following 1954, spawning continued to be widespread in the northern center and the commercial catch also has continued fairly good.

As background for describing what happened in 1957, I should like to discuss the seasonal distribution of sardine spawning of the previous six years, which is illustrated in figure 157. In the northern spawning center (areas two and three in figure 157), eggs were taken mostly between February and July. Eighty percent occurred in May and June off Southern California and in April to June off northern Baja California. You will note that in the southern spawning center (areas four and five) the seasonal distribution is definitely bimodal. It builds up to a peak in March, then falls off to a minimum in June, and then there is again quite heavy spawning in July and August. This late-season spawning has no counterpart in the northern spawning center.

Also, the late-season spawning occurs at quite different temperatures than the early spawning. The earlier spawning during recent years has occurred at temperatures of about 15.5°C on the average, while the late-season spawning has been at an average temperature of somewhat over 18°C, or about 2.5° difference in temperature. These are the temperatures measured at the 10-meter level. Sardine eggs are confined to the upper mixed layer and usually are most abundant in the upper 20 meters of this layer. The 10-meter temperature is usually a good indicator of the temperature of the whole upper mixed layer.

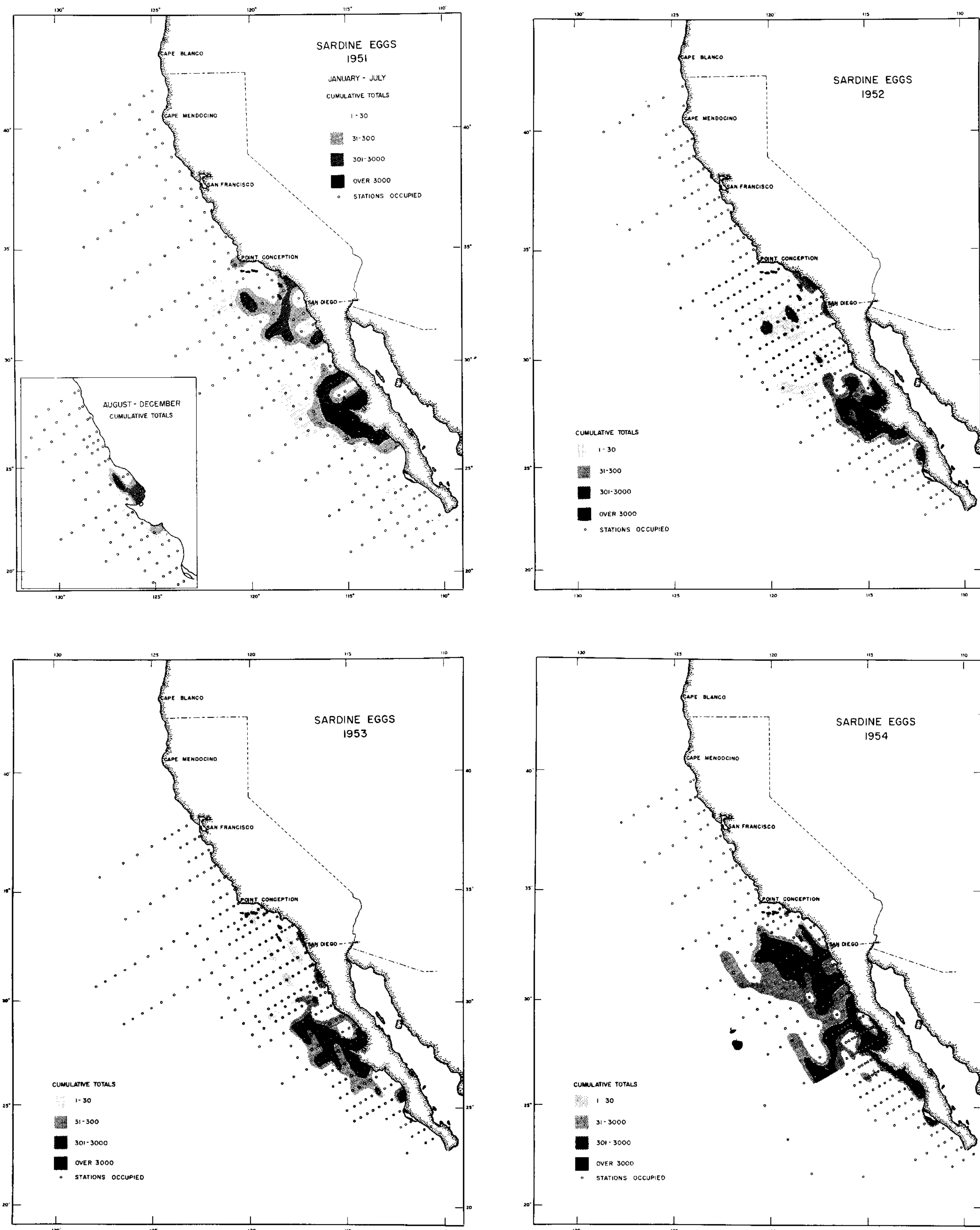

FIGURE 155. Distribution and abundance of sardine eggs.

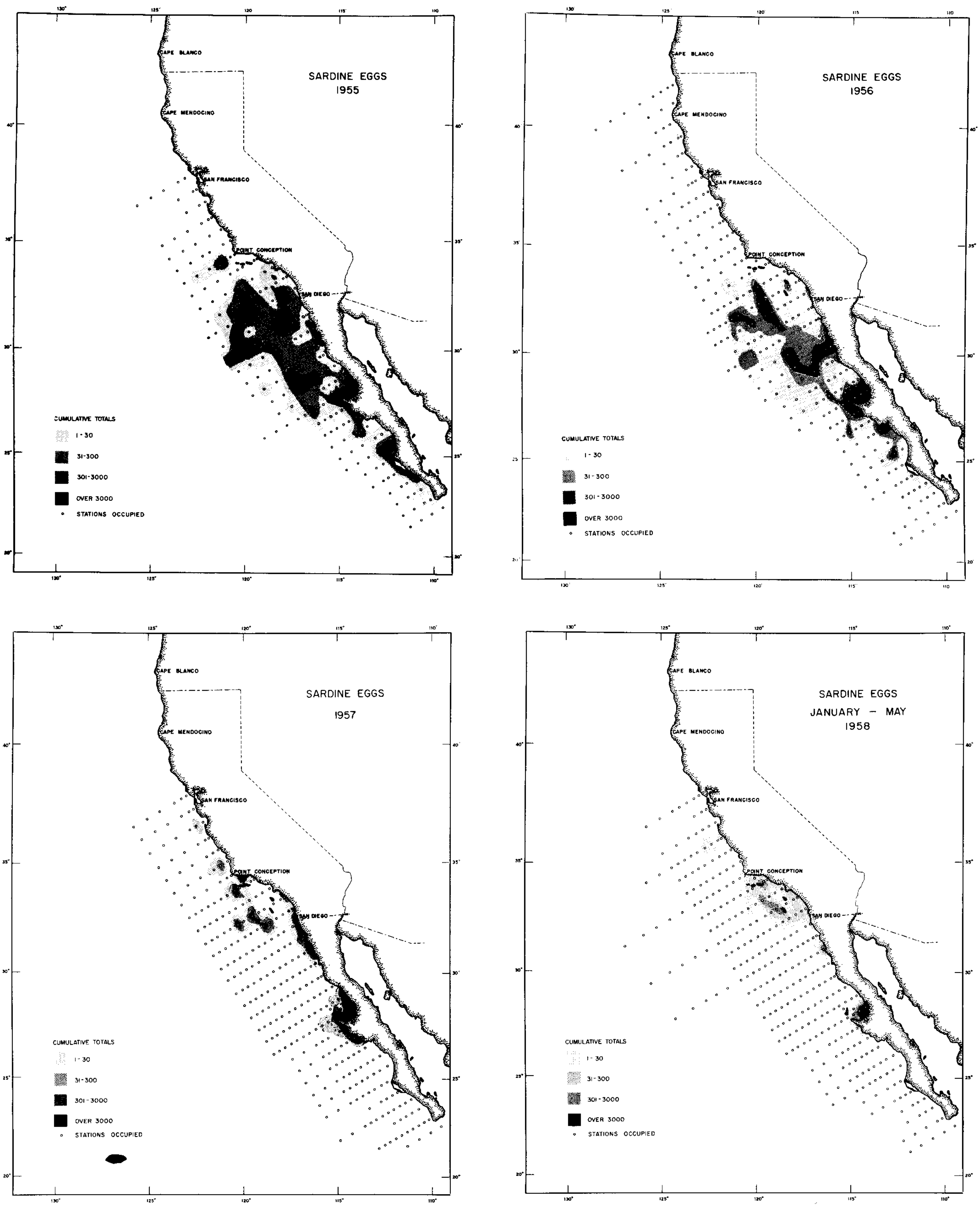

FIGURE 156. Distribution and abundance of sardine eggs.

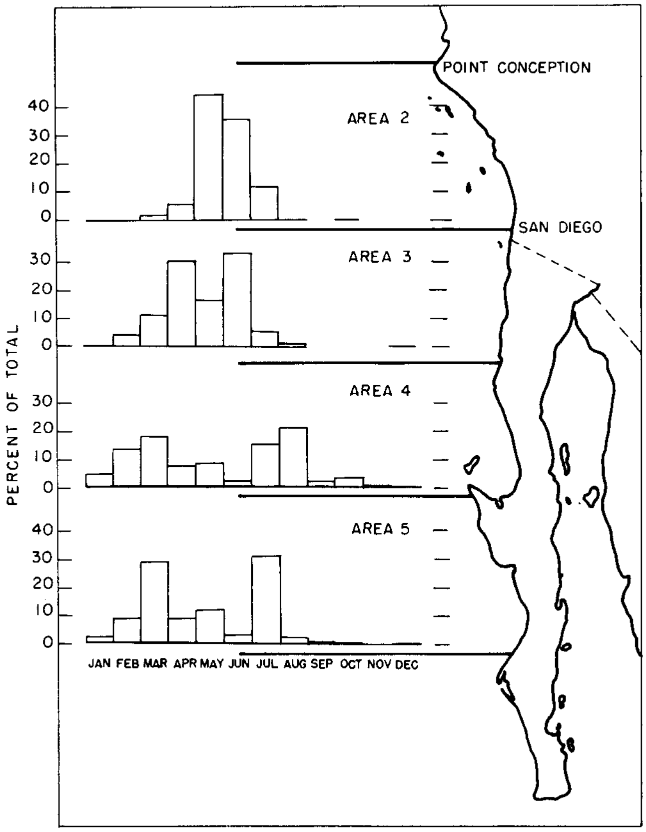

FIGURE 157. Seasonal distribution of sardine spawning, as indicated by the monthly mean number of eggs for the six-year period 1951 to 1956, by major areas.

It might be pertinent to give some information on the temperature at which sardine spawning has occurred in the different areas. In the Southern California area during the six-year period 1951 through 1956, the average temperature at all stations yielding sardine eggs was 14.7°C. There was then a gradual progression in temperature going southward. In the northern Baja California area the average was 15.1°, or 0.4° higher; in the upper central Baja California area it was 15.4°, and in the lower central Baja California area it was 15.7°.

In 1957, spawning in the Southern California area which occurred from March to July, was at an average temperature of 14.9°. This is quite close to the average of the preceding six years. Off northern Baja California, the 1957 temperature was 15.4°, as compared with the average of 15.1° during the preceding six-year period. In the upper central Baja California area the temperature was 16.4°. This was a degree higher than the average of the preceding six years. In the southernmost area it was 16.2° at the only two stations where eggs were taken.

These temperatures I have given were the averages for January through June, that is, the earlier months of the season. The fall spawning temperature had a still more marked difference. For fall, the six-year average was 18.1° (areas four and five combined). Last year it was 21.2°. During the latter part of 1957 we know that temperatures increased all along the coast. The increase occurred after July, mostly. Spawning in the northern center was over by July, so it was not much influenced by the change in temperature.

By 1957 spawning was light compared to some of the preceding years. The most striking thing is that there were practically no spring spawners off central Baja California in 1957. In this area the spring spawning was only about 2 percent as large as the fall spawning in 1957. In preceding years, the spring spawning had been (with one exception) considerably larger than the fall spawning. We are pretty much convinced that the early spawners of central Baja California are the same group of fish that spawn off Southern California and northern Baja California. The fall spawning group may be a distinct subpopulation or race. I think the evidence of 1957, when we had practically no spawning in the southern center during the first six months and heavy spawning during the last six, is good support for considering this group to be a separate subpopulation. As I said earlier, the fall spawning was at a higher temperature than usual, the average being over 21°.

The distribution during the first four months in 1958 was very unusual. As we have seen in the average seasonal distribution chart of figure 157, practically no eggs are taken off Southern California in January or February and a small amount in March. April has only about five percent of the seasonal total. In 1958, however, fairly heavy spawning started in February and spawning in March had the distribution shown in the chart. Spawning centered in the area of the Santa Barbara Channel Islands, with a strip along the coast of Southern California and another zone offshore from Santa Catalina and San Clemente Islands. The only other center of heavy spawning was in Sebastian Viscaino Bay. Spawning did not occur offshore in the southern center, which is the usual distribution in this center during the spring months. Also, there was hardly any spawning between Sebastian Viscaino Bay and Southern California.

Spawning temperatures off Southern California in 1958 during January through May have averaged 15.1°C. This is only about 0.4° higher than the six-year average. The 1958 season has paralleled the 1941 season off Southern California in time of spawning, but not in temperature of spawning. As you will recall from a number of discussions, 1941 was an unusually warm year; the average temperature of spawning off Southern California was 16.1°C. This is not only a full degree higher than the spawning temperatures off Southern California during January-May 1958, but a higher value than that found for any of the four areas during the regular spawning period in 1951-1956. This points up the fact that one area like Southern California, over a period of time, will have a wider average temperature range than is ordinarily found throughout the spawning range of the sardine.

DISCUSSION

Berner: Is the implication here that early spawning sardines off Southern California might be the first hump in the seasonal graphs of the southern center and these fish have migrated north?

Ahlstrom: Yes.

Fleming: May I ask another: Do they spawn only once a year?

Ahlstrom: This is the kind of information that we would like to have but which is very difficult to get. When you examine the frequency distribution of diameters of ova in the gonads of maturing female sardines, you find that there are several modes. From this kind of evidence it is usually thought that three batches of eggs are destined to be spawned during the year. Whether all three actually are spawned or not is the controversial point.

Wooster: You may be going to get to this, but as I understand it, one of the uses you make of these data is to estimate the size of the spawning stock. To do this you eventually integrate these charts and multiply them by some factor which gives you the number of adult sardines. If this is true, then there was an extremely great decrease in the size of the population of sardines from 1956 to 1957, if I am integrating properly. Is this the conclusion you have drawn?

Ahlstrom: Yes. In estimating the number of spawners we have assumed that a female spawns on the average about 100,000 eggs. Allowing this figure and a 50-50 sex ratio, we estimate that the spawning population in 1956 numbered approximately five billion fish, while in 1957 it was reduced to about two billion fish.

Isaacs: What is the population drop over a period of seven years?

Ahlstrom: From twelve to two billion fish.

Wooster: To continue, in these first six charts your average population in each year is more or less the same size population.

Ahlstrom: No. The distribution charts are simple sums of eggs taken at each station through the season. For estimating the total number of eggs spawned the numbers of eggs are integrated over time and space. The total spawning population is estimated from these integrated egg numbers. There was a very big drop in spawning population between 1951 and 1952. The numbers, including both the northern and southern populations, dropped from twelve billion down to below three billion. The 1953 and 1954 values are around eight billion.

We think these variations in adult population are caused by variations in year-class strength. From the age-composition and quantity of the California sardine catch we have a very good estimate of the relative year-class strength. There was a series of three big year classes in 1946, 1947, and 1948. In 1949 and 1950 the survival to commercial size was one one-hundredths as good as in 1947. For example, the 1947 year class, produced from a relatively small population, amounted to over 3000 million fish as measured by the subsequent catch. In 1952 we were feeling the influence of the weak 1949 year-class as three-year-olds, and the weak 1950 year-class as two-year-olds. Since 1948 the only moderately good year class we have had in the fishery was the 1952 year-class. The 1951 class was somewhat better than 1949 and 1950, but not as good the 1952. In the spawning population of 1954, especially, we have this influence of the 1952 year-class, and there has been a gradual decrease in population since then, because succeeding year classes have not been very good. But as John Radovich probably mentioned yesterday, present indications are that from this small spawning population in 1957, there has been fairly good survival.

Radovich: Right now the picture is extremely confused because the samples we are getting in the 1958 bait catches contain fish about the size they should be if they were of the 1957 year-class, except they seem to have an extra ring in their scales.

Isaacs: What happened to all these fish? Two-thirds of them disappeared between 1956 and 1957. Is this explained on the year-class strength chart?

Radovich: The 1952 year-class actually supplied more than 50 percent of the catch for a period of about three years and tapered off without another year-class coming in.

Murphy: What is the typical number of year-classes in a spawning population?

Radovich: I would say, predominantly three. Only about half of the fish are spawning at the age of two.

Question: Why do you take the 10-meter temperature when it comes to spawning?

Ahlstrom: We have done a number of studies on vertical distribution of sardine eggs, and have found that the distribution varies in different places and at different times. There are several things that are constant in the vertical distribution, however. The eggs are always in the upper mixed layer above the thermocline, and where the thermocline is shallow, the vertical distribution is shallow. When there is a deep mixed layer as is true at some offshore stations, the eggs may be distributed quite deeply. But in most cases where this happens, the temperature is rather uniform throughout the mixed layer so that the 10-meter temperature is a very good representative temperature of that layer. In a situation where you have a gradual change in temperature from the surface downward, there is no single temperature that would be fully representative.

Arthur: Would you not expect to find the eggs at their density layer? I am thinking of the waters with more shallow thermocline along the coast.

Ahlstrom: In one set of the vertical distribution data we had an interesting difference in spawning. In a sample there are usually the eggs of the three previous days of spawning, and these can be distinguished by the stage of the embryo. At this station, one-day-old eggs and the three-day-old eggs had their center of distribution at about the 40-meters depth. The two-day-old eggs had their highest concentration near the surface and declined quite rapidly downward. The same distribution was found in both day and night series. So to me, at least, this suggests that sometimes the sardines may be spawning as deep as 40 meters, and at other times spawning near the surface in the same water mass. The temperature was uniform throughout the egg-bearing stratum so that temperature did not affect the distribution.

Fleming: Can you tell us something about the frequency of the cruises here, and whether you have any time series at stations?

Ahlstrom: The cruises are roughly a month apart. The coverage has been the most intensive during the first seven months of the year, and somewhat less intensive during the later five months, so that the fall spawning has been undersampled.

Fleming: Have you ever stayed in an area for a month to see what happens?

Ahlstrom: We have stayed in an area on several occasions for a period of three to five days.

Fleming: Does this spawning seem to go on fairly regularly day after day? What I am wondering is how valid is the sampling? Could you miss a whole spawning population when you only sample once a month?

Ahlstrom: Although each station is sampled only once a month, the samples contain two or three days' eggs so that time coverage amounts to about one-tenth of the period between cruises.

Fleming: I shudder to think how little you are actually comparing.

Ahlstrom: I will point out one thing. When there is spawning of any intensity in an area, there are usually several days' eggs present. Take 1941 that you mentioned earlier. The first cruise of that year covered 41 stations off Southern California. We found eggs in every haul taken in this area, and in every haul there were all of the previous days' eggs that could have been expected from continuous daily spawning. This suggests that the spawning was continuous rather than intermittent during the month.

Johnson: Wouldn't the presence of larvae indicate whether or not you had missed spawning?

Ahlstrom: Yes, this is a nice thing about larvae; they are more conservative in time than the eggs. A sample of larvae represents the accumulation over approximately a month and a half.

Fleming: So your larvae do reassure you.

Klein: Does the spawning in Viscaino Bay follow the general pattern, or does it remain fairly constant?

Ahlstrom: Fall spawning was very heavy in 1953. In most seasons it was less than 20 to 25 percent of the total spawning.

Hubbs: Do you get any periodicity in frequency of spawning?

Ahlstrom: We have looked for this a number of times but we have not been able to see anything in our data suggesting this.

Radovich: Since sardines in Sebastian Viscaino Bay spawn in warmer waters and at a different time-space interval than the rest of the sardine population, they may be a genetically different population. Do you have any indication of fall spawning off Southern California during the time the Sebastian Viscaino Bay fish are spawning?

Ahlstrom: The 1957 material examined thus far does not show this. When we have completely worked up the 1957 collections we might find this.

Radovich: I am trying to identify these small fish that seem to be older than they should be. They may represent that southern group, which are smaller for their age than the others.

Question: What species, other than sardines, do you get in your hauls?

Ahlstrom: Eleven or twelve species dominate the material making up about 80 percent of all the larvae we take. In recent years the most abundant has been the anchovy. Also of considerable importance are jack mackerel and hake. The latter is a commercial species in some parts of the world but is not caught commercially in our area. Next to anchovy, hake larvae have always been the most abundant. In last year's collections a notable thing was the large number of the larger sizes of jack mackerel larvae. It seems that the apparent survival of the jack mackerel year-class was surprisingly good. This will have to be determined later by the fishery.

Radovich: Numerous young jack mackerel were caught in Morro Bay in 1957.

Ahlstrom: This probably is an indication that it is a successful year-class. Certainly the larval collections suggest that it should be the best year-class of jack mackerel since we began our surveys.

I might mention one other thing about the apparent relation between success in sardine spawning and temperature. It has been known for a number of years that there often is good survival in years that are warmer than usual. Mr. Sette made a preliminary study of this some years ago, I believe. It showed very well the success of the 1926 class and 1931 class during these two warm years. 1941, another warm year, was somewhat of an exception to this; it was not a bad year-class but it was not spectacular. Again, the 1957 class is a good one in a warmer than usual year. The 1939 year-class was an outstanding class that was produced during a partly very cold, and partly very warm year. The 1932 class was also very successful, although the temperature in 1932 was somewhat below normal. It is interesting to note that the years 1932, 1939 and 1957 have one thing in common. During the last half of each of these years the winds died down. This, at least, was a condition that would favor a shoreward transport of larvae and also it might have been conducive to the heating up of the water.

Murphy: Do you ever have any very striking indications of mortality among the eggs and larvae?

Ahlstrom: We almost always find abnormal looking eggs in our hauls, but as far as these might relate to unusual mortality we have found nothing noteworthy.

Hubbs: Didn't Carl Oppenheimer find tremendous numbers of diseased eggs?

Ahlstrom: There is a problem in collecting viable eggs. Among the early stage eggs there is a large proportion of abnormal appearing eggs in our plankton hauls. This does not apply to sardines alone, but to eggs in general. In fact, this condition makes identification of fish eggs difficult because they become milky and so opaque that the embryonic features cannot be seen. Some of the later stages of eggs are in the same condition, but it is not nearly as prevalent after the embryo has encircled the yolk at the anal pore closure. I suggest that this condition may be due to rupture of the embryonic membrane and that it may be caused by mechanical injury during the collection of the eggs.

Sette: I would like to remark that this condition is quite general in fish eggs of various species in both the Atlantic and Pacific.

Hubbs: Carl Oppenheimer showed me some preparations of eggs he had recently collected locally. The milky area was swimming with bacteria. Although a little time elapsed while getting them under the microscope, there seemed to be much too much bacterial increase in a short time to be due to infection after collecting.

Ahlstrom: We have had difficulty with Carl's results. We have collected a number of fish eggs recently, sardines and other species, and have incubated them at constant temperatures. We have had good survival. We take out normal looking eggs and it is very unusual to have these die. Carl experienced a very high mortality, but ours is much better.

Hubbs: Have you picked out some milky looking eggs? This would be very interesting.

Sette: Are not the milky eggs dead when first seen?

Ahlstrom: All the live ones are translucent—transparent. You can hardly see them unless you get the light reflecting the right way.

NOTE: The editors offer their opinion that Hubbs and Ahlstrom were discussing two different phenomena. The former related Oppenheimer's findings on the nature of the "milky" eggs, i.e., whether or not they were diseased; the latter was relating his experience as to the viability of eggs other than the milky ones, i.e., normal healthy eggs.

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

OSCAR E. SETTE

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

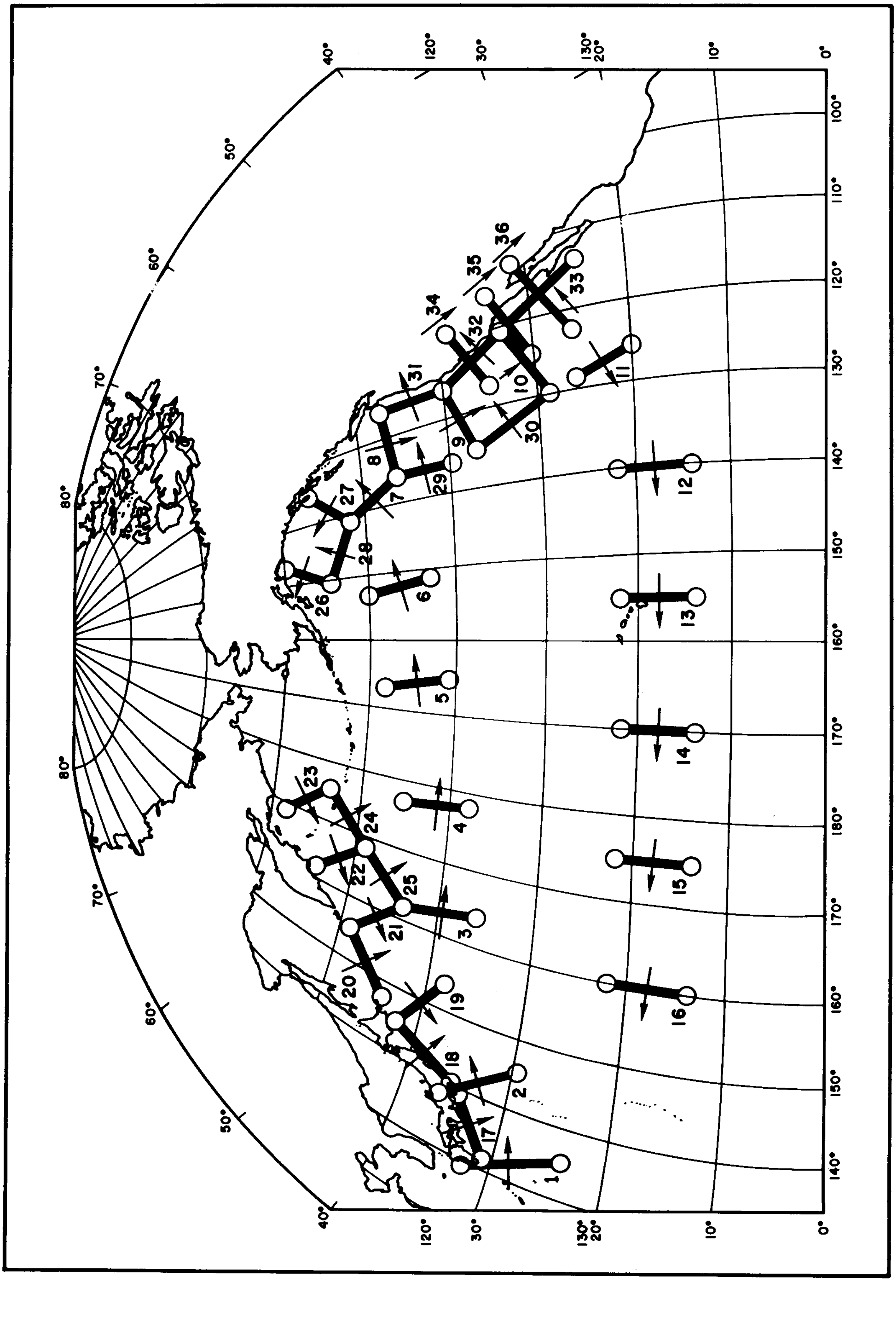

FIGURE 158. Location chart showing points, marked by open circles, between which pressure differences were read in deriving wind indices. Arrows indicate the positive direction of the geostrophic wind component associated with each point pair.

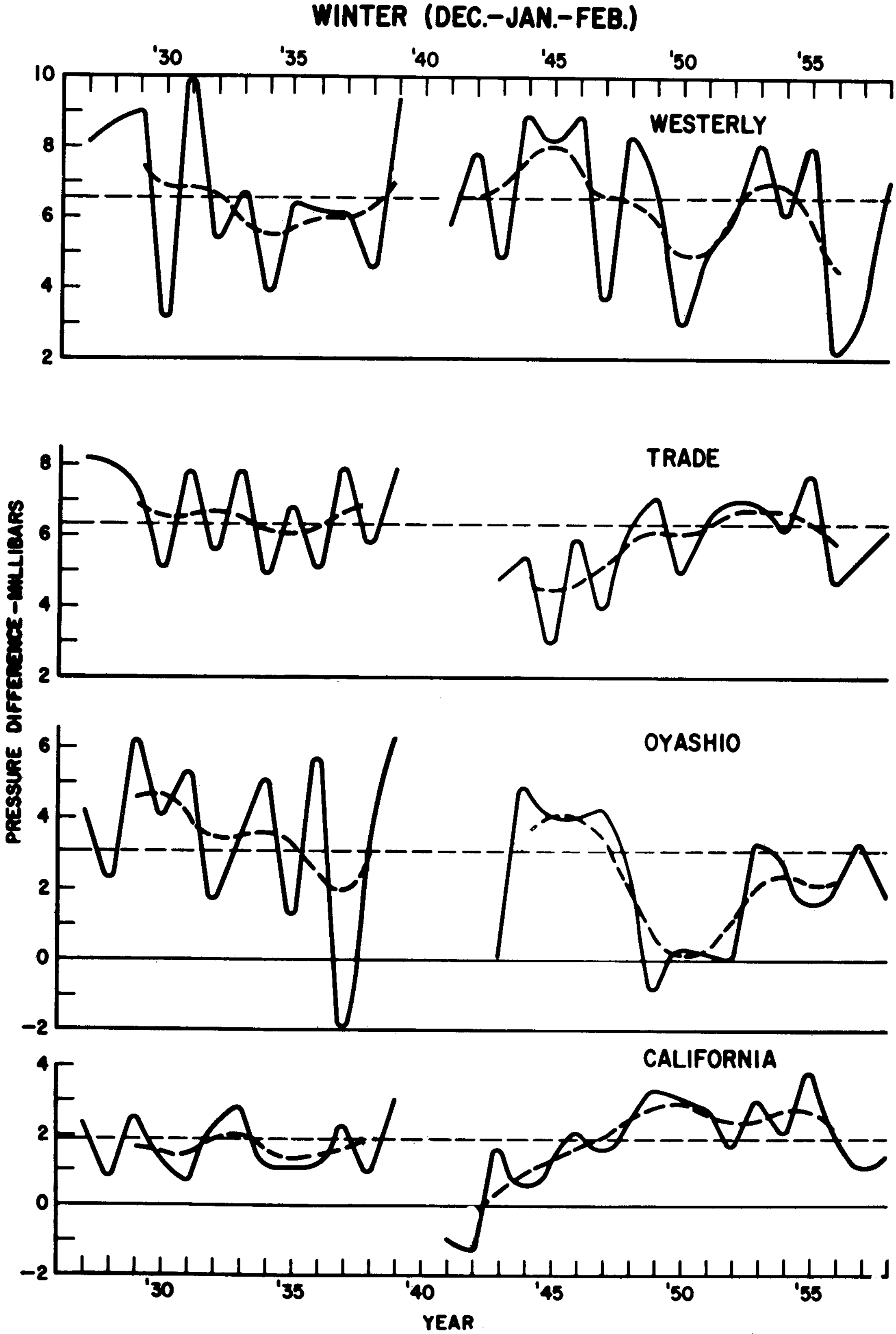

FIGURE 159. Wind index values for westerly (locations 3 to 7), Trade (locations 11 to 16), Oyashio (locations 21 to 23) and California(locations 34 to 36) wind fields for the three winter months: December, January, and February, 1926 to 1958. Dashed line is an unweighted running average of three.

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.

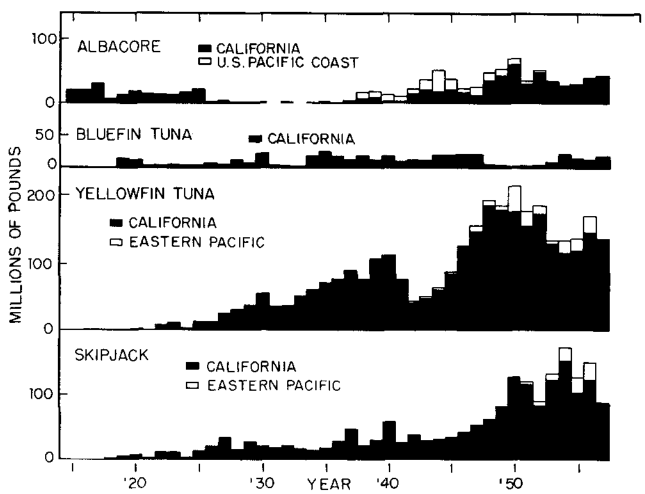

FIGURE 160. Total commercial catch, in millions of pounds, of the four principal coastal pelagic fish species of the Pacific Coast, 1916 to 1957.

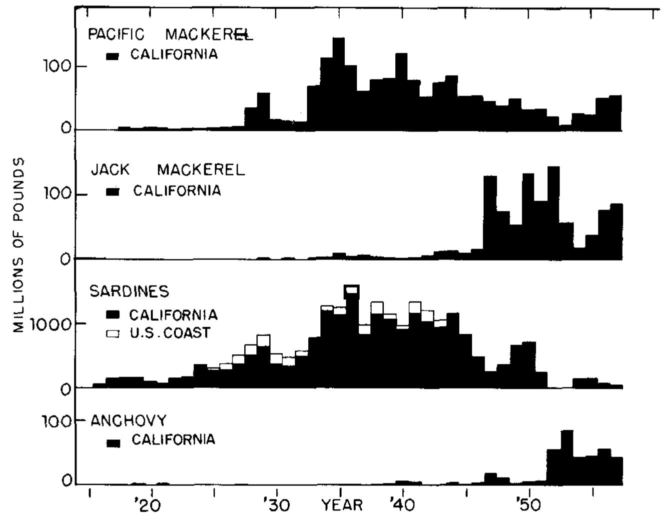

FIGURE 161. Total United States commercial catch, in millions of pounds, of the four principal coastal pelagic fish species of the Pacific Coast, 1916 to 1957.

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-

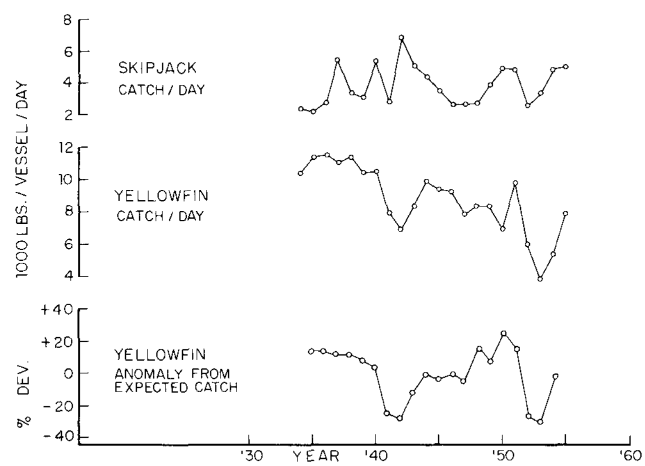

FIGURE 162. Annual mean catch per standard vessel per day of skipjack and yellowfin tuna by the California live-bait fishery and the anomaly from "expected catch" of yellowfin tuna, 1934 to 1955.

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

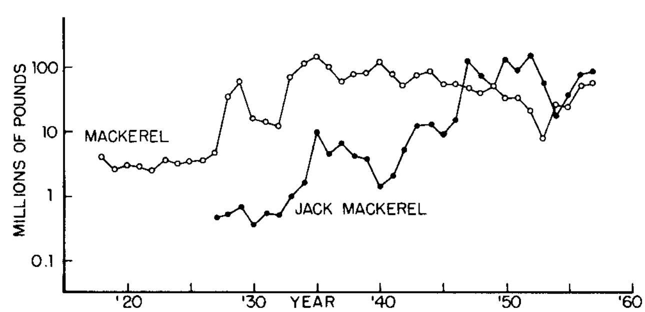

FIGURE 163. Logarithmic plot of the Pacific mackerel and the jack mackerel catches shown in figure 161.

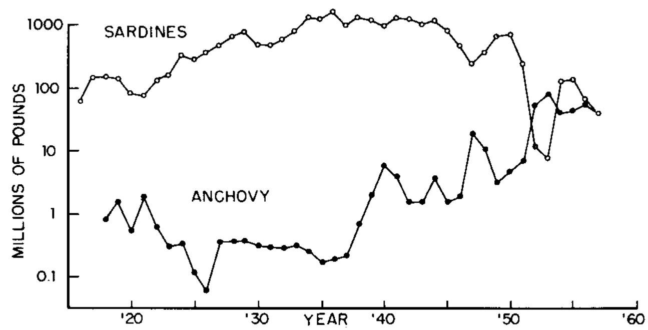

FIGURE 164. Logarithmic plot of the sardine and anchovy catches shown in figure 161.

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.

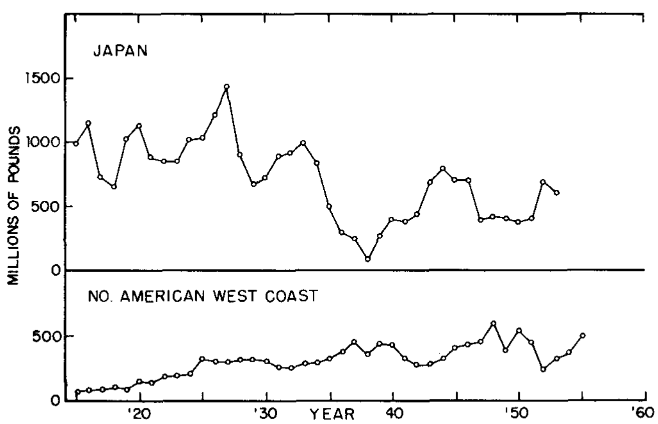

FIGURE 165. Total annual commercial catch, in millions of pounds, of Pacific herring by Japan, 1915 to 1953, and by Canada and the United States, 1915 to 1955.

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 significance to this, but I think it invites an effort to get some more refined data that would indicate whether those two series reflect econômical 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 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 offshore 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.

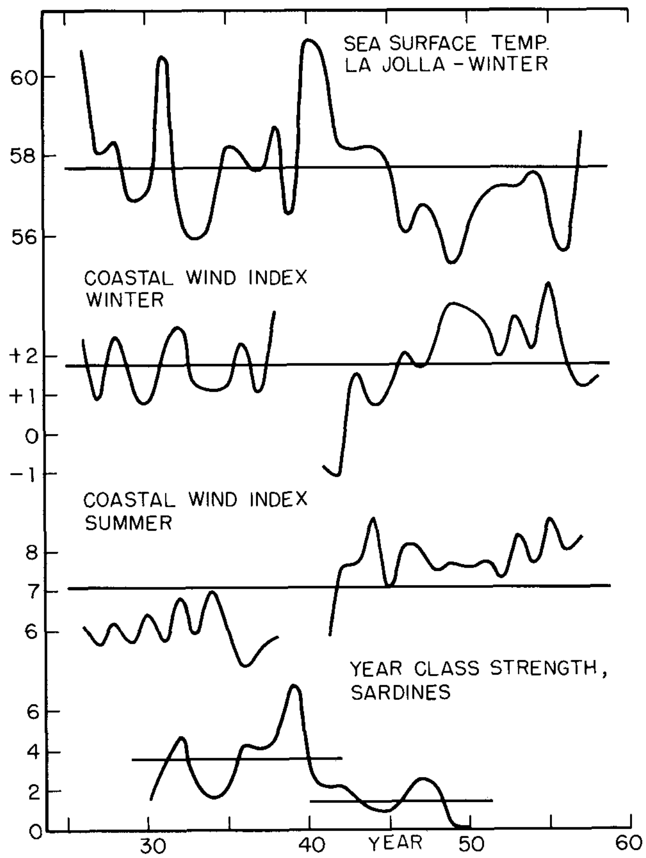

FIGURE 166. Mean winter (December, January and February) surface sea water temperature (° F.) at La Jolla, 1925 to 1957, mean winter (December, January and February) and mean summer (June, July and August) wind indices (millibars) for the relative year-class strength of sardines as indicated by the total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

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 countercurrent, tends to be better developed with weak spring or summer northwesterlies than with strong northwesterlies. 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 summertime 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 survival. 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-

* 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?

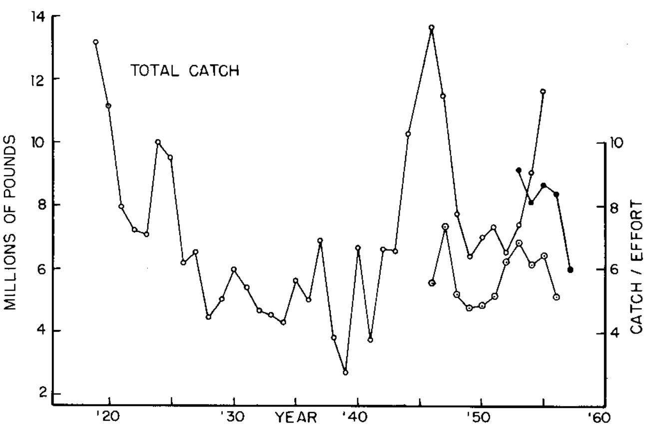

FIGURE 167. Total commercial catch in California of chinook salmon, in millions of pounds, 1919 to 1955 (open circles); mean catch of chinook salmon per boat per season, in thousands of pounds, by California trollers, 1946 to 1955 (dotted circles); and mean catch of chinook salmon per boat per day, in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

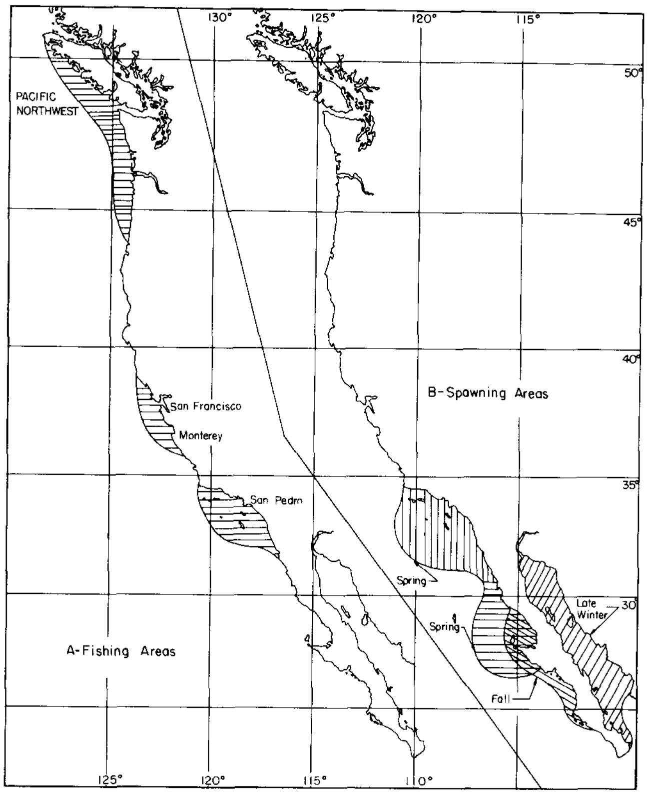

FIGURE 168. Schematic representation of: A) fishing area; B) spawning areas of the Pacific sardine.

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 information 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 countercurrent (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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GENERAL DISCUSSION

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

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

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

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

Of what kind of observations does this evidence consist? I have listed the following kinds of evidence: (1) temperature and salinity of the water; (2) various kinds of direct measurements of the water motion: drift bottles, drogues, and G.E.K., (3) evidence from tide gauges of changes of sea level, (4) properties of the water other than temperature and salinity, such as the oxygen content. Possibly in this category of direct measurement there falls also some biological information, and we can add a fifth sort of evidence, (5) the distribution of the phytoplankton and the non-swimming zooplankton. And perhaps in this discussion of what happened in the ocean, we ought to talk about (6) the area and time of fish spawning—how it was affected by temperature; (7) survival of fish larvae as they were perhaps affected by the greater on-shore movement of the water; (8) the distribution of the adult fishes, both sardines and related species, and the larger fishes. In addition we might discuss: has there been any effect of the environmental conditions upon the availability of the fish apart from their actual distribution? Any effect on schooling behavior? On the depth at which they are normally present or other aspects of their behavior that make them easier or harder to catch? What has been the effect of the environment conditions on distribution and growth of phytoplankton and the distribution and numbers of zooplankton?

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

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

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

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

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

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

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

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

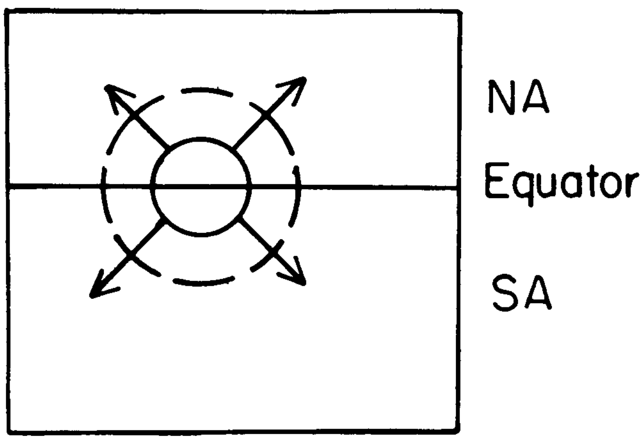

FIGURE 169. Straw man I (Revelle)

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

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

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

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

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

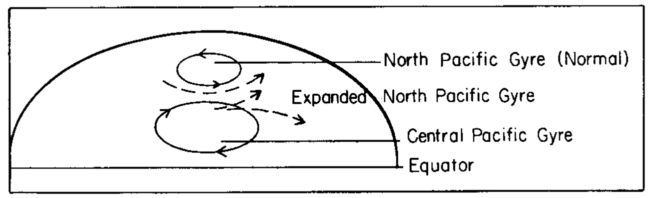

FIGURE 170. Straw man II (Isaacs)

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

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

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

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

A summary of events implied by this model is: first, warmer water moves into the Gulf of Alaska intensifying the Aleutian Low, and off California, and colder water appears off Japan immediately. Sometime later colder water moves into the Central Pacific areas, and later cold water moves into the Northern Gyre. This may react on the meteorology to terminate the entire sequence.

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

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

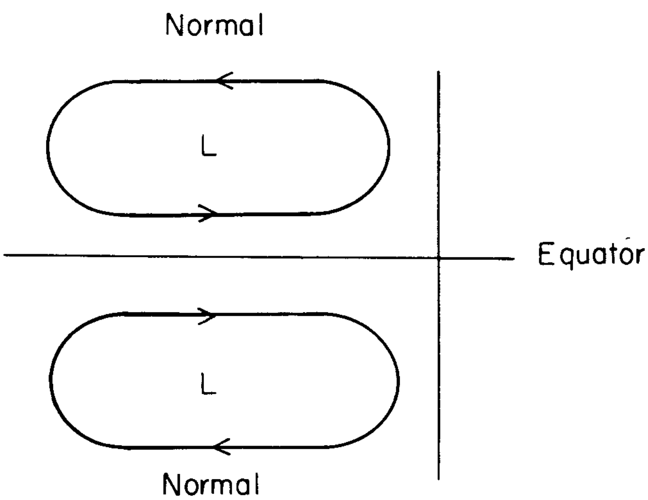

FIGURE 171. Straw man III (Munk).

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

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

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

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

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

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

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

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

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

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

Stommel: Unfortunately, we labor under some difficulties in trying to relate the behavior of any of our models—or straw men—to what presumably may actually be happening in the ocean. One difficulty is that there are no theories that can deal properly with Straw men I and II. The other is that it is difficult to think of any way of obtaining the kind of observational material that can properly be compared to the theoretical deductions of Munk's Straw man III. We do not have a theoretical model that enables us to describe the mean vertical thermal structure of the ocean, and that permits us to decide what perturbations of the mean state would be like. On the other hand, the evidence that we have at hand is not the kind that permits computation of vertically integrated geostrophic transport over the vast central regions of the Pacific, yet this is what we would like to compare to any predictions of the Straw man III.

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

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

Revelle: This is what Straw man I says. If the deepening occurs only in a narrow region near the coast, say in only 1/20th of the rest of the ocean, the related shallowing of the layer depth, in the central region of the ocean might be small. The change in heat content off California is about 40,000 calories per cm^2. This may be an extreme area so we might consider 20,000 calories as average. It would take about 1,000 calories in the central regions to make up the 20,000 gained on the periphery or a shallowing of the mixed layer in the central ocean of only 5 meters. This might be too small to be observable and would have a very small effect on the dynamic topography in the center.

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

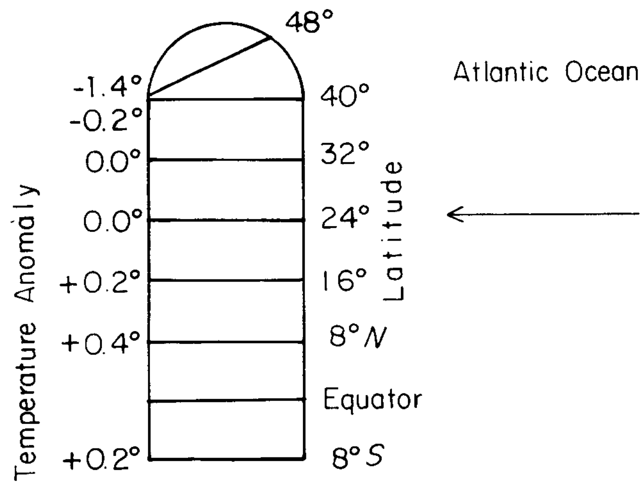

FIGURE 172. Temperature anomalies at 200 meters, Atlantic Ocean.

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

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

ANOMALY TABLE

TEMPERATURES ARE IN CENTIGRADE

	Winter 1956-1957	Spring 1957	Summer 1957	Fall 1957	Winter 1957-1958	Spring 1958
Longitude 140°-165° W Latitude 20°-50° N	(3° to 4°)[6] Warm	2° Warm	0[2]	0[1]	Warm	Cold
Hawaii[3] (Sal) (Temp)	 0.7° Cold	+ Sal 0.1° Cold	+ Sal 2° Warm	++ Sal 1.1° Warm	++ Sal 2° Warm	++ Sal 1.8° Warm
Gulf of Alaska (Salinity)	----	----	+ Sal	+ Sal	++ Sal	++ Sal
Entire Coast[5] (West Coast)	—.14 ft.	+0.06 ft.	+0.018 ft.	+0.11 ft.	+0.41 ft.	+0.22 ft.
CALIFORNIA N. of Pt. Conception S. of Pt. Conception	 0 0	 0 1° Warm	 1° Warm 1° to 2° Warm	 1° Warm 1° Warm	 1° to 2° Warm 1° to 2° Warm	 2° Warm 2° Warm
Peru[4]	----	----	----	----	----	4° Warm
Japan North South	Cold 0	Cold Cold	Cold Warm	Cold Warm	0 Warm	Warm Warm

[1] Considerable cooling in September in NW part of area, warming in the SE part, net effect was about zero (0).
[2] In August, there seems to be a net warming.
[3] These are comparisons with the 30-year mean for the Hawaiian Area.
[4] Warming in Spring 1958 is all that is known.
[5] Stewart: Anomolous sea level heights from tide gauges.
[6] Added by Editors.
(EDITORS) The above table was assembled on the blackboard from the evidence presented in the contribution of the various participants.

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

Isaacs: Was this a vertical motion of the thermocline?

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

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

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

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

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

Fleming: Extending how far south on the Atlantic Coast?

Namias: About Cape Hatteras.

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

Stommel: On the whole I would think it would not have sufficient time for a seasonal adjustment. I think that the topography would not have a chance to fully readjust. That, in a way, makes it a little awkward to see the picture. The heat content was associated with the topography and if overall adjustment of the topography did take place, it would be warmer. On the other hand, you can construct an entirely different picture from the anomolous low if you say that the temperature is in an upper thin layer, the Ekman Layer, and then the low blows this surface water away and makes it colder in the center of the low, then the topography adjusts. If you do not have some kind of a model of what is doing the heating, you can make the temperature do the opposite there off the coast, according to the Straw man given us.

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

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

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

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

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

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

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

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

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

Revelle: What I would be inclined to say is as Dr. Munk said, I think this is very primitive. Speaking of the slack of the north winds, they reduce the divergence along the coast to allow offshore water to come in, reduce the upwelling, and increase in temperature. The upwelling affects the countercurrent in some way. In some way these have a sequential occurrence. They do not all occur at the same time, but they may be all the same kind of phenomena, but not all due to the same fundamental cause. I lean to the same kind of change in the wind system. I will make a somewhat different remark than Charney made. You actually do not have to have very much movement in the area a couple of hundred miles offshore to show a temperature anomaly of 2 degrees everywhere.

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

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

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

Munk: How about the Alaska drift bottles?

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

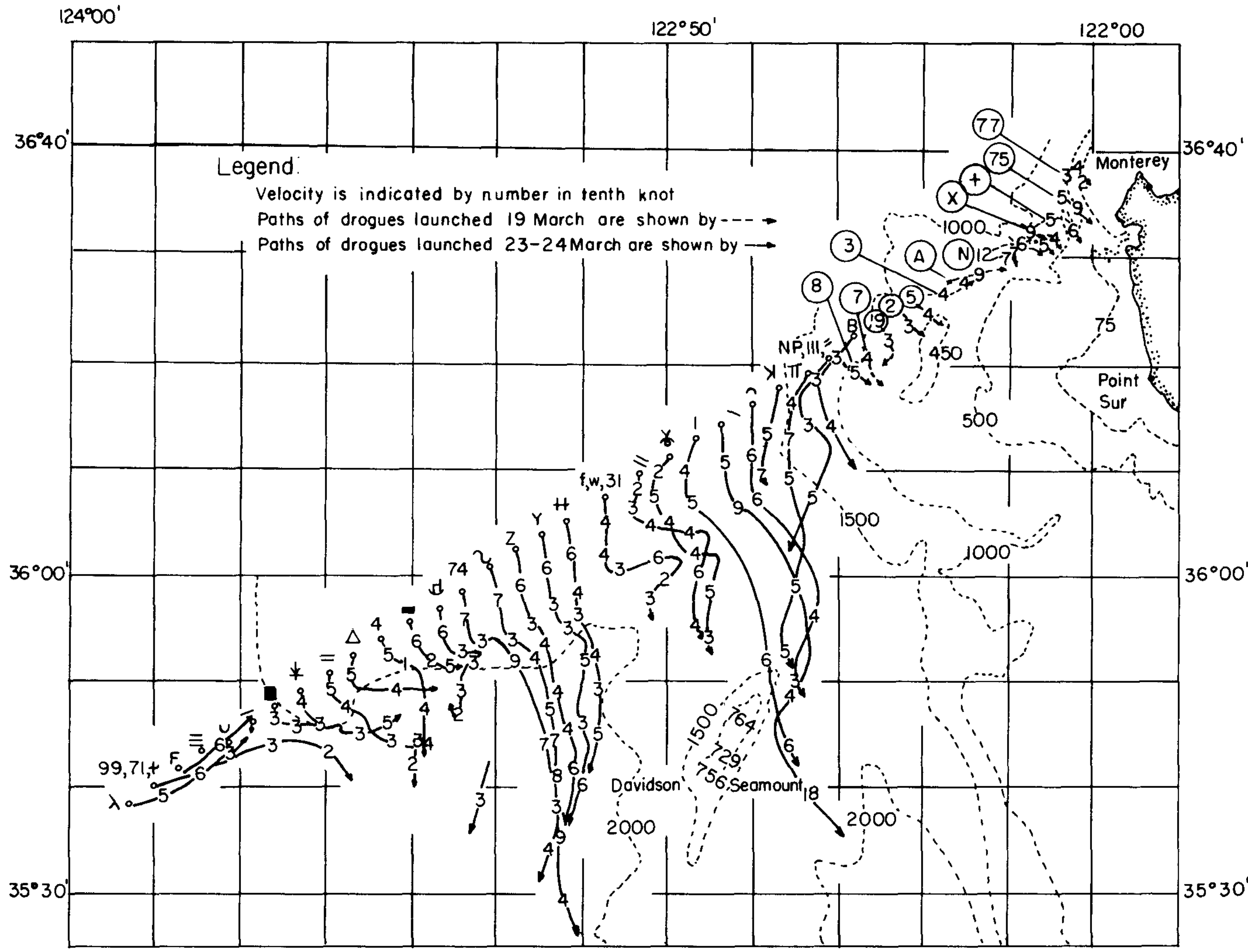

FIGURE 173. Current survey during March 1958, offshore from Monterey, California. The vessel determined the drogue positions as many times as possible during the survey. The velocities and direction of movement was determined between each positioning point for each drogue.

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

As long as we are discussing this countercurrent, I would like to mention one matter that appears to me to require explanation. All of the drift bottle recoveries above Point Conception are from bottles that were released within fifty miles of the shore. Some of these traveled over a thousand miles up the coast on the countercurrent! Many traveled several hundred miles. Of the hundreds of bottles released more than fifty miles offshore, there have been *no recoveries.* This curious matter seems to me to argue that there must be a very special structure to this countercurrent that can convey bottles along a route that has a "thinness ratio" of twenty. Some mechanism must keep these bottles offshore, but not more than fifty miles offshore, and some associated mechanism prevents offshore bottles from entering the countercurrent. If some helicoidal flow exists in the countercurrent with the surface waters moving north and offshore, this possibly would account for the first of these problems. If so, surface species of organisms should be transported north more rapidly than shallowly migrating ones, and this is precisely what we have!

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

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

Revelle: Do we have any information for the oxygen?

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

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

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

Revelle: When was that?

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

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

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

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

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

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

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

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

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

Takenouti: I have no material on oxygen change.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

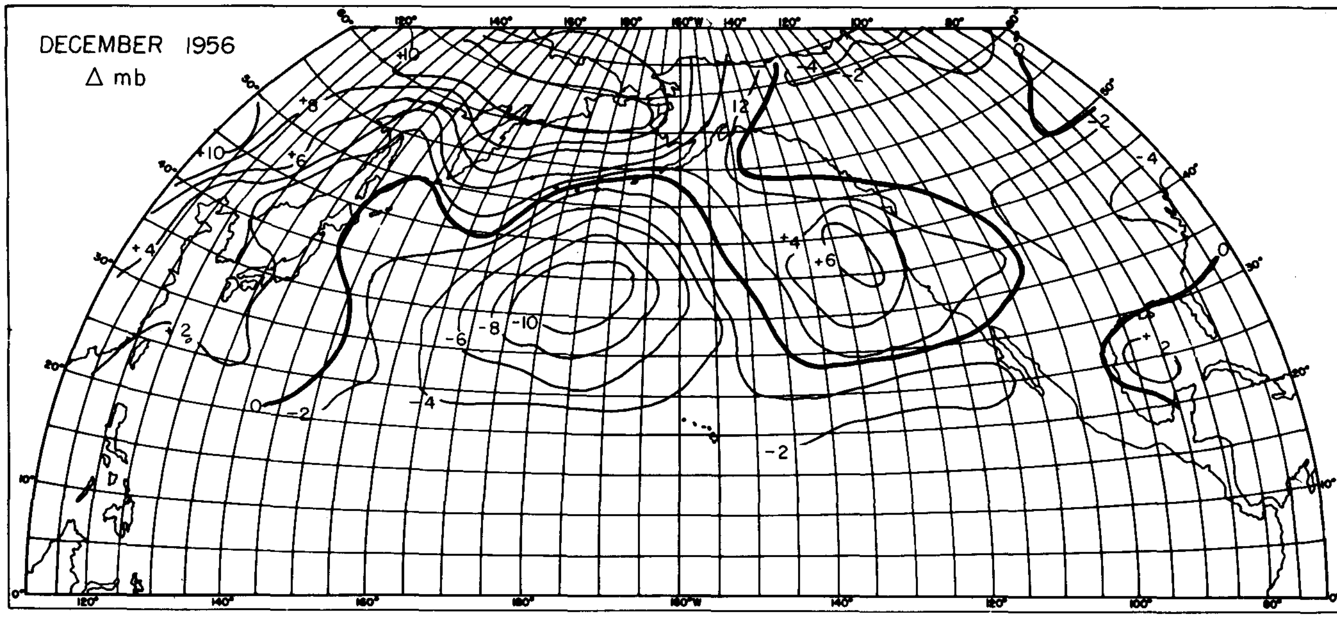

FIGURE 174. Sea level atmospheric pressure anomaly (Δ mb) in the North Pacific in December 1956, showing two cells. According to the advection theory, temperatures should be colder than average on both the Asiatic and American coasts, and quite warm in the Central Pacific.

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

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

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

Revelle: I think we now ought to discuss the last three questions. How does the ocean affect the persistence in the atmosphere? Do we have possibilities of predictions? What more research needs doing? Let us take the number three question first, particularly concerning motion in the Ekman Layer and baroclinic adjustment as a function of changes in the wind stress. Charney, would you like to lead off on that?

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

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

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

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

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

Charney: I am speaking of a highly idealized model in which the continuous density distribution, shown as the continuous curve, figure 175, is replaced by the discontinuous distribution, shown as the dashed curve. A rapidly-moving wind system will excite currents reaching all the way to the bottom. (Note added in proof: The slope current in the layer O Q should not be confused with the drift current in the Ekman Layer O P. This is probably the source of my misunderstanding with Revelle. J.C.)

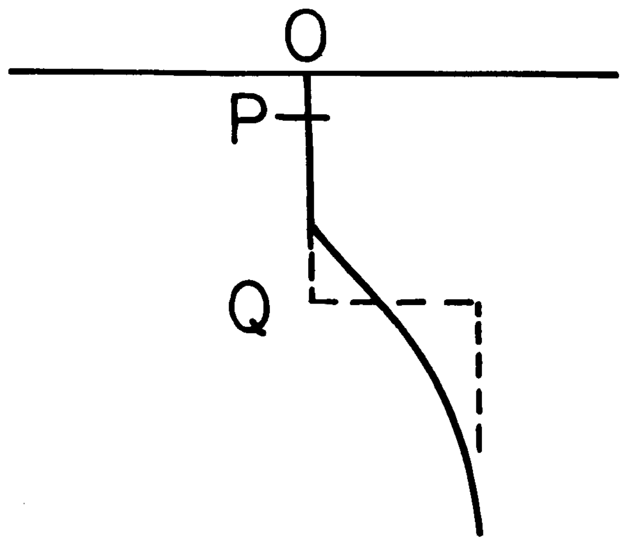

FIGURE 175. Idealized model of density distribution.

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

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

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

Isaacs: In the final results, the deep currents did not quite do that. They made a long arc with no relationship at all to the wind, whereas this surface current described another big arc in response to wind shift. Curiously, if the wind diminished suddenly, the surface drogues moved into it for a while!

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

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

Charney: If one ignores inertial effects the currents become quasi-geostrophic and the mass-velocity adjustment takes place instantaneously. Let me illustrate this point, which I think is important. Following Rossby, we consider an infinite homogenous ocean and imagine that an infinite wind stress directed northward (into the page) acts on the strip AB and instantaneously imparts a velocity U to the prism of water ABDC extending north and south to infinity (Fig. 176). The coriolis force acting on this volume will then deflect it to the right, so that there will be horizontal convergence and a rising of the free surface to the right of BD and divergence and a falling of the free surface to the right of AC. In the process the entire system will execute a series of gravitational-inertial oscillations, and some of its energy will be radiated off to infinity as a train of gravity waves. In the end the free surface will acquire a permanent deformation A′ B′ to produce the pressure force necessary to balance the current displaced from CD to C′ D′. Rossby calculated the difference in energy between the initial and final states and found that very little energy is converted to inertial oscillations, even in this extreme case.

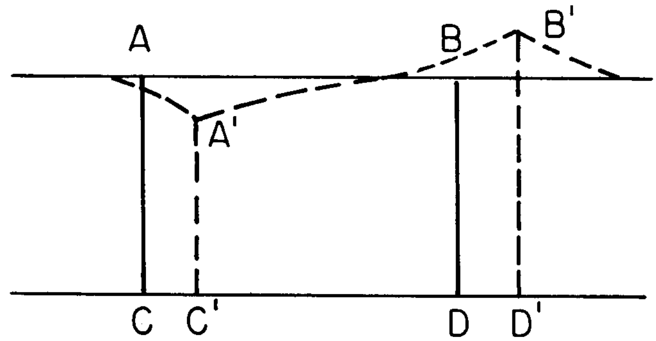

FIGURE 176. Rossby's Model.

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

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

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

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

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

Munk: Who has this geostrophic versus observed current data?

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

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

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

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

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

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

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

Thus we can still be assured that the density structure of the ocean will not respond sufficiently to a large-scale wind-storm to permit any baroclinic adjustment in the geostrophic flow. This means that in Straw man I the movement of a thin surface lens of low density water will occur, by virtue of a storm, almost entirely as an Ekman drift. There will be no need to make any correction for changes in the geostrophic currents produced by shifting surface temperature patterns. There *will be* changes in the geostrophic currents, but since these extend to the bottom, the velocities will be much smaller than the velocities in the Ekman-drift layer and relatively negligible. For somewhat longer periods, a partial response in the density field will occur, and it will be necessary to allow for the development of baroclinic geostrophic currents which themselves have appreciable surface velocities and hence modify the picture obtained from consideration of the Ekman drifts alone. Of course it scarcely needs to be mentioned that the details of the Ekman drift as a function of depth, in contrast to the vertical integral of the Ekman drift, depend upon the nature of the turbulence in the surface layers and are therefore essentially uncomputable. This is one of the reasons why the theories of Rossby and of Veronis and Stommel deal with average velocities in each layer. The more I think about this, the more I realize that the results of the theoretical models are rather susceptible of misinterpretation when applied to the real ocean, and so perhaps it has been worthwhile to have made these comments at length. I hope they will help to dispel any suspicion which I may have inadvertently fostered, that there is some deep and irreconcilable discrepancy between the well-known results of the theories, and the common-sense picture of the wind driving the surface few meters of water.

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

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

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

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

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

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

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

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

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

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

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

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

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

Isaacs: Fleming is right in my opinion. The computer might help us as a computation means, but to go heavily at this time into computational problems of unreal models where we have not yet solved realistic and typical problems seems a very questionable step. I am afraid there will be a new generation of oceanographers growing up who are going to throw words around about computational instability, and electronic wind transients, and get wholly tied up in such things. If you accept Charney's suggestion you will need increasingly more observations, and these must be more direct, unequivocal, and critical than present classical oceanographic observations, to enable you to check the success of the calculations. What can oceanographers do when they have only charts such as we have to check computations according to some model of what happened in the ocean? You are not going to check it against the geostrophic flow. You might check against temperatures. Temperatures, or against sea level, perhaps. This seems very dubious, but possible.

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

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

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

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

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

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

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

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

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

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

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

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

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

Fleming: It may anchor the meteorology geographically.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Isaacs: The drogue technique worked out quite well.

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

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

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).

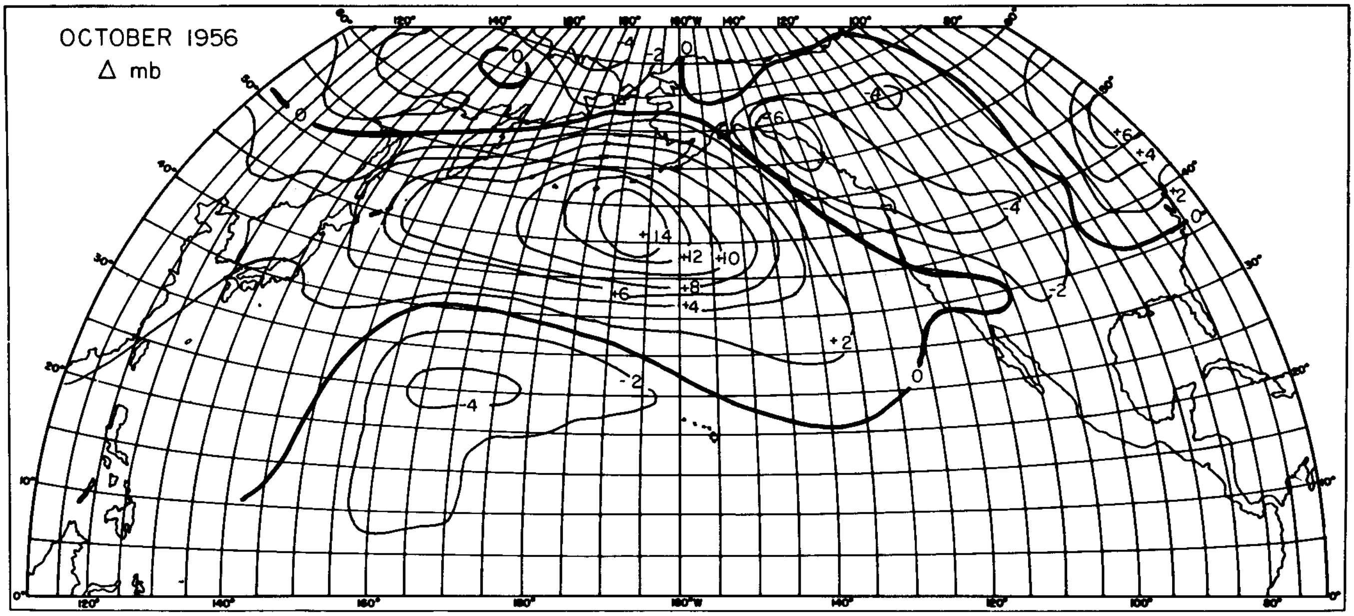

FIGURE 177. Sea level atmospheric pressure anomaly (Δ mb) in the North Pacific in October 1956.

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-

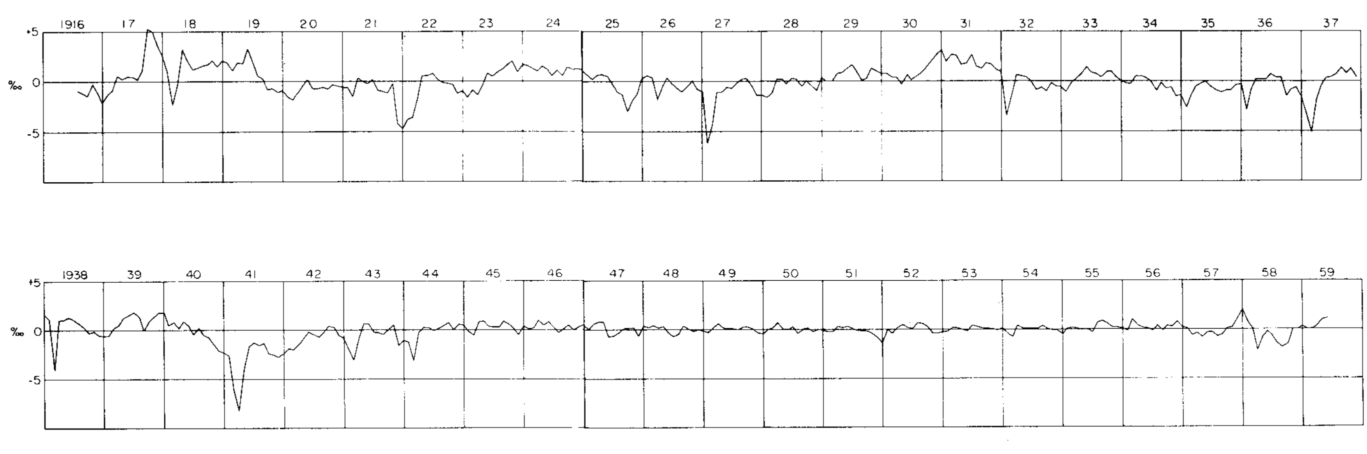

FIGURE 178. Salinity anomalies at Scripps Pier, 1916-1959.

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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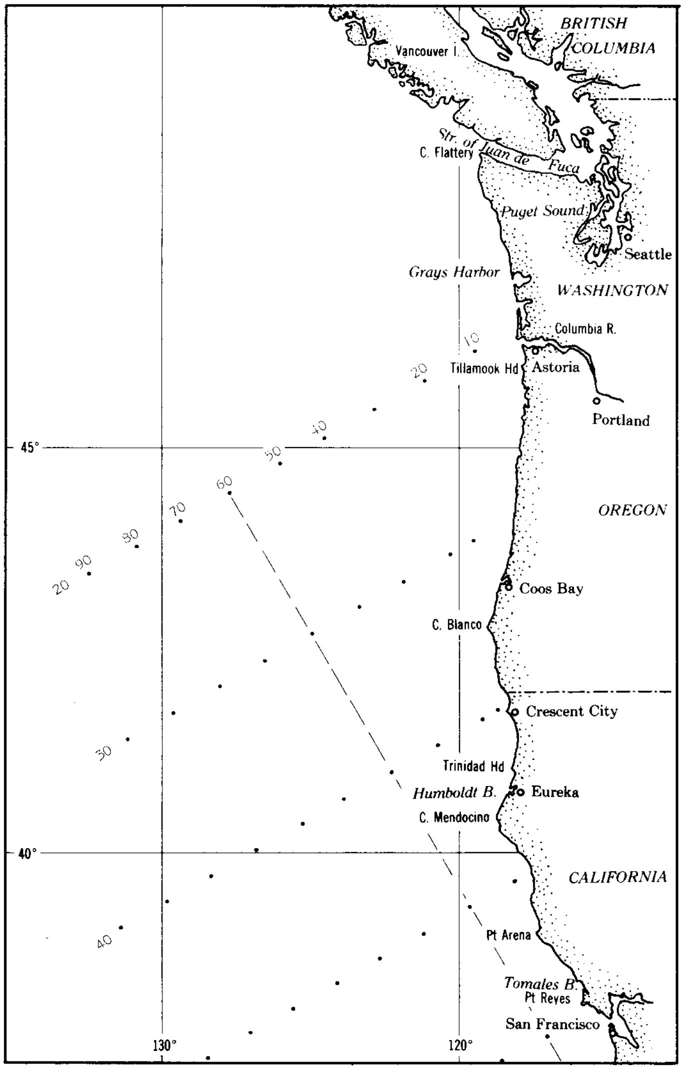

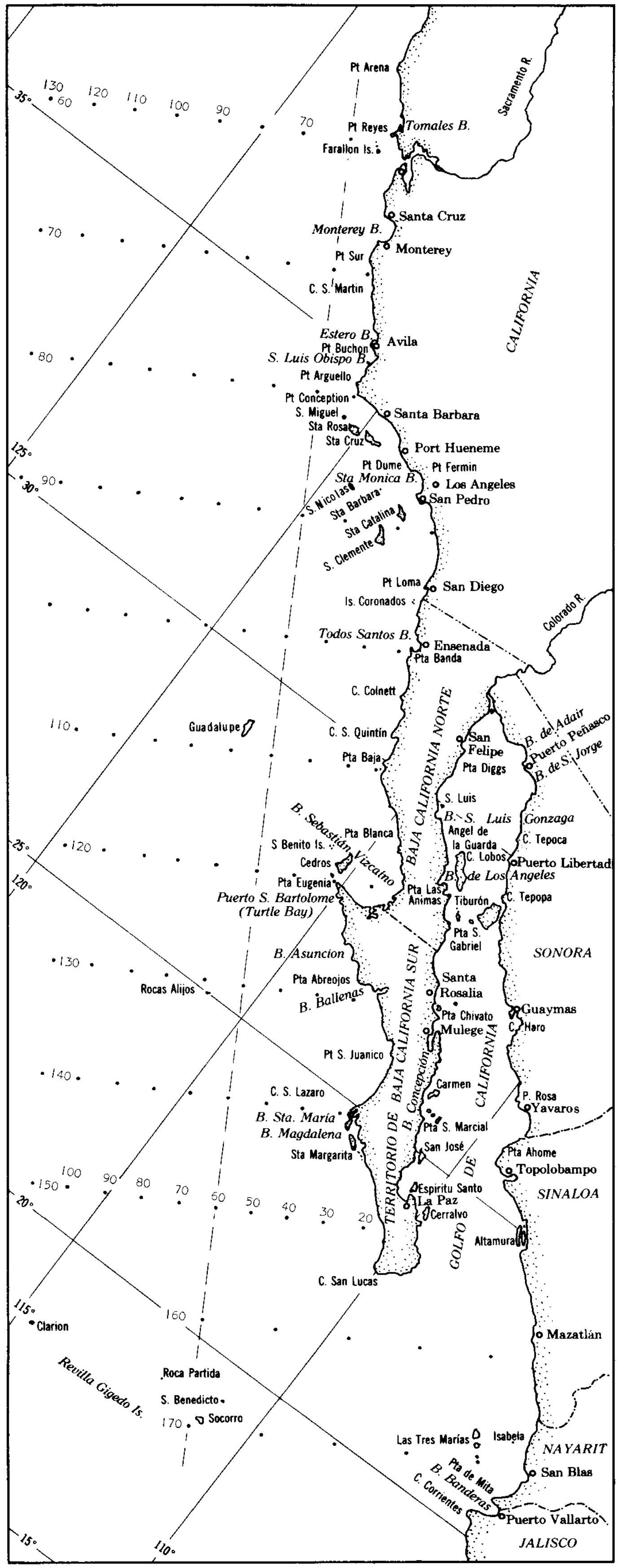

These maps are designed to show essential details of the area most intensively studied by the California Cooperative Oceanic Fisheries Investigations. This is approximately the same area as **is shown in red on the front cover. Geographical place names are those most commonly used in the various publications emerging from the research.** The cardinal station lines extending southwestward from the coast are shown. They are 120 miles apart. Additional lines are utilized as needed and can be as closely spaced as 12 miles apart and still have individual numbers. The stations along the lines are numbered with respect to the station 60 line, the numbers increasing to the west and decreasing to the east. Most of them are 40 miles apart, and are numbered in groups of 10. This permits adding stations as close as 4 miles apart as needed. An example of the usual identification is 120.65. This station is on line 120, 20 nautical miles southwest of station 60.

The projection of the front cover is Lambert's Azimuthal Equal Area Projection. The detail maps are a Mercator projection. Art work by George Mattson, U. S. Bureau of Commercial Fisheries.