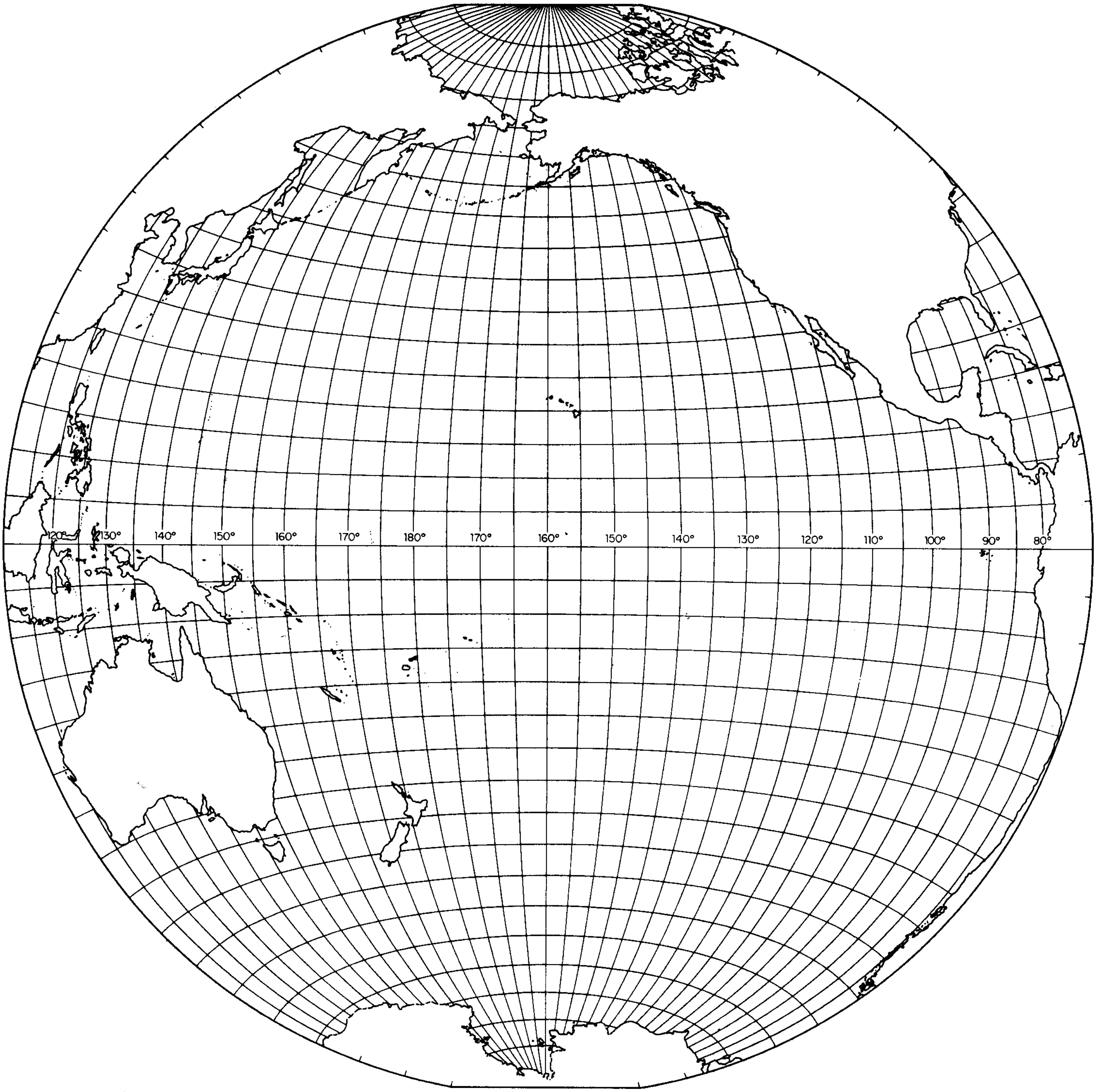
STATE OF CALIFORNIA
MARINE RESEARCH COMMITTEE



CALIFORNIA COOPERATIVE OCEANIC FISHERIES INVESTIGATIONS

REPORTS

VOLUME XII
MAY 1968

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STATE OF CALIFORNIA
DEPARTMENT OF FISH AND GAME
MARINE RESEARCH COMMITTEE

CALIFORNIA COOPERATIVE OCEANIC FISHERIES INVESTIGATIONS

Reports

Volume XII
1 July 1966 to 30 June 1967

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 May 1968

STATE OF CALIFORNIA—RESOURCES AGENCY

RONALD REAGAN, *Governor*

DEPARTMENT OF FISH AND GAME

MARINE RESEARCH COMMITTEE

1 May 1968

The Honorable Ronald Reagan
Governor of the State of California
Sacramento, California

Dear Governor Reagan:

We have the honor to submit the twelfth report on the work of the California Cooperative Oceanic Fisheries Investigations.

The report consists of two sections. The first contains a review of the administrative and research activities during the period 1 July 1966 to 30 June 1967, a description of the fisheries, and a list of publications arising from the programs. The second section consists of papers prepared for a special symposium on wide scale studies of the ocean held at La Jolla in December 1965.

Respectfully submitted,

Charles R. Carry

THE MARINE RESEARCH COMMITTEE
Charles R. Carry, *Chairman*

CONTENTS

Page

I. Review of Activities 1 July 1966–30 June 1967

Report of the CalCOFI Committee 5

Agency Reports 10

Review of the Pelagic Wet Fisheries during the 1966–67 Season 22

II. Symposium on Wide-Scale Studies of the Ocean

Joseph L. Reid, Editor 25

Introductory Statement

Joseph L. Reid 27

Long-Range Forecasting of the Atmosphere and its Oceanic Boundary—An Interdisciplinary Problem

Jerome Namias 29

Ocean-Wide Surveys, both Meteorological and Oceanographic

Harris B. Stewart, Jr. 43

Some Future Trends in Ocean Research as seen by the Geophysics Branch of the Office of Naval Research

Hugh J. McLellan 50

A Discussion of Some Critical Indices of Primary and Secondary Production for Large-Scale Ocean Surveys

T. R. Parsons and *R. J. Le Brasseur* 54

What Might Be Gained from an Ocean-Wide Survey of Fish Eggs and Larvae in Various Seasons

Elbert H. Ahlstrom 64

Summary by the Chairman

Joseph L. Reid 68

Calif. Mar. Res. Comm., CalCOFI Rept., 12: 5–9, 1968

PART I

REVIEW OF ACTIVITIES

1 July 1966—30 June 1967

REPORT OF THE CALCOFI COMMITTEE

PARTIAL REVIEW OF AND PROPOSED PROGRAM FOR RESEARCH TOWARD UTILIZATION OF THE CALIFORNIA CURRENT FISHERY RESOURCES

PROEM

In 1961 the CalCOFI Committee proposed to the MRC a number of programs that were an outgrowth of the realization that the CalCOFI research had obtained much understanding and thrown much light on a far broader spectrum of the living resources of the California Current system than the original object of study—the sardine. The programs were in addition to the basic studies and were aimed toward the development of these unused resources.

During its meeting of 8 August 1967 the Marine Research Committee requested the CalCOFI Committee to review these several proposed programs and to report on the development and present validity of the programs and to modify and augment the discussions, programs and needs in the light of present understanding.

The following restatement of the 1961 proposals comprises the CalCOFI's evaluation, report and present proposal for expanded research.

INTRODUCTION

Much of the basic work that the CalCOFI Program has performed is of fundamental significance to the understanding of the abundant species of fishes in the area. These species include the hake, anchovy, saury, squid, jack mackerel, Pacific mackerel, myctophids, deep-sea smelts, elasmobranchs, and, of course, the sardine.

The basic work has provided a substantial fund of information on the relative and absolute abundance, distribution, population stability, and other aspects of these species. For some time, this information has been quite adequate for a preliminary evaluation of the economic significance of these resources. In general, the level of knowledge is greater than that commonly available when fisheries are established elsewhere, and even greater than the understanding that now exists in many established fisheries. The private sector of California industry has not acted or been able to act on these resources, and the question naturally arises as to what restricts the utilization of the resources, i.e., are the constraints scientific, economic, social, or political?

The CalCOFI Committee adheres to the principle that the individual scientist's work is finished with publication but that the committee itself has an obligation to recognize and, so far as its capabilities permit, aid in placing these findings in perspective within the social, economic, and political milieu.

The CalCOFI Committee has thus undertaken to design a program of added research that will lead to an identification of the factors that constrain the utilization of those living resources of the California Current system which are presently underutilized.

It should be made clear that the program proposed in the following is an addition to the existing program rather than a major reorientation of the basic program of ecological studies that has yielded the present level of insight. This basic program, its continuance and its subtle reorientation is as essential to the presently proposed course as it is to the identification of the future opportunities that are presently unsuspected and unidentified.

It is the purpose of the present discussion to propose inquiry and research, drawing on the existing fund of knowledge, and ultimately permitting the conservative utilization of the great resource represented by the underdeveloped and underutilized living pelagic resources of the California Current system.

The *primary purpose* of the proposed research is to obtain an understanding of the factors associated with the development and management of fisheries on these pelagic populations of the eastern North Pacific.

The evidence points to the existence of populations of hake and anchovies in these waters that are measured in millions of tons. The populations of the other species are also of large magnitude. A presently proposed fishery on the anchovy of 200,000 tons per year is based on highly conservative estimates for a beginning experimental fishery. Without question the anchovy can be conservatively harvested at a considerably higher level (which can be determined as a fishery progresses) and several of the other species have an equal or greater potential.

Several *secondary objectives* will guide the approach to the primary objective. These include attaining understanding of the factors that might permit:

A. Diversification of the commercial fishery.
B. Reduction of competition between sport and commercial fisheries.

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C. Alteration of the fish population toward a composition of preferred sport and commercial species.
D. Accommodation to foreign utilization of local resources, and
E. Development of broadly-applicable fisheries science and technology.

A. Diversification of the Pelagic Commercial Fishery

The economic success of the pelagic commercial fishery (principally sardine) has undergone a serious collapse. Now that the causes of this collapse of the sardine fishery are better understood, it is apparent that some abundant species have relationships with oceanographic (and marine biological) conditions that are complementary to those of others. So, for example, when the sardine has a series of poor year classes, the anchovy may have a successful one and vice versa.

This probability is supported by very similar experience elsewhere. The Japanese *iwashi* fishery takes a number of fish that are ecologically in close relationship, as are the sardine and anchovy. Fluctuations in the *iwashi* fishery are much less than is general for fisheries on a single species. In addition, the availability of the mature fish may also be complementary in two species, so that one or the other is more readily available in any season.

In addition, the population of certain species, such as the jack mackerel and saury, have a far wider distribution than the sardine, anchovy, etc., and it is probable that a coastal fishery for these would draw from an enormous fish population spread over the entire eastern North Pacific. A large fishery for these species therefore might place much smaller demands upon the productivity of the area.

Other implications of a fishery developed around these species will be discussed below, but, for the purposes of the objective of diversifying the pelagic commercial fishery, the committee feels it should develop understanding for a fishery that can have success under a greater variety of oceanographic conditions than the previous pelagic fishery and draw its product from a greater part of the Pacific.

B. Reduction of Competition Between Commercial and Sport Fisheries

The State of California can afford to neglect neither the development of her great commercial fishery resources that exist off her coast, nor the full development of the wholesome, rewarding, and commercially valuable sport fishery.

It shall be among the purposes of this proposed inquiry to elucidate the relationships between these two utilizations of the pelagic marine resources, understand their areas of conflict and competition, and recommend development in directions to reduce these conflicts and competitions.

The sport and commercial pelagic fisheries of California enjoy a major advantage as compared to the analogous fisheries of other states in that their direct competition for species of fish are relatively minor. For example, except for the mackerels to some degree, the sportsfisherman seeks none of the species under discussion. Conflicts are thus principally indirect, quite unlike the case of the northern salmon, where both activities directly compete.

C. Alteration of the Fish Population

An important element of the inter-relationship of the sport and commercial fisheries stems from the highly selective demands of both of these fisheries.

Among the abundant species of fish in the waters of California, these fisheries now seek only a very limited number. These preferred species include mostly fish with relatively coastal distributions.

It is the opinion of many that if these fisheries continue to develop around this limited number of species, the general fish population will become increasingly replaced by a population of the less desirable species, whose numbers are not now reduced by either fishery. This opinion holds that the yellowtail, halibut, barracuda, seabass, anchovy and sardine may become replaced by the shark, ray, hake, saury, squid, and other presently unsought species.

This opinion presupposes that fishing can alter a population and, assuming this to be so, there is only one tool available that might reduce the number of these presently undesirable species—the commercial fishery.

Some of these unsought species have complex relationships with the preferred fishes: competitors as young, juvenile, and adults; predators on the young at several stages; and as food for the adults.

It shall be one of the purposes of this research to elucidate these relationships and to recommend development of fisheries that, if the population is affected by the fishery in any way, give the best promise that the alteration is toward an overall increase of the preferred species. The present anchovy fishery, of course, has been in part initiated with such an experiment in mind.

D. Accommodation to Foreign Utilization of Local Resources

The pelagic fishes of the California Current system inhabit international waters. The only constraint to foreign harvesting of these fish—aside from economics and in the absence of treaties—will be for foreign fishermen to cooperate in any scientific management plan on the species. Such a management plan need not involve only the utilization of the fish for commercial purposes, but should consider sport requirements as well.

Foreign fishermen already have begun using some of these fishes (hake and rockfishes) and have surveyed the potentials of others, including mackerel, anchovy, saury, bonito and sardines.

The only hope of retaining some degree of California control over these resources is to establish a scientifically managed fishery.

E. Development of Broadly-Applicable Fisheries Science and Technology

It is evident that a high level product of local fisheries has been the evolution and extended application of the fishery and processing technology developed in local waters. Among the examples of such products, the tuna industry is outstanding, where both the availability and utilization of tunas was explored, pioneered and developed in local California waters, and later extended ocean wide with important economic results to California fishermen, processors, and investors, and to the State as a whole. The sardine fishery likewise provided entries into processing opportunities elsewhere in the world.

It is thus, important that the research evaluate these supereconomic potentials of each of the resources considered, not only as extended economic opportunities for California entrepreneurs but as scientific and industrial contributions to the worldwide humanitarian development of marine food sources.

PROPOSED RESEARCH

In order to accomplish these objectives the following program is proposed:

Phase 1

This phase is a preliminary study, coalescing existing, available information on the principal undeveloped and underdeveloped pelagic fishery resources of the California Current system. The primary question is *"What restricts the proper utilization of these resources?"* To answer this question, this study will attempt to determine the probable size, value, and utilization of the resources.

The study will include the following elements:

a. Estimate from available data the probable population size, stability, and distribution of the undeveloped and underutilized pelagic fisheries including the hake, jack mackerel, saury, squid, elasmobranchs, etc., and further perfect estimates of the population size of utilized species.

b. Elucidate the probable inter-relation of these species with the preferred sport species and with each other.

c. Appraise availability of these species to present and eventual fishing potentialities.

d. Investigate the utilization of similar species elsewhere.

e. Understand the present and future demand, and market for these species, and their value.

f. Understand the legal and institutional problems of developing a fishery, including both domestic and foreign aspects.

g. Estimate the probable influence on the preferred fish stocks of a fishery supported by these unsought species.

It is proposed that Phase 1 be directed by the CalCOFI Committee and coordinated by a fisheries biologist of senior standing, with technical assistance added to each of the three CalCOFI agencies of Scripps, B.C.F., and California Fish and Game, for the purpose of extracting and consolidating the information that exists at these agencies. It is proposed that the research in which the CalCOFI agencies possess no especial competence be carried out as direct contracts or grants by the MRC and supervised by the coordinator, who would then serve to represent these programs in the CalCOFI and to the MRC.

These programs would include the portions of the work dealing with the social and institutional factors, such as economic, legal, and ethological problems; some of the industrial problems of processing, etc.

It is proposed that these latter programs also be guided by the CalCOFI Committee under the Marine Research Committee, because of the close relationship of the proposed work with the CalCOFI, and the success of this committee organization.

Phase 2

A specific research program should follow the elements listed above, as a profitable program emerges from Phase 1.

We have tabulated what appear to us to be the subdisciplines of fishery research in the broad sense, and we feel that eventually, for each resource, all these topics will have to be considered. This tabulation is attached as an Appendix to this paper.

It is clear that in the context of the present-day situation in the California fisheries not all of these topics have the same urgency, and priorities may differ from resource to resource. We have identified, and listed below, what seem to us to be the urgent fields of study for each of the main resources that previous work has identified. We do not feel that within these listings further relative priorities can be set, and the sequence the problems are listed in does not imply priority.

By excluding a problem from our listing we do not mean to imply that work on it should be halted, or not started if it seems appropriate and funds and facilities are available. This statement is particularly intended to apply to new or continuing long-term projects whose value may be enhanced as the period of their operation increases.

In particular, we recognize that multi-species fish population dynamics represents the scientific core problem and is properly the subject of greatest concern to fisheries laboratories, even though it is not generally included in the priority problems listed below.

It is important to remember that many of the sorts of data which we recognize to be essential to regulating a fishery properly will not become available until after a fishery develops and its effect on the total mortality and other dynamic parameters of the population can be measured.

I. *Northern Anchovy*

a. Stock identification and estimates: How many distinct genetic stocks are there and what is the distribution, abundance and migration patterns of each.

b. Availability and vulnerability: Proportion of each subpopulation that is available during all or part of its migration pattern; seasonal differences in availability as influenced by environment; vulnerability (catchability) in relation to differential behavior patterns of fish at different times of the year and to their varying environment.

c. Food chain studies and interspecific competition: Analysis of stomach contents to determine kinds and amount of food consumed in different areas and seasons with comparable studies of what was available in the environment; possible competition for food with other plankton-feeding fish, particularly sardines. Role of anchovy as a forage species.

d. Management problems: Multiuse, especially use of anchovy for bait by sportsfishermen and role played by anchovy as forage for game fishes; need of a comprehensive management program for the balanced use of all ocean resources.

e. Market economics: Investigations of possible use of fish in forms other than fish meal, and consideration of the needs of reduction plants for modernization on setting catch quotas.

II. *Hake*

a. Stock identification and estimates: Need to determine distinct genetic stocks, if any, and the distribution, abundance and migration patterns of each—the latter by a tagging program.

b. Availability and vulnerability: Marked seasonal migrations, made by each subpopulation of hake, seasonal differences in availability as influenced by environment; vulnerability in relation to different behavior patterns of fish at different times of the year and to their varying environment; need to establish techniques for capture in commercial quantities off southern California.

c. Food chain studies: Analysis of stomach contents to determine kinds and amounts of food consumed in different areas and seasons; role as predators on other marine fishes and as a prey species.

d. Marketing problems: Technological problems in handling hake for fresh market use; the microsporidium problem; development of MPC processing plants adjacent to fishing grounds.

e. Relationship with foreign fishery for this species.

III. *Jack Mackerel*

a. Stock identification and estimates: Need to determine whether jack mackerel stock is made up of one or more genetic subpopulations and what is the distribution, abundance and migration patterns of each.

b. Differential distributions and habits of age groups: Older fish distributed offshore—fishery depends on younger age groups, which are available inshore, but what portion of the total are available to the fishery has yet to be determined. How much is the varying abundance of young fish dependent on year class strengths?

c. Analysis and publication of age composition data collected from jack mackerel fishery since 1947 (in progress but not yet available).

d. Processing and related problems, especially with regard to the utilization of older size groups of fish.

IV. *Pacific Sardine*

a. Competition (food chain studies): Interaction between this species and other plankton-feeding fish, especially the northern anchovy.

b. Scientific management: Problems arising from fishing on this species in adjacent countries; continuous monitoring of status of populations in light of regulations recently applied to fishery.

V. *Pacific Mackerel*

a. Scientific management: Problems arising from drastic reduction in population abundance and competitive fishery in Baja California which accentuates need for management, including the need for international agreements.

b. Food chain studies: Analysis of stomach contents to determine food preference at different stages of the life history and proportion of plankton versus nekton in diet.

Parenthetically we note that the stakes in scientific management are greater than the potential yield of the Pacific mackerel fishery. Despite scientific evidence attesting to the Pacific mackerel's decline, presented over many years, no action has yet been taken which might rehabilitate this resource. This prima facie evidence substantiates allegations that the State cannot manage its resources on a scientific basis. The failure of the State to do so could vitiate this entire proposal for research.

VI. *Squid*

a. Resource identification: Studies to determine what part of the total resources in the California Current is made up of the single utilized species, *Loligo opalescens.*

b. Racial studies in *L. opalescens* to determine if the stocks that are known to spawn in definite localities such as La Jolla, Catalina, Hueneme, and Monterey, are discrete or intermingling populations.

c. Studies of the feeding habits of squid and their role as predators or prey in the marine food chain.

d. Fishing operations research to develop understanding which might lead to year-round operations and to extend known areas of fishing.

e. Market: Need to explore the possibilities of a greater market and need to investigate new products.

VII. *Saury*

a. Problem of sampling juveniles: Few larvae are now taken in oblique plankton hauls; we further need to test surface hauls as a means of sampling relative abundance of larvae and young juveniles.

VIII. *Other Abundant Resources*

Included in the group are the abundant mesopelagic fishes (myctophid lanternfishes and deep sea smelts), sharks and rays, and abundant small crustaceans such as red crab, *Pleuroncodes planipes*.

a. Fishing operations research to determine how mesopelagic fishes might be caught in commercial quantities.

b. Processing and related problems: Research is needed in possible uses for bathypelagic fishes (which may or may not be catchable in quantity), for *Pleuroncodes,* which certainly can be caught in quantity, and for sharks and rays.

Tabulation of the Subdisciplines of Fisheries Research

Environment
- Physical Oceanography
- Chemical Oceanography
- Phytoplankton—biogeography
  —dynamics
- Zooplankton—biogeography
  —dynamics
- Food Chain Studies and Interspecific Competition
- Historical Studies
- Correlations

Primary Resource Estimates
- Eggs and Larvae
- Fish Surveys
- Exploration
- Availability

Behavior and Physiology
- A. Physiology
  - Metabolic Studies
  - Gonadal Studies
  - Developmental Studies
- B. Behavior
  - Feeding
  - Migration and Distribution
  - Spawning Behavior
  - Schooling
  - Environmental Response

Fisheries Population Dynamics
- Age, Weight and Growth Studies
- Fecundity
- Recruitment
- Mortality—Natural, Fishing, Fishing Associated, and Total
- Fishing Effort and Statistics
- Stock Identification

Problems of Resource Development
- Multiple Use
- Legal Restrictions
- Fishing Operations Research
- Scientific Management Strategy
- Market Economics
- Processing and Related Problems
- Exploitation of Resources by Foreign Fisheries

—*J. L. Baxter, J. D. Isaacs, A. R. Longhurst and P. M. Roedel, February 1968*

AGENCY REPORTS

CALIFORNIA ACADEMY OF SCIENCES

The investigation of food habits and feeding behavior of the northern anchovy in California and Mexican waters initiated on July 1, 1965, has been continued. The number of anchovy stomachs collected to date has been about 1,000, the majority of which were collected and examined by Anatole S. Loukashkin. Of these, between six and seven percent were empty.

Recording of the stomach contents was made by wet volume and the components were then subjected to microscopical analysis. To date some forty kinds of food items have been recorded, and it is anticipated that this number will grow as a greater number of stomachs are analyzed.

It appears that the anchovy feeds on both zoo- and phytoplankton, somewhat in proportion to their availability. In the presence of both types of food the anchovy appears to show a definite preference for zooplankton, especially crustaceans (Copepods, Euphausiids, etc.) over phytoplankton. This modus vivendi has also been displayed by sardines and herring of the Pacific coast as reported by earlier investigators.

So far as the investigation in progress is concerned, the planktonic crustaceans—adults, larvae and eggs—appear to be consumed by preference when available. This conclusion is based on frequency of incidence, and on the wet volume contained in the stomach. The anchovy is either a particulate feeder or a filter feeder, depending on the size of the planktonic organisms available.

The presence of very minute forms of dinoflagellates and silicoflagellates in the anchovy stomachs is apparently to be explained by the ingestion of small copepods which had been feeding on these forms.—*R. C. Miller.*

CALIFORNIA DEPARTMENT OF FISH AND GAME PELAGIC FISH INVESTIGATIONS

The Department's research under the California Cooperative Oceanic Fisheries Investigations is concerned chiefly with studies of the pelagic wet fisheries and with studies of the fishery resources of the California Current System based on echo-sounder surveys. These studies are directed toward assessing the distribution, abundance, and age structure of the northern anchovy, Pacific sardine, jack mackerel, Pacific mackerel and other important fish populations. This information is basic to developing an understanding of fish population dynamics relative to their proper utilization.

The Pelagic Fish Investigations include four research projects; (i) Anchovy, (ii) Sardine-Mackerel, (iii) Sea Survey, and (iv) Sea Survey Data Analysis.

Anchovy

The anchovy tag and recovery study initiated in 1966 has continued with considerable success. Initially the internal, metal tags were recovered on permanent magnets located in the final stages of the reduction process. During the last 6 weeks of the 1965–66 reduction season, mid-March through April, 22,853 anchovies were tagged and 150 recovered. It soon became obvious that tags could not be assigned to the vessel that recovered them, but only to the fleet and consequently to a major fishing area such as Monterey Bay, southern California, or Ensenada. Gross movements, between fishing areas, could be determined but not local movements. Recovery efficiency within and between reduction plants was extremely variable. Project scientists concluded that one of the reasons for poor and varied recovery efficiency was that the abdominal cavity of the anchovy breaks open very easily especially if the fish is small and/or has been dead more than 6-10 hours. This meant that the tags frequently fell out of the fish and never got far enough along in the reduction system to be recovered by the magnets then in use.

During the second period of tagging and recovery, May 1, 1966 to May 1, 1967, project scientists tagged 77,261 anchovies and recovered 323 of the 100,114 tags released since the inception of tagging. We also developed an improved system of magnetic recovery which takes advantage of the size and decomposition rate of the anchovy by pulling the tags out of the anchovies as they are unloaded. This will allow us to assign some of the tags recovered at Terminal Island plants to the vessel that recovered them thus enabling us to determine local movements. A price dispute, in effect since this system was installed (summer, 1967) has curtailed landings at Terminal Island plants and prevented testing the system under production-run conditions.

Assessment of local movements in the Monterey Bay has not been attempted because fishing occurs over a small area. Recovery efficiency at the major plant was increased from 0–10% to 80–100% by installing (summer, 1967) a more efficient magnet.

During the third tag and recovery period, May 1, 1967–January 31, 1968, 124,058 anchovies were tagged and 57 recovered. Total fish tagged and recovered March 14, 1966–January 31, 1968 was 224,172 and 530 respectively.

Since the inception of tagging, considerable has been learned about anchovy movements between major fishing and/or tagging areas. Tag recoveries demonstrate that anchovies moved from Sausalito (San Francisco Bay) to Monterey Bay, Monterey Bay to southern California, southern California to Ensenada, and southern California to Monterey Bay. Movement between southern California offshore areas and the Los Angeles-Long Beach Harbor was also demonstrated.

Of the 530 tags recovered, 53 demonstrated gross movement: 43 were recovered during the third tag and recovery period, 40 in Monterey Bay. Several had been at liberty over 18 months. The numbers are interesting but should be viewed with caution. Practically no fishing has occurred in southern California during the third tag and recovery period, which accounts for the lack of recoveries in this area. Detailed reports of the tag and recovery study and of the fishery during the first two seasons are nearing completion.

Analysis of fishery and sampling data for the first two seasons of the anchovy reduction fishery (November 1965–April 1966 and October 1966–April 1967) is nearly complete. In southern California the fleet registered gross tonnage increased from 1,406 to 1,950 tons. The gear composition changed from almost 100% lampara when the fishery started to nearly 100% purse seines by the end of the second season. In central California most of the vessels are small lamparas but about 60% of the tonnage is taken by purse seiners.

In southern California the catch per unit effort dropped from 9 tons per hour during 1965–66 season to 7 tons per hour during 1966–67 because the quota in the San Pedro zone was reached before the fish concentrated close to the port of delivery.

Landings in central California increased from 589 tons to 9,275 tons while catch per unit effort increased from 4.5 to 9.2 tons per hour. The fact that the second season's fishery occurred during the fall, the period of best availability, accounts for the increased landings and success.

Preliminary age analysis indicates that fish of age group I dominated the live bait landings followed closely by age group II; together they comprised about 75% of the landings. In the commercial landings, II's dominated (48%) followed by III's (25%) and together accounted for 73% of the landings.

Mackerel-Sardine

Design of a log-interview program to obtain catch and effort data from the San Pedro wet-fish fleet was completed. The program was initiated in May, 1967 and has been successful in monitoring 90 to 95% of all jack mackerel, Pacific mackerel and Pacific bonito landings. Fishing information obtained from successful and unsuccessful trips includes: time, date, and number of sets; area and hours scouted; species and tons per set; airplane assists; and other data incidental to fishing operations.

Sampling of mackerel and sardines continued on a routine basis. A major change was made in the jack mackerel age and length sampling procedure. In May, 1967 we initiated a new age and length sampling plan with probability of selection of a boat for sampling proportional to the weight of its load, and with equal probability for selection of the sample units (fish) from the boat. The new southern California sampling system obtains 50 random samples (determined from a table of random accumulated tons) for every 5,000 tons of fish landed. The samples are by weight (5 pound units) and are obtained as a single unit. In central California sampling differs slightly as the sampling unit has been increased to 15 pounds.

Analysis of the backlog of jack mackerel data continued. All otoliths collected between the 1947–48 and 1965–66 seasons have been processed. Final analysis and preparation of manuscripts describing these data are in progress.

Analysis of the Pacific mackerel age composition data was completed for the 1964–65, 1965–66, and 1966–67 seasons and a paper presenting these data neared completion.

Sea Surveys

During the year, 7 fish surveys and one experimental cruise were conducted aboard the R/V ALASKA which logged 148 operational days at sea. A long delay in the research vessel's annual overhaul resulted in cancellation of 2 other scheduled surveys.

Anchovy. This species has been found in every area surveyed in highly varying densities. Most fish were found within 20 miles of shore but at times considerable quantities were present 50 to 80 miles offshore. They are generally found in coastal waters of moderate to high turbidities. Thus far none have been detected in the very clear deep waters of oceanic character. Southern California appears to be by far the area of greatest population density with Baja California ranking second. Central California waters contained light, scattered distributions with concentrations in a few localized areas. Although no cruises were made this year in northern California, past surveys using different methods indicate low anchovy abundance in the area.

A diurnal schooling behavior, occurring in all seasons and areas, was discovered. During the day anchovies are densely schooled well below the surface. At the approach of darkness they rise to the surface and disperse into the uppermost portion of the scattering layer where they feed on organisms constituting this layer. The most common food item in stomachs of night-caught fish was euphausiids. At the approach of dawn, the fish aggregate again into schools and submerge. This behavioral pattern, which is extremely common, is unfavorable for effective harvest with purse seines. The daytime schools were usually too deep and the night schools dispersed. Occasionally in localized areas, schools remain intact during the night and rise close to the surface where they are visible as large bioluminescent spots. It is this behavior that is necessary for efficient harvest by existing purse seining methods. As yet no seasonal or spatial occurrence of this behavior has been established.

In the more southern areas surveyed a preference for cool water by anchovies was observed. During the warm months fish were usually found at deeper levels or concentrated in areas of the coolest temperatures. Off Baja California anchovies were found at the deeper levels or concentrated in areas of upwelling.

Fish off southern California moved to the northern parts of that region during late summer and fall.

Central California surveys are difficult to conduct due to bad weather conditions throughout much of the year and lack of anchorages over long stretches of the coast. This region was surveyed in July and November of 1966 and March and June of 1967. The March survey was very superficial due to poor weather conditions.

It appears that this region is populated by a portion of the older and larger fish from southern California. The fish in this area may be a marginal extension of the main population located in southern California and consist chiefly of migrants. Tagging studies have established that migration between the 2 regions occurs. There is also some evidence of extensive migration from southern California into the southernmost portion of central California in summer and early fall. Surveys next fiscal year will check this possibility.

Surveys thus far have found anchovies distributed in low densities and in discontinuous patches within 20 miles of the coast. Densities of schools per square mile ranged from 12 to 16 during fall and early summer surveys. The fall survey found a more widespread distribution than that of summer. Local concentrations were found in Monterey Bay, off Pfeiffer Point and in a larger area from San Simeon to Point Arguello. Very large schools exceeding 50 tons were found close to shore in the first 2 areas.

The fish were almost exclusively large adults. Length frequency data from this region contained the largest size groups found anywhere. The highest proportion of older age groups was also found. Only in the extreme southern part of this region were younger and smaller fish present.

Southern California surveys were made in October of 1966, April of 1967, and June of 1967. Extreme variations in school numbers were experienced between all cruises. School densitites per square mile averaged 14.0 in October, 45.4 in April and 132.9 in June. Similar variation has been experienced in previous years.

This region had by far the highest anchovy school abundance. No other region has yet equalled the number of schools detected or observed. During the spring months hundreds of thousands of small surface schools are distributed over extensive areas from shore to 85 miles seaward. Echo sounding has given evidence of school densities of up to 1,200 schools per square mile in localized areas. An estimated 1,895,000 schools were present during the June 1967 survey. A somewhat similar survey in April of 1965 produced an estimate of over 2,000,000 schools. Although no accurate measure of school size has been developed, these schools are obviously small, probably averaging less than 2 tons as judged by experienced biologists on the cruises.

Schools observed or detected during the spring surveys are comprised of adult fish in or near spawning condition. Samples, taken by trawl, have consisted of fish that were spawning at time of capture. It appears that the anchovy population concentrates in this region during spring to spawn. Very low densities or a complete absence of fish were found in areas adjacent to southern California at this time.

During the fall survey only about 10 percent of the number of schools present in spring were detected or observed. Schools were found much closer to shore and were of larger size. A shift towards the northern extremity of the region and deeper schooling were also evident. The location of the larger numbers of fish present in spring remains unknown. There is some evidence that they migrate into the southern extremity of central California.

The northern portion of Baja California is usually surveyed at the same time as southern California. To date very low densities have been detected with some local abundances in Todos Santos and Soledad Bays.

Baja California was surveyed in September 1966. Anchovies were found over much of the region except in southern Sebastian Vizcaino Bay where sardines prevailed. Anchovy schools were generally quite small with most estimated at less than 5 tons. An average of 22.7 schools per square mile was estimated by echo sounding. San Roque Bay and San Carlos anchorages produced the highest school counts and midwater-trawl catches. Concentrations suitable for commercial harvest were found at these locations. The principal areas of abundance are from Cape Colnett to Point Canoas and from Cedros Island to Point Abreojos. Both areas are characterized by upwelling and accompanying low surface temperatures.

Anchovies taken by midwater trawl were conspicuously smaller than those taken in regions to the north. Adult fish were much smaller for their age and maximum sizes were far smaller than fish taken off California. It has been hypothesized that Baja California anchovies comprise a separate stock such as has been established for sardines.

Other Species. This group consists chiefly of lanternfishes, deep sea smelts, and juvenile jack mackerel. They are generally not detected by the echo sounder because they do not school compactly. Small catches of 1–50 fish are frequently taken by midwater trawl. The first 2 groups are found in low densities over vast areas. The total amount present is probably quite large but due to lack of concentrations they are not suitable for present methods of harvest.

Lanternfishes are the most common group. They are regularly taken in midwater trawls over bottom depths exceeding 200 fathoms and occur in 40 to 50 percent of all trawl tows. Catches usually consist of 1–20 fish and rarely exceed 200. A large number of species have been taken but 10–12 different species per tow comprise a large percentage of the total. Lanternfishes are distributed over all regions with the highest densities in central California.

Deepsea smelts are usually taken in catches mixed with lanternfishes but in far less quantity. Most catches consist of 1–3 specimens. Spring surveys in southern California produced some unusually large catches of smooth tongues (*Bathylagus stilbius*).

Catches of 10,000–12,000 individuals per tow were made, indicating seasonal schooling.

Juvenile jack mackerel have been taken in all regions during late summer and fall. Catches usually consist of less than 10 fish per tow ranging from 30–120 mm SL. Most are young hatched the previous spring. In southern California and upper Baja California small schools of up to 500 fish have been regularly observed under patches of floating kelp.

Larger jack mackerel are occasionally detected on the offshore banks of southern California and infrequent catches are made in the midwater trawl. Making abundance estimates is hazardous due to their ability to avoid the trawl and the uncertainty of identifying the schools at depths where they occur.

Sardines have been detected and sampled only in Baja California. The population center and area of greatest density is located in the southern half of Sebastian Vizcaino Bay. Sardine schools were found in fewer numbers and in more scattered locations from Point Eugenia to Point Abreojos. The midwater trawl has been very successful in determining the presence or absence of this species.

Other species occurring infrequently include Pacific hake, juvenile rockfishes, and whitebait smelt. Rockfishes are occasionally found over shallow banks off southern and central California. Hake and whitebait smelt are most often taken in central California waters.

Pacific sauries have been visually observed, but none have ever been taken by trawl or identified from echograms. The present survey apparently is ineffective for this species.

Invertebrate species consisted chiefly of jellyfishes, salps, and pelagic red crabs. These organisms frequently hinder trawl operations by clogging the meshes and often damaging the net. Jellyfishes, which abound in central California waters, have prevented trawl operations in areas near San Francisco and Monterey. Salps are dominant in southern California and pelagic red crabs in Baja California. The midwater trawl is highly effective for capturing red crabs. Catches of up to several tons have been made in tows of 20 minutes duration.

Data Reports: A computer program has been written to produce reports directly from computer output through a lithographic process. All data will be entered on punch cards for utilization by the program and for future analytical studies. Progress on this aspect has been minimal due to serious personnel shortages and delays in obtaining age data of completed surveys.

Data reports for surveys from 1950 to 1965 have been published through 1962.

Future Plans: A sonar will replace the echo sounder for anchovy surveys. This will greatly increase the search area which in turn will give more accurate estimates of school numbers.

A major effort will be made to determine school sizes from echograms. If a successful method can be developed, reliable estimates of absolute population sizes can be made.

Sea Survey Data Analysis

This project was established in June 1966. Most of the year was spent learning computer programming techniques, evaluating the many approaches to summarizing the large amount of data obtained during the past 15 years of Sea Survey and devising suitable computer programs for extracting pertinent information from this backlog.

About mid-year it was decided, that for the present, we would concentrate our effort on summarizing the age and size composition of the Pacific sardines taken in samples collected during past Sea Surveys off California and Baja California. The computer program can be adapted to other species such as anchovies, Pacific mackerel and jack mackerel with minor changes.

By years end, the final program was de-bugged and operational, and we are now in position to summarize Pacific sardine age-size composition in each of six geographical areas covering 1,000 miles of coast line.

Our plans for the future are to, as soon as possible, process all years for which we have data on edited, punched cards (1950 through 1960). This will give us 11 consecutive years of sardine data broken down into two time periods (January–June, July–December) per year and six geographical areas. The geographical areas correspond by design quite closely to those used by the U.S. Bureau of Commercial Fisheries to summarize the results of their egg and larva surveys.

Within the above time periods and areas, sardine data will be summarized in a manner which will give us a relative measure of year class composition and abundance as well as a monthly summary of surface temperature conditions.

It must be kept in mind that this first program has many of our subjective concepts incorporated into it and is by no means final. We do, however, feel that it gives us a summary that can be used to formulate further concepts and hypotheses.—*J. L. Baxter*

HOPKINS MARINE STATION

The Hopkins Marine Station of Stanford University at Pacific Grove, California, conducts studies on the environment and organisms of the coastal waters of central California. Under the CalCOFI program, the marine station monitors the marine environment and phytoplankton of Monterey Bay.

Approximately weekly cruises to six stations on Monterey Bay are made. Cruise data consist of: temperatures at 0 and 15 meters as recorded on reversing thermometers, salinities at 0 and 15 meters determined by titration, temperatures at 0, 10, 20, 30, 40, and 50 meters as recorded on a bathythermographic slide, and plankton wet volumes collected in a ¼ meter net towed vertically 15 meters. Also general comments on the weather, condition of the sea, marine mammals and oceanic birds are made.

In addition, daily shore temperatures are recorded at Pacific Grove and Santa Cruz.

Both shore and cruise data are compiled and distributed to interested agencies and individuals in the form of quarterly and annual reports.—*D. B. Seielstad*

SAN DIEGO STATE COLLEGE

Studies on the Sablefish and the Pacific Hake

The food relationships of the Sablefish (*Anoplopoma fimbria*) and the Pacific hake (*Merluccius productus*) were studied under contracts M-4 and M-8 between the California Marine Research Committee and the San Diego State College Foundation. Samples, taken weekly, from Newport Beach setline dory fishery yielded 1071 sablefish stomachs and 23 hake stomachs. Hake were not taken in great abundance because of low consumer demand. Analyses were made of the stomach contents, identified to the lowest possible taxonomic category, on the basis of weight and number of food items. Additional data were obtained on horizontal and vertical position of capture, condition of the gonads, the length and weight of the fish, and sex.

As soon as possible after the fish were displayed for sale the stomachs were taken and preserved in buffered formalin. In the laboratory, the stomachs were opened and their contents sorted to the lowest possible taxonomic category. The blotted wet weight of each component was measured. These data were grouped by monthly intervals. The stomach contents composition was also partitioned by the age of the fish. Ages were determined by length frequency analysis as well as scale ring counts. Ripening ovaries were also taken and the frequency of ripe ovaries in the catch was computed for each month.

Some inherent problems with data collected by the methods used in this study are: 1) the sample is restricted to those fish large enough to be taken by a specific hook size (5/0 and 4/0), 2) the varying periods of time between catching and processing the fish on the beach allow continued digestion of food organisms to take place so that the stomach contents are more difficult to identify, and 3) completely random sampling of the fishery is difficult because the dories are positioned in such a way that it is impossible to sample from more than one boat at a time. Weekly sampling resulted in field and laboratory examination of 1071 *A. fimbria* and 23 *M. productus.* An additional 113 juvenile *A. fimbria*, collected by miniature purse seine off the coasts of California and Baja California, were examined in order to study the smaller size groups that could not be obtained from the Newport Beach fishery.

A. fimbria, the Sablefish, is a species of minor commercial importance that in the later juvenile and adult stages is very abundant on and above a clay or firm mud bottom at depths of 400 to 1200 meters off the Pacific coast of North America. The geographic range of *A. fimbria* extends from Cape Colnett, in northern Baja California, to Cape Spencer, Alaska. Individuals attain a maximum size of 914 mm in fork length. Approximately 50% of the females are mature at a total length of 610 mm and an age of five years. The eggs are of the free-floating, pelagic type, and fertilization is external.

M. productus, the Pacific hake, is also a dominant benthic or near benthic species of very limited commercial importance that occurs off the coast of California at depths of 55 to 366 meters. Available information indicates that it is abundant throughout its geographic range, which extends from the Gulf of Alaska to the Gulf of California. Males and females attain maximum sizes of 660 mm and 760 mm in total length, respectively. Individuals over 400 mm in total length are sexually mature. The eggs are pelagic, round, and just over 1 mm in diameter when spawned. Fertilization is external.

The bathymetric distribution of *A. fimbria* appears to be strongly correlated with fish size, with the largest individuals in general occurring at the greatest depths. There is fairly conclusive evidence that this species occurs in deeper water in southern California than in the northern part of its range and that the bathymetric ranges of particular size groups in the population may be modified accordingly. The population of *A. fimbria* sampled off Newport Beach exhibits a typical age structure, in which the younger age groups (II and III) predominate. This population exhibited no apparent changes in its age structure distribution throughout the year sampling period, suggesting that the population is quite stable and that there were no major seasonal migrations of particular size groups.

Separate length-weight relationships were determined for adult *A. fimbria* sampled at Newport Beach and for age 0 fish sampled near the surface off southern California.

A. fimbria were grouped into size categories approximating age groups to study the influence of size on the diet of the fish. Juvenile fish (age group 0), which occur near the surface, feed primarily on small pelagic organisms, including copepods, amphipods, euphausiids, and the urochordate, *Oikopleura* sp. The urochordates constituted 53% of the diet by weight, while the crustaceans 23%. Generally a single type of food organism constituted the major portion of the stomach contents at a given locality.

The diet of age group 2 fish includes squid, salps, pyrosomes, and polychaetes, all larger prey species than found in a juvenile diet. Small zooplankton makes up a very small portion of the diet. Age 2 individuals thus appear to be undergoing a transition in food habits in that both benthic and pelagic food organisms are utilized. *A. fimbria* of age group 3 and older are primarily piscivorous (72% by weight) and at least in the area sampled, show a definite preference for one species of benthic fish, *Sebastolobus alascanus. A. fimbria* thus exhibits a shift in its food habits and position in the food chain from a primary and secondary carnivore as a juvenile fish to a higher order carnivore as an adult fish.

Considering all *A. fimbria* examined in this study, fishes Urochordata, Cephalopoda, and Crustacea were the major contributors to the total biomass of the diet, comprising 92% of the total wet weight of food material in all stomachs. Fish alone contributed 93% of the total stomach content biomass of the *A. fimbria* taken on the benthic setlines off Newport Beach. *A.*

fimbria of all ages appear to be highly opportunistic in their feeding habits, ingesting whatever suitable prey species are most abundant or available in their immediate surroundings. However, adults appear to be more selective in their choice of prey than the young.

Seasonal changes in the occurrence of fish in the diet of adult *A. fimbria* appear to be a real phenomenon. The main prey species, *S. alascanus,* was most abundant, in terms of stomach content biomass, during the period from September to February and was low in abundance during the remaining spring and summer period. The occurrence in stomachs of other fish species and squid also followed this general seasonal trend. Salps and pyrosomes remained a constant diet item, in terms of stomach content biomass, throughout the year. A larger variety of food items were found in the stomach contents during the spring and summer months (Echinodermata, Decapoda, and Cumacea), suggesting that *A. fimbria* may supplement its diet with less desirable prey when preferred prey species are not available.

Females grow at a faster rate and attain a greater maximum length and weight than males and thus predominate in the larger sizes. Individuals age 6 or younger comprise the largest part of this population (97.6%) and the majority (53%) of the fish sampled were in age group 2. Thus, in common with other fish species, the *A. fimbria* population consists primarily of young fish still undergoing rapid growth.

Female *A. fimbria* are reported to reach reproductive maturity at five years of age (larger than 470 mm). This was confirmed by the fact that all gravid female *A. fimbria* observed at Newport Beach were over 500 mm in fork length. The spawning season of *A. fimbria* is thought to be from December through April in northern California, with peak spawning activity in January and February. Mature eggs were found in *A. fimbria* sampled at Newport Beach during the period October through December, indicating that there may be a different or more extensive spawning period in southern California. As a result of this long spawning period, recruitment of young occurs over an extended period of time, resulting in a wide range of lengths for each corresponding age group and considerable size overlap between age groups.

Digestive tract parasites of *A. fimbria* were examined. These included *Anisakis* sp., a nematode parasitic on the stomach lining, a trematode of the family Hemiuridae, and an isopod, *Livoneca* sp., normally found in the gill region.

Most of the *M. productus* sampled at Newport Beach were mature individuals. They ranged from age group 3 to 13, with the majority in age group 6 or older. The length-weight relationship for *M. productus* sampled at Newport Beach compares favorably with that determined by Best (1963).

No definite conclusions can be drawn about the food habits of *M. productus* based on the small sample examined in this study. Three species of fish, *Anchoa compressa, A. fimbria* and *M. productus* and a single squid occurred in the stomach contents examined. Based on these results and information reported in the literature, *M. productus* appears to be primarily piscivorous, and thus can be classified as a higher order carnivore.

Because of their high population densities, size, and food habits, both *A. fimbria* and *M. productus* are undoubtedly important members of bottom communities in which they occur and both represent important latent food resources.—*J. B. Conway, D. A. Farris and R. F. Ford.*

SCRIPPS INSTITUTION OF OCEANOGRAPHY MARINE LIFE RESEARCH PROGRAM

The Marine Life Research Program carries out the portion of the California Cooperative Oceanic Fisheries Investigations that is the responsibility of the University of California. The program is principally concerned with the ecology of the California Current system—that is, with its currents, temperatures, populations of organisms, chemistry, climate, etc. and with the fluctuations in all of these.

In the period of existence the Marine Life Research Program has considerably extended its scope, principally through research grants and contracts from Federal Agencies such as the Office of Naval Research, the Atomic Energy Commission, the National Science Foundation, and by informal cooperation with the Environmental Scientific Services Administration (including the Weather Bureau, and the Geodetic Survey), with Naval Activities such as the Fleet Numerical Weather Facility, and with other research programs of the University. All of these expanded activities of the Marine Life Research Program have stemmed from the broadness of the basic concept of the California Cooperative Oceanic Fisheries Investigations as conceived and initiated by its genitors.

In addition to the broadened program, the central responsibility of the University to monitor the California Current and its fisheries remains highly viable.

The entire Marine Life Research Program was discussed in some detail and depth in the CalCOFI Reports Volume XI, issued in January, 1967. The present statement will thus serve mainly to point out some of the most recent developments and to enter in some depth into a discussion of developments in the important problem of plankton sampling.

Recent Oceanographic Conditions in the Pacific

The northeast Pacific Ocean was generally warmer than normal by 2° F from July, 1966 to June, 1967. A colder than normal area in the Gulf of Alaska in August, September, and October, 1966 became approximately normal the remainder of 1966 and the first half of 1967; by mid-1967 it had warmed to 2° to 4° F above normal. A cold area off the west coast of the United States and Baja California developed in April, 1967 but it had become much smaller by July, 1967.

North Pacific Study

Preparations for a rather large program to study conditions in the North Pacific were reported in Volume XI. This study is progressing well.

A newly-designed stable mooring has been designed and will soon be installed in the Western Pacific in deep water to appraise its ability to survive typhoon conditions. The experience of the four moorings already placed some hundreds of miles offshore of California has shown the survivability of such instruments and the value of their data. By the fall of 1968 we plan to have a number of such stations in the North Pacific where the ocean conditions and weather of the California coast receive some of their most important inputs.

Deep Benthic Conditions

Deep fish populations have been under investigation and were reported upon in CalCOFI Reports Volume XI. This research has continued and is yielding interesting and important findings. These can be summarized as follows:

1. Very large predators (sharks) are common in the deep water between 600 m and 2000 m. At 5 stations out of 8 sharks estimated to be longer than 6 m were photographed.
2. All stations showed abundant deep fish. Surprisingly very deep stations, 3400 m and 6000 m, photographed many fish—at the latter station and in a single photographic frame, there were more than 50 fish, mostly between 0.5 and 1.0 m long.
3. All but one station (10 total) photographed fish within the first 30 minutes after the camera had reached the sea floor and five stations photographed fish within 5 or 15 minutes after the camera reached the bottom.
4. The tanner crab and the sablefish are important underdeveloped resources of the California and Mexico coasts and are present in great abundance at depths of 600 to 2000 meters.

Deep currents have now been measured in many places and are found to be both simpler and somewhat stronger than expected. The fluctuating component of these deep currents mainly results from the semidiurnal surface tides.

Zooplankton

During the last few years, the ecology of the California Current and adjacent regions, both in the near-surface waters and the deeper waters, has become much better known through the research of the zooplanktologists in the Marine Life Research Program at Scripps Institution of Oceanography.

Some of this research has been to evaluate various plankton nets with species diversity, and avoidance of zooplankton. Other research that is being carried out now is an attempt to determine patchiness of zooplankton. These studies are of primary importance to the evaluation of the past zooplankton collections and will help determine what is necessary to improve our methods of sampling in order that more accurate estimates of populations can be made. This work is highly relevant to the population estimates of pelagic fish using fish larvae, since the larvae react much the same as zooplankton to various nets.

The findings of Dr. Fleminger, Dr. Clutter (Bureau of Commercial Fisheries) and Dr. McGowan are the following:

1. The smaller the net, the smaller the number of species caught, even though the volume of water filtered is held constant for each net.
2. The smaller the net, the smaller the species abundance in the collections.
3. The degree of avoidance varies among species.
4. All nets used had bridles, tow lines, cable clamps, and cables preceding them; this hardware may have been an important cue to various zooplankton to facilitate their escape.

This research brought about the development of the Brown-McGowan Opening and Closing Paired Zooplankton Net (BMOC) and the opening and closing mid-water trawl. These nets have been designed to minimize the problems encountered by many others. They are helping to solve the problems of zooplankton patchiness and avoidance. Also they filter large volumes of water in a reasonably short time and can be opened and closed at known depths so that zooplankton diversity at various depths plus vertical migration of zooplankton can be studied more meaningfully.

The BMOC net has two nets with a combined mouth area that is almost exactly the same as the one-meter net used for the last 17 years on the CalCOFI patterned cruises. No bridle, tow line, or cable precedes the mouth openings. The nets can be opened and closed at any desired depth. In tests between the one-meter net and the BMOC net, it was shown that a single BMOC tow caught as many species as a cumulative species list based on five one-meter net tows. In a comparison between a BMOC tow and tow using a net with a mouth diameter of 1.4 m, the latter caught 23 percent less species than the BMOC net. The BMOC net has since been used for zooplankton studies to study the important problem of the vertical distribution of zooplankton.

Dr. Angeles Alvariño has recently published on the vertical distribution of Chaetognatha, Siphonophorae, Medusae, and Ctenophorae between the surface and 3000 meters from data obtained using this equipment.

An important study to understanding replenishment of various zooplankton that live primarily in the California Current has been carried out by Dr. Abraham Fleminger. There are certain California Current copepods that are most abundant at or above the thermocline. Where these copepods encounter the mixing of central water with California Current water, these zooplankton are found in greatest abundance between 250 and 350 meters below the surface, the locations of the northerly flowing current underlying the California Current. By this mechanism, these animals are able to maintain their population despite the net southward transport of the upper layers of the California Current.

The determination of the zooplankton biomass of selected cruises is continuing and the first Biomass Atlas is being prepared.

Historical Study

During the period, Dr. Alvariño of Scripps visited Spain to examine some of the early documents of Spanish exploration of the California coast. It was expected that such documents clearly would contain valuable clues of the past, and hence, to the anticipated climatological and oceanographic conditions in the California Current, but the extent of the documentation found far exceeded any expectations. For example, the Malaspina Expedition visited this coast in 1784 to 1790, and described the conditions, fishes, invertebrates, harbors and peoples. Over 50 extremely fine unpublished color plates record details and identifiable marine organisms. Clearly a wealth of data rests in the archives of Spain and Portugal, all awaiting attention to extend our knowledge of the conditions and fauna of the California Current system back almost two centuries, and hence vastly to extend our knowledge of the range of events to be expected in the future.—*J. D. Isaacs*

U.S. BUREAU OF COMMERCIAL FISHERIES FISHERY-OCEANOGRAPHY CENTER

At the end of the period under review a major reorganization of Bureau of Commercial Fisheries activities at La Jolla occurred with the merger of the California Current Resources Laboratory and the Tuna Resources Laboratory into a single laboratory, subsequently to be known as the Fishery-Oceanography Center. Within the new unit all research programs of the earlier laboratories were organized into four unified programs: *Fishery-Oceanography; Behavior and Physiology; Population Dynamics;* and *Operations Research.*

Since the new research programs are essentially based on disciplines rather than species groups or geographical regions as were the previous laboratories, CalCOFI-coordinated research may be performed in several administrative units within the new laboratory. In fact, most has been placed in Dr. Smith's Population Dynamics Program, with some elements in Dr. Lasker's Behavior and Physiology Program. Dr. Flittner's program, though not formally associated with the work of the CalCOFI Committee is very germane to fisheries research in general in the California Current.

In order to avoid future confusion, the present organization of the Fishery-Oceanography Center will be used in this report, though in fact during most of the period under consideration the older laboratory arrangement was still used at La Jolla. This report is concerned only with research activities directly related to the interests of the CalCOFI Committee and coordinated by it; other research, even if performed in the region of the California Current, is not described here and interested persons are referred to the general report of the Fishery-Oceanography Center for information. Much of what follows is extracted from the report for the fiscal year 1966–67.

Vessel operations

During calendar year 1966 monthly coverage was resumed over part or all of the CalCOFI pattern between lines 60-140, using the new research vessel *David Starr Jordan* and cooperating vessels from the SIO and the CF&G.

The *David Starr Jordan* returned from CalCOFI cruise 6606 on July 1. On July 7, the *Jordan* teamed with the Scripps research vessel *Alexander Agassiz,* departed on CalCOFI cruse 6607. The *Jordan* covered lines 60-97 (San Francisco, California to Descanso Point, Baja California) taking 81 hydrographic stations with net tows and 26 stations with net tows only. The *Agassiz* covered lines 100–137 (Todos Santos Bay to Point San Juanico, Baja California) taking 81 hydrographic stations with net tows and 52 stations with net tows only. The vessel completed the cruise and returned to port on July 28.

On August 4 the *Jordan* departed San Diego on CalCOFI cruise 6608. Despite returning to port briefly for repairs, the *Jordan* completed its assigned pattern station, lines 80-137 from off Point Conception, California to off Santo Domingo, Baja California, making 134 double net tow stations. The *Jordan* returned to San Diego on August 27.

The *Jordan* left San Diego on September 6 to begin CalCOFI cruise 6609. Taking up the first station on line 80, off Point Conception, the ship worked south until September 12 when it returned to San Diego to land a crew member who was ill.

Leaving again on September 14, the *Jordan* continued south to the Sebastian Viscaino Bay area where failure of the rudder bearing made it necessary for the ship to return to San Diego for emergency repairs.

The *Jordan* worked in cooperation with the Scripps research vessel, *Alexander Agassiz,* in conducting CalCOFI cruise 6610. From October 7–24 the *Jordan* occupied 98 stations in that portion of the pattern from just north of San Francisco to San Diego. The *Agassiz* began work on October 13 from south of San Diego and worked to lower central Baja California, occupying a total of 126 oceanographic-biological stations; the *Agassiz* returned to port on October 29.

On October 28, for the first time, the *Jordan* was used to operate trawling gear during a 1-day trip off San Diego when a pelagic trawl belonging to the CF&G was rigged and operated successfully on a number of tows.

The *Jordan* was chartered in November by the Scripps Tuna Oceanography Research program (STOR) for a survey off southern Baja California. In addition to the STOR survey, 22 net-tow stations were occupied between San Diego and Punta Eugenia, Baja California for the CCRL. The STOR survey was conducted between Punta Eugenia and Cape San Lucas.

On December 1, *Jordan* teamed with the Scripps research vessel, *Alexander Agassiz,* departed on CalCOFI cruise 6612, *Jordan* covered lines 60-93 just north of San Diego, taking 20 net-tow stations and 72 hydrographic stations with net tows. *Agassiz* covered line 97, off Ensenada, to line 137, occupying 52 net-tow stations and 72 hydrographic stations with net tows. The *Jordan* returned to home port on December 19.

In January, 1967, the *Jordan* was turned over to Eastropac for outfitting for Eastropac cruises.

Stock assessment

This program has as its objective an understanding of the population dynamics of fishes found in the California Current system, that is, to determine at various levels of fishing intensity the quantity and quality (sizes of fish) in the average annual catch. For many years, the principal research effort was centered on the Pacific sardine. Since 1952, however, age determinations have been made of the anchovy catch as well. With the start of a modest 75,000 ton reduction fishery on the anchovy, authorized by the California Fish and Game Commission in November, 1965, attention has been focused on this species.

Information for this program is derived, in part, from age and growth studies carried out cooperatively by scientists of the CF&G and the BCF. Through a contract with the California Academy of Sciences, the Bureau samples landings of anchovies as well as sardines in Baja California and gathers statistics on landings of sardines, anchovies, Pacific mackerel, jack mackerel, and thread herring.

A paper on anchovy fecundity and its application to estimation of anchovy biomass in conjunction with egg census data is in the final stages of revision. Examination of anchovy catch data shows that female anchovies are somewhat more numerous than males in the commercial catch. Females are also somewhat larger and heavier than males. Females made up 57 percent by weight of the catch and males 43 percent, or the biomass of males was only 75 percent of the biomass of females. This would reduce total anchovy biomass estimates by 12½ percent from those based on the assumption that the biomass of the two sexes is equal.

The California sardine fishery produced less than 300 tons in the 1966–67 season, the lowest catch on record. The catch, mainly from schools of mixed species, consisted entirely of northern subpopulation fish as it has for the past several seasons. As in other years of low catch, the fish were older, larger and fatter than in years of very high catch. Analysis of sardine scales during the 1965–66 season indicate that 98 percent of the fish samples were 4-ring and older. During periods of high catch, the dominant ages are 2 and 3. Average length of the fish samples was 231 mm and the average condition factor 141, both much higher than in years of high catch. Although the scale reading of California sardines is incomplete for the 1966–67 season, preliminary results show that the few fish taken were as large and old as in the past few seasons of low catch.

It was planned to have monthly survey cruises during calendar 1966. Due to a breakdown in the *David Starr Jordan* during the initial January cruise, this vessel was not available again until May. As a consequence, the February cruise, made by the *Alexander Agassiz,* was an abbreviated one and there was only one full coverage of the CalCOFI area during the March-April period (mostly made in April). During the remainder of the year two-vessel coverage was obtained during the months of July, October and December, single vessel coverage on the remaining months. The November cruise, cooperative with the STOR group, occupied only two inner stations on each line between 97–120, but full coverage was obtained on lines 123–153. Despite the abbreviated coverage on some cruises, 1,990 plankton hauls were obtained during the year.

PLANKTON HAULS OBTAINED ON CALCOFI DURING 1966

Cruise	Station Lines	Jordan	Agassiz	Alaska	Total
6601	60-137	86	134		220
6602	80-120	--	140		140
6603–4	60-137	--	136	101	237
6605	80-137	195	--		195
6606	60-103	134	--		134
6607	60-137	104	133		237
6608	80-137	177	--		177
6609	80-120	130	--		130
6610	60-137	98	126		224
6611	97-153	80	--		80
6612	60-137	92	124		216
		1,096	793	101	1,990

The reintroduction of the monthly survey in the CalCOFI survey program during calendar 1966 permitted precise information to be obtained about the distribution and abundance of the larvae of a number of species of present or potential commercial importance, including the northern anchovy, Pacific sardine, Pacific hake, and jack mackerel. During the preceding 5 years, surveys had been limited to four cruises per year, spaced at 3-month intervals.

As expected, anchovy larvae dominated the collections. The population size based on abundance of larvae is comparable to that found during the preceding 4 years, 1962–65. There has been a northward shift in the abundance of larvae. In the 1966 collections, sorted to date, over 50 percent of the anchovy larvae have been obtained off California.

There is a great deal of interest in determining the present abundance of hake in CalCOFI cruises, because it can be used for producing Marine Protein Concentrate and because the Russians are developing a fishery on this species. Hake has a spawning season restricted to 4 months, January–April. The peak of spawning usually occurs in February or March, months not adequately covered by the quarterly survey cruises of the preceding 5 years. Hake larvae have been abundant in CalCOFI collections during the January–April period. Spawning in 1966 appeared to be earlier than usual, and a higher portion was obtained off central California.

The numbers of hake larvae taken on the first four cruises of the year were as follows:

Hake larvae	Station lines	Number of occurrences	Standard haul totals
January cruise	60-137	87	28,820
February cruise	80-120	102	29,600
March–April cruise	60-137	122	13,460
May cruise	80-137	40	350
			72,230

The 1966 estimate of hake larvae was one of the larger on record, despite the fact that only one cruise was made during the 2-month period, March–April. The indicated high abundance estimate is gratifying inasmuch as the 1959 and 1960 estimate, the last ones available, were considerably below average.

The scant year for the jack mackerel fishery in California during 1966 has stimulated interest in determining whether this was due to poor availability, poor success of year classes or to an actual decrease in the size of the spawning population. Only the younger ages are taken by the fishery.

Jack mackerel larvae mostly are obtained during the months of April through July. The number of larvae obtained during this period was as follows:

Cruise	Area covered	Occurrences	Number of larvae standard haul totals
March–April	60-137	68	2,390
May	80-137	82	3,660
June	60-137	71	2,950
July	60-137	109	6,185
		330	15,185

Yearly estimates of jack mackerel larvae of CalCOFI cruises usually range between 8,000–20,000 larvae. The 1966 estimate is somewhat above average. Hence, both the wide distribution of jack mackerel larvae in 1966 and their abundance in the CalCOFI area indicate that the spawning population of jack mackerel is still large and of comparable size to that measured during the 1950's.

A distributional atlas of the anchovy-larvae is being prepared for all cruises of the CalCOFI from 1951–1965. The data will be presented on two charts for each cruise; one will be for the size group, 6.26–12.25 mm, the one most adequately sampled, and the other will be for total numbers collected.

Data processing

In an effort to make the egg and larval data collected on the CalCOFI oceanographic-biological surveys more readily accessible, a program was initiated for recording the data for automatic data processing. The decision was made to record not only current and future data in this manner, but also to transfer historical data of important species for automatic data processing.

The historical data for the 2 years 1958 and 1959 were selected for a pilot study of sardine and anchovy larvae data and this was completed first. Subsequently, transferral of data of these two species for the years 1951–57 and 1960–64 was undertaken. Supplementary haul data and information on other species are also being incorporated as follows:

Identification	Haul data	Larvae data
1. Cruise 2. Station 3. Date 4. Position coordinates 5. Type and number of tows	1. Starting time 2. Stopping time 3. Depth 4. Volume strained 5. Plankton volumes 6. Standardizing haul factor 7. Area factor 8. Local sunrise 9. Local sunset	1. Sized anchovy larvae 2. Sized sardine larvae 3. Total jack mackerel larvae 4. Total Pacific mackerel larvae 5. Total hake larvae 6. Total rockfish larvae
Physical data	**Egg data**	
1. 10 meter temperature 2. 10 meter salinity 3. 10 meter O_2	1. Total sardine eggs 2. Total anchovy eggs 3. Total saury eggs	

The recording and standardization of current data has become simplified. Information from the original data sheets and the meter calibrations is reproduced directly onto punched cards. Tables of standardized eggs and larvae and standardized plankton volumes, together with the calibration and regression plot are thus obtained without further hand manipulation.

Sampling techniques

Previous studies have established the importance of sampler performance and plankton avoidance in quantitative sampling. A study has been made of mesh selectivity and the size distribution of zooplankton in the California Current. One phase is concerned with 2,000 plankton samples taken over the entire California Current area in 1965 and 1966 using paired nets of different mesh size and the other involves a comparison of four different mesh sizes used in a series of samples taken at an inshore and offshore station.

The CalCOFI survey has included a fine mesh net dubbed the "anchovy egg net" which is towed simultaneously with the standard CalCOFI net in a rectangular-shaped frame. Displacement volumes of the 0.55 mm mesh width CalCOFI net averages 60 percent of the volumes taken by the 0.333 mm mesh anchovy egg net. The differences in plankton volume change with region, being greater onshore than offshore. The differences are greater in the northern section of the California Current than in the southern section while the fine mesh volumes exceed the regular mesh volumes to a greater degree in winter and spring than they do in the summer and fall. Although both nets indicate the same areas to be productive, the catch of the fine net includes much of the food of filter-feeding fishes missed by the regular net.

The geological technique of successively graded sieves has been applied to the preliminary sorting of plankton samples. The eight grades of mesh sizes were used ranging from 0.15–2.20 mm. The technique appears most useful for organisms of simple form, such as fish eggs, although it can be used for removing salps, ctenophores and filamentous algae from the samples.

Extreme variability in replicate and paired plankton tows have emphasized the importance of small-scale patterns in the microdistribution (the study of vertical, etc.) of zooplankton organisms. One implication of this finding is that such organisms as pelagic fish larvae having limited mobility and large food requirements may depend on coincidence to locate dense concentrations of food organisms. This is shown by Dr. Schumann's experiments in fish rearing where he has observed that a sardine larva, at the onset of feeding, is capable of searching approximately one cubic centimeter of water in one hour. Since it is known that copepods, for example, tend to aggregate in the sea, larvae in a barren area would incur an energy deficit and die.

Sonar studies

The sonar cruises of May, June, August and October, 1966, on the Bureau research vessel, *David Starr Jordan*, have produced a record of abundant targets likely to be schools of fish and have demonstrated that these are frequently massed in large groups extending over tens of miles. The largest group was encountered in May and extended over a distance of 30 miles with a peak registration of 190 schools in 10 miles. Two other groups of half this magnitude were also encountered in May. In June five large groups were recorded, with a peak abundance of between 100 schools and 170 schools per ten miles. These extended over distances of 20 to 80 miles. In August, on the other hand, there were only two groups of any size, one with a peak of 100 schools, the other with a peak of 40 schools per ten miles. Again in October only two groups were recorded. The largest, extending over a distance of fifty miles from Point Fermin south to Del Mar, had a peak of 70 schools per ten miles, and the other, off Santa Monica Bay, had a peak of only 30 schools per ten miles.

Most, but not all of these aggregations were close to the coast or off islands. The two largest in May were centered about 40 miles off the coast between Point Fermin and San Diego and did not extend any closer than 30 miles to the coast. These were identified visually as anchovies. Two of the five groups encountered in June were over 100 miles southwest of Ensenada Bay. No offshore aggregations of comparable magnitude were recorded in August or October.

Since the distribution of many fishes is known to be associated with temperature, the sonar data from these cruises were examined in respect to surface temperature distribution. Only one dynamic event indicative of association was found. The two major concentrations of May were approximately centered on the 16° C isotherm, which was about 50 miles offshore from San Diego. In June the isotherm had moved offshore and northward and no concentrations existed in these locations where the water was about 5° warmer, but a large concentration occurred along the seaward side of the Channel Islands, and another along the coast from Point Conception north, again in association with 16° isotherm. Other large groups in June were far to the south in warmer water, and no large groups were associated with this isotherm in August or October. These data are too scant to warrant generalizations about associations between school groups and temperature, especially since most of the groups were unidentified, but they do indicate that any such associations can be delineated as survey information accumulates.

Temperature is important in another sense in regard to acoustic survey work. The directional dispersion of sonar sound emission is strongly influenced by the vertical temperature profile. Sound transmission patterns generated by the U.S. Navy Fleet Numerical Weather Facility in Monterey for temperature profiles taken on other cruises in the region vary from situations where the center four degrees of the emission cone propagates horizontally close to the surface to situations where the entire cone is refracted sharply downward. Such differences would alter the volume of water searched by the sonar at different depth strata, an important consideration if estimates of school abundance are to be attempted from acoustic records. As a first approach to examining the relation between temperature structure and sonar records those BT's taken closest to the large concentrations recorded during the above cruises have been selected for processing by the U.S. Navy Fleet Numerical Weather Facility.

The records of these large concentrations do vary considerably in character, possibly in relation to temperature structure, but probably also in relation to the nature of the targets themselves and to their depth in the water. Some register only close to the vessel, others at only a considerable distance. Some show echoes that are stronger and larger than others for equivalent distances from the vessel, and some show two groups of echoes at different distances. The importance of school depth in interpreting such variation is illustrated clearly by matched depth sounder and (horizontal) sonar records of the one notable concentration registered on the October cruise. During the afternoon the schools of this group were at a depth of 250 meters, as shown by the depth recorder, and were registering at distances of 800 to 1,500 meters from the vessel horizontally, as shown by the sonar recorder. These schools started to rise in late afternoon and by 1800 hours had disappeared from the depth sounder record. As this occurred the distance at which schools registered on the sonar record shortened so that by 1800 hours echoes were all within 500 meters of the vessel. The distance of registration continued to shorten and by 1900 only occasional schools appeared on the sonar record at distances no greater than 100 meters from the vessel. It is obvious that as schools become shallower they intercepted the conical beam of the horizontal sonar transducer sooner, and hence closer to the vessel. It is also obvious from this sequence of events that special care must be taken to avoid bias in estimates of school abundance. In estimating this group, for example, the nighttime record should probably be discounted on the assumption that many schools close to the surface do not show on the sonar record; or on the assumption that the schools tend to break up when they reach the surface. In a more general sense, however, the changes seen on the sonar record as schools ascend, suggests that the water volume attributable to any recorded group for the purpose of estimating abundance over a broad area will be a function of its vertical distribution as well as of the temperature profile of the water. Obviously many more combined sounder-sonar records should be obtained for study to establish the effect of target depth on the nature of the sonar record.

In addition to the problems of understanding the variable character of sonar records in relation to refraction of sonar sound emission and depth of schools, there are the problems of counting criteria and identification of targets. When schools are close to the vessel they produce strong but small echoes which are not always distinguishable from random background noise. As distance from the vessel increases echoes become larger and eventually weaker, so that at the outer margin of any registered group there are some considerable number of faint traces that merge into the background. To deal with the near-vessel problem when the sonar transducer is fixed at right angles to the course of the vessel we have arbitrarily adopted the criterion that only traces composed of at least three successive echoes are valid targets. No criteria have yet been established for dealing with uncertainties at the outer margins of recorded groups. Overcoming these and other counting difficulties is of little consequence where the objective is only to delineate general distribution, but would be quite important, where the numbers counted are to be the basis for abundance estimates.

Identification of targets will be necessary, of course, whether the purpose is to define distribution or to estimate abundance. Prospects of achieving identification from sonar or sounder traces themselves where the schooling species most commonly encountered are of similar size are not promising, and we have concluded that sampling by midwater trawl will be the most reliable if not the simplest procedure. For this purpose we have assembled and tested a small midwater trawl. Though not yet used on a survey cruise, the device tows satisfactorily and is easily set and retrieved.

Genetic studies

The subpopulations project is investigating the population structure of the northern anchovy off California and Baja California, Mexico, through genetic studies and tagging. In the genetic studies we are looking into blood-typing and electrophoresis of various tissue proteins; the tagging work is reported on in the following section.

In order to blood-type anchovies it is necessary to develop the specific typing reagents. Experiments have been conducted with several species of fish as sources of these reagents by immunizing them with a series of injections of washed anchovy red blood cells. Four or five species proved to be adequate producers of immune sera but the best was the ocean whitefish, *Caulolatilus princeps,* because it was quite readily available, easy to keep in captivity, withstood frequent anesthetization and handling, produced a relatively large volume of blood per unit of body weight, and most important—produced quite high titer immune sera. There was however some variability between individuals; for instance, the average titer from the weakest individual was 1/512 and the strongest was 1/32,768. A paper is being prepared on the results of these experiments.

Electrophoretic analysis of the proteins from various anchovy tissues is also being tried as a means of determining the genetic make-up of our anchovy population. The patterns produced in polyacrylamide gel from the soluble proteins of the eye lenses showed no differences in anchovies sampled from southern Baja California to San Francisco.

The lactate dehydrogenase isoenzymes were investigated by electrophoretic separation of starch gel but no polymorphism was found among local individual anchovies in the LDH of the three tissues looked at. Anchovy serum contains a single isoenzyme band, muscle has two bands, and eye lens homogenate has three. Additional studies will be made of LDH of other tissues of local anchovies as well as anchovies from other areas.

The proteins which show the greatest promise now for genetic analysis of the anchovy population structure are the transferrins. Transferrins are a specific group of iron-carrying proteins found in blood serum. Genetically controlled polymorphic types of transferrins have been found in fish, reptiles, mammals, and birds. Anchovy transferrins labeled with radioactive iron and electrophoresed on starch gel show six phenotypes which appear to be controlled by a three allele genetic system. The frequency of occurrence of these genes in the anchovies from various areas is being investigated.—*A. R. Longhurst*

REVIEW OF THE PELAGIC WET FISHERIES DURING THE 1966–67 SEASON

One bright feature of the pelagic wet fishery was the developing anchovy reduction fishery in which landings were more than twice the 1965–1966 season. While the value of the calendar 1967 anchovy landings was the greatest since 1956, there was a decline in both landings and value of Pacific mackerel and sardines and a decline in landings only of jack mackerel (Tables 1 and 2). Value of pelagic wet fish to the fleet remained about the same as for the past two seasons.

The activation of retired vessels for the reduction fishery increased the California wet fish fleet to 58 vessels, 4 more than during 1965–66: 25 large purse seiners (60 feet and over), 13 small purse seiners, and 20 lampara vessels. During the season 1 large and 2 small purse seiners fished in central California and 15 lampara boats fished in Monterey Bay. The southern California fleet consisted of 24 large and 11 small purse seiners and 5 lampara vessels. Four of the Southern California vessels occasionally fished in central California.

Sardine (June–May)

On June 6, 1967 the Governor of California signed an emergency bill declaring a two year moratorium on sardine fishing. The law, which allows a 15% tolerance by weight in any load of mixed fish, took effect the following day.

A total of 344 short tons of sardines were landed during the 1966–67 season, the poorest catch in the history of the fishery (Table 2). Landings for central and southern California were 23 and 321 tons respectively. Only 11 catches were pure sardines. Fishermen received from $200 to $400 per ton at fresh fish markets and from $70 to $75 per ton at canneries; most sardines (about 90%) were used for dead bait. Primary areas of catch were Point Sur, inshore between Point Vicente and Newport Beach, Santa Catalina Island, and La Jolla. The catch was composed almost entirely of large, old fish.

Anchovy

The California Fish and Game Commission again authorized a quota of 75,000 tons of anchovies for reduction to be taken from October 1, 1966 through April 30, 1967. Fishing began October 4, 1966 in Monterey Bay. The San Pedro purse seine fleet did not enter the fishery until December 19, 1966. The reduction season closed April 30 with a catch of 37,615 tons. Canning, reduction and fresh fish market landings for calendar 1966 and calendar 1967 were 31,140 tons and 34,000 tons respectively (Table 3).

Primary catch areas for the 1966–67 season were Monterey Bay, Anacapa Island, and the San Pedro Channel. Fishermen received $20 per ton.

In central California three-year-old fish were dominant in both 1965–66 and 1966–67 reduction fisheries. In southern California two-year-old fish were dominant during both seasons.

Los Angeles-Long Beach Harbor provided almost 50% of the live bait caught during 1966 and 1967. In the 1965–66 and 1966–67 seasons one-year-old fish (1964 and 1965 year-classes respectively) contributed about 50% of the catch. South of San Pedro fish of the year (less than one year old) were the second most important year-class while north of San Pedro they were of lesser importance. The estimated live bait catches for 1966 and 1967 were 6,773 tons and 7,760 tons respectively (Table 4).

Mackerel (May–April)

Pacific mackerel landings continued to decline and only 2,038 tons were landed during the 1966–67 season (Table 5). Most of the catch consisted of fish more than four years old. Fishermen received from $70 to $75 per ton for both Pacific and jack mackerel. The primary catch areas for both species were Cortes and Tanner Banks, and San Clemente and Santa Catalina Islands.

The 22,879 ton jack mackerel catch represented a decline of about 50% from the previous season. Fish less than three years old comprised the bulk of the catch. *James E. Hardwick, California Department of Fish and Game.*

TABLE 1

LANDINGS OF PELAGIC WET FISHES IN CALIFORNIA IN SHORT TONS, 1963–1967

Year	Sardines	Anchovies	Pacific Mackerel	Jack Mackerel	Herring	Squid	Total
1963	3,566	2,285	20,121	47,721	315	5,780	79,788
1964	6,569	2,488	13,414	44,846	175	8,217	75,709
1965	962	2,866	3,525	33,333	258	9,310	50,254
1966	439	31,140	2,315	20,431	121	9,513	63,959
1967*	74	34,000	583	19,046	135	9,800	63,676

* Preliminary.

TABLE 2

VALUE TO FISHERMEN OF WET FISH LANDINGS

Year	Anchovy	Jack Mackerel	Pacific Mackerel	Sardine	Total
1960	$73,816	$1,582,173	$757,443	$1,185,813	$3,599,245
1961	110,083	2,029,236	954,495	1,145,990	4,239,804
1962	50,050	1,868,948	1,026,963	490,329	3,436,290
1963	77,585	1,989,146	861,316	298,879	3,226,926
1964	82,061	2,109,269	666,435	500,149	3,357,914
1965	98,799	1,829,432	223,842	125,530	2,277,603
1966	643,925	1,424,327	188,510	152,051	2,408,813
1967*	703,120	1,428,450	87,450	29,600	2,254,320

* Preliminary.

TABLE 3

SARDINE CATCH IN TONS, 1963–64 THROUGH 1966–67
(Period June Through the Following May)

Year	California	Baja California	Total	Year	California	Baja California	Total
1963–64	2,942	18,384	21,326	1965–66	729	22,252	22,981
1964–65	6,103	27,120	33,223	1966–67*	344	19,156	19,500

* Preliminary.

TABLE 4

COMMERCIAL LANDINGS AND LIVE BAIT CATCH OF ANCHOVIES IN SHORT TONS, 1963–1967

Year	Commercial landings	Live-bait	Total	Year	Commercial landings	Live-bait	Total
1963	2,285	4,442	6,727	1966	31,140	6,773	37,913
1964	2,488	5,191	7,679	1967*	34,000	7,760	41,760
1965	2,866	6,148	9,014				

* Preliminary.

TABLE 5

JACK AND PACIFIC MACKEREL CATCH IN SHORT TONS, 1963–64 THROUGH 1966–67
(Period May through April)

Year	Jack Mackerel	Pacific Mackerel	Year	Jack Mackerel	Pacific Mackerel
1963–64	42,038	17,105	1965–66	33,831	3,794
1964–65	39,548	12,437	1966–67*	22,879	2,038

* Preliminary.

PREFACE

These thoughts on wide-scale studies of the ocean were presented in a manner as informal as possible before such a large group. The speakers used rough notes in some cases and finally edited manuscripts in others. The style of the presentations will be seen to vary. This variation reflects both the individual style of the speaker and the nature of the topic he was asked to discuss: some topics lent themselves to a precise discussion of data while others could be approached only through the necessarily coarse appraisals of future capabilities and future support.

Unfortunately it was not possible to record completely the discussions that followed. Rapid interchanges defied the traveling microphone as well as the scribbling chairman.

I regret that Lee Alverson has been unable, for various reasons, to prepare his paper "How an Ocean-Wide Survey of Adult Fish Might Be Carried Out, and What Might Be Gained From It" for publication at this time. It was presented as the sixth paper of the Symposium and would have been a valuable contribution.

On behalf of the participants in the Symposium I wish to thank those who made the Symposium possible:—to Dr. William A. Nierenberg, Director, and to the Scripps Institution of Oceanography for their sponsorship and their arrangements; to Mr. Julian G. Burnette, Chairman, and to the Marine Research Committee of the State of California, for their support and encouragement of the purposes of the Symposium; to the secretary and recorder, Mrs. Lorayne Buck and Miss Barbara Edwards and to Mrs. Ruth Ebey for her valuable assistance to the editor in preparing these proceedings.

Joseph L. Reid
Editor

INTRODUCTORY STATEMENT

JOSEPH L. REID
Scripps Institution of Oceanography
La Jolla, California

The last few years have produced many new ideas and discoveries in oceanography and many new techniques and instruments for studying the ocean.

On the other hand, many of the discoveries may turn out to be discoveries of new problems, and many of the instruments and techniques, which were developed to solve older problems by making more precise and continuous measurements, have finished by revealing such complexity that we have to reformulate the original problem and start anew.

A classic example of an instrument that works beyond reproach, and measures just what we asked for is the GEK—it does indeed measure the surface velocity in the ocean remarkably well. Yet when we see the results we realize that for many, and perhaps most, purposes, this isn't what we wanted at all. The results show such complexity and such rapid fluctuation in both time and space, that a single observation, no matter how accurate, can tell us only what the average velocity was in one place during the 10 minutes required to make the observation. If we want the average flow during a day, or a week, or a year, we can get this from the GEK, but we have to continue the measurements for a day, a week, or a year to get these averages. This is not a practical instrument, then, for measuring the large-scale circulation of the ocean very economically, though it is extremely good for other purposes.

If we are to get at large-scale phenomena we must use instruments and methods which will provide data over large areas and over long periods, and we must consider very carefully whether the ideas and tools at hand can be used economically for the study of these phenomena.

At a recent meeting, the Scripps staff reported its immediate plans for research and I have chosen two or three examples of things mentioned there which bear upon or could be brought to bear upon large-scale phenomena.

One of the proposed tools was a modification of the Hardy plankton sampler to be towed vertically to reveal with one tow the stratification of various plankton. If the results constitute a usable integration in space so that some immediate quantification can be made, this may well be useful for large-area studies. On the other hand the results may show such a complexity in both time and space that the instrument can be used only to study this complexity.

Likewise, continuous records of nutrients and chlorophyll can be made. When a reasonable amount of data are at hand, we will have some notion of how close in space and time we need to measure these properties in order to quantify and study them, and how one ought to plan large-area studies of productivity in the ocean.

Quite a bit of development has been done in instruments to measure at abyssal depth the time variations of such properties as temperature, pressure and flow. Curiously, the first results of deep current measurements suggest that there is a reasonably large-area coherence of the deeper flow, and that perhaps we don't need 10,000 deep current meters each operating for 10 years to make a useful estimate of the deep circulation, but that a few dozen operating for a few months may reveal the major circulation. This is almost certain to be true for tides, of course, since they do have a very large-scale coherence.

For studies of longer-period fluctuations there are the sediments, which record in a more or less obscure form many aspects of the past environment. In areas of rapidly depositing and seasonally undisturbed sediments it may be possible to establish a great deal about the very recent past—perhaps details of fluctuations of populations and hence environmental conditions during the last 1500 years. This is especially exciting to physical oceanographers and to meteorologists. Most of us believe we could do something useful with a 1500-year record, since the fluctuations would probably be within a range we could compare with the last 30 years or so. We are not so sure how to use bits of information from 50 or 100 million years ago, when the ocean itself might have been very much different, and our present notions not at all applicable.

Of course, the CalCOFI program has itself contributed a great deal that can be used in planning the study of ocean-wide variations of many of the important items. Fish eggs and larvae, young and adult fish and general zooplankton studies in the California Current ought to give some useful time- and space-scale information about how to study the biology of the wider areas. The California Current temperature and flow measurements cover quite a large area and some 15 years of time, and during the last 10 years have been augmented with the eastern Pacific monthly surface temperature maps. These have been among the first series of reasonably large-scale synoptic data for the ocean and have led to some ideas about the relation between ocean and atmosphere. The Rancho Santa Fe symposium in 1958 (published as vol. 7 of these Reports in 1960) was called to describe and discuss one of the remarkable fluctuations in ocean temperature and populations and to try to relate this to the atmosphere.

The papers presented there and many others written since have dealt with large-scale matters in many fields, including trans-oceanic migration of fishes, zoogeography of the entire Pacific, and general studies of the ocean circulation of the Pacific and its relation to the atmosphere as only a few examples.

For this reason it has seemed appropriate to convene a symposium on wide-scale studies of the ocean at this CalCOFI meeting, and to invite these distinguished people to tell us about the newest developments and plans, or indeed to suggest a few plans, about how we might approach the broad problems in their fields in useful ways.

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LONG-RANGE FORECASTING OF THE ATMOSPHERE AND ITS OCEANIC BOUNDARY—AN INTERDISCIPLINARY PROBLEM

by
JEROME NAMIAS
U.S. Weather Bureau, ESSA

INTRODUCTION

The physical sciences of the atmosphere and of the ocean are each extremely complex; the physical sciences of the combined atmosphere-ocean system is more than twice as complex. It is becoming increasingly apparent, however, that we cannot separate these two systems because of their continual interaction, seemingly on all time and space scales.

The oceanographic and air-sea interaction problems thus far treated at great length and with great care have involved phenomena of two principal space and time scales:

1. Microscale and short-period phenomena, which often occupy the efforts of an oceanographic cruise lasting a few weeks; and
2. The large-scale fields of some oceanographic element or perhaps ocean-atmosphere exchange parameter averaged by tens of years to obtain climatological averages.

Indeed, studies such as these have laid the foundation of modern oceanography and will continue to be of major importance in its development. Between these two ends of the spectrum there is a large range of scales of motion and interaction which have up to now received comparatively little attention, and whose discovery, description, and explanation await systematic research.

The importance of attacking these intermediate-scale problems arises not only intrinsically, but especially because many other scales of activity, large and small, short- and long-lasting, may be influenced—or in Stommel's (1963) words—"contaminated," so that their neglect can render ineffective otherwise well-designed observational programs.

Meteorology offers a good analogy. Not so long ago (some thirty years), many meteorologists thought that the largest-scale systems with which they need be concerned for prediction purposes were the extratropical cyclones and anticyclones—until the abundance of surface and upper-air data over the Northern Hemisphere illuminated the fact that these elements were often responsive to planetary waves 3 to 4 times their size. Moreover, these planetary waves in turn were found to be interactive not only with the cyclones but with each other and thus with the entire hemisphere's general circulation. Some work currently proceeding in extended forecasting suggests that even crossequatorial interactions are likely.

I shall describe in this paper a few suggestive studies whose results point to the need for greatly expanded work in both oceanography and meteorology in quest of knowledge of ocean-atmosphere interactions on a hemispheric space scale and on time scales of months, seasons, and years. It is only when solutions to such problems are obtained that meteorologists and oceanographers will be able to aim at validating and applying their research by making more accurate predictions of events on these scales.

THE EFFECT OF ANOMALOUS ATMOSPHERIC SYSTEMS ON SEA-SURFACE TEMPERATURES

In view of the heterogeneous nature of this audience, I shall first describe—as background—the relationship frequently found on daily weather maps between cyclones and anticyclones (and their associated fronts) and the larger scale pressure and wind patterns. Figure 1 shows a schematic diagram on which the cyclones are represented as waves along the Polar front, moving from southwest to northeast and developing and occluding as they progress. These waves are embedded in a broad low pressure trough between two extensive high-pressure areas—one to the northwest composed of cold air masses and the other to the southeast composed of warm air masses. Associated with this group of short wave disturbances is a long (or "planetary") wave in the westerly winds aloft which has a length of the order of four times that of the cyclone waves. There may be about 4 to 6 of these waves present around the hemisphere on any one day's map for the 500-mb level. The long waves and the cyclone waves provide the atmosphere's mechanism for exchange of heat, momentum, and water vapor between the tropics and polar regions. Long waves provide a means for deploying cold and warm air masses into the temperate latitudes where cyclones and anticyclones are generated. It must be constantly borne in mind that both systems, the long waves and the short waves (the cyclones) are interactive so that they cannot be treated independently.

Even if one averages the upper-level or sea-level pressure and wind distributions over periods of weeks, months, or seasons, the long waves do not disappear. They assume positions and amplitudes which vary from one period to another, and thus the prevailing air masses, storm tracks, and associated weather conditions may be inferred from the mean configuration of the long waves. We stress the latter point because most of the charts shown in this article refer to the observed mean upper-level flow patterns.

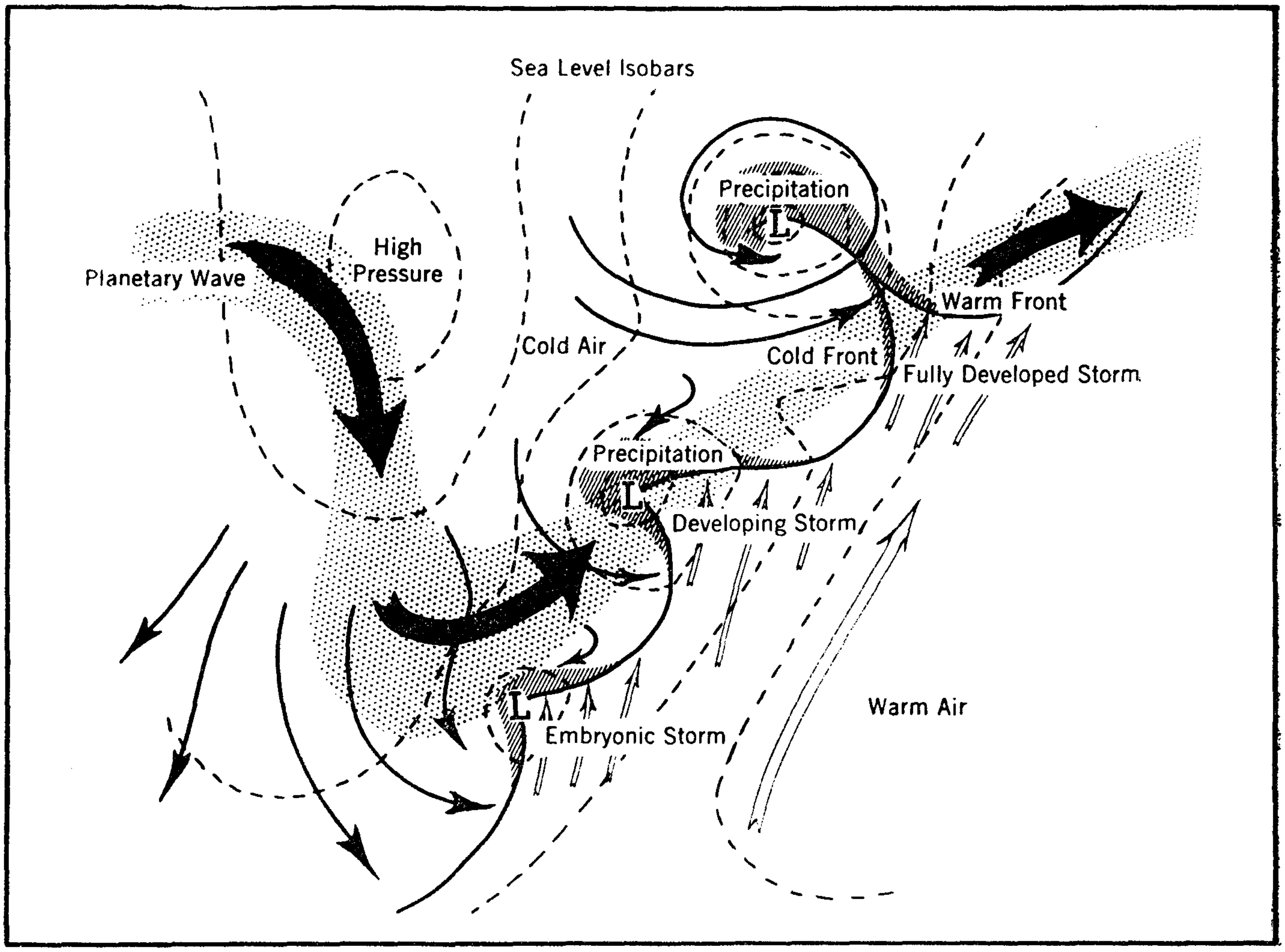

Figure 1. Schematic illustration of the relationship between planetary waves in the middle and upper troposphere and air masses, fronts, cyclones, and anticyclones customarily seen on daily weather maps. Note that the wavelength of the planetary wave is several times the size of cyclone waves along the surface polar front.

The mean pressure distribution—let us say at sea level—gives a very good approximation of the resultant wind streams for the period. Furthermore, the mean wind can be considered as being composed of a normal component and an anomalous component. The latter component may be determined by subtracting from the observed mean pressure distribution the long-term normal, thereby obtaining a field of isopleths of anomaly. Such a map for July 1958 is shown in the upper left corner of Figure 2. For the shaded area the frequency of daily observed and normal wind directions during June and July 1958 is shown in the upper right-hand part of the figure. It is clear that whereas the normal prevailing wind over the shaded area is from the west, in July 1958 it was mostly from the south and southwest, in agreement with inferences from the isopleths of anomaly. These anomalous components of the wind represent an anomalous drag on the surface water and force an anomalous Ekman drift. The southerly components in July 1958 also imply warm air transport and less than normal latent and sensible heat transfer from the underlying water. It is not surprising, therefore, to see that in the observed sea-surface temperature distributions, warmer-than-normal surface water is found in the eastern part of the Gulf of Alaska and colder-than-normal water in the western. The colder-than-normal water must also be associated in part with the upwelling near the center of the negative anomaly, where strong horizontal divergence of surface water takes place—as pointed out by Bjerknes (1962). The preceding concepts have been incorporated into a methodology for estimating numerically what the sea-surface temperature distribution is likely to be with a given mean pressure distribution whose effects are superimposed upon an initially abnormal sea-surface temperature pattern. This method has been described by the author (Namias, 1965). An example of such a computation for July 1958 is given in the lower lefthand portion of Figure 2 and for October 1963 in Figs. 4 and 5. It is interesting to note that the large anomalous drop in sea-surface temperatures in the area south of the Gulf of Alaska between September 1963 (Fig. 3) and October 1963

(Fig. 5) was associated with a very strong development of Gulf of Alaska cyclones. This can be seen from Figure 6 where the pressure distribution for October and the change from September are shown. From this figure it is clear that cooling of the surface water was induced (a) by greatly increased losses of latent and sensible heat from the surface water as windy Arctic air masses entered the Gulf from Alaska and the Bering Sea; (b) by especially pronounced vertical destabilization and stirring of the lower layers of the atmosphere, which removed heat and water vapor rapidly aloft; (c) by strong upwelling and water mixing produced by the Gulf of Alaska cyclone. James Johnson (pers. comm.) of the Bureau of Commercial Fisheries has made some calculations for this case and finds that about two-

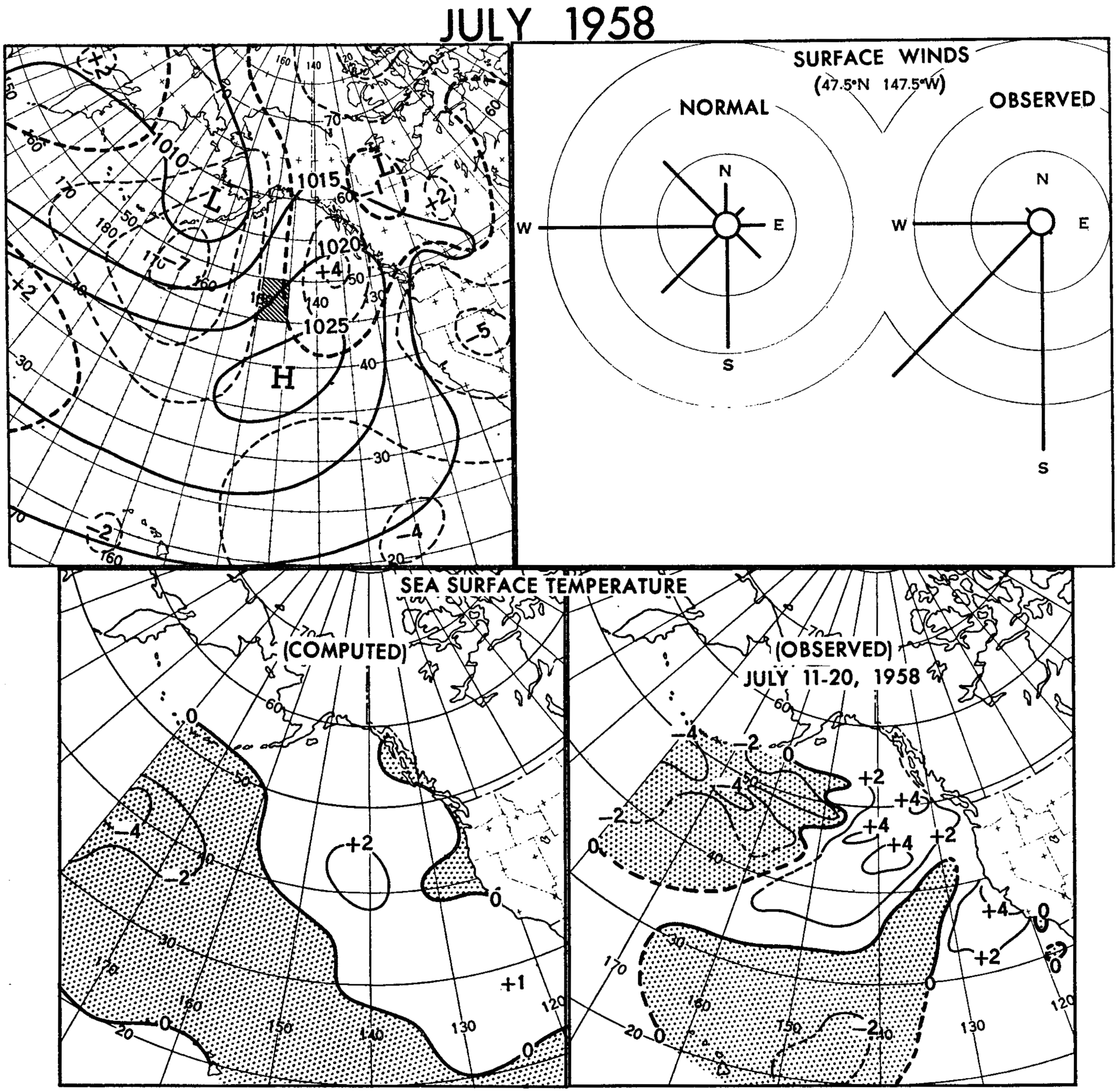

Figure 2. *Upper left:* Sea-level mean isobars (solid) and isopleths of departure from normal (dashed) for July 1958.
Upper right: Normal surface wind rose for July and that for July 1958 as computed from surface weather map analyses for the hatched 5° square shown in chart in upper left. Length of bars indicates relative frequency of winds for indicated direction.
Lower left: Computed sea-surface temperature anomalies using advection in the Ekman layer for July 1958 (°F).
Lower right: Observed sea-surface temperature anomalies for July 11–20, 1958 (°F).

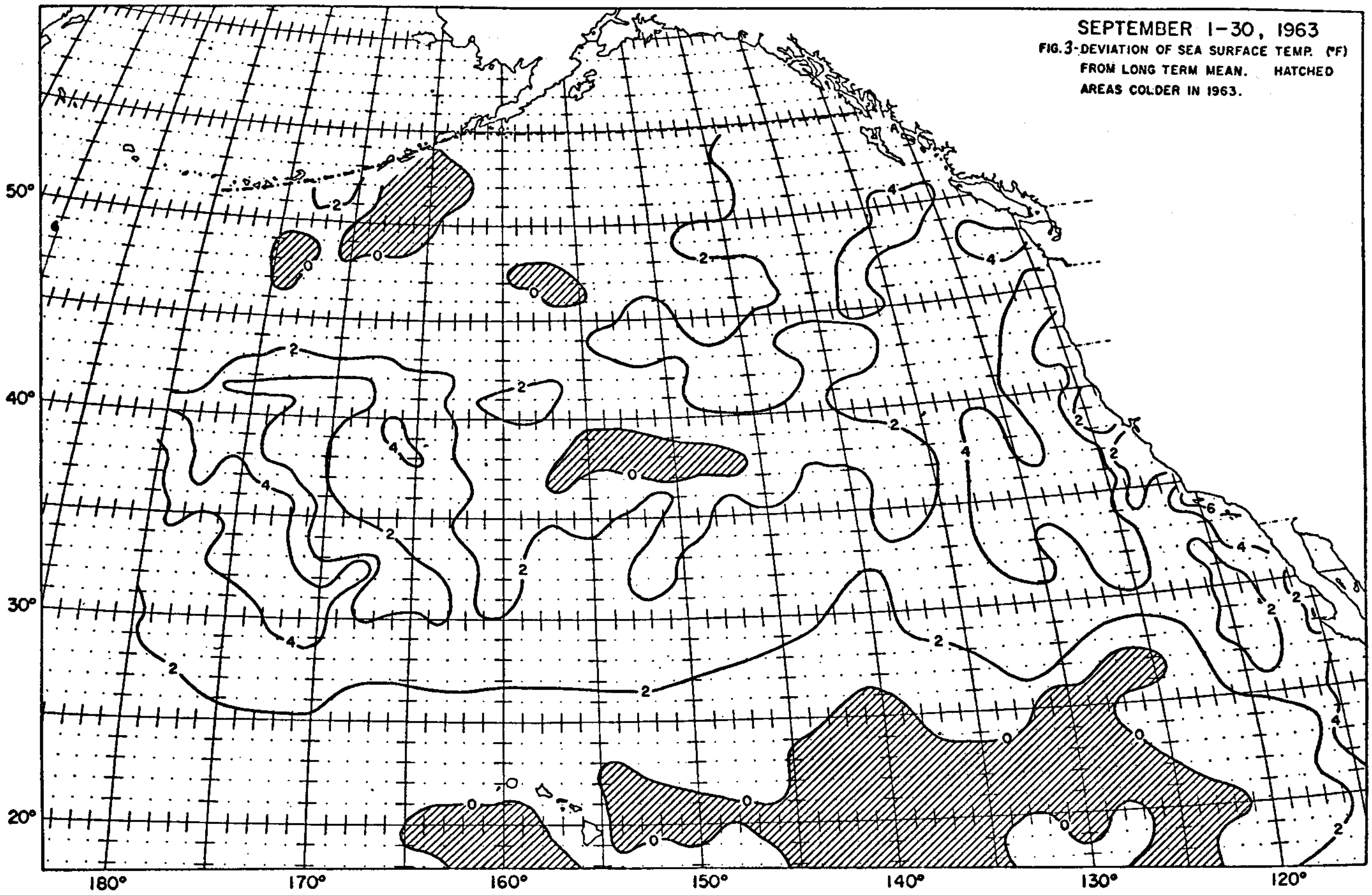

Figure 3. September 1963 departure from normal of sea-surface temperature (°F) from long-term mean. Hatched areas colder-than-normal (from Bureau of Commercial Fisheries).

thirds of the cooling was due to the increased latent and sensible heat exchange, while about one-third was due to upwelling and mixing.

The continuation and extension of abnormal warmth to the west is associated with the positive change in pressure distribution found there and particularly with the suggestion of horizontally converging surface water and resultant lack of upwelling.

Of course, it is desirable to be able to *predict* the sea-surface temperature distribution a month or so ahead. With the help of the concepts described above some first approximations are now being made by using *predicted* mean sea-level pressure distributions. Naturally, these sea-surface temperature predictions leave much to be desired because their accuracy depends not only upon the accuracy of the atmospheric pressure prediction, but also upon the complex physical problems associated with air-sea interaction and the response time of water masses to wind drag and other factors. The first trial estimate of sea-surface temperatures, made for March 1965, is given in Figure 7, together with the corresponding observed state. Considering the fact that the initial sea-surface temperature data were the means for February 1965 (although atmospheric data up to March 14 were used), this prediction was reasonably successful. Tests are proceeding on additional cases and attempts are being made to improve the method by incorporation of terms not previously considered. Professor Robert Arthur has been especially helpful in making suggestions along these lines.

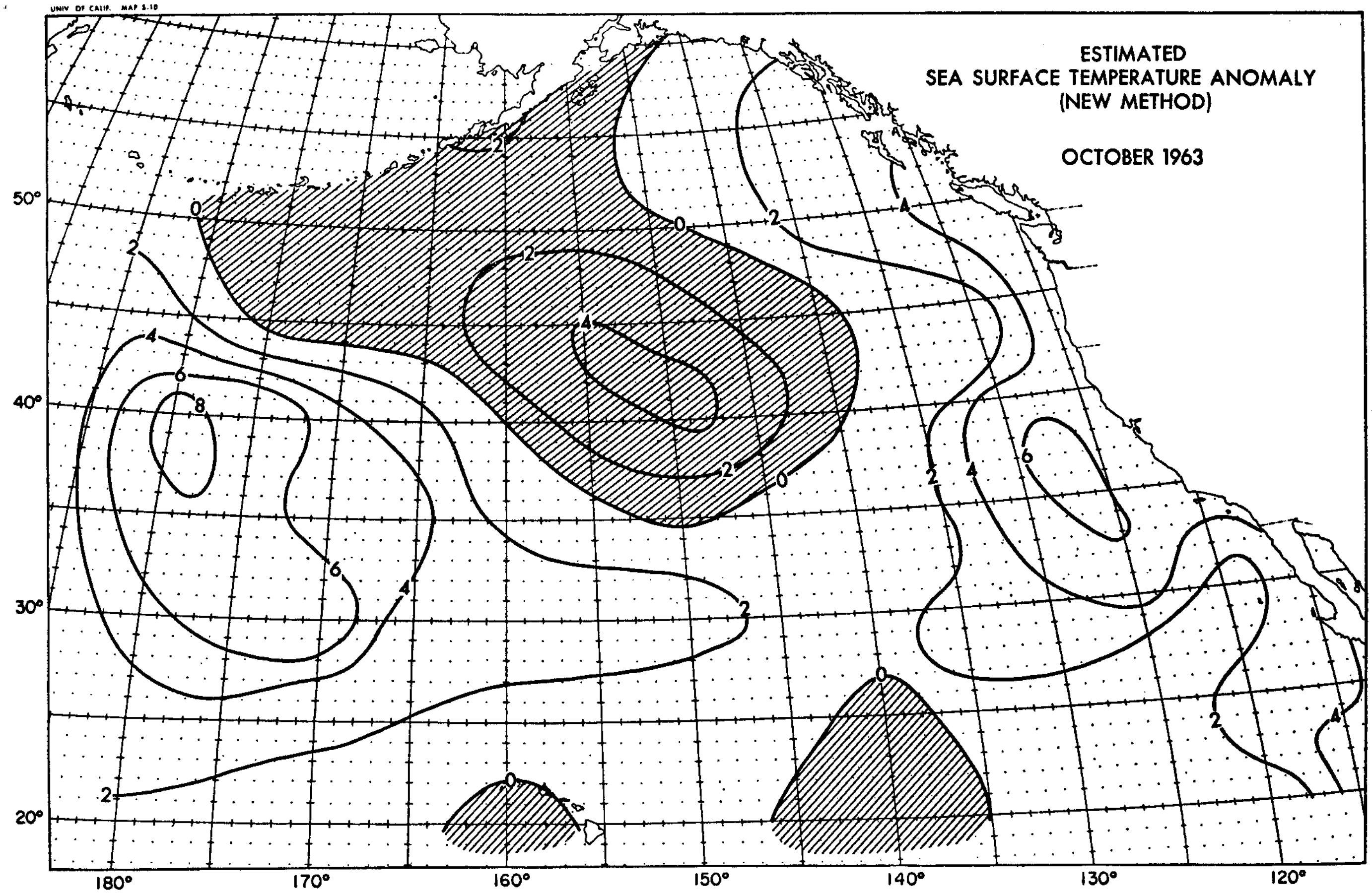

Figure 4. Estimated sea-surface temperature departures (°F) for October 1963 (see text).

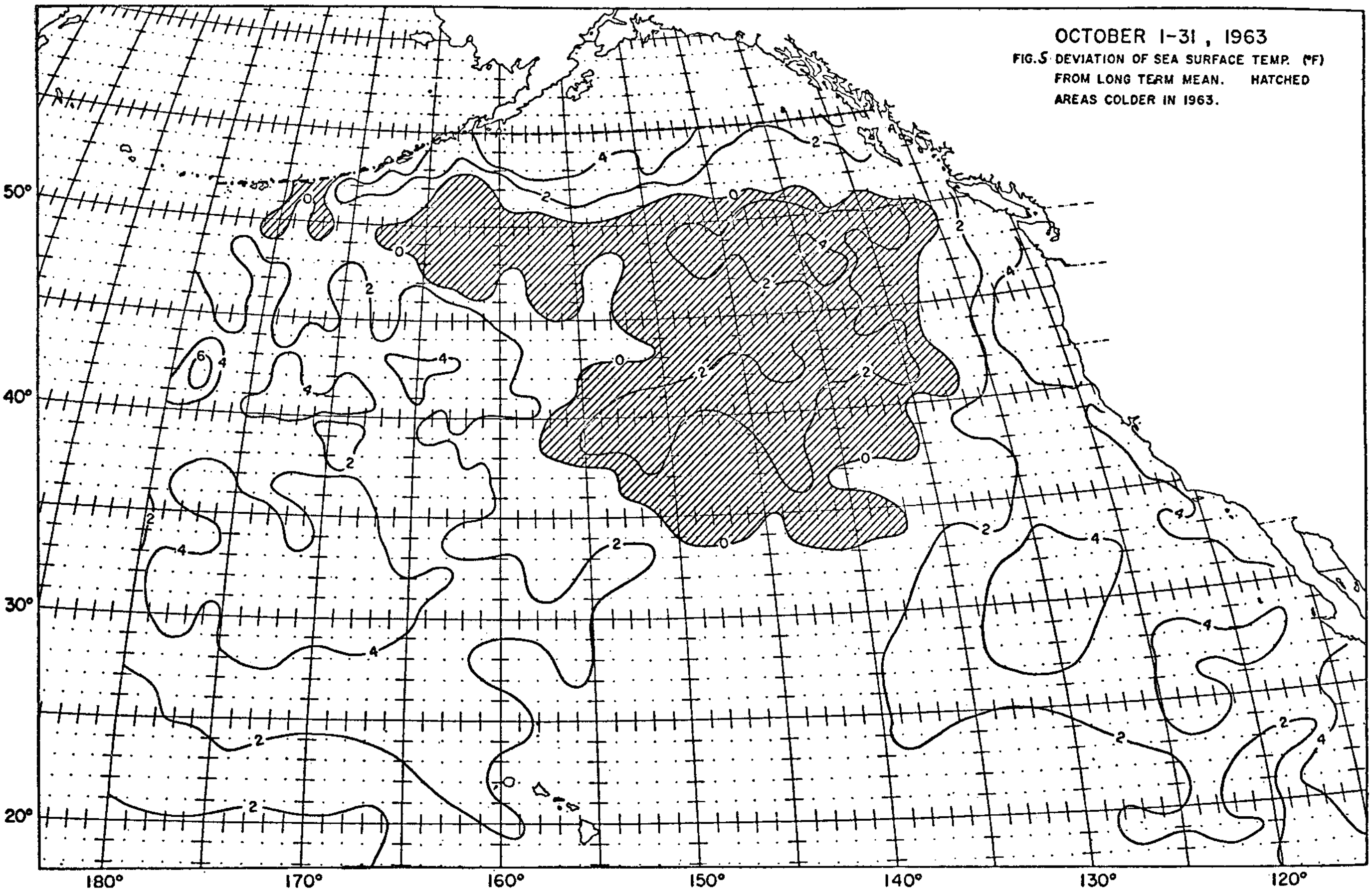

Figure 5. October 1963 departure from normal of sea-surface temperature (°F) from long-term mean hatched areas greater-than-normal (from Bureau of Commercial Fisheries).

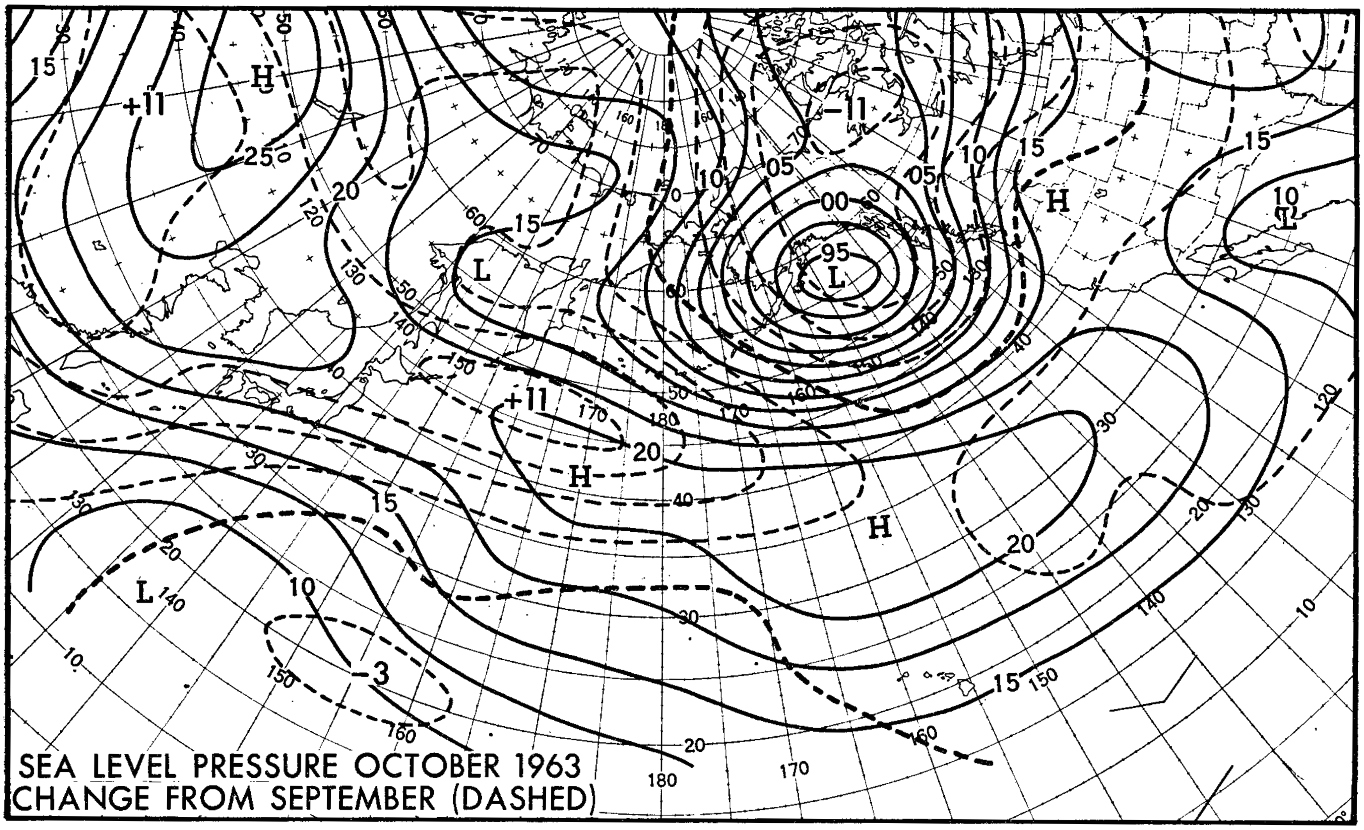

Figure 6. Mean sea-level isobars for October 1963 (solid) and changes in sea-level pressure from September 1963 to October 1963 (changes indicated by broken lines drawn for every two mbs pressure change with centers of maximum change indicated numerically).

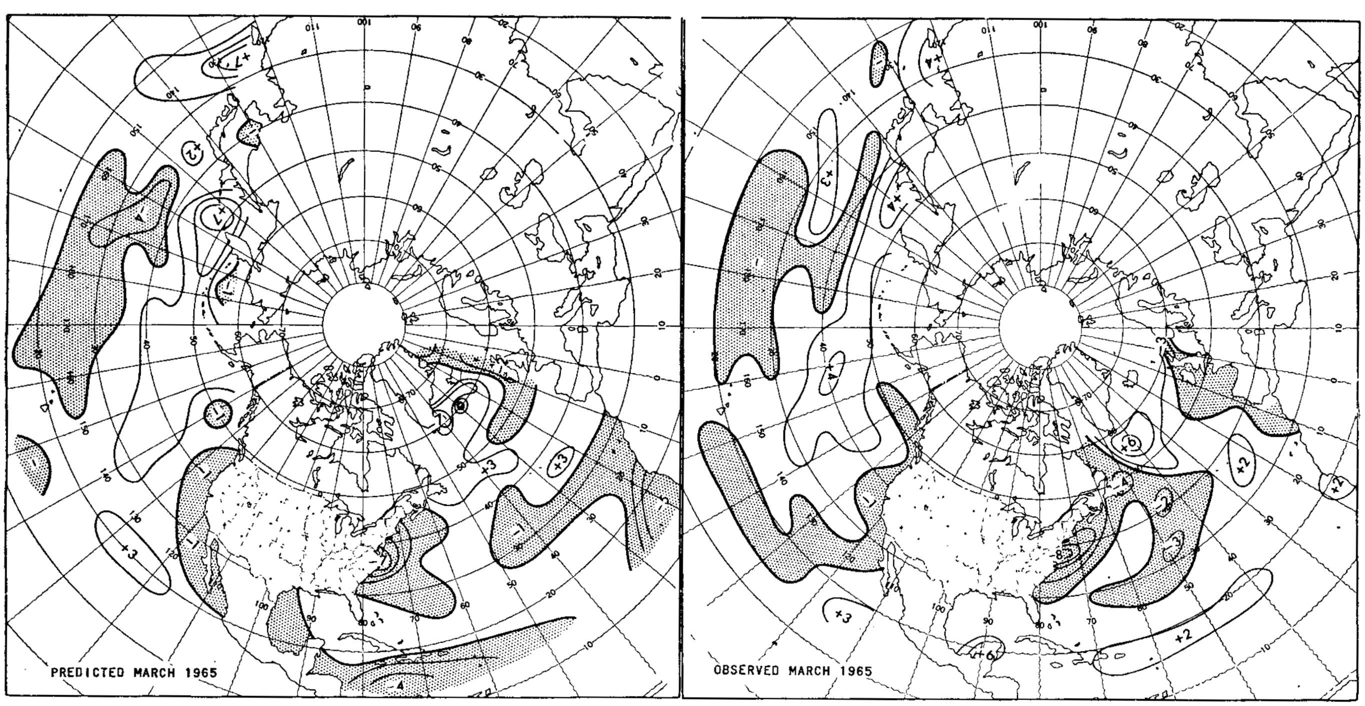

Figure 7. *A.* Predicted sea-surface temperature anomaly (°F) for March 1965. *B.* Observed sea-surface temperature anomaly (°F) March 1965.

THE COUPLED AIR-SEA SYSTEM VIEWED ON A MONTHLY AND SEASONAL SCALE

In an earlier paper (Namias, 1965) I described a slowly migrating upper-level trough which came to light on seasonal mean charts and which could be followed as it moved eastward from the west-central Pacific in summer to near the West Coast in the subsequent spring. A theory was proposed to explain how this slow motion may have come about as a result of climatological effects and wide-scale air-sea interaction. Since 1958 other cases of eastward motion of Pacific troughs from summer to fall and winter have been observed on mean maps. We shall treat below one of the most spectacular of these, which led to the unprecedented rainy period in southern California in November and December of this year, as well as to a protracted heat wave there in October and successive cool spells in September.

The upper-level charts for each month from July through November 1965 are reproduced in Figures 8-12. Superimposed upon the mean contours (solid lines) are the contemporaneous mean sea-surface tem-

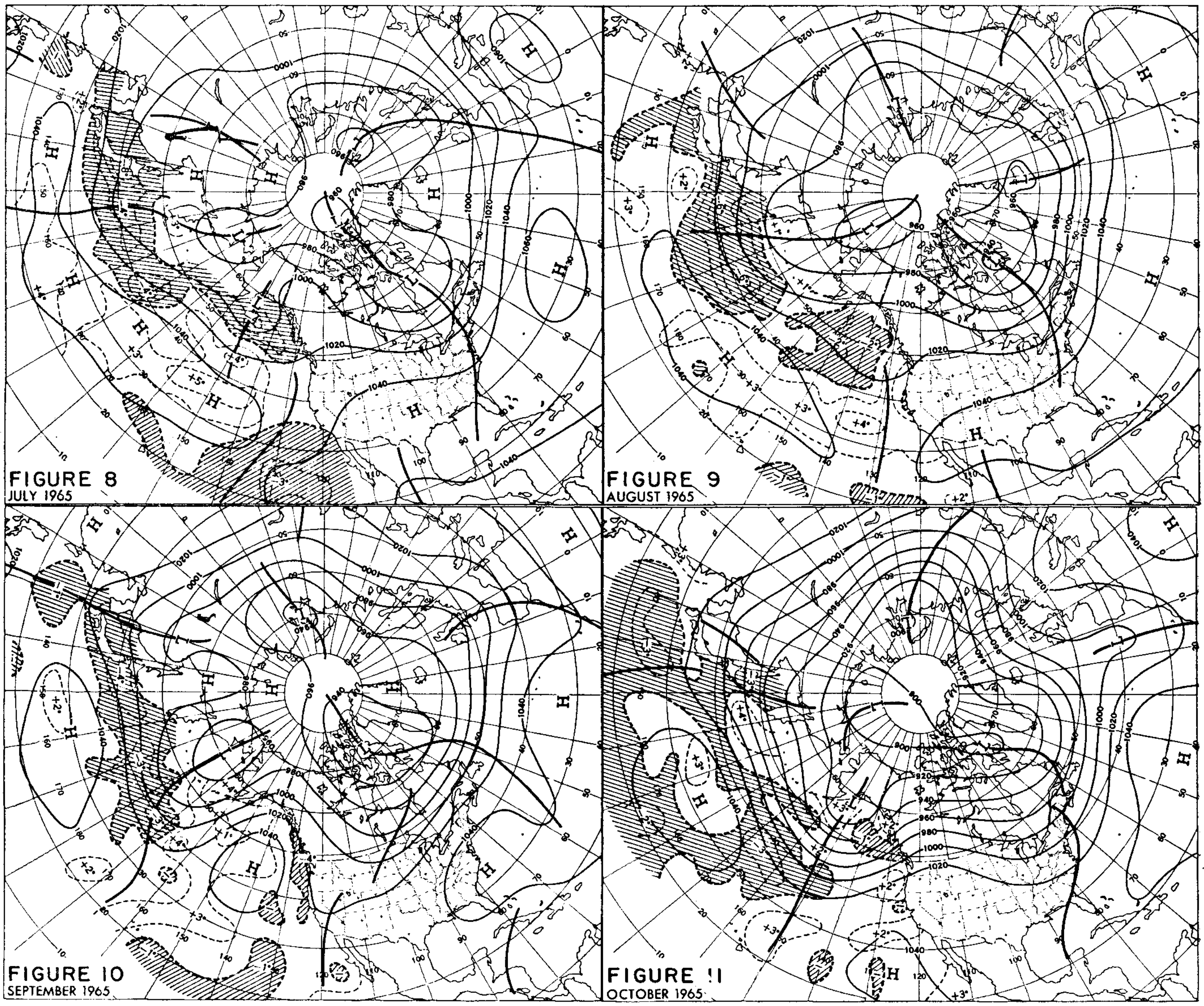

Figure 8. Mean 700-mb contours for July 1965. Heavy lines running roughly North-South are major trough positions. Broken lines are isopleths of sea-surface temperature departure from normal drawn for each 2°F.

Figure 9. Same as figure 8 but for August 1965.

Figure 10. Same as figure 8 but for September 1965.

Figure 11. Same as figure 8 but for October 1965.

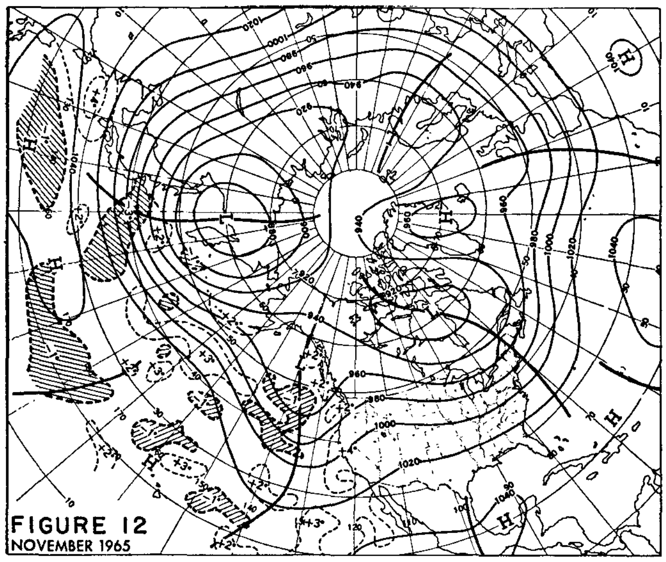

Figure 12. Same as figure 8 but for November 1965.

perature deviations from normal.* At first let us consider only the evolution of the contour field over the North Pacific and adjacent continental areas.

In July a trough (solid heavy line) extended southward from Kamchatka, roughly along longitude 155° East. In the subsequent months up to November, this trough advanced slowly eastward so as to have arrived near the West Coast in November. In addition to advancing eastward some 70° of longitude, the trough increased in amplitude, most abruptly between August and September. Associated with the moving trough are families of cyclones (not shown) more or less like these in the schematic diagram shown in Figure 1. The cyclones, generated in and to the east of the trough, are the principal mechanisms for release of precipitation—such as that falling on southern California in November.

The question arises as to how such regular trough motion might be accounted for, and here we will briefly restate the theory proposed in the 1958 report (Namias, 1959). With seasonal progression from July into late summer, fall, and early winter, there are two climatologically probable and in fact dependable, changes in the general circulation in the vicinity of Asia and over the western Pacific. As the Asiatic monsoon anticyclone develops over the continent and the contrast between air coming from this anticyclone and air overlying the warm water strengthens, a series of cyclones of increasing frequency and intensity move along and off the Asiatic Coast and an associated upper-level trough "locks-in" somewhere in the vicinity of the Asiatic coastal region, within a narrow range of longitudes (perhaps 20 degrees). As the cyclones move northeastward and intensify they carry momentum into the prevailing westerlies, and the strength of the westerlies is thereby increased over the western Pacific. Now it is well known from the early work of Rossby (1939) that the stationary wavelength between troughs is proportional to the square root of the zonal wind velocity flowing between them. Therefore, as the Asiatic trough locked in somewhere along the Asiatic Coast in 1965 and the westerlies increased, stationary wavelength considerations demanded that the trough to its east move progressively eastward. Support for the above-described mechanism is shown in Figure 13, where

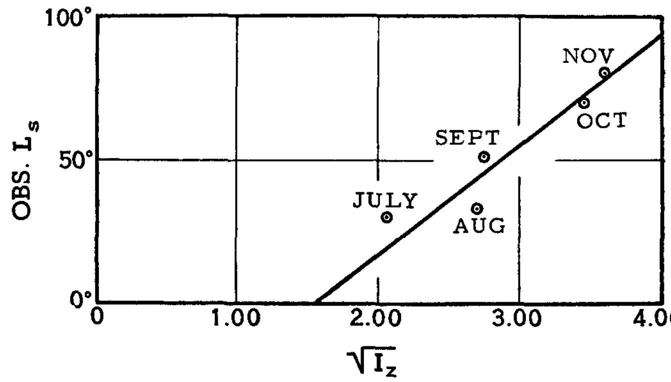

Figure 13. Relation between square root of zonal velocity between Asiatic coastal trough and next downstream trough and wavelength between troughs for months of July through November 1965. Zonal index is measured between latitudes 35° and 55°N and wavelength between troughs is measured at 45°N.

the square root of the zonal velocity between the two troughs is plotted against the observed wavelength for each month from July through November 1965. From this figure it is seen that the wavelength increase (at 45°N) was from 30° in July to about 80° in November while the zonal velocity increased from about 4 meters per second in July to 13 meters per second in November.

From Figures 8 to 12 and 13 it will be noted that while the distance between the Asiatic Coast trough and the Pacific trough increased some 50°, the latter trough actually moved 70° from the initial position south of Kamchatka in July. The additional 20 degrees was associated with eastward migration of the Asiatic coastal trough. This is also a climatologically frequent event associated with development of the Asiatic High. The eastward motion of families of cyclonic storms was sufficiently pronounced during this 5-month period that the mean Aleutian Low at sea level could be followed from off Kamchatka in August to off Vancouver in November, as indicated by the inset diagram in Figure 14.

The question next arises as to why a migrating trough of this kind possessed such longevity and why its abnormally large amplitude continued from September on. In the following discussion one must always bear in mind the fact that air-sea interactions vary with season and geographical location.

We cannot attempt to explain why a trough appeared south of Kamchatka in July without treating events both in the sea and the atmosphere leading up to July. It is conceivable, however, that the cold water off Japan and south of Kamchatka in July and

*The eastern Pacific sea-surface temperature departures have been taken from the maps of the Bureau of Commercial Fisheries, while those over the western Pacific are from the Japanese Meteorological Agency.

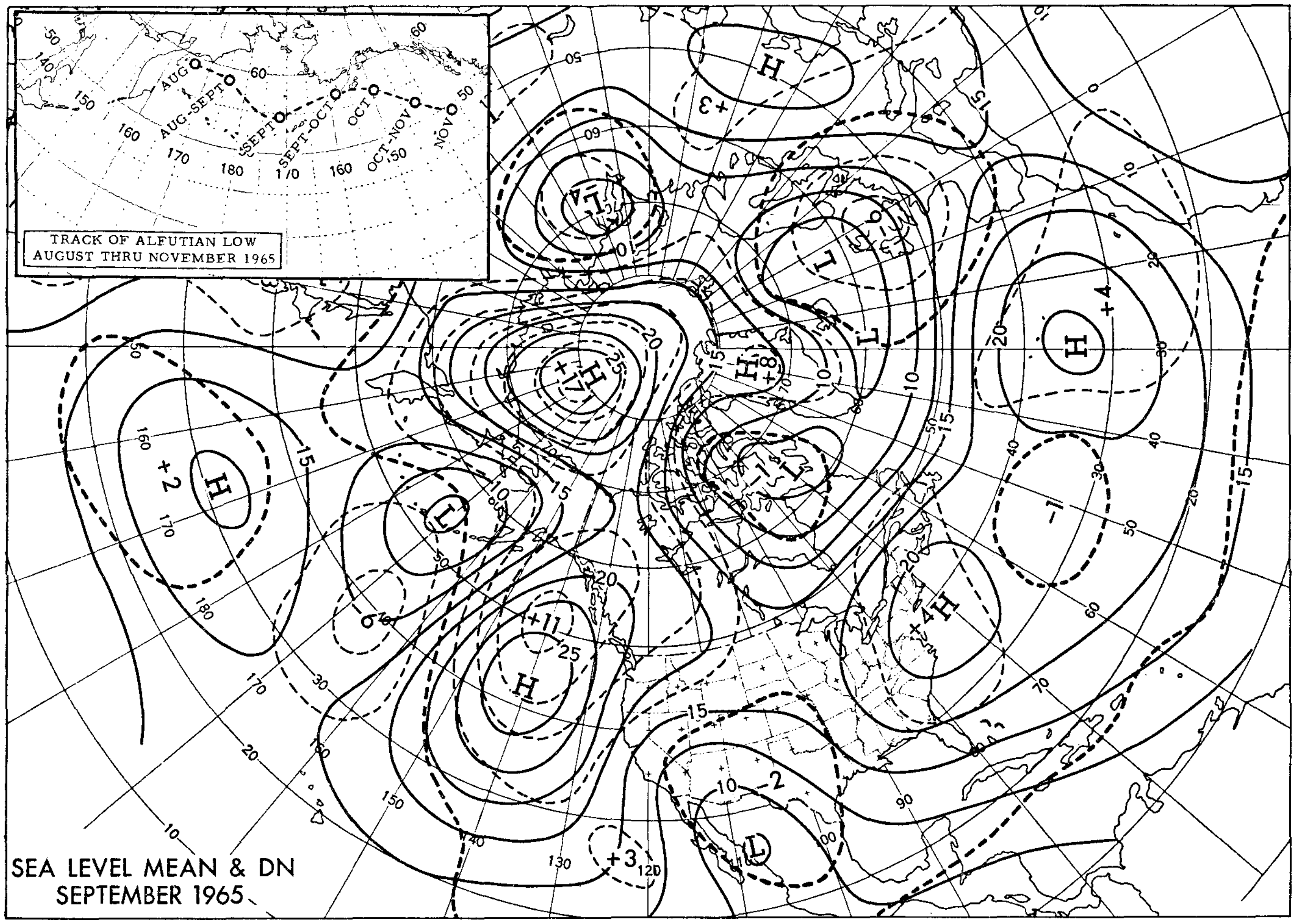

Figure 14. Mean sea-level isobars for September 1965 and isopleths of anomaly (broken) for September 1965. Numbers represent centers of maximum positive and negative departures from normal. Isopleth for zero departure is indicated as heavy broken line.
***Inset:* Track of Aleutian Low center for 30-day periods from August through November 1965.**

August (Figs. 8 and 9) had something to do with generating this trough, because during mid-summer the East coast of the Asiatic continent is appreciably warmer than the sea and colder-than-normal water would tend to create an accentuated thermal gradient in the atmosphere favoring upper-air troughs in this area. Frequent cyclones were steered into this trough area (charts not reproduced) and, because of the associated increased cloudiness, upwelling and water stirring would maintain the cold water mass. Note that in August a strong temperature contrast existed between this cold pool and warmer-than-normal water in the central Pacific. As the locus of the mean trough changed so as to appear over this area of contrasting water temperature, its intensity was sharply increased as shown by the increased cyclonic curvature of the contours and also increased amplitude. This increase was probably associated with the increased vigor of cyclonic activity introduced by contrasting air masses in the vicinity of the trough, for these air masses receive a large share of their heat from the surface water. The increase in intensity in the ridge to the east (along 140° West) from August to September (Figure 10) is largely an inertial (barotropic) response to the central Pacific trough, as is the trough over the western United States. The latter trough-ridge complex resulted in the deployment of very cold air masses into the far west in September.

Note the change of water temperature in the Gulf of Alaska from below normal in August to above normal in September. This anomalous warming appears to be a result of advection of warm water from more southerly latitudes, less sensible and latent heat extraction from the water as prevailing winds shifted and brought warmer southerly air currents over the Gulf, and less upwelling associated with the anticyclogenesis. Note also the generation of a pool of cooler-than-normal water just off the west coast with greater upwelling engendered by increased northerly flow. The above-described wind regime in September is perhaps more clearly illustrated with the help of sea-level pressure distribution and its anomalies for September as shown in Figure 14.

By October, when the trough moved eastward to 150°W, cold water was formed just to the west of the trough and the sharp change in prevailing winds off the West Coast resulted in a dramatic warming of the surface water with respect to normal (Figure 11). As the western ridge moved into the plateau, foehn winds (Santa Anas) along the West Coast led to a protracted heat wave. With further eastward motion of the trough to an area just off the West Coast in November (Figure 12), the stage was set for the heavy rains in California, which continued into the first half of December. Since most people in this audience experienced these rains and have probably read many newspaper accounts, I need not bore you with the rainfall statistics.

Thus, the abnormal weather in California, the associated appearance and disappearance of the Pacific anticyclone, and many other phenomena relating to North Pacific ocean weather in the last half of 1965 appear to have their instigation in the coupled ocean-atmosphere continent system and the forcing influence imposed by changes in the radiation balance associated with change of season.

FALL WATER TEMPERATURES AS A POSSIBLE CAUSE OF WINTER ABNORMALITIES

Because of the vast reservoir of heat which may be established in the ocean during summer and fall, it is possible for the overlying atmospheric circulation to become greatly affected in the subsequent winter. This may arise because the climatologically determined position of the prevailing westerlies and storm tracks change materially between summer and winter. Thus, over the North Pacific, the great Pacific anticyclone dominates much of the area during summer, and cyclonic activity is usually restricted to areas off Kamchatka and north of the Aleutians. In the winter, on the other hand, as the Asiatic monsoon anticylcone develops, cyclones form and intensify as they move northeastward off the Asiatic Coast, and as the westerlies progress southward, various air mass and frontal interactions occur over the central and North Pacific which are weak or often absent during summer. The normal condition during winter is one in which cold air masses over the Asiatic Continent contribute to cyclonic intensification over the open warm ocean to the east. Diabatic heating of these air masses usually contributes to the growth rate of cyclones over the open sea. If, therefore, a large pool of warmer-than-normal water has developed in the path of the migratory fronts and cyclones, it is likely that cyclonic growth will be more rapid than normal. Furthermore, the greater-than-normal rate of growth of the cyclones may provide a further means of inducing more cold air from the north and from Asia, thereby reintensifying the cyclogenetic process. While the above ideas require much more research to prove and implement, some preliminary empirical-synoptic studies have indicated that they do have substance.

Thus, in the summer and fall of 1962 a vast pool of warm water was generated in the central Pacific, particularly between latitudes 30° and 40°N as indicated by the hatched areas in Figure 15. It appears that this warm pool developed in the preceding summer and fall because of greater-than-normal anticyclonic domination over the area, leading to lighter-than-normal winds, less-than-normal cloudiness, and more-than-normal horizontal convergence of surface water. These factors led to less transfer of latent and

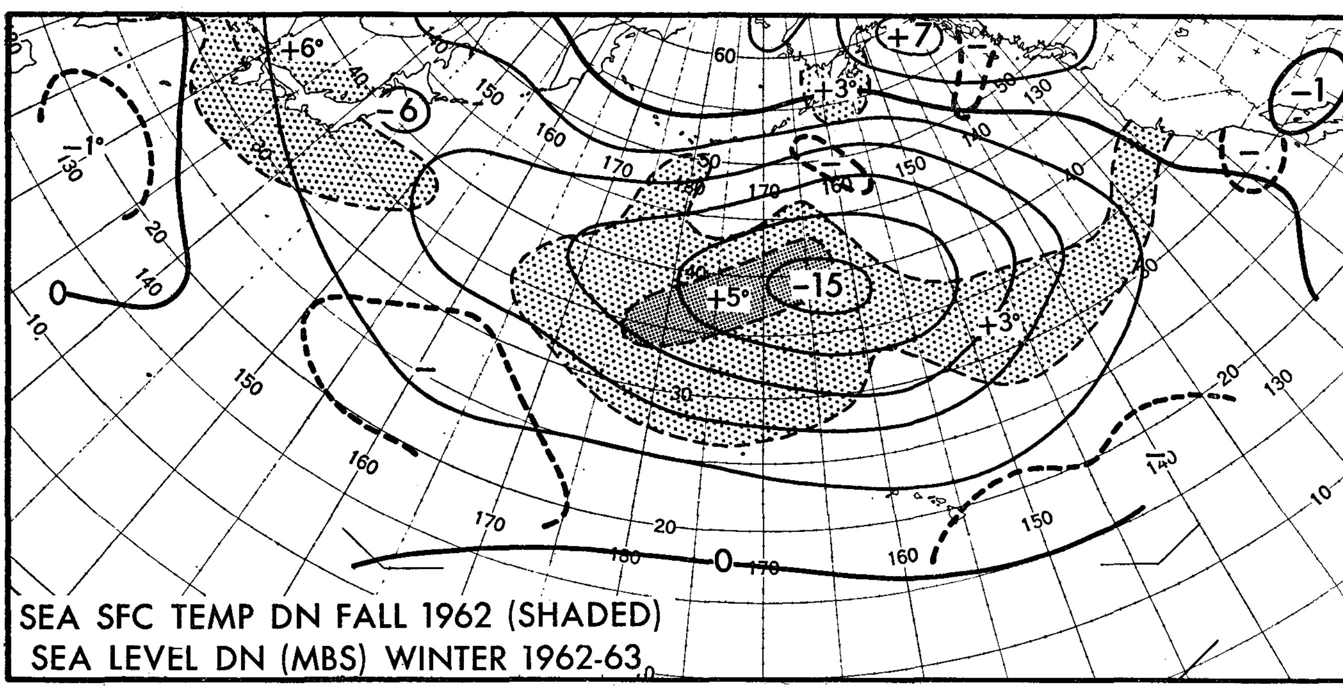

Figure 15. Warm pool of surface water observed during fall (September, October, November) 1962. Stippling indicates areas greater than 2°F above normal while heavy hatched areas indicate temperatures greater than 4°F above normal. Solid lines are isopeths of sea-level pressure from normal drawn for each 2 mbs; −15 at center indicates departure of 15 mbs below normal wintertime value.

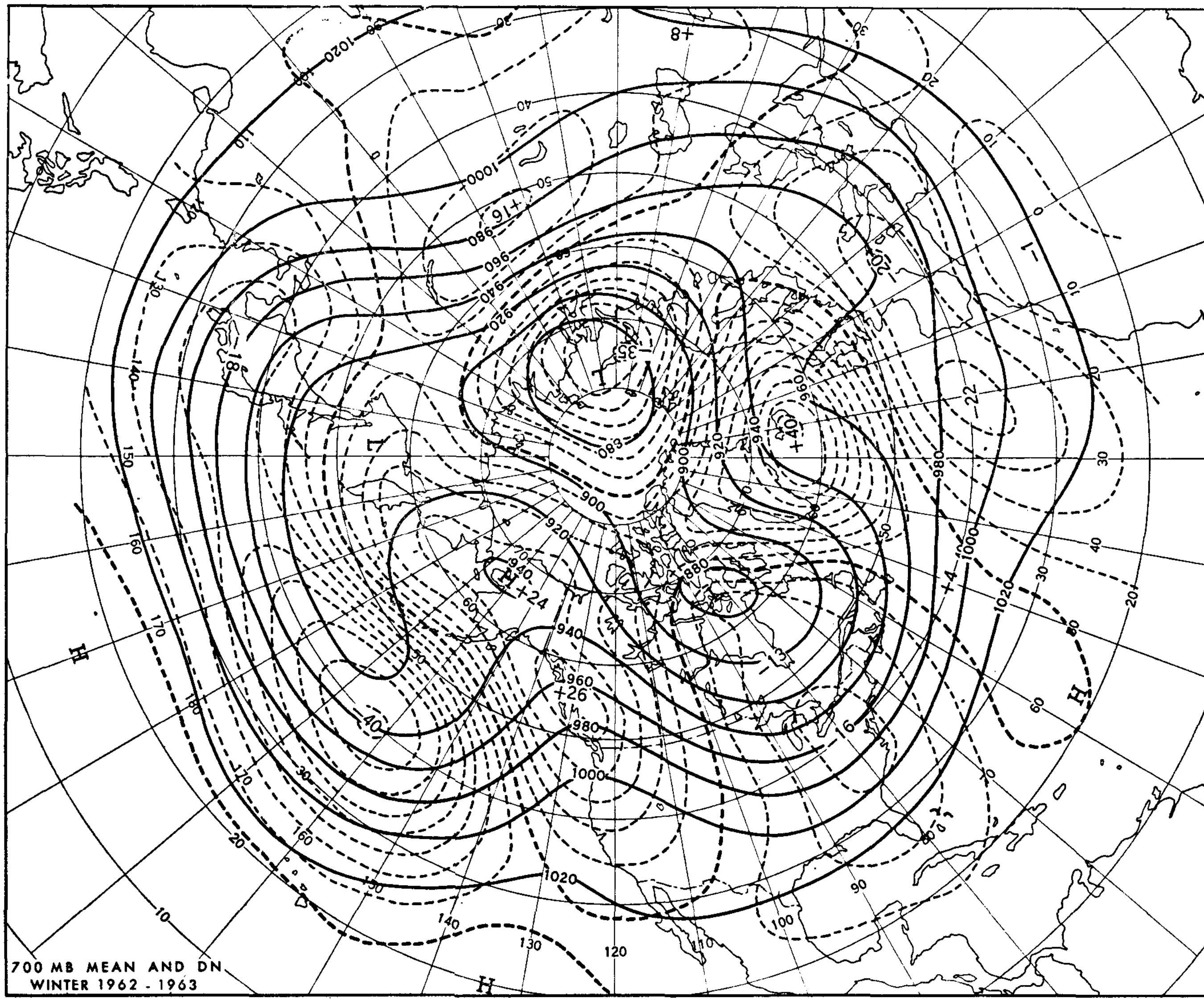

Figure 16. Mean 700-mb contours (solid) and isopleths of anomaly (broken) (both in tens of feet) for winter (December, January, February) 1962–1963.

sensible heat to the air, to diminished upwelling and to the development of a warm surface mass. As cyclones developed off Japan and the cold air masses behind them swept eastward, this warm pool seems to have been responsible for intensifying these storms materially and therefore forcing the Aleutian Low much farther south than normal. The winter departures from normal of sea level pressure shown in Figure 15 up to —15 mbs, appear to be situated near the fall warm pool and oriented like it. In Figure 16 is reproduced a map of the upper-level contours and isopleths of anomaly for the winter of 1962–63. This is taken from a paper (Namias, 1963) which treats the 1962–63 winter and the wide-scale air-sea interaction in some detail. The departures from normal over the eastern Pacific (—15 mbs at sea level and —400 ft. at 700 mbs) are almost 3 times the standard deviation of seasonal values of winters in the historical record. The negative anomalies associated with the strong cyclonic curvature aloft shown in Figure 16 undoubtedly played an important part in amplifying the ridge along the west coast of North America and into Alaska. This reaction is an inertial effect caused by redistribution of the vorticity generated by the strong trough in mid-Pacific. In turn, the strong western North American ridge repeatedly deployed cold Arctic air masses into the eastern two-thirds of the United States and also, through redistribution of vorticity, helped create the trough observed in the eastern United States during winter. This trough, however, was so placed as to generate a strong air temperature contrast between the eastern half of the United States and the adjacent waters of the Gulf of Mexico and western Atlantic. The cyclonic wave disturbances generated along the polar front separating these contrasting air masses then were afforded a favorable environment for abnormally fast growth. Firstly, they were situated in an area associated with an upper-level trough, and secondly, they could draw upon the potential energy provided by the enhanced horizontal temperature contrast. In addition, the contrast between the coastal area and the off-shore waters was further aggravated by frequent snows (rather than rains) along the Atlantic seaboard, since Arctic air infiltrated the cyclones. It is quite possible that the fast-occluding disturbances along the eastern seaboard during this winter set up conditions favoring a blocking mechanism in the eastern Atlantic—that is, relatively high pressure near Iceland and low pressure in the Azores and Mediterranean. This latter condition was associated with one of the most severe winters ever recorded over much of Europe. It is interesting to note that this prevailing wind distribution over Europe led to the westward penetration of cold continental air masses from Siberia, and consequently an unusual snow blanket was provided for much of Western Europe. By increasing the surface albedo, the snow acted to refrigerate the cold air. When this very cold air was fed into Mediterranean storms, an additional stimulus was provided to the usual atmospheric heating over the warm sea. The extra storm intensification resulted in further westward transport of the cold air to the north, and heavier snow.

Thus, during the abnormal winter of 1962–63 a remarkably steady and highly abnormal circulation and weather pattern dominated the Northern Hemisphere. Perhaps the warm pool in the North Pacific played a vital role in instigating this pattern, although it is clear that developments in other areas must have cooperated and thus reinforced the instigated pattern; that is, positive feedback loops may have been generated to provide sustained weather abnormalities. Another example illustrating the probable effect of a warm pool of water in the Pacific in displacing the Aleutian Low southward came to light during the winter of 1964–65, although the displacement was not nearly as pronounced as during the winter of 1962–63.

NUMERICAL PHYSICAL OBJECTIVES

The above treatment, admittedly sketchy, is designed to suggest causal factors which may help to explain short-period climatic fluctuations of the order of months, seasons, and up to a few years. The evidence for these factors up to now has been more or less circumstantial, and quantitative procedures must be devised to test and expand upon these ideas. Therefore, it is especially important that theoreticians in both oceanography and meteorology work closely with empiricists. If collaboration does not take place, it is difficult to see how either group can succeed in unraveling the tangled skeins of such interdisciplinary problems.

The first dynamic and thermodynamic studies of this kind have involved simplified numerical models, the aims of which have been to answer the question of how solar energy is ultimately utilized and how the atmosphere and ocean respond in order to transfer heat most efficiently. A great deal of reasonably successful work has been completed within the U.S. Weather Bureau (Smogorinsky et al., 1965; Manabe et al., 1965; Adem, 1964) and by some of the meteorological research organizations in this country. This research has thrown light on the primary mechanisms in the atmosphere which give it its characteristic time and space scales and also on the variability of these scales.

The next question to be answered involves the details of the atmospheric and oceanic evolutions, details involving such matters as variations in the location and intensity of storm tracks, the regional variations in heat supplied to and taken from the water, variations in the snow and ice cover over the continents and northern oceans, and the like. Unfortunately, these "details" may require something approaching exact knowledge of such elusive physical processes as release of latent heat, momentum and water vapor exchange, internal turbulent exchange, radiative transfers, and in fact the entire gamut of meteorological processes—to say nothing of complex oceanic processes. It is possible that some of these air and sea processes can be omitted or perhaps greatly simplified for certain predictions. On the other hand, there is no guarantee that a completely physical solution will be found, and for this reason alone the empirical approach must proceed. Besides, it appears likely that as with short-range numerical forecasts, empirical knowledge will be necessary in making optimum practical use of any physically-based long-range forecasting model.

In recent years, attempts have been made in the Extended Forecast Division of the Weather Bureau, particularly by Adem (1964) to formulate a numerical prediction model for periods of a month or a season. The basic equations used are those of conservation of thermal energy at the surface of the earth and in the mid-troposphere. The model predicts the anomalies of temperature of the underlying surface and in the mid-troposphere. Besides radiation, other forms of heating are generated within the model. This is done by expressing such heating as a linear function of variables predicted in the model.

The anomalies directly incorporated are those in the storage of thermal energy, which are introduced by using the previous month's temperature of the surface water in the ocean and the temperature of the mid-troposphere, and by calculating from the observed albedo (snow cover) at the end of that month, the anomalies in the short-wave radiation absorbed by the surface.

The numerical experiments show that important anomalies of evaporation at the surface, vertical turbulent transport of sensible heat from the surface, condensation of water vapor in the clouds and cloudiness are implied by anomalies of the computed temperature fields. Furthermore, these induced anomalies of the heating function and of the cloudiness in turn imply changes in the anomalies of the computed temperature fields.

Numerical experiments are also carried out to test the possibility of using satellite data in this model.

Despite the frightening complexity of such a program, there are reasons for optimism at this point. In the first place, there are now greater and improved sources of data in the world (including satellite measurements) and steps are being taken to press forward with a still greater implementation of data collection through establishment of the World Weather Watch. Secondly, fast and economical methods for processing these data and doing statistical and physical computations are now at hand. Thirdly, there is now better understanding of the dynamics of the general circulations of the atmosphere and the oceans. Finally, there is a new generation of young meteorologists and oceanographers well versed in physics, mathematics, and electronic computing. One of the tasks of the more senior meteorologists and oceanographers is to acquaint these young men with the real problems of long-period atmospheric and oceanic behavior in the hope that they may be able to apply their talents and achieve greater successes with long-period air and sea prediction than earlier generations could.

DISCUSSION

Schaefer: The recent 1965 meteorological and surface-temperature regime is possibly similar to that in 1957, and also the sea temperature off Peru seems to be following a similar development. It is possible, therefore, that we have an "El Niño" developing similar to 1957–59, and we would hope this time to get a better understanding, through the series of oceanographic sections which are being taken every three months along the coast from Panama to central Peru. We would also hope that we would all follow the development in the North and South Pacific, and, if we get an abnormal year, bend every effort to studying it in more detail than previously.

With limited time series, one can always find empirical correlation which may not reflect real relationships, if enough things are tried. The more complex the set of observations, the greater the chance of the computer, told only to try empirically to find relationships that explain the observed data, coming up with apparently higher significant, but actually spurious correlations. (The test is, of course, how well the prediction works for additional future observations.) The fisheries people have fallen into this trap a number of times.

Laevastu: Most properties of ocean surface layers, including sea-surface temperature anomalies, can be computed from meteorological analyses. In fact such quantitative computations of interactions form the bases of synoptic oceanographic analyses/forecasts at FNWF. However, the influence of sea-surface temperature anomalies on the atmosphere is very difficult to assess quantitatively. A moderate anomaly of sea-surface temperature would have the effect of small North-South displacement of sea-surface isotherms or be the equivalent of a slight difference of actual direction of surface wind with reference to the direction of sea-surface isotherm. The latter condition is one of the important factors determining the energy exchange, but changes rapidly on synoptic scale, thus diminishing the possible effects of sea-surface temperature anomalies.

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OCEAN-WIDE SURVEYS, BOTH METEOROLOGICAL AND OCEANOGRAPHIC

HARRIS B. STEWART, JR.
ESSA Institute for Oceanography, U.S. Department of Commerce
Washington, D.C.

Any ocean-scale survey activity would be less than efficient if it were purely oceanographic or purely meteorological. Oceanic and meteorological phenomena are too closely interrelated to attempt to understand one through ocean-wide surveys without considering the other at the same time.

To digress for a moment, this same philosophy has been extended to the administrative end of the scientific spectrum with the recent merger of the Weather Bureau, the Coast and Geodetic Survey, and the Central Radio Propagation Laboratory of the National Bureau of Standards to form a new group within the Department of Commerce which labors under the burdensome—but descriptive—title of the Environmental Science Services Administration. ESSA is a new concept—"lumping" versus the usual governmental operation of "splitting." So far it is working well, and we who are involved in it have great hopes for the future.

ESSA, to carry out the research required to provide adequate services in relation to the marine environment, is establishing the ESSA Institute for Oceanography. Its establishment will be formally announced on the 26th of December. Just a word about this might be in order, since institutes of and for oceanography are fairly close to a lot of our hearts out here. What we're doing is this: We hope to be providing to ESSA the research needed in order to carry out their missions in the field of marine products and services. It is not to be a purely basic research institute as such in competition even scientifically or financially with Scripps or Woods Hole and Lamont, rather we envision this as a research group that will be trying to bridge what some of us consider a considerable gap between basic research conducted elsewhere and the people who are banging on our doors for information on the ocean. This involves everyone from the Jerry Namiases in the long-range forecasting business screaming for sea-surface information, through the people concerned with putting up oil rigs who want to know the currents that they are going to be involved in as they work in the ocean, to the minerals people, the fisheries people, everyone concerned with the ocean as an environment. This will be the thing then that we will be working on in our Institute for Oceanography. It will be located, at least temporarily, in Washington, D.C. Eventually, of course, we hope for a coastal site somewhere, but that will be long in coming, I'm afraid.

Joe Reid had my talk this morning all scheduled and a name tied to it, and my first reaction was to change the name, but my second reaction was to become sufficiently informed myself so that I could speak to the same title Joe had planned. Therefore, what I would like to do is tell you something of ocean-wide surveys both from the meteorological point of view and the oceanographic point of view.

Jerry Namias alluded briefly to something called the World Weather Watch, and I think it's worth cluing you in briefly on the World Weather Watch as a concept. As I discuss this, please keep it within your oceanographic frame of reference. Think of this as perhaps an oceanographic possibility although being developed primarily for meteorological purposes. To commemorate the United Nations' 20th birthday, 1965 was designated as International Cooperation Year, and in speaking of this President Johnson said: "We will move ahead with plans to devise a world-wide weather system, using the satellite facilities of all industrialized countries. The space age has given us an unparalleled capacity to predict the course of the weather. By working together on a global basis we can take new strides toward coping with the historic enemies: storms, droughts and floods."

The World Weather Watch is an interesting idea. The rapidly evolving capabilities of modern weather instrumentation together with the very large advances which have been made in the understanding of the atmosphere have led people to plan a truly global observation network for meteorology. The motivating idea was that better weather service for all nations really offers the best hope for understanding the atmospheric environment in which we all live. Again keep the oceanographic framework in mind as I go through this meteorological approach.

The World Weather Watch is a system of observing, collecting, processing, and distributing weather information, using the latest developments in communications, data processing, instrumentation, and space technology. Its objective is to remedy age-old deficiencies in weather operations which have prevented meteorologists from providing weather predictions of longer range, greater accuracy, and more usefulness. The plan is being worked out through WMO (World Meteorological Organization). There are some 125 nations now involved in WMO. Already there is an international weather network, but it reflects the widely differing capabilities of the internal weather services of the various countries involved. In many instances several nations cooperate in joint efforts to collect vital weather information. The United States and some western European countries, for example, share the task of maintaining the ocean weather stations.

But the present weather observing and communication networks fall far short of providing the weather services that are required today. The network of upper air observations, for example, is way below the minimum requirements for about 80% of the earth, for half the globe these observations are totally inadequate, so the meteorologists have considerable problems. But one thing they have done is to establish this World Weather Watch. No one nation could be expected to provide it all—it had to be done on an international basis. As Jerry Namias said, the perfection of mathematical models is coming along, but they need the information that is to be put into them, and I agree with him in that I think it quite probable that when we have good mathematical models and adequate input of global weather data to these models, we will be able to increase our ability for long-range forecasting. But again, this depends on global-scale data—it's nothing that can be done with one man and one lab somewhere—it requires a large network for obtaining meteorological data.

In 1961 after the first Tiros satellite, President Kennedy expressed the desire of the United States to cooperate with other nations in space technology for peaceful purposes, and, speaking to the United Nations, he said the United States "would propose cooperative efforts between all nations in weather predictions and eventually in weather control" (they keep talking about weather control, but this is a long, long way away). And United Nations Resolution 1721, which concerned international cooperation in the peaceful uses of outer space and embodied the idea of cooperative meteorological work, was approved unanimously by the General Assembly of the UN on December 20, 1961, just four years ago today. In the field of meteorology the resolution proposed that the World Meteorological Organizations study means of developing a global weather network to receive, process, and transmit information received from weather satellites.

Specifically, the resolution requested WMO in collaboration with UNESCO and ICSU (the International Council of Scientific Unions) to draw up a proposal for appropriate organizational and financial arrangements and get going with it. In the first report to the United Nations submitted in 1962 WMO recommended the creation of a World Weather Watch combining satellite information with an expanded network of continental information to bring better weather services to all nations of the world. And it is coming along. Plans are developing, and—as these things do—it requires committees and panels, and meteorologists, at least in the United States, are working closely with the National Academy of Sciences Committee on Atmospheric Sciences, and it is coming along—the plan is taking shape, funds for it are being planned—are being budgeted (no one knows how these will come out, it's always sort of a problem), but the point to be made is that the meteorologists are moving right along in this field of developing a global observational network for improving weather prediction and especially long-range weather forecasting throughout the world.

It would be very interesting to me to see the word "meteorological" replaced with "oceanographic" throughout the foregoing discussion. And there are trends in this direction. One of the aspects of the World Weather Watch is that over much of the ocean there are no means of obtaining meteorological data. The early plans for the World Weather Watch include a series of ocean buoys transmitting meteorological data. It is our intention that the buoys will also be able to obtain oceanographic information.

Again this creates something of a problem because I personally am convinced that right now we do not have a buoy that can sit out there and operate six-eight months with high reliability. It just isn't here yet. There are a lot of plans, ideas, lots of developmental work, particularly the work that ONR is doing, but to get the buoy that will sit in one place for as much as six months with the sensors operating effectively with no deterioration of the data right now is not possible. There are people working on it, but it seems to me that we have to stop talking about great global buoy networks without having the backup to go along with it, and we just don't have it yet. Good buoys are one thing that a lot of people are working on, some of you here are involved in it. It is primarily a technological problem rather than a scientific problem, and I'm a firm believer that if enough bucks are poured into it, it can be solved. Never underestimate the profit motive as a means for getting something done. I think this is also true for oceanography as a whole. People say, for example, that there are not enough oceanographers to justify adding funds to the program on a large scale. My answer is "horsefeathers." With enough money poured into it, oceanographers "will come out of the woodwork." There will be people from other disciplines who are currently working on problems unrelated to the ocean who can just as well translate their effort to similar problems in the ocean.

The same thing happened when we were first talking about a large effort in space. If you look back at some of the Congressional hearings, the standard question was "How can we possibly mount a large-scale space program, we have no space scientists in the United States." Yet when the dollars were placed on the line, "space scientists" came from everywhere. The same thing could happen in oceanography. But I get sidetracked. That then is a brief summary of the World Weather Watch as it now stands.

Now what about the ocean business? Certainly the problem of systematic surveys in the ocean is nothing new to the CalCOFI people. Your organization's program has been one of continuing systematic surveys for some 17 years. Within the Federal Government, where we have been trying to do this on an oceanic scale, we had some of the same problems that CalCOFI has had. Processing, working up, and publishing the data has been a real bugaboo for us. First, a few words as to how this effort got started.

The National Academy of Sciences Committee on Oceanography (NASCO) in their monumental 12-chapter report had a Chapter 9 called Ocean-wide Surveys. This was no brand-new concept. This had been proposed over and over again. It was proposed by the International Council for the Exploration of the

Seas at the end of the last century. It was either ICES or what later developed into ICES. It was proposed by the Navy in the early 1920's—something called the Matthew Fontaine Maury Oceanographic Research Expedition. I've seen a volume this thick of justification as to why the Navy should get into the oceanographic survey business and I have framed in my office a copy of the letter from the Bureau of the Budget saying that the President had looked at this program and found that submission of this program at this particular time was not consistent with the President's present budgetary ideas. This circumlocution means no bucks, so it died again. We still have the same trouble; but within the Federal Government in Washington, we've tried to see what could be done about implementing this latest recommendation to get going on taking a systematic look at the world's ocean.

The Interagency Committee on Oceanography, in trying to put things into categories so that you can label them and add them up to find out how much money is going into what, made an unfortunate split between surveys and research. I think it was a mistake—we have had to live with it ever since, and it has been darn difficult at times. In the Coast and Geodetic Survey, for example, we knew we couldn't get any money for research, we never got any money for research. Thus if we called our effort a research effort, we never would have gotten started. So we had to call it a survey effort. We are a survey organization, and by calling it a survey effort we were able to get it started. Other organizations doing exactly the same thing called it research. But when you start totalling this up to see how much money is going into one aspect of oceanography and how much money into another, you run into definitional problems. The way I like to sort it out is to say that the systematic survey effort is looking at the "what," the "where," and the "when," whereas research-motivated work is looking primarily at the "why" and the "how." I think it is a legitimate way to split them up. In other words, the survey effort is primarily a descriptive effort, looking at the "what's," the "where's," and the "when's," whereas the research effort is trying to understand why and how these things are as they are.

Now in order to find out about the "why's" and the "how's," you have to go back and do the descriptive work first, so these two approaches are inextricably related, and it is criminal that we had to try and make a split between them for budgetary purposes. The hydrographers, the nautical charting people, for years have been doing systematic surveys along the coasts of the world producing nautical charts. These guys are good at it—they have what I like to call a tolerance for tedium that most of us just don't share. If something exciting isn't happening or if we can't see some scientific problems that we want to attack, oceanographers feel it's pretty dull. But fortunately the hydrographers have this tolerance for tedium—this ability to do the same thing day in and day out over and over and over, and they're good at it. What we have done is try and utilize this capability of doing good, systematic, technical, accurate work and utilize this capability for oceanography. What we have tried to do is translate the recommendations of Chapter 9 of the NASCO report, Ocean-wide Surveys, into an operational reality. To say it is coming along well would be stretching the point a bit. It's limping, just barely limping, but we're beginning to get some interesting results out of it.

As to what the program is, we have, in the classic Washington tradition, given it a code name (you seem to do better getting funds for things when they have a code name) and we struggled to find one for this for a long time. There had been considerable confusion between the continuing historical missions of the agencies—that is, the nautical charting of the Geodetic Survey and of the Naval Oceanographic Office—and the attempt to get started on the ocean survey program. Both ended up as survey items in the budget presentation, so there was confusion as to what bucks were for what. So it became imperative to point out that the specific ocean survey program was different, that it was separate from the continuing historical mission of the agencies. So to point out this difference very strongly during the hearings before the Panel on Oceanography of the President's Science Advisory Committee (PSAC), we called it the "Federal Oceanic Exploration and Mapping Program" which came out "FOEMP" as an acronym, and actually FOEMP! wasn't a bad description of what we had been able to accomplish to date. It has been coming along really very slowly. A lot of work, a lot of good work, has been done but the support has not been there. So we have come up with a better name: it's called Project SEAMAP for Scientific Exploration and Mapping program, and we're going to stick with that title now, Project SEAMAP.

What SEAMAP involves is the systematic mapping of the bottom topography, gravity, and magnetics on underway surveys plus such meteorological observations and sea-surface observations that can be made underway. In this respect I'll be particularly interested this afternoon to listen to Lee Alverson, Ahlie Ahlstrom and Tim Parsons and the rest of the papers on your agenda.

For a long time we have been trying to instill in biologists a feel for the systematic approach to biological surveys, the point being that if we can get this survey rolling, there will be ships doing systematic survey work in the world oceans, ships that can be biologically useful. What is it that the biologists want that can be obtained on a systematic global basis? We are sure that there are such data. I was interested to hear from Maurice Blackburn just recently of developments on measuring pigment material well underway. In the early stages we talked with the Bureau of Commercial Fisheries people in Honolulu. They said, in effect, "Yes, we're very sympathetic; there are a lot of things we would like to learn on this basis, but we don't have the people to do the work. Our shelves now are just stocked with plankton samples that we can't work up—what good would it do to get another 500,000 plankton samples?" So there are some real problems on taking a biological look on a global basis. But the point I want to make is that we're still fighting to get this project SEAMAP going, and to me it would be criminal to have the project going without a solidly based biological program. The meteorological part is coming along very well—why can't we get the biologists to cooperate?

The work done to date on this program has been in the North Pacific between the Hawaiian Islands and the Aleutian Islands starting at 153°W and working all the way over to 180°. We have done a series of north-south lines; generally they have been 10 nautical miles apart. This, of course, is meaningless unless you have accurate navigational control. I won't beat the navigational drum any more. It has been one of my pet ones for a number of years; but the point is that we had Loran-C control. We now on the *Pioneer* have TRANSIT—the Navy Navigational Satellite System, and hopefully we can hang on to it. We got ours through a little different route from that of the private institutions, and so far we have been able to hang on to it. It hasn't been pulled back by the Navy; we hope to hang on to this thing. The system works—it's a good system. The accuracy is classified, other aspects of the system are also classified, but the point is that it works and we can tell where we are on the surface of the ocean. This really is the fulcrum on which this whole ocean survey business depends—having good navigational control.

Question: What observations are planned for the SEAMAP program? The plan as proposed by the ICO covers the whole spectrum—it reads like a Montgomery-Ward catalog of oceanography, and it is meaningless as far as I'm concerned. It is ridiculous to try to do everything at once; we're under political pressure at this point of the game. But what is actually being observed now? There is one ship working on this —the *Pioneer* of the Coast and Geodetic Survey. The observations being made underway are continuous echo sounding, gravity, magnetics, BT's every two hours (hopefully next year some expendable BT's will be added to this), meteorological observations, regular radiosonde balloon releases, and surface weather observations. We are also using the logs of the Bureau of Commercial Fisheries for fish and bird sightings. We monitor sea-surface temperature, and sea-surface salinity is determined on all BT bucket samples. These make up the underway observations.

In addition each year we have hove-to or station operations which include a research input. For four years we had a series of stations—hydrographic stations—running from the Hawaiian Islands to the Aleutians. These data have been worked up, and they are now in the process of publication by the Seattle laboratory. Cores have been taken when there was a requirement for them. Our own feeling has been that the best place in the world to store cores, if nobody is going to look at them, is at the bottom of the ocean where they were in the first place. So when there are specific requirements, we will do coring.

We've cooperated with the University of Washington and the Geological Survey in doing some dredging on the rift zones seaward of the Hawaiian volcanoes. Dredge samples from these studies enabled the people from Hawaii, the geologists, to come up with some very interesting correlations between the size of vesicles in pillow lavas, lavas extruded under water, and other characteristics of the lavas as a function of the depth at which the lavas were originally emplaced. This is turning out to be a very interesting new tool for geologists to use to determine the depth at which pillow lavas were extruded on the ocean floor. In the early days of the SEAMAP program we also did some cesium 137 collections for Ted Folsom. We have done other specific projects like this. We collected water samples at depth in the North Pacific for NIO in England, and we have done some biological work for the Bureau of Commercial Fisheries, particularly for the Hawaiian group that wanted samples in specific places. We did work on magnetics with Vic Vacquier who was interested in the possibility of extending some of his crustal displacements. He wanted to see how far these things ran, so we ran some specific magnetic crosslines for him.

We have been concentrating, so far, primarily on the time-independent variables: gravity, magnetics, topography, and so on. The whole problem of the time-dependent variables, as Warren Wooster has pointed out over and over again, is a different problem. Joe Reid alluded to it this morning. We also include within the philosophy of project SEAMAP the systematic collection of information on the time-dependent variables. This problem is a real stinker as you all know. We are proceeding very slowly. Perhaps we are overly conservative, but I don't think so. I personally am tired of the grandiose schemes of loading our ocean with buoys (a) before we have the buoy that will do the job or (b) before we know what we really want to measure, or where. What we hope to do, following the suggestion of the new NASCO report now in the draft stages, is to carry out their suggestions that a small test buoy network be established, and so far the item has been able to remain in our 1967 budget (how long it will stay there is hard to say). We are requesting funds to plant on the east coast shelf an array of five buoys of which the prototype is being delivered to us this week. The system was developed within the Coast Survey and will measure current direction and speed, pressure, temperature, and salinity. These will be in sensors that can go on the cable. We plan to plant five, if we have funds for them, in a fairly tight network on the east coast shelf somewhere out of the Gulf Stream system. With these we will take a look at the whole spectrum of variations, the range of frequencies, and the scales of these variations. And when we have accumulated a volume of data, what we then hope to do is to make copies of these and farm them out to physical oceanographers in the United States and elsewhere. Hopefully we will then have a meeting to sit down, take a look at these data and see what people feel are the things to measure on the larger, more systematic scale.

Question: What sort of an array of buoys do you plan? It's not set yet, we don't know and are open to suggestions; probably they would be arranged in a square with one in the middle. How far apart, we are not yet sure. We would plan it to be close enough to shore so that we can intersperse the buoy measurements with ship observations so that we can fill in some of the space holes. Question: What is the cost per copy? By the time you get your anchoring gear and the buoy itself we think it's going to run about $30,000 per buoy. It isn't terribly expensive as buoys go. Question: How do you get the data back? These are both telemetered and/or stored in the buoy on incremental magnetic tape. The man handling the whole project is Mark Goodhart of the Coast Survey. They have done considerable modification to the Geodyne current meter and the tests so far show it has worked very well not only in slow currents but also in currents of two to three knots. So we will continue to be working on buoys, for within this SEAMAP project is a requirement for the measurement of the time-dependent variables. But, this phase is going very slowly and conservatively, which I think is as it should be.

One thing that Jerry Namias mentioned this morning struck a very responsive chord. This was the requirement for long series of data so that you can take a look at time variations systematically. What this always brings to my mind is tidal data, for here in fact is one of the best—if not the best—long series of oceanographic data. They go back into the last century. Generally these are available, with some gaps in the record, on an hourly basis. This is an incredible time series of data. Some people have been well aware of this. Gunnar Roden, for example, has dug many times into our tidal data bank and has utilized these data to come up with new ideas. Walter Munk has done a lot with these long series of tidal data, things that couldn't be done before electronic computers were here. Bernie Zetler from the Institute of Oceanography has been working with Walter Munk on this and has come back from his work with Walter this summer with something which to me was very interesting. I'll pass it on as far as I understand it and suggest that you talk with Walter to get the details on it. To me it was very intriguing. They were using a very long series of hourly tidal height data at San Francisco. They were applying to it new analytical techniques using the BOMM program developed here at Scripps. They were taking a look at the whole range of spectrum of frequencies that occurred in this tremendously long series of hourly tidal heights running back 80 years or so.

Instead of looking for what they thought would be there, they looked at the whole thing to see what actually was there. They found some interesting things. For example, there was a 5-day cycle that appeared as a line on their frequency chart that no one ever suspected. You'd never think of a 5-day frequency in tide; but they also came up with another thing as they looked at these. They found what they are calling a "radiation term." What they feel is that this is a variation in sea level that is a function of the incoming solar radiation. Actually the sun warmed up the water column sufficiently during the day to put a measurable steric variation in sea level, and they are convinced that is what it is. To me what this meant is that all of a sudden we have a tool for going back historically and taking a look at the variations in what I would call the "effective incoming radiation." So here is a whole storage bin of air-sea interaction data that suddenly people tripped on, and it's all there in the records just waiting for someone to go ahead and take a look at long-term variations in this effective incoming radiation. This may tie in with some of the solar activity we were speaking of a minute ago. In other words, this is the sort of thing that can happen when you get long series of dependable data.

One other thing, while we're on this ocean-scale survey subject and talking on tides, is a program that is now in the thinking and planning stages, and funds have been budgeted here and there for. Hopefully it will come off. This is the IAPO-Walter Munk plan for an ocean-wide look at deep-sea tides. There has been a lot of interest generated in taking a look at deep-sea tides on a global basis. One nice thing, of course, is that this does not have to be done synoptically, so you are not going to have to have instruments all over the ocean at the same time. Rather the plan is to run a profile dropping the instruments, say, across the Pacific, then coming back and picking them up later on—hopefully. We now have, as you know, cotidal charts that are theoretical. They are based on coastal and island data. We don't really have much of a feel for what happens to a tidal wave as it comes up on to the Continental Shelf—what sort of modification takes place. The idea of going out with bottom-mounted tide gauges on a global scale and taking a look at the whole movement of tide in the ocean is a fascinating idea, and it can very probably be done. People have been working on deep-sea gauges. Aeries in France, Jim Snodgrass and Walter Munk here, Steacy Hicks of the Coast Survey, and many others have been working on deep-sea gauges, and they are beginning to get pretty good results. These things will work. So here is another look on a global scale—this one at the phenomenon on tides.

One other aspect of this large-scale business is one other look at the time-dependent variations which is going on even now. This is called Gulf Stream Studies—'65. I realize it isn't your ocean, and I apologize: but it's a lot like your Kuroshio, so what we Atlantic oceanographers can say is that we are looking at the Gulf Stream and maybe this will help with understanding your Kuroshio. This was a project dreamed up three years ago when air-sea interaction was an especially good budgetary word, and we thought that maybe by using that word which people were latching onto, we would get some additional funds to do something we had been wanting to do all along. This is the way you have to play it in Washington, as you know. So what we proposed was to take a long look at the Gulf Stream. We knew perfectly well that if we in the Weather Bureau and the Coast and Geodetic Survey said that we were going to go into a Gulf Stream program, that it probably would be shot down in flames before we ever got started. So what we did was this: we called in the Gulf Stream people, brought them to Washington for three full days of sessions. This was Henry Stommel, John Knauss, Fritz Fuglister, Tak Ichiye, Bill Richardson, and Ray Montgomery. All came in for three days, and all sat down in a conference room on the top floor of the Department of Commerce. We said, "Let's be perfectly frank about it. We have the facilities—both meteorological and oceanographic. What we would like to do is take a look at the Gulf Stream, but we want your guidance. We want to know what are the major scientific problems that have to be solved; don't worry about the justifications, we'll tie it in with fish and weather and national defense in the national budget; all we want to know is the scientific problems involved." They were fairly good sessions. On the basis of those sessions we planned a Gulf Stream survey—Gulf Stream Studies 1965. It is going along pretty well. It started actually in August and it's going for one full year. Let me just briefly show you the way it's working, and then I'll get back to these survey studies.

This program is in three phases. For the first at Miami and at Bimini in the Bahamas we had continuously recording tide gauges. Originally we hoped to have the one at Bimini telemetered into Miami so that we could have these on a two-pen recorder. This way we could see immediately the variations and the difference in sea level across the straits. We ran into some telemetering problems with the Canaveral people who were a little touchy about what radio frequencies were used; and rather than get all the new equipment that would be required, we decided that it wasn't really that important to have these data in real time. Thus we waited until we could see the records that would now come in, and we could get hourly heights. Bill Richardson of Miami was working with these data, and we hoped to get some feel for variations in the volume flow through the straits as indicated by variations in sea level across the straits of Florida. Also as part of this program, Bill Richardson has been working with the pop-up current integrator, a very clever gadget. With accurate positioning, you drop it to the bottom and then wait until it comes up to the surface, and the difference between the point were it was dropped and the point where it is recovered is a measure of the net transport that was going on at the time.

The second phase of Gulf Stream Studies—'65 is a standard section running about 150 miles out from Charleston, S.C., done by the Coast Survey Ship *Pierce* with meteorologists aboard making regular upper air observations. The *Pierce* occupies 28 deep stations of which every other one goes to the bottom. This profile is run once every two weeks. This projection is not very accurate. There is no great directional change in the Gulf Stream at Cape Hatteras. If you look at it on the globe, it is one straight run all the way out.

The third aspect, and the one most interesting to me, is continuing the work that Fritz Fuglister was doing, that is, taking a look at the Gulf Stream meanders in the area northeast of Cape Hatteras. Using the Braincon vee-fin towed at 200 meters we pick up the 15° isotherm. Actually the ship is navigated as a function of the temperature at 200 meters. If the temperature gets warmer, we come to the left; if colder, we come to the right, and this way we are able to follow the left-hand (downstream) edge of the Gulf Stream. These meander trips are made once each month, with the first one made in August.

What we have found is that these large-scale meanders are the norm rather than an exception. When we first started, Henry Stommel became particularly interested in looking at an eddy if we found one. He was sure that these large circular eddies formed and broke away, but he was hoping we would find one so he could go out and look at it. It turned out that we found several of these. The first one we found south of the main stream in September. On the October trip we found that it was still about in the same place, but it had moved to the west, and we found another one over to the east. These large eddies do break away and maintain their integrity at least during periods on the order of three months. We also found very large changes in the position of these meanders. Where at one time we followed a meander like this (drawing on the blackboard), the next time a month later when the ship went out, the meander had moved some 50 miles to the east.

This, then, is another way of taking a look at some of these time-dependent variables. I think this program is going to work out pretty well—I think we will learn a good deal about the Gulf Stream. We held a meeting in November at which the Fuglisters and the Knausses, the Richardsons, and the Ichiyes were all there, and we had a delightful time hashing out what we found to date and what modifications should be made in the program as we go along.

But I want to get back to your ocean and what has been found in some of these systematic surveys in the North Pacific—the area covered through last June, primarily north-south lines with occasional cross lines. The cross lines have not been adequate to date, and this is being improved. We'll talk right now about the work underway. The 1961 data have been almost completely processed and are already being used in papers by George Peter on the geophysics of this area.

The results of the magnetic observations from the area across the Aleutian trench show the same general thing that the only previous systematic survey of this type had also shown. As you recall, the work of Mason, Raff, Vacquier, et al., showed the magnificent magnetic topography off the west coast of the United States. That was magnetic topography found as the result again of systematic surveys off the west coast done by the *Pioneer,* but done on a classified survey for the Navy. We still don't have the bathymetry from that survey in our own shop, but the magnetics were not classified, and Vacquier and company found very intriguing ridge and trough magnetic topography off the west coast. If we examine the Aleutian Trench area with the magnetic anomalies superimposed on top of the topography we find these long magnetic trends, the same sort of thing that Vacquier, Raff, and Mason found farther down off the west coast of the United States. In other words, what these are are magnetic trends that do not follow the pattern of the topography. Now if you took a single trackline of a research ship going through this area and plotted the magnetics, it would look the same way it does in any other ocean—just a single trackline. If you tried putting two or three of these tracklines together, it would help some, but what I'm contending is that it is the systematic, back-and-forth, tedious survey job that turns up information like this.

I checked with George Peter before I left on Friday. He was quite excited. He has continued to work up this information farther to the South and has found that these lineations do not, as he at first suspected, continue down to join up with the magnetic trends found off the west coast, but these magnetic lineations peter out and become quite irregular in the general area of the Mendocino fracture zone. They then pick up below that, so that the Mendocino is having some reflection in the magnetic data. But this again is the sort of thing that can be discovered only by a systematic survey. We're looking from 45°N to 55°N and from 150°W to 159°W.

One other thing about how this ocean survey program is progressing. I'd like to be much more optimistic than I can—we just had budget sessions this past week and I'm anything but optimistic. A ship that we had in our '67 budget for doing this type of work was disallowed—we're not even sure where we are going to get the funds to repair the *Pioneer* which is badly in need of major overhaul before she can go back to sea. However, we do have two bright spots in the horizon with the delivery sometime this winter of the *Oceanographer* and the *Discoverer,* two large oceanographic survey ships. Each is 3,800 tons, 303 feet length over-all, with 4,200 square feet of lab space. Lots of versatility was designed into them. For example, the lab is of modular construction so you can switch it around to do what you wish. These ships have really good possibilities. One will be operating in the Pacific, one will be in the Atlantic, and both doing project SEAMAP, laced, we hope, with a good deal of research work going on at the same time. We've done a lot of homework for this program. A recent operations research study carried out at considerable expense has come up with mathematical planning models which we are already using. I can go into details later, perhaps, for those of you who are interested. It was very interesting, though, that after a very detailed research analysis of the whole thing, they came up with almost the identical number that the NASCO people had come up with for the number of ship-years operation required to survey the whole ocean. NASCO did it over a few drinks at the Cosmos Club one night, and these people to whom we paid X number of bucks came out with the same number, so it was legitimate. It was about 285 ship-years. Granted this would have to be done like the World Weather Watch, on an international basis. The oceans are just too big to try to do it by ourselves. However, we approach the international basis cautiously, for if you have to strike the median level of competence of all the countries involved, this would perhaps fall short of the achievement level that we have in mind. Probably what will be done internationally at first—again a recommendation of Warren Wooster and the NASCO group—is to have some of the larger maritime countries, perhaps the United States, Canada, and the United Kingdom, undertake a portion, say, of the North Atlantic. Then the others come along as they can meet our standards.

The only point that I really want to make is that be it meteorology and the World Weather Watch, or be it oceanography and project SEAMAP, over and above research activity, the systematic collection of meaningful data in both the atmosphere and the ocean can contribute tremendously to our knowledge of both of these environments. Both areas must be pursued with considerably more vigor than in the past if we are ever to realize any real benefits of new and needed knowledge.

DISCUSSION

Schaefer: Are the buoys being planned by ESSA as part of World Weather Watch designed to obtain sub-surface temperatures through the mixed layer? It would be important to do this both for oceanography and also for the weather-forecasting problem.

Stewart: They are being planned to have this capability. No bid proposals have been requested—the buoys are still being considered by ESSA. Not only NASCO and MASCAS, but also the oceanographic element within ESSA has insisted that oceanographic capabilities be included in any buoys for the World Weather Watch.

Calif. Mar. Res. Comm., CalCOFI Rept., 12: 50–53, 1968

SOME FUTURE TRENDS IN OCEAN RESEARCH AS SEEN BY THE GEOPHYSICS BRANCH OF THE OFFICE OF NAVAL RESEARCH

HUGH J. McLELLAN
Geophysics Branch, Office of Naval Research
Washington, D.C.

Every oceanographer in this country has had as an article of faith, belief in the existence of a money barrel in Washington, labelled ONR, maintained for the benefit and promotion of the affairs of those people engaged in activities loosely classified as "oceanography".

For seven years I tested this dogma myself and found that, if not a demonstrable truth, it was at least a faith one could live by. What more can one ask of any faith? Not content with worshipping from afar, I was led to become a servant in the temple. At a very disturbing time it turned out. Things are changing around the money barrel and lamentations and wailing have been heard from the far reaches of our land.

Now we all expect our work to change, in fact we wouldn't have it otherwise. It is a far different matter to tamper with an article of faith. Even though everyone interpreted the dogma in his own way to start with—the forthright statement that things are different from what they were believed to be constitutes heresy. Anyone guilty of one heresy is clearly capable of any heresy. Therefore one can make up his own heretical proclamations and impute them to the heretic and have this imputation generally believed.

Let me review some of the things that are being said of us:

Item: ONR is no longer going to support basic research in oceanography.

Item: It has to be of direct application to planned weapons systems or ONR is no longer interested.

Item: ONR is cutting off support of graduate students in oceanography.

Item: (From directors stomping into our office) "You are trying to run the research program of my institution."

Item: (From an October 1 Chapman report) ONR is going towards research with a purely military payout and therefore to classified research only.

To my best knowledge these five statements at least are false. I want it clearly understood, though, that by selecting only these five, I don't admit the truth of any or all other statements of this sort.

I am going to try to tell you what the situation really is and some of the things we feel may happen in the next few years.

Let us review the bidding.

1. ONR's mission has been, and is, to have basic research done that will lead to an improved capability for our forces to be defensive or offensive at sea. As a spender of public funds, ONR has an obligation to buy the best product at the best price. There is no getting around the argument that it is impossible to predict which basic research will pay off. Nonetheless, with any real limitation on funds, the responsibility for deciding how the funds shall be apportioned among the possible efforts is inescapable. Such decision making is the task of those public servants assigned to work in the office. A citizen who thinks this task is being ill done should agitate to have the decision makers replaced by more competent individuals from among the citizenry.

The first decision that had to be made was what portions of our resources should go to physics, mathematics, psychology, geography, oceanography, etc. Difficult decisions of this type have been made for many years, and oceanography has fared the best by far. Other fields have had no substantial increases for years, many good programs have been completely sacrificed and ocean science has grown rapidly. The reason for this is implicit in the title of a speech given by the Chief of Naval Research to the recent Navy Underwater Sound Symposium. It was: "Prospects in Oceanography—Central Science of the Navy".

Now, with the limitations that your Congress in its wisdom, and Mr. MacNamara impose, we are entering an era where hard decisions must be made as to how the effort should be split within oceanography.

Fortunately, this point was not reached before other sizable sources of public support have appeared in agencies which have quite different missions than has ONR.

2. With a few minor exceptions, ONR has never made *grants* to institutions for research in oceanography. However, we have had a policy of broad contracts covering most of the types of work at an institution, and have relied heavily upon the directors to decide how best to use our resources. They have proposed what they wish to do and we have contracted with them to do it. We have given them as much latitude as possible and, in most cases, this has resulted in maximum mileage for our dollars.

We are not displeased.

However, within the last two months the director of one of our larger contracts referred in a letter to a very high Navy official to an "annual grant for research" from the Geophysics Branch—and we have concrete reasons to believe that others thought of our contracts as Institutional Grants which they had a right to expect would be renewed annually and that we had no right to interfere concerning the way the funds were spent. This must change quickly if our usefulness to the oceanographic community is to continue.

The solution is not, in my mind, for us to staff up with a large group of hard-nosed project officers. The solution lies in our finding a way to work more closely with the directors in designing the kind of program that, to the extent we can divine, best fulfills the Navy's need for research in this area.

3. ONR has no mission specifically to promote education in oceanography or other fields. Nevertheless, our office has in the past made research support commitments to universities that made it possible for them to set up teaching programs. We have supported, and now are supporting, a large fraction of the country's graduate students in oceanography through part-time employment on our contracts. We are extremely proud of the role we have played here. But, consider the rational!

More well-trained scientists were absolutely essential for the research our office felt the Navy required to be done. Students have never been paid for being students but for assisting their professors in their research and for carrying out the very productive research that was an essential part of this graduate education. Thus, education in oceanography has developed to its present state largely through Navy sponsorship. Meanwhile, the Navy has gotten a return for its money that can easily be demonstrated to have been a good return.

NSF has entered the scene with a mission in both basic research and education. We gladly defer to this agency the responsibility for looking after the well being of the academic institutions. This is not a matter of choice but necessity. We know that the need for continuity in support is recognized within NSF and hope that they will soon learn how to provide it. Numbers of small one-year grants based upon the collective whim of review committees is not the method.

One of the real dangers we face today is that the institutions to which we give broad support will sell their best packages to NSF and we will end up having our funding go to the supporting roles and to the expensive efforts that are not given good reviews by NSF panels, whose members hope to feed themselves from the same trough. We would, in effect, supply all the bread for the sandwich while NSF provides the ham or cheese to go between. I think you must agree that, in light of our mission, this is an unacceptable way to dissipate our resources even though on a national basis the results might be excellent.

Let me say here emphatically that it is not our intent to change our role precipitously. We feel a real responsibility to the institutions that have grown up with our encouragement and which, by and large, have served the Navy as well as it has served them.

We must, however, gradually back out of the position of supporting institutions because they are, or aspire to be, centers of oceanic research.

What then do we feel should be ONR's role in oceanography? Remember that it must be a role that is defensible within the Navy's total R&D effort.

We must have a definable, balanced program that we can demonstrate is as good as, and as agressive as, the program of any other agency, and that looks ahead to Navy problems in a way the others don't.

We must support new departures in the field. This incidentally is easier for us (provided we are not broke) than for other agencies, because we, in our office, can make arbitrary decisions without applying to the reasoned deliberations of those who have become respectable through application of current or classical approaches.

We must keep a broad-fronted attack going on the ocean—supporting some work even in areas that seem to have no conceivable bearing on naval operations—ours is a basic research program and if there is any single characteristic that makes research basic it is that you don't know all the implications. A recent example that could be cited is research into dissolved organics in sea water. Two years ago we would have had difficulty in showing any naval relevance besides surface slicks. Today we know enough that some serious applied research could be justified.

We tend to be optimistic about a continuing role for ONR in oceanography. If we can get organized to the point that we can tell those to whom we report precisely what activities we are supporting, and why, our ability to continue support will be retained. If we can identify new things that really should be done, I'm sure we can get resources and do them.

To illustrate our optimism we began this summer, in the face of what then looked like a 15% funding decrease, to hold discussions with oceanographers about types of endeavor that need to be initiated. We realized that this meant looking for projects in "Big Science" since everyone already has money to do little science that needs doing.

For the first go it appeared that Physical Oceanography is the part of the field most ripe for improvement and we are pursuing our search for the most important things we can help with.

The task which originally faced physical oceanography was that of describing the "steady state" field of physical properties and evaluating the ordered motion of the waters. It was early found that, through the geostrophic assumption, a relationship between the field of motion and the field of mass could be formulated which gave first order agreement with the few available direct current measurements.

There followed an era where careful measurements were made of temperature and salinity to a precision that permitted accurate estimates of relative density. By assuming a "depth of no horizontal motion" the flow field for the upper layers of most parts of the world ocean has been mapped. By invoking continuity for heat and salt, and adding information on nonconservative constituents, a gross picture of the deep circulation has been added.

This task has been well done. Critical observers have shown the extent that the mass field departs from the stationary and, in some cases, have advanced physical reasons for observed fluctuations. The necessary fiction of a "depth of no horizontal motion" which the geostrophic assumption demands has been called into question so that, although the surface circulation is well described, the mass transport of certain major currents may be uncertain by a factor of two.

Beginning in earnest about twenty years ago theories of currents in a stratified fluid on a rotating earth controlled by wind stress, friction and inertia have been developed and refined. These can adequately explain the major features but suffer because adequate observations are not available to check refined features of the models. Indeed the feature of geostrophy, which is retained to some extent in most theoretical models, has yet to be subjected to a quantitative check in the deep open ocean.

Theory has also suggested a complexity of small to medium scale motions that could be excited by impressed external forces, by inertial instabilities in shear flow, etc. Some of these should be predictable from the physics of the system and the boundary conditions, while others represent the degradation of ordered motion through the process we term turbulence and, if predictable, are predictable only in a statistical sense. What few pertinent observations are at hand confirm only that oceanic motion in detail is at least as complex as theory would indicate.

The obvious task now facing the physical oceanographer is to devise and use systems of instrumentation which will yield a true picture of motion in the important scales. This will allow for the refinement of theoretical models and hopefully an advance toward the goal of prediction.

Four types of experiments are under discussion within the scientific community.

The first looks to the gross behavior of circulation within an entire ocean basin. In each basin the surface circulation is dominated by a large anticyclonic gyre, wind driven, with marked intensification along the western boundary. These anticyclonic circulations are perturbed in respect to intensity and to local geographic position of their boundaries. These perturbations give rise, especially in the eastern parts of the basins, to major departures of temperature structure from the climatological mean. The questions to be asked are: What is the frequency and magnitude of the variations?; How are variations in one part of the system related time wise to variations in other parts?; and What is the underlying cause of the perturbations? Required for the answer are long time series of observations from fixed stations around the periphery of the gyre in its minimum and maximum expected extent. For a first effort temperature observations alone might suffice and the network might omit the region of the strong western boundary current. Such an operation has been proposed for the North Pacific.

A second type of experiment under consideration would concentrate on a western boundary current where there has been the greatest amount of theoretical study. The Gulf Stream north of 32°N would be a logical starting point. The experiment would require a three dimensional array of temperature, salinity and current sensors across and along the stream and time series of observations from them. The questions to be asked are: What is the nature of the flow in space and in time?; To what extent can geostrophic computation be trusted to yield flow information?; What augmentation in mass transport and momentum takes place down stream?; and What are the fluxes of heat, momentum, mass, and vorticity down stream and cross stream? This sort of study would have maximum impact upon theoretical studies but would present severe technological problems in mooring instruments in such a strong current.

A third type of experiment asks: What are the temporal and spatial scales within which significant amounts of kinetic energy are found in the ocean?; How is the energy distributed through the spectrum of scales and periods?; Can this knowledge lead to a formulation of the physical laws, and evaluation of controlling parameters, governing the motion? The initial experiments would go to a comparatively quiet part of the ocean away from strong known currents and, after setting lower limits for the significant space scales and upper limits for the frequencies containing significant energy, set a three dimensional array of current, temperature, and salinity sensors to record for a sufficiently long period that spectra and cross spectra could be computed. Once sufficient information was collected on this background the experiment would be moved or expanded into regions with well defined currents.

The fourth type would take new instrumentation which has been yielding direct measurements of mass transport for the Florida Current into the open ocean. The first problem is to achieve precise relative navigation beyond VHF radio range from shore. This is being investigated and there is promise that buoy mounted systems may work. Direct measurements in major currents would yield a model of the ocean circulation with more reliable numerical values.

Experiments of these types cannot be embarked upon until firm, scientifically valid, plans are worked out; the technological feasibility of carrying them out has been assured; and a nucleus of talented scientists has been identified who will dedicate a significant fraction of their lives to the prosecution. Nevertheless, this is the direction to which we believe one must look for the next major increase in understanding the ocean as a physical system.

In conclusion I would like to quote the final passage from the Chief of Naval Research's speech referred to earlier.

> "To those who compare our efforts in science with our efforts in space, the comparison is apparently odious. The fact remains that the Navy

tries hard to do the most it can with the financial and other resources it gets by permission of your representatives and mine. You need not fear that our approach to ocean science will be less than it could be; in fact, it will be all that the law allows. The law allows enough for a dynamic program and that is what we intend to have—*period.*"

DISCUSSION

Wooster: How would ONR funds for oceanography be divided between "big science" and "little science", and how would this ratio compare with the ratio of publishing scientists involved in the two "sizes"? Back of this question is my fear that a large proportion of ONR support would go to the "big science" projects which might involve only a small number of scientists, the rest of us withering on the vine.

McLellan: This would be hard to predict. Certainly one could not think of cutting out all of the small, isolated efforts since many of the very productive scientists in any field can only function this way. "Size" is just not a valid basis for decision. Neither is "number of publishing scientists" or "pages of published material generated." What you fear is recognized and is a valid point of concern. On the other side of the coin one might fear that we would fail to get on with the job because everyone is comfortable doing what was very productive ten years ago.

Laevastu: If the results of recent works on problems of currents and variability, especially those by O. Saelen (Norway) and by NATO's La Spezia laboratory, have not been taken into consideration in planning the relatively expensive current and variability studies, they should be, as these works seem to answer to a considerable extent the questions and problems raised in the proposed studies.

Calif. Mar. Res. Comm., CalCOFI Rept., 12:54–63, 1968

A DISCUSSION OF SOME CRITICAL INDICES OF PRIMARY AND SECONDARY PRODUCTION FOR LARGE-SCALE OCEAN SURVEYS*

T. R. PARSONS and R. J. LeBRASSEUR
Fisheries Research Board of Canada
Pacific Oceanographic Group
Nanaimo, B.C.

INTRODUCTION

In the following presentation we have considered different types of observations which might be applied to the study of the primary and secondary production of large areas of ocean. From an initial consideration of this problem it was apparent that in order to carry out these studies a large number of ships and personnel would have to be employed for many years to come on this one facet of marine research. As an alternative, therefore, we felt that it was necessary to consider observational platforms other than research ships. Thus the type of observations that we believed would serve as critical indices of primary and secondary production have been limited by the extent to which we consider that meaningful measurements could be made from these alternative platforms. In a decreasing order of ability to cover large areas in time and space we have considered as possible platforms: satellites, aircraft, instruments towed in the surface layers by commercial shipping and simple sampling procedures carried out by fishing vessels.

One additional aid to these data gathering platforms is a fixed point in the ocean such as Ocean Weather Station "P" (50°N 145°W). It has been said that it is impossible to understand the results of a synoptic survey without having performed a time series study at a point in the area. It is equally true that a time series study is difficult to interpret without a synoptic survey of the adjacent area—the two are complementary and while we have to place certain limitations on the types of observations which may be made over large areas, virtually no limitation need be placed on the types of observations carried out from a weathership. Thus it is part of our contention that such ships should be considered an integral part of large scale oceanographic surveys and from an organizational point of view they should be considered oceanic field stations of various agencies.

Finally we have felt that in order to achieve some progress toward a solution of the problems of large scale surveys we should employ indices and models which have become well established among marine scientists as acceptable measurements and explanations of processes in the marine environment. The extension of these measurements and the use of more sophisticated models will depend greatly on the results of future basic research. For the present, however, we have attempted to extend the use of basic concepts in order to test their validity for use in large scale studies of the ocean.

EXPERIMENTAL

Two parameters which we have initially considered to be appropriate for large scale monitoring are stability and the penetration of light into the water column. In temperate latitudes conditions for the onset of the spring phytoplankton bloom have been shown to be largely dependent on these two variables. One approach to an examination of their effect was suggested by Gran and Braarud (1935) and developed into a prediction model by Sverdrup (1953).

The approach described by Sverdrup (1953) is based on a comparison between the depth of the mixed layer (D_m) and the depth at which light conditions (radiation and transparency) are sufficient to allow a net increase in the primary production of a water column. The latter depth is known as the 'critical depth' and is defined as the depth above which the total production (of the water column) is equal to the total respiration. It follows that if the critical depth is greater than the depth of mixing, a net increase in production can take place. Sverdrup (1953) determined a mathematical expression for the critical depth as follows:

$$\frac{D_{cr}}{1 - e^{-k_e D_{cr}}} = \frac{I_e}{I_c\, k_e}$$

where D_{cr} is the critical depth in metres, k_e is the extinction coefficient (m^{-1}), I_e is the average energy which passes the sea surface per unit time and is available for photosynthesis, and I_c is the energy at the compensation depth. (The compensation depth is defined as the depth at which the energy intensity is such that production by photosynthesis balances destruction by respiration.) I_e and I_c have been expressed in langleys (ly) per hour—one langley being equal to one gram calorie per cm^2.

In using this model we have tried to accumulate data over as long a period of time as possible so as to present an average picture from which future anomalies could be judged. The following sources were used for these data:

I_e, the average energy available for photosynthesis per unit time, has been determined at Station "P" from the total solar radiation measured with an Ep-

* Part of this presentation was submitted previously to the Journal of the Fisheries Research Board of Canada as a paper entitled "The advent of the spring bloom in the eastern subarctic Pacific Ocean."

pley pyrheliometer. These values, averaged from January 1960 to February 1964, have been expressed as the mean hourly radiation and corrected for reflection losses by determining the mean sun altitude for each month (Sverdrup, 1953). The amount of energy available for photosynthesis has then been determined by reducing the total radiation by a factor of 0.2 to allow for absorption of non-photosynthetic energy in the first metre of sea water (Sverdrup, 1953).

For the rest of the eastern Subarctic Pacific, solar-radiation estimates computed by the U.S. Bureau of Commercial Fisheries, San Diego (Marine Weather Observation Summary for the Pacific Ocean) have been utilized. These data, which were available by 5° squares of latitude and longitude for the years 1962 through 1964, were averaged and expressed as the hourly photosynthetic radiation, corrected for cloud cover and for reflection, for each month.

Estimates of the depth of the mixed layer (D_m) during February have been taken from Giovando and Robinson (1966). For the areas 55–60°N, 150–160°W, and 40–45°N, 150–160°W, where no statistical estimation of the depth for the mixed layer has been made by these authors, an approximation of the mean mixed-layer depth plus or minus one standard deviation has been made from bathythermograph data (available at the Pacific Oceanographic Group, Nanaimo) for January to April during the years 1957 and 1962 through 1964. Data on the mixed-layer depth obtained by Giovando and Robinson (1966)

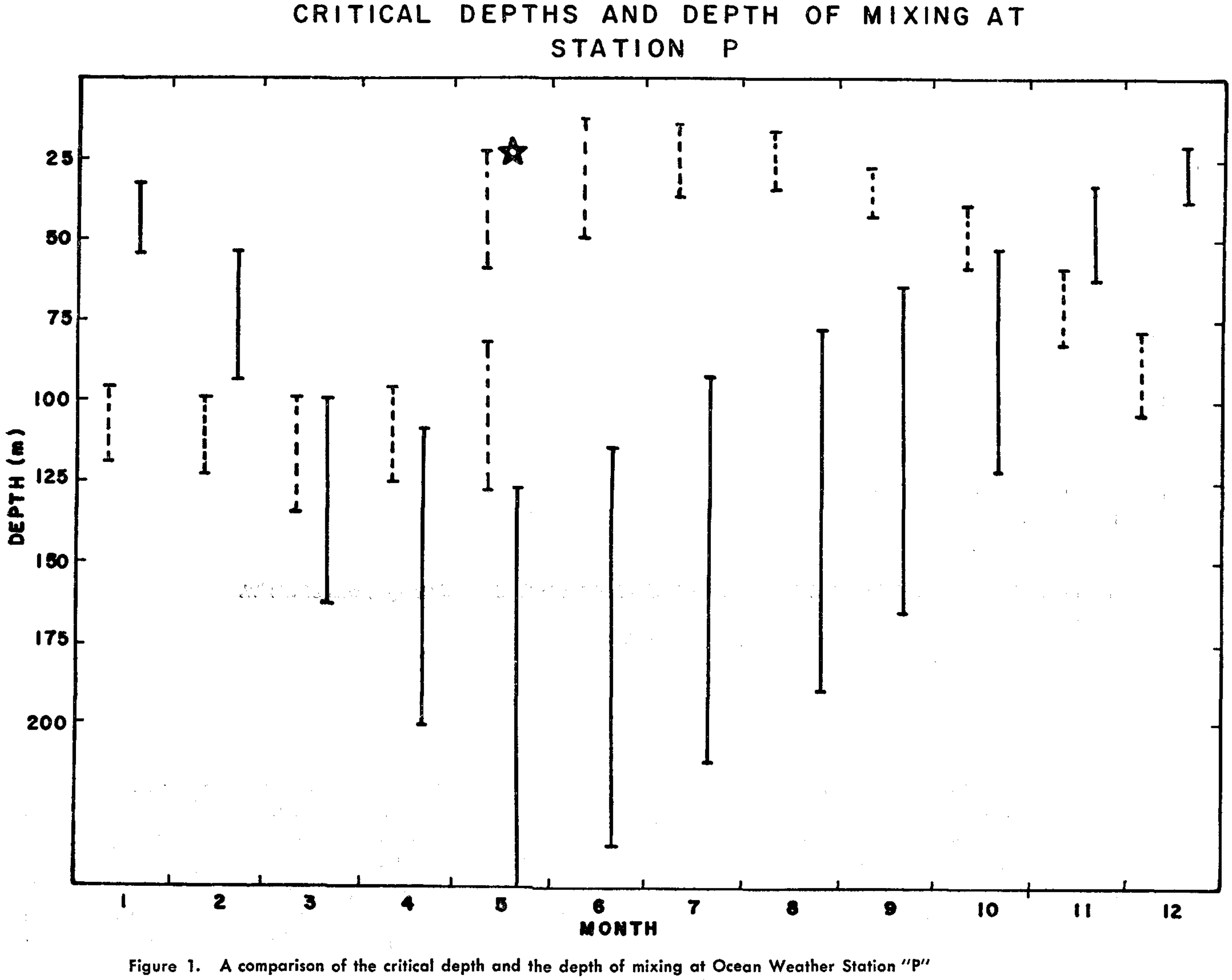

Figure 1. A comparison of the critical depth and the depth of mixing at Ocean Weather Station "P"

Legend: Maximum and minimum critical depth

Approximate mean mixed layer depth, plus or minus one standard deviation (data from 1947–1963)

* During the years 1956–59, 1962–63 the "new" seasonal thermocline was formed near the middle of May.

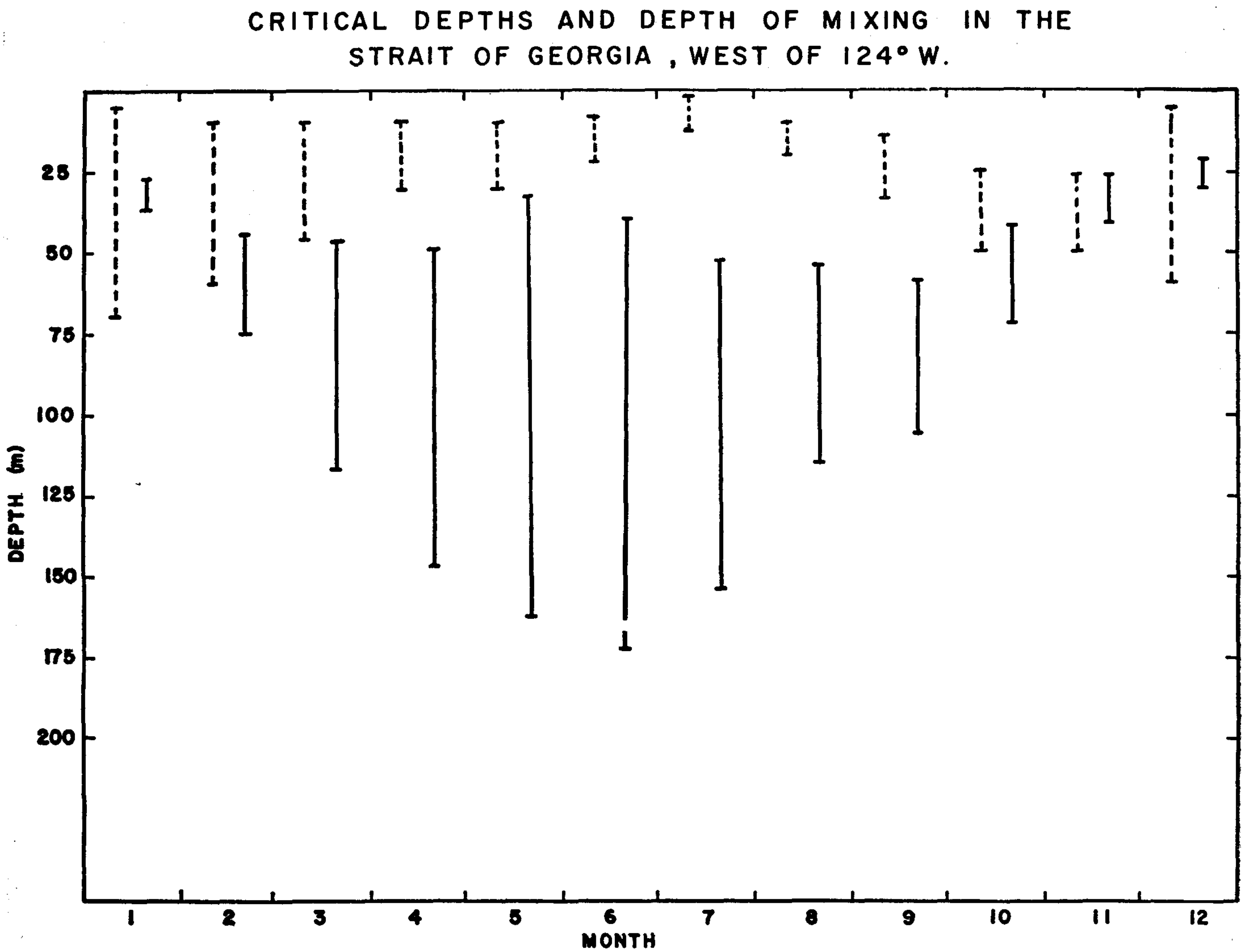

Figure 2. A comparison of the critical depth and the depth of mixing in the Strait of Georgia, west of 124°W.

Legend: Maximum and minimum critical depth

Maximum and minimum depth of mixing

have been represented in Fig. 1 for each month of the year at Station "P". In Figure 4 are shown values of the mixed-layer depth from north to south along lines of longitude, at 10° intervals, from 125° to 155°W. In this manner, estimates of the critical depth, which were made by 5° intervals of latitude and 10° intervals of longitude, can be compared with estimates of the mixed-layer depth which have been reported by areas composed of 5° intervals of latitude and 2° intervals of longitude, (Giovando and Robinson, 1966). In reporting the latter results, where two areas of latitude and longitude are adjacent (east and west of a line of longitude), the larger maximum and the smaller minimum values for the mixed-layer depth have been entered in Fig. 4.

The extinction coefficient, $k_e(m^{-1})$, has been expressed as maximum and minimum values for each month from Secchi disc data accumulated during 1957 through 1962 at Station "P" (Parsons, 1965). These values have been expressed as extinction coefficients for blue light which have been derived from the formula given by Poole and Atkins (1929):

$$k_e = \frac{1.7}{D}$$

where D is the Secchi disc depth in metres. For the remainder of the eastern Subarctic Pacific, Utterback and Jorgensen's (1934) oceanic-average extinction for the area, 0.073 m^{-1} at 4800 A, has been employed. This value is representative of minimum extinction values at Station "P" during the spring. Maximum extinctions during the spring at Station "P" also have been included in calculations of the critical depth in the remainder of the northeast Pacific by using the maximum extinction for March of 0.13 m^{-1} (Parsons, 1965).

The value used by Sverdrup (1953) for I_c, the energy at the compensation depth, was taken from Jenkin (1937) as being 0.13 ly/hr. This value was determined for a day length of 16 hours, which suggests that a slightly larger value might be considered for the spring when the day length is less. However, in the absence of sufficient information on the effect of light and dark periods on the compensation light intensity, the value of 0.13 ly/hr has been employed here for purposes of comparability with other authors (Marshall, 1958, Sverdrup, 1953 and Cushing, 1962).

Copepod weights used in this presentation are from data accumulated by LeBrasseur (1965 and 1966). Collections were made with a standard North Pacific net hauled vertically from 150 m to the surface.

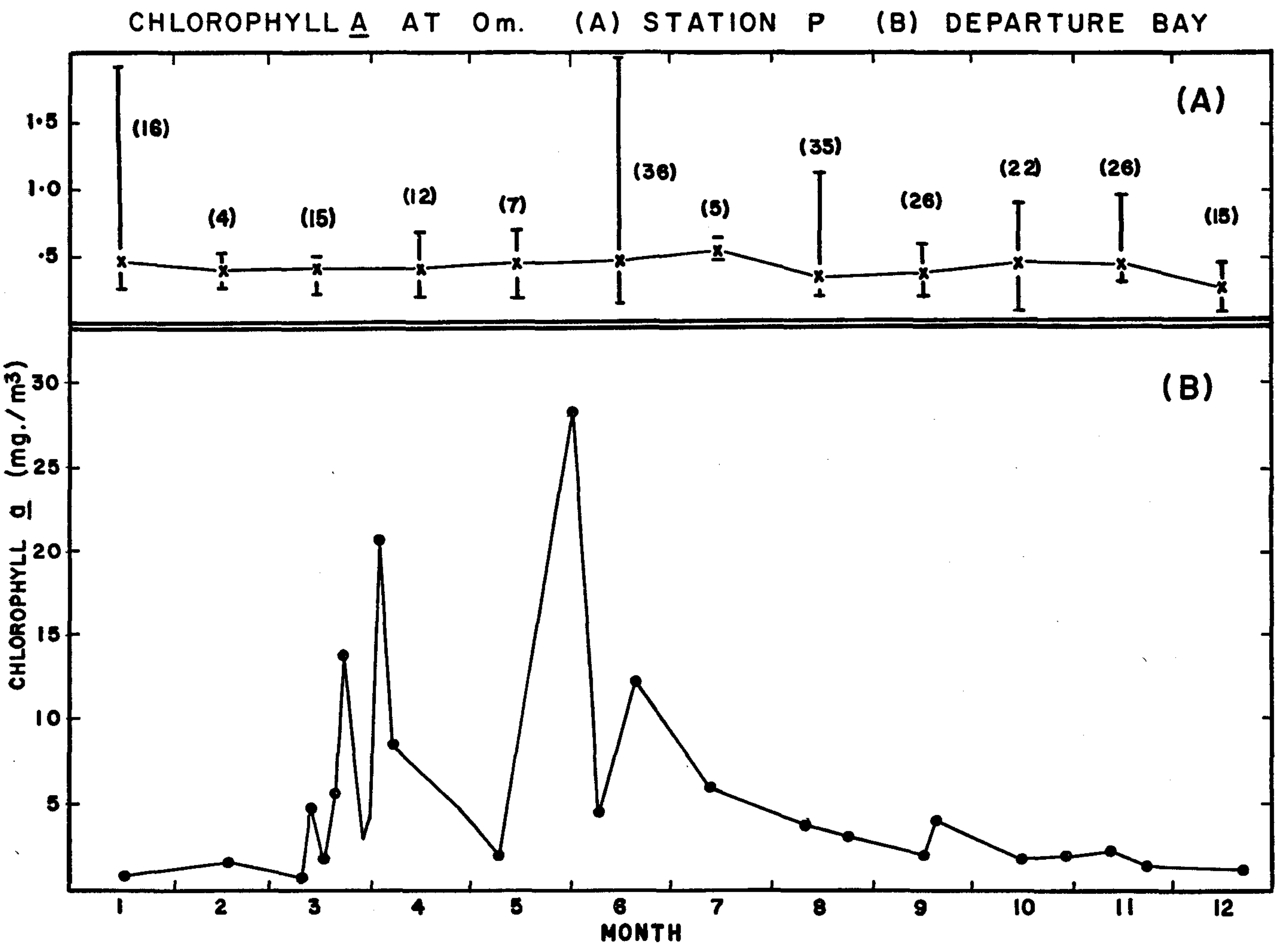

Figure 3. Surface chlorophyll *a* values at Station "P" and in Departure Bay.
Legend: Station "P"; average values, x; number of determinations, 1958 to 1964, (25); maximum and minimum values represented as bars. Departure Bay; data from Parsons (1960)

DISCUSSION OF FIGURES 1 TO 6

Figure 1

In this figure the range of maximum to minimum critical depths at Station "P" in each month of the year is shown as solid bars. The depths of mixing, plus or minus one standard deviation, are shown as dotted lines. Two depths of mixing are shown for the month of May since the "new" seasonal thermocline in some years is established in this month rather than June.

From this figure it may be seen that during the period December to February the maximum critical depth is less than the minimum depth of mixing. This would indicate that there could be no net increase in primary production at Station "P" during this period. On the other hand, from May through September the minimum critical depth is equal to or greater than the maximum depth of mixing, indicating that this is the principal period in which a net increase in primary production can occur. For the months of March and April, and again in October and November, conditions are such as to permit some increase in net production under favorable conditions. It appears therefore that for the advent of the spring bloom, the months of March and April will be the most important in determining the timing of this event.

Figure 2

Data are shown here on the mixed-layer depth and the critical depth for each month of the year in a coastal region at the same latitude as Station "P". It may be seen that in contrast to Station "P" the much higher stability, due to freshwater runoff in this area, has allowed conditions to develop so that a net increase in primary production would be expected during March. Further it appears that throughout the winter some production could occur in this area providing relatively stable conditions and/or a suitable degree of transparency were maintained for a sufficient period of time. To approach a plankton bloom, however, assuming a maximum generation time of 48 hours for winter radiation levels, suitable conditions would have to be maintained for about two weeks. This period is considerably longer than is generally encountered during the winter in the Strait of Georgia.

Figure 3

Data are shown here on the seasonal variation in the surface concentration of chlorophyll a/m^3 at Station "P" and at Departure Bay in the Strait of Georgia. The latter values, collected during 1958–1959 show that a marked increase in the standing stock of phytoplankton occurs during March which is in agreement with the prediction shown in Figure 2. At ocean Station "P", however, average concentrations of chlorophyll a show little change with season. There is good evidence that this absence of a marked change is due to intensive zooplankton grazing (McAllister *et al,* 1960) which must commence

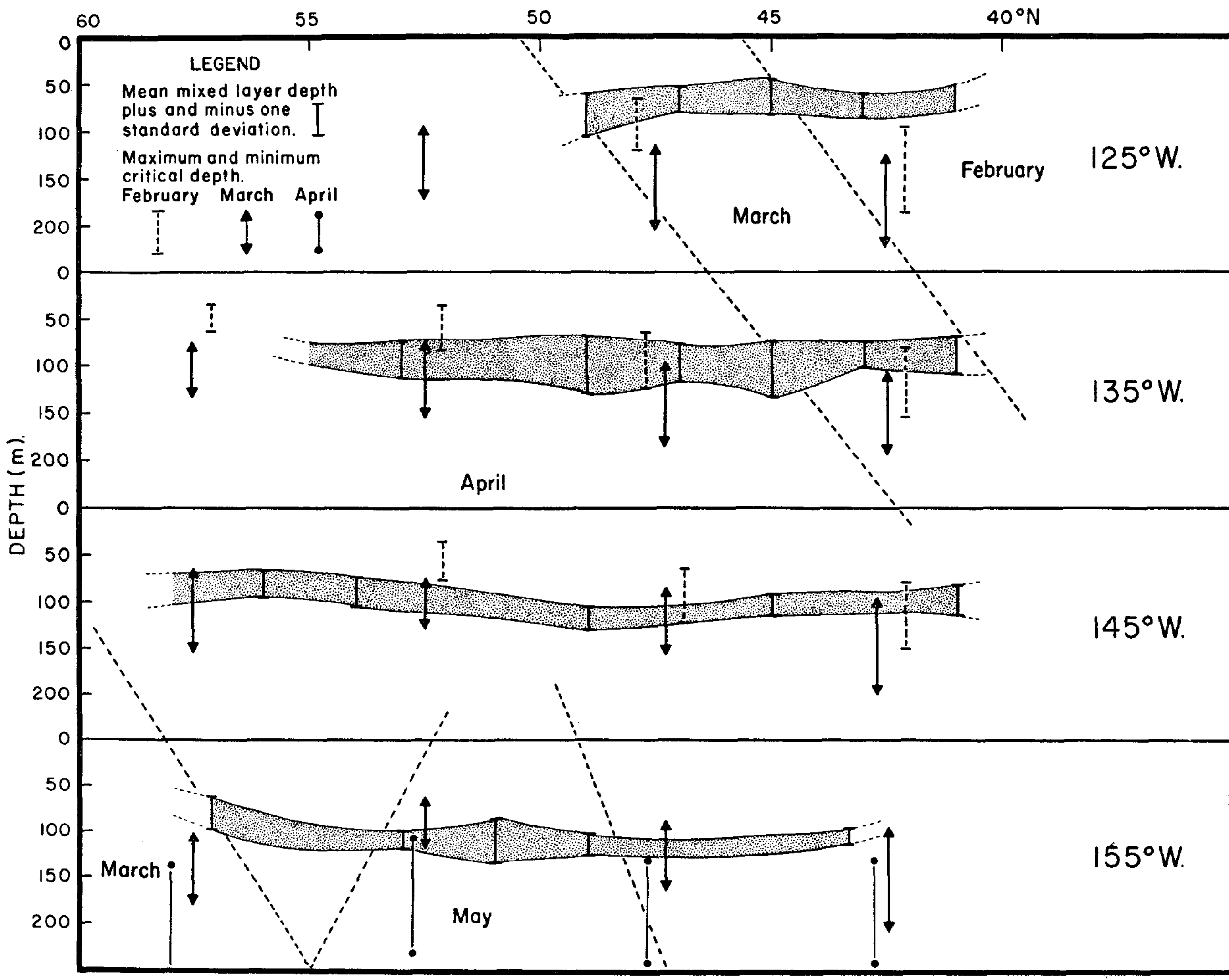

Figure 4. A comparison of critical depths and the depth of mixing in the eastern Subarctic Pacific Ocean, February to April.

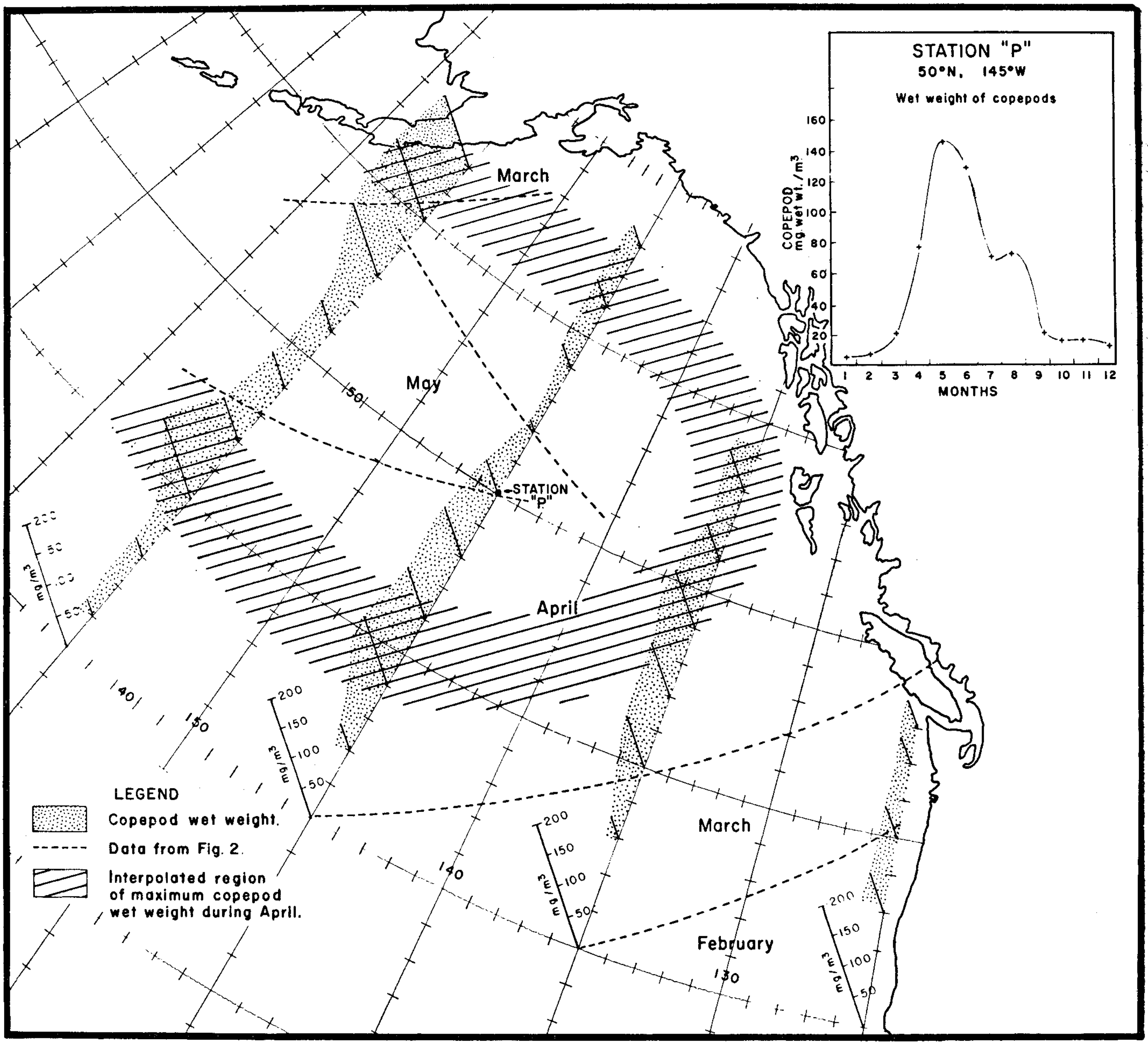

Figure 5. A comparison of copepod wet weights during April and the occurrence of the spring phytoplankton bloom in the eastern Subarctic Pacific Ocean, February to May.

simultaneously with a net increase in the primary production during the spring. (Another example of this suppression of changes in the concentration of phytoplankton is discussed by Heinrich, 1962).

Figure 4

In this figure we have confined the reported data to the mixed-layer depth and the critical depth during February, March and April, but extended the area of observations from 40 to 60°N and by lines of longitude at 10° intervals from 125 to 155°W. The mixed-layer depths are shown as solid bars and are joined by the shaded portion.

Critical depths are shown for February as broken bars and if we take as a criterion of timing the month in which the minimum critical depth is greater than the maximum depth of mixing, then only at 125°W between 40 and 45°N are conditions firmly established for a net increase in primary production during February.

During March, the minimum critical depth is greater than the depth of mixing up to 50°N at 125°W and out to 135°W at 40°N. Another small area exists at this time at 155°W and 55 to 60°N.

For simplification of the figure the only April critical depths shown are along the 155°W line. In the rest of the area, except at Station "P" (Figure 1), the April minimum critical depth is below the depth of mixing. The only other exception is in the central

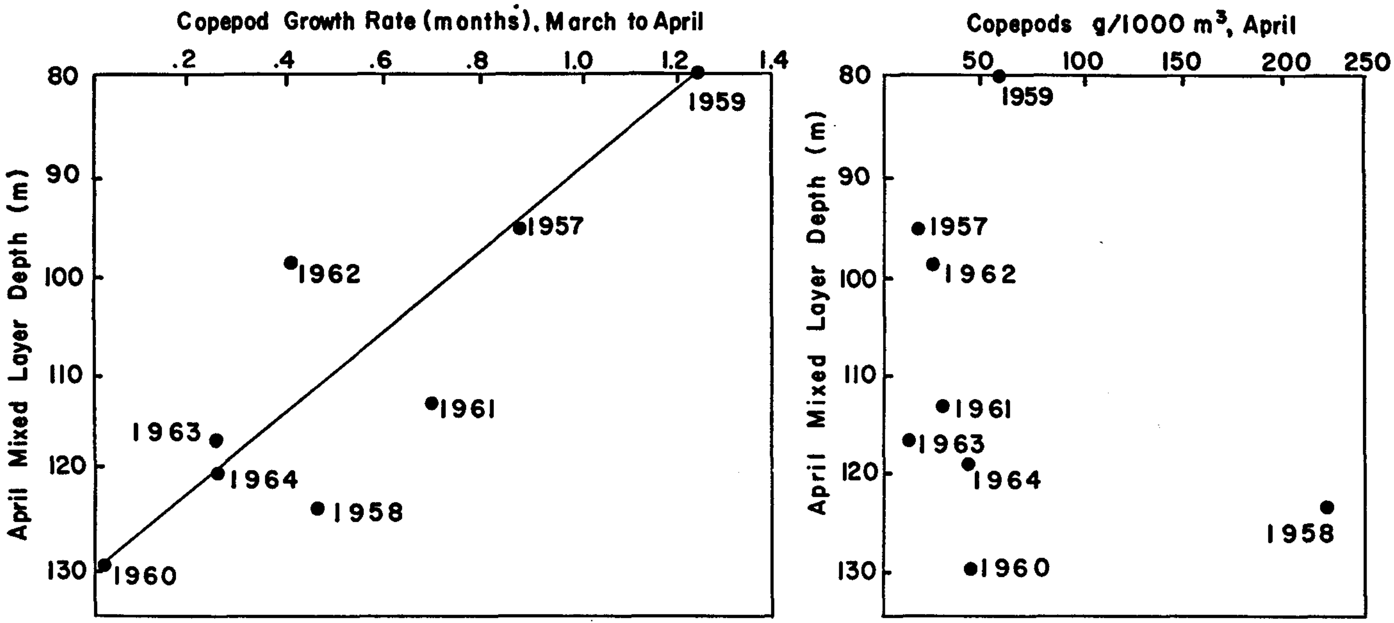

Figure 6. A comparison of the mixed layer depth at Station "P" and the growth rate of copepods (left); (right) the standing stock of copepods. Data from 1957 to 1964.

part of 155°W where the minimum critical depth does not exceed the depth of mixing until May.

Lines drawn diagonally across this figure separate, by month, areas in which hydrographic and radiation conditions are such as to firmly establish conditions for a net increase in primary production. These lines have been transposed to the next figure.

Figure 5

Dotted lines drawn on Fig. 4 which separate areas in which conditions for a net increase in primary production become established in the same month, are compared in this figure to the average copepod wet weight for the month of April. An area of approximate copepod maxima has been drawn in as an interpolation of the results of copepod wet weights shown in this figure. From a comparison of these results it may be seen that maximum copepod weights are encountered in areas north and south of Station "P", in a semicircle approximating the areas in which a net increase in primary production occurs during March to April. Minimum copepod weights are encountered in a central portion of the northeast Pacific between a wide area at 155°W and a narrower area extending east beyond Station "P". This area of low copepod weight approximates the area in which conditions for a net increase in primary production are not firmly established until May. These results are further substantiated by the inset to Fig. 5 which shows that maximum copepod biomass at Station "P" does not occur until the period May to June.

Other evidence supporting the description of the spring bloom discussed here can be found in primary production data for Station "P" (McAllister, 1962). These data show values of up to 1100 mg C/m²/day during the period May-June compared with values of 200 or less mg C/m²/day during March-April. From data reported by Stefansson and Richards (1964) it is apparent that in the area of 40–45°N and 125–130°W, nitrate depletion starts in February and that the nutrient becomes exhausted from the surface layers by May. These results are also in keeping with the sequence of events shown in Figs. 4 and 5.

There are, however, some inconsistencies in Fig. 5 in that the central copepod minimum at 155°W is greater than some of the maxima at 145°W. This may be partly explained if we were to consider that there are other effects of the environment, such as currents, which determine the actual numbers of copepods present. An alternate and possibly more plausible explanation is that the critical-depth model does not predict the total biomass but only growth-rate of copepods in this area.

This may be demonstrated in the following manner: The species of copepod primarily responsible for the increase during the spring is *Calanus plumchrus* (generally from 40 to 90% of the biomass). Further it is widely distributed during March and April, and as spring progresses the increase in copepod biomass is in a large part due to this animal getting fatter and changing from Stage IV to V. Thus if, on the inset to Fig. 5, the exponential increase in copepod biomass is taken as an approximation of growth-rate during March and April at Station "P", then this value in different years should be predictable from the ratio of Dcr/Dm.

Figure 6

In this figure the measure of copepod growth-rate ($\log_{10}$ biomass in April minus $\log_{10}$ biomass in March divided by the time-interval in months) has been plotted against the depth of the mixed layer from 1957–1964. Radiation data are not available for the same period so that we have had to leave out half of the model (Dcr) in plotting the ordinate. The indication is still clear, however, that there is a measure of growth-rate which is related to the mixed-layer depth; but that, as is shown in the second part of the figure, there is no apparent relationship between the standing stock of copepods and the mixed-layer depth. Thus it is apparent that the model gives some measure of growth-rate but not of recruitment of copepods.

Conclusions from Figures 1 to 6

In conclusion to this first part of our presentation we feel that we can now at least partially answer Dr. Stewart's question when he asked earlier, what types of data does the biologist want collected for large scale oceanic studies? If we return to the right hand side of the equation for the Sverdrup model, then we want an improvement in the collection with time, by area and in accuracy of the three terms, I_e, k_e, and I_c. I_e, the effective photosynthetic radiation, should be monitored over the northern (north of about 40°N) hemisphere and, initially at least, reported as a monthly average for each 5° square. This value could be improved in accuracy by correcting for reflection due to the effect of wind on the sea surface and by an accurate measure of the energy in the photosynthetic portion of the spectrum. The extinction coefficient, k_e, should also be routinely measured over the same area and it is suggested that some kind of disposable light meter for use with aircraft might greatly assist in the collection of these data. Our knowledge of I_c, the compensation light-intensity, might be initially improved by laboratory studies but field investigation of this value from research ships would ultimately be desirable.

Also inherent in the model is the continued collection and improved coverage of data on the mixed-layer depth, D_m.

Finally, while three of the above terms can probably be best collected from satellites or aircraft, the actual biological data with which the model can be correlated should also be routinely collected. As has already been mentioned in the introduction, these data (e.g. chlorophyll *a*, primary production, zooplankton biomass) are probably best collected from commercial shipping or from fishing boats in the area.

So far we have been discussing essentially the timing of events in the N.E. Pacific Ocean and their relation to the rate of increase of primary and secondary producers. Looking at a different aspect of this problem it is possible to divide up the effects of stability and radiation on the production of a water column and compare these effects at different latitudes and in different oceans. As in the previous study, the quantity and time at which data have been collected permit only a broad assessment of this problem but we believe that the following discussion gives a good appraisal of the effect of stability and radiation on primary production.

Table I shows the effect of increased radiation, February to May, on the growth-constant of a chrysophyte. The actual production at Station "P" (line 1) has been taken as representative data from McAllister (1962) as reported by Parsons (1965). Average photosynthetic-radiation data measured with a pyrheliometer at Station "P" is reported in the second line (Parsons, 1965). In the third line the effect of this increase in radiation on the growth constant of *Monochrysis lutheri* has been determined from data given by McAllister, *et al*, (1964). This organism was chosen since it is a chrysophyte and most of the crop at Station "P" is believed to be coccolithophores. It is also the only rate-versus-light intensity curve we could find in which the study had been carried out over a sufficient period of time to reflect the true growth-response of a chrysophyte to light-intensity rather than the adaptive response of organisms taken from one light-intensity and incubated for a short period of time in a light-intensity gradient. Finally in line 4 of Table I the increase in growth-constant for March, April and May is represented as a multiple of the growth-constant in February.

TABLE I

INCREASE IN PRIMARY PRODUCTION DUE TO RADIATION AT STATION "P"

	February	March	April	May
1. Approximate average production (1959–1961) mgC/m²/day	25	75	220	500
2. Radiation (PAR) ly/hour (1960–1964)	0.82	1.56	2.12	2.94
3. Growth constant for *M. lutheri* using radiation values (2) above*	.008	.02	.04	.05
4. Increase in production from (3) above	--	X2.5	X5	X6.2

* Data from McAllister, C.D., N. Shah and J.D.H. Strickland. J. Fish. Res. Bd. Canada 21: 159–181. 1964.
PAR, photosynthetically active radiation.

TABLE II

INCREASE IN PRIMARY PRODUCTION DUE TO STABILITY AT STATION "P"

	February	March	April	May
1. Average mixed layer depth (m) (1959–61)	113	128	101	68
2. Compensation depth (m) (1960–64) (k_e = 0.075)	25	33	37	42
3. Dc/Dm	.22	.26	.36	.62
4. Increase in production from (3) above	--	X1.2	X1.6	X2.8

In Table II the mean mixed-layer depth at Station "P", February to May has been reported from Robertson *et al*, (1965). In line 2 the compensation depth at Station "P" has been calculated from the radiation data in Table I, assuming an average extinction-coefficient of 0.075. The effect of stability on the compensation depth has been determined in the third line by taking the ratio of the compensation depth and the depth of mixing. The increase in this ratio for the months of March, April and May as compared with February is shown in the last line.

Although the major effect in increasing the ratio Dc/Dm from February to April is due to the increase in Dc, the effect of an increase in Dc is only made possible during this period by the relative constancy of Dm. Thus while Dc is a function of radiation, the efficacy of the compensation depth is determined by the stability of the water column which is quite different from the effect of increased radiation being considered in Table I.

TABLE III

TOTAL EFFECT OF CHANGES IN RADIATION AND STABILITY ON PRIMARY PRODUCTION AT STATION "P"

	February	March	April	May
1. Approximate average production (1959–1961) mgC/m²/day	25	75	220	500
2. Increase in production compared with February	--	X3	X8.8	X20
3. Increase due to radiation (Table I)	--	X2.5	X5	X6
4. Increase due to stability (Table II)	--	X1.2	X1.6	X2.8
5. Total effect of radiation and stability	--	X3	X8	X17

Table III provides a summary of data in Tables I and II and a comparison of the effects of radiation and stability on the actual increase in primary production. Thus the initial increase in production during March at Station "P" is primarily due to the effect of increased radiation on the growth-rate of the primary producers. This is also true for April but a larger proportion of the increased production in this month is due to stability. From April to May there is little effect of increased radiation on the growth rate of individual cells but there is a marked increase in the production of the water column due to stability. Finally the total effects of radiation and stability at Ocean Station "P" show that the increase in radiation on the growth-rate of the phytoplankton is about twice that of the increase in production due to stability. Further these combined effects (line 5) are in quite good agreement with actual increase in primary production at Station "P" (line 2).

While at Station "P" radiation apparently determines the increase in growth-rate as well as to a large extent the increase in stability (through the formation of the seasonal thermocline) in other areas these effects assume a different proportion. Thus from Marshall's (1958) studies in the Arctic it is apparent that a shallow mixed-layer is strongly maintained during the spring by a salinity gradient and the effect of increased radiation on production is much greater than further south in the Atlantic where the winter mixed-layer depth extends to 200 m. In the northern Sargasso Sea, however, there is sufficient radiation throughout the year for production, but the stability of the water column is believed to limit production during a few months of the year (Riley 1957). A similar discussion of these changes from north to south in the Atlantic may be found in Cushing (1962).

Finally there appears to be a marked difference in the onset of the spring bloom in the North Atlantic and Pacific Oceans. Throughout the subarctic water mass in the Gulf of Alaska a halocline at about 100 m exists throughout the year. Thus even in the absence of a seasonal thermocline at some shallower depth, conditions for a net increase in the production of the water column are firmly established by May (Fig. 1). The productive column is relatively shallow, however, being limited by the halocline at *ca* 100 m. In the North Atlantic the mixed-layer depth, at the same latitudes and off the continental shelf, extends to 200 m during the winter. Under the latter conditions the onset of the spring bloom is more dependent on the formation of the seasonal thermocline. Thus primary production during the spring in the North Atlantic will tend to start later but develop more rapidly than in the North Pacific. One possible explanation, therefore, for the lack of synchronization between the phytoplankton bloom and the zooplankton crop in the North Atlantic is that the phytoplankton are produced initially in the spring at a greater rate than can be grazed by the zooplankton. In the North Pacific, on the other hand, the mean generation rate of the phytoplankton in the water column must be equal to the grazing rate of the zooplankton. The difference in the species of secondary producers has also been given as another possible explanation for the differences in the spring bloom in these two oceans (Banse, 1964). It is probable in fact that both the relative stability of the environment and the grazing patterns of the zooplankton play a role in determining the extent of the spring phytoplankton bloom in each ocean. It is imperative, however, that for a study of the former effect there should be an improvement in the large scale collection of the types of data discussed in the first part of this presentation.

To conclude it is perhaps worth considering the extent of variations within small areas of ocean. So far in this discussion we have been considering a unit area of about a 5° square of latitude and longitude and a time period of one month. From studies on surface pigment-concentrations in the Gulf of Alaska (Parsons, 1965) it is apparent that the range of variation of chlorophyll *a* in 5° squares regardless of season is about 0.2 to 0.8 mg/m³. A similar order of variation can be found during a time interval of one day in a seven mile-square in the same region (Antia *et al*, 1962). Thus, while we have shown in Fig. 3 that the monthly-mean surface-chlorophyll *a* concentration at Station "P" remains virtually constant, small variations in time and space occur which may provide an insight into local production processes or grazing patterns. These sub-area variations must be studied with different techniques than those suggested in the first part of this presentation and the development of a variety of automated recorders for measuring nutrients, particulate material and zooplankton are particularly desirable for these studies.

DISCUSSION

Smith: Were there other measurements of non-conservative properties on this fine-scale sampling pattern?

Parsons: When we carried out the study on the distribution of chlorophyll *a* within a seven-mile square the only other measurements made were for salinity and silicate. Since neither of these parameters reflected the same degree of variation as the chlorophyll *a* data, we felt that the latter might be a result of zooplankton grazing patterns. No environmental evidence was obtained, however, for this suggestion.

McGowan: What proportion of the standing crop of zooplankton is made up of grazers in your study area?

Parsons: The data which I have reported in the previous figures are for copepod biomass. Assuming that the copepods were the only grazers, then the proportion of these animals was about 90% of the total zooplankton standing stock.

Schaefer: It has been suggested (by Banse?) that differences in seasonal phytoplankton between open North Pacific and open North Atlantic, north of about 45°N, are due to differences in the life cycle of major zooplankton forms. The egg production of *Calanus* sp. in the North Atlantic is directly related to availability of food and commences only in spring. The two dominant Pacific *Calanus* species produce eggs from reserve materials, independent of phytoplankton concentration. Because of reproduction in winter, the offspring of the Pacific forms may prevent the spring bloom.

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WHAT MIGHT BE GAINED FROM AN OCEANWIDE SURVEY OF FISH EGGS AND LARVAE IN VARIOUS SEASONS

ELBERT H. AHLSTROM
Bureau of Commercial Fisheries
Fishery-Oceanography Center
La Jolla, California

This presentation is an extension of the talk I made 2 years ago, which was published in CalCOFI Reports, Vol. X. The first talk, presented at the Symposium on Larval Fish Biology was titled, "Kinds and abundance of fishes in the California Current region, based on egg and larval surveys."

We know a great deal about the kinds of fishes and their relative abundance in the California Current region off California and Baja California. Most of these fishes have a more widespread distribution than we cover on CalCOFI surveys. However, it is the exceptional species whose distribution is completely delimited by our surveys. *Bathylagus wesethi*, a deep sea smelt, is an example of a species that may occur wholly within our survey area. The eggs and larvae of the Pacific sardine and the Pacific mackerel have been effectively delimited in Pacific waters but both have sizeable populations in the Gulf of California that we have not surveyed regularly. The Gulf sardines are now known to be genetically distinct and resident only in the Gulf; hence they have not complicated our problems. In all probability this also will prove true for other fishes found in and out of the Gulf, e.g. Pacific mackerel, hake.

I would like to raise a basic consideration in conduct of egg and larva surveys. CalCOFI surveys initially were oriented to the studies of the Pacific sardine. This species was found to have a widespread and variable distribution, especially off southern California and along the length of Baja California. We had to survey a wide area of ocean frequently. From the standpoint of evaluating other pelagic fish resources from egg and larval surveys, this was a fortunate circumstance. Fortunately, we began the systematic identifications and enumerations of all our egg and larva material, at the inception of the CalCOFI cruises. We soon came to realize that we were investigating a complex of interacting species. Each had its own season of maximum abundance and distributional range. Furthermore, nothing was static. Temporal and areal distributions of each species changed in response to varying ocean conditions.

It is now my firm conviction that fish egg and larva surveys should never be oriented to a single species or genus. They should be ecologically, rather than species oriented. The whole complex of species should be evaluated. I have no sympathy or rapport with studies that specialize in a particular group of fishes, such as tuna, to the neglect of everything else. It costs so much money to conduct surveys at sea that, in comparison, the monies needed to fully work up the collections of fish eggs and larvae, once obtained, are quite modest sums.

I have gone through several changes of mind in thinking about my topic. At first, I was thinking principally of how informative Norpac had been. Norpac was the Joe Reid inspired, oceanwide survey of the North Pacific (from 20° N lat.) made principally in August 1955. If I were to place a subjective value on Norpac, I should say that it gave me insights that were worth many CalCOFI surveys. I am not derogating our systematic CalCOFI surveys. From these we have not only learned a great deal about our fishery resources, but also have documented changes in their abundance, such as the remarkable increase in the anchovy population. Although informative, limited surveys have to be placed in perspective. We are looking at a small fraction of the Pacific Ocean on our CalCOFI surveys and at partial distributions of most fishes. It takes wider-ranging surveys to delimit these.

But not exclusively oceanwide surveys. We could learn a great deal by increasing our coverage, both to the north and south of the CalCOFI area. I would like to illustrate this by discussing two species, the northern anchovy and the Pacific hake. As you see, I choose to lead gradually to the subject of oceanwide surveys.

Our CalCOFI surveys have pinpointed the importance of two pelagic fishes, the northern anchovy and Pacific hake. The larvae of these two species consistently have been the most abundant in the CalCOFI survey area. However, neither of the distributions of these species is completely delimited by our surveys.

Anchovy:

The distribution of the northern anchovy is "open-ended" at the northern end of our survey pattern. We have fenced it in very well at the southern end of its distribution and at its offshore extent. Unlike the sardine, it does not occur as far north as British Columbia. Furthermore, in 1949 and 1950 we sampled anchovy larvae in moderate abundance off Oregon. We have not been north of California since then on CalCOFI cruises, except on Norpac, so we do not know from larva surveys what the state of the anchovy population is off Oregon, Washington and British Columbia.

It is difficult to generalize from our survey data. For example, we know that abundance of anchovy larvae in plankton hauls decreases markedly as one goes from southern California to northern California. I would like to look for a moment at the data on numbers of anchovy larvae obtained from various parts of California. We usually divide the CalCOFI pattern off California into three areas that we simply term the northern, central and southern California areas. The northern California area extends from the Oregon border to just above San Francisco (station lines 40-57); the central California area extends from San Francisco to Point San Luis (station lines 60-77); the southern California area from Point Conception to the Mexican border (station lines 80-93).

Inasmuch as anchovy abundance has increased over the years, it is necessary to take such increase into account. For this example I am doing this simply by dividing the time span of surveys into two equal periods, the first is the 7 years of 1951–1957 and the second is the 7 years of 1958–1964.

The area off southern California has had the best coverage of any in the CalCOFI pattern. During the decade of the 50's, the area was covered on 9 to 12 cruises a year—average 10.7. The area off central California was covered on seven cruises per year, on the average. The northern California area has been surveyed only about 30 times since the inception of CalCOFI, and the last surveys made in this area were in 1960.

The number of anchovy larvae taken off southern California were 86 larvae per occupancy during the first 7 years, and 210 larvae per occupancy during the second 7 years. The number taken off central California was lower in both periods, but a marked increase in abundance has been evident on the station lines adjacent to the southern California area. Numbers of larvae taken on lines 70-77 averaged seven larvae per occupancy in the earlier period, but 100 larvae per occupancy during the latter period. The upper portion of this area, off San Francisco to Monterey, has been less productive—the increase being from less than one larvae per occupancy on the average to just over 10 larvae per occupancy.

Few larvae have been taken off northern California at any time. During the decade of the 1950's, anchovy larvae were taken in 17 of the 267 hauls made in the area; the average number per occupancy was only about 0.4 of a larva.

Since we have not surveyed this area since 1960, we do not know if numbers are now increasing there. Also it is dangerous to assume that because numbers are low off northern California they will also be correspondingly low off Oregon and Washington. Anchovy larvae could be markedly more abundant in the water off Oregon and Washington than off northern California. There is even the possibility that anchovies in the Pacific northwest constitute a separate genetic stock. We are looking into the latter possibility using blood antigens. The former can only be determined by systematic surveys for fish eggs and larvae off Oregon and Washington.

We are intensifying our surveys in the CalCOFI area during this coming year (1966) in order to have a base year of data about fish egg and larva abundance at the same time that a controlled anchovy fishery is begun. The surveys will not cover any area north of San Francisco. Obviously, to know the state of the anchovy population over its whole distribution we would have to survey the ocean off the Pacific northwest as intensively as we have between San Francisco, California and Magdalena Bay, Baja California. I would like very much to stimulate such coast-wide surveys.

The Pacific hake is of more immediate interest in the Pacific northwest. A fishery for that species is getting underway there. Hake are only seasonally present in the shelf waters off Washington. There is still speculation as to where they go during the off season. Do they move south to spawn in waters off California and Baja California? Or do they merely move offshore to spawn in waters off Washington and Oregon?

Since there is a large population of hake in shelf water of Washington in the summer and fall months, it should be an easy matter to determine if they move offshore to spawn. If they do, there should be plenty of eggs and larvae to sample. I would like very much to see this point resolved. My guess is that hake will not be found to spawn off Washington in most years, but that they may spawn there when the ocean is unusually warm.

This educated guess is based on several lines of evidence:

One of these is the distribution of hake eggs and larvae within the CalCOFI survey area. In most years, the greatest number of hake larvae are taken off Baja California rather than off California. The percentage of the CalCOFI total of hake larvae taken off Baja California has been as high as 97.6%. This was in 1956, a year colder than average. In contrast, 70% of hake larvae were obtained off California in 1958 and 90% in 1959, during warmer-than-average years. Hence, it appears that the distribution of hake spawning shifts markedly in response to ocean conditions.

Another line of evidence is the temperature range over which hake eggs and larvae are collected. Our data are from vertical distribution studies and from regular CalCOFI surveys. Hake larvae have been taken over a wide temperature range—8–16° C with most occurrences between 10.5–15° C.

As part of our study for the Atomic Energy Commission, we have looked at our larva from the Point Arguello area fairly intensively. Point Arguello is immediately north of Point Conception. We define the Point Arguello area as that within a 75-mile radius of the Point. Hake larvae were common in the Arguello areas only during years with warmer-than-average temperatures, especially 1958 and 1959. During these 2 years most temperatures, at depths where hake larvae occurred, were above 10° C.

I have spent some time looking at water temperature data from off Oregon and Washington at 75 and 100-meter depths—the depth range at which we take most hake larvae in the CalCOFI area. In a cold year, such as 1956, winter and spring temperatures at these depths ranged between 6 and 8° C. In 1958, water temperatures occasionally were above 10° C, but most observations were between 7° and 9° C. It seems to me that water temperatures in the Pacific northwest ordinarily are too low for hake spawning. To substantiate or disprove my conclusion, we need only systematic surveys for fish eggs and larvae off Oregon and Washington.

I have given my argument for more extensive egg and larva surveys in the eastern North Pacific which raises an interesting point. Such surveys are not particularly needed for hydrographic observations which have been made with fair frequency. The problem is simply that systematic collections of fish eggs and larvae were not an integral part of those surveys. There is an education problem here. We must sell fishery oceanographers on the value of egg and larva surveys for resource-evaluation.

Now to discuss ocean-wide surveys. I would like very much to see the equivalent of Norpac repeated in all seasons, but with more systematic coverage and with uniform methods of sampling fish eggs and larvae. Furthermore, to answer some of the questions I am going to pose, it would be necessary for one person to examine collections from all parts of the Pacific. The studies of fish eggs and larvae should be centralized, not particulated.

Broad-scale surveys are, of necessity, cooperative. Financing for such surveys does not come easy; surveys have to be justified. If we are to launch repeat Norpacs, I think that support will have to come from major fishery investigations, such as those dealing now with temperate tunas.

In this area, fortunately, some spadework has been done. The need for such surveys was recognized in one of the resolutions agreed upon at the FAO-sponsored "World Scientific Meeting on the Biology of Tunas and Related Species," held at La Jolla in July 1962. The resolution, numbered 10, is titled "Cooperative Study of Albacore and Bluefin Tuna in the North Pacific Ocean." One of the recommendations is for "further cooperative oceanographic surveys of the North Pacific Ocean such as the Norpac and EQUAPAC expeditions, for the purpose of obtaining synoptic coverage, preferably at all four seasons of the year."

We know little about spawning season and areal distribution of eggs and larvae of the two temperate tunas. In fact, there is some question as to whether we are able to identify the larval stages of the albacore. I am certain that this is a problem that would be resolved if we had adequate material.

The bluefin and albacore are two species of North Pacific fishes that travel across the Pacific. We know this from the capture off Japan of fish tagged off California and Baja California and vice versa. We do not doubt that both species must eventually be studied on an oceanwide basis. There are other species, common as eggs and larvae in CalCOFI collections, that we suspect must also have oceanwide distributions. Two species of particular interest are jack mackerel and Pacific saury. We have taken jack mackerel eggs and larvae 1,100 miles at sea off Washington in the Norpac collections. We have looked at fish eggs and larvae collected by the Bureau of Commercial Fisheries, Honolulu, on Norpac to see if we could extend the distribution of jack mackerel further seaward. Their samples were from the mid-Pacific. We found no jack mackerel eggs or larvae in these samples, but we did find saury eggs. Hence, present evidence supports the trans-Pacific distribution of saury, but it is inconclusive for jack mackerel. Additional systematic Norpac-like surveys, especially during the spring season, would permit better evaluation of jack mackerel distribution.

Having made my justification with bread and butter species (and, parenthetically, all of these offer very interesting problems), I will proceed to discuss taxonomic and distributional problems with some of the simon-pure species—those without any potential except their ecological role. There are many such as these, but I wish merely to select a few examples.

Among the abundant kinds of fish larvae we sample in the CalCOFI area are those of deep-sea smelts. The common deep-sea smelts belong to two families, Bathylagidae and Argentinidae. The larvae of both families are easier to identify to species than are the adults. This is one of the groups in which larval taxonomy is a distinct aid to adult taxonomy.

We had an excellent demonstration of this from material obtained on one of the first, wide-ranging cruises made by Scripps, "Northern Holiday," which worked into the Gulf of Alaska near the Aleutians. On looking over the fish eggs and larvae from the cruise, I was pleased to find eggs and larvae of a species of *Leuroglossus* quite distinct from those of the common *Leuroglossus* in the CalCOFI area. These eggs were half again as large as those of the CalCOFI. They went through a somewhat different embryonic development. The larva had more pigment than *Leuroglossus stilbius* larvae, but, most striking, they had 10 vertebrae more. They obviously belonged to a quite distinct species. These differences were not as apparent to ichthyologists working with adults. A Russian scientist first described the northern form as a subspecies of *Leuroglossus stilbius* under the name *schmidti*. An American ichthyologist proceeded to synonymize the two, finding no important differences. Neither of these scientists had looked at all trenchant characters, especially the number of vertebrae. When these characters were pointed out, the northern species was recognized as a valid one.

Interestingly, it is apparent that this northern species of *Leuroglossus* is the same fish that was described from skeletal material taken from fur seal stomachs by Lucas in 1899. He gave his skeletonized fish the name of *Therobromus callorhini*. Our northern species of *Leuroglossus* should be known as *Leuroglossus callorhini*. This first example is drawn from a problem that larva surveys helped resolve, but there are many more yet to be resolved.

Some of such problems are in the related family Argentinidae. From studies of larvae, we know we have four species of Argentinidae belonging to three genera in the eastern north Pacific. One of these which poses no problems is a species with a localized, neritic distribution, *Argentina sialis*. A second, *Mic-*

rostoma microstoma, is a widely distributed species, which occurs on both sides of the Pacific, in the Atlantic, and Mediterranean. The other two belong to the genus *Nansenia*; one has a subtropical to tropical distribution, the other a subarctic to temperate distribution. At least four species of *Nansenia* have been described from the North Pacific, two from the western Pacific, two from the eastern; one or more of these may be synonyms. Here again, larvae from both sides of the Pacific would help us resolve such problems. The larvae of *Nansenia* also are more distinctive than their adults.

Larvae are useful in helping to resolve taxonomic problems in other groups of pelagic fishes, for example, *Scopelosaurus*. This is an interesting deep-sea fish that is a curiosity as an adult. A few species have been described, from the Atlantic, South Pacific and off Japan. Adult material is limited to one or two specimens per species, these usually in poor condition. One species of *Scopelosaurus* is rather common in the CalCOFI area in larval form. It took us some time to figure out what kind of fish it was, since so little is known about adult *Scopelosaurus*. When we compared the larva material from the CalCOFI with larvae from Norpac, on Shellback, and off Peru and Chile on Step I, we found that there are some six species of *Scopelosaurus* in the eastern and central Pacific between California and Chile. This is another one of the groups of fishes that has very distinctive larval characters. The problem remains of what to call our CalCOFI species and most of the others. A species of *Scopelosaurus* was described by Mead and Taylor from a very young juvenile specimen taken off Japan. It could be the same species as occurs off California. This could be settled if larvae were available from all of the north Pacific Ocean.

Such examples could be multiplied, using larvae of myctophids, paralepedids, etc. There are a multitude of problems that could be clarified if adequate material were available for study from trans-Pacific surveys.

DISCUSSION

McGowan: If the objective of your proposed program is to study the total ecology of larval fishes, won't it be necessary to include in it organisms other than fish larvae? Usually the larval fish make up a very small proportion of the total number of individuals present in a plankton tow. This indicates that larval fish are living among an overwhelming number of invertebrates which are potential predators, prey and competitors.

J. Johnson: The Coast and Geodetic Survey will soon start an expanded program of routine surveys of physical properties of the oceans. Recently there has been considerable discussion on what biological sampling should be included in the C&GS ocean survey program. I should like to know what the consensus of the scientists in this group is as to how worthwhile plankton sampling would be on the ocean survey program. Should samples be collected even though there might be no immediate plan for their analysis?

(The answer seemed to be that it would be worthwhile to collect the samples.)

Stewart: For some time now, we have been deliberating whether to barge ahead and make such tows routinely or to wait for specific requests for specific types of observations in certain areas. When we have gotten such requests, we have filled them. The special daily tows for tuna larvae and copepods made from the PIONEER at the request of the Bureau of Commercial Fisheries' Honolulu Laboratory is an example. NASCO and others have reiterated the need for data for zoogeographical studies, but requests to BCF and to individual biologists to come up with a definition of exactly what they want to have, have not yet resulted in a biological survey component of the ocean survey being developed. When I approached BCF Honolulu in 1962, the answer I got was—'Don't take any more North Pacific plankton samples for us, we have more now than we can process'—so we did nothing on it. I maintain that the samples should be stored in the ocean rather than on a shelf somewhere if nobody is going to work them up.

Longhurst: Isn't the problem about whether or not to take a plankton sample rather one of curating than processing? Plankton samples taken when opportunity offers may not cost much to collect on top of the cost of an expedition and should cost little to curate if properly organized; yet a library of such samples can be of enormous use to future work.

McGowan: Since a zooplankton sample represents an ecological situation which exists at a particular place and time, no sample which has been properly collected should be thrown out; further, no opportunities to collect samples should be missed.

Blackburn: The sorting of planktonic fish stages can be kept reasonably current, provided that there are people with a lively interest in the information and that they are not snowed under with other jobs. If such work is important, it should be staffed in an adequate way—qualitatively as well as quantitively.

M. Johnson: With respect to (1) the accumulation of plankton samples which have not been adequately analyzed for their contents and (2) the reluctance to add to this burden by more plankton collecting because of lack of plankton sorting personnel and facilities, is it possible or practical to send samples to the Smithsonian Sorting Center in Washington, D.C., for sorting?

In light of the huge and growing plankton collection on this coast, I should like to propose that consideration be given to establishing a sorting center in this area.

Chapman: There is an International Sorting Center now in operation at Cochin-Ernakulum, Kerala State, India. It is working quite well. The United States has tens of millions of dollars' worth of rupees resulting from PL480 grain and other food shipments. They must be spent in India. The Sorting Center in Cochin requires additional support. Why can it not be given this from the PL480 rupee fund, plankton samples air-shipped from here to there for sorting under contract, and shipped back sorted with the only new cost to the United States being the air freight?

SUMMARY BY THE CHAIRMAN

JOSEPH L. REID

In trying to summarize what has been learned today I face the problem that all instant summarizers face—it's hard to be correct and harder to be brief, but hardest to be both correct and brief.

We have heard a lot about wide-scale studies. Some of these studies are being carried out, though perhaps in very preliminary stages. Others are being considered and planned; others are being considered, and still others have not got this far even: they are being proposed, discussed or just dreamed about.

But all of these levels are perfectly natural stages in the development of research, and in fact they occur in the reverse order to what I have listed.

Jerome Namias' work at the moment is at almost the largest possible scale—a hemisphere. He is one of the few who are really trying to do large-scale comparisons and fitting together of atmospheric phenomena. What he is asking for is not a larger scale so much as a more intensive and continuous array of data. And we can see how it will soon be possible to help him by giving him closer arrays and more nearly continuous time series. Our techniques are improving and though they have not reached the ultimate they are much better than 10 years ago.

Harris Stewart has talked about some data collections which are going on now in a systematic way, and will be expanded, and can contribute to many fields of ocean research. The ocean is being covered by research vessels now, more than ever before, and we must learn to use them all.

It seems likely that the most rapid increase in data coverage through national and international cooperative efforts will be in the meteorological field. There is an obvious need for such material and the weather people seem to recognize now that the conditions over the ocean cannot be neglected if they really want to understand even the land weather. Such meteorological observations at sea will also include sea water temperatures at the surface, and all of these measurements can be considered now as standard observations, with no really new techniques required if all they want is an increase in coverage of the older forms. Any ship at sea can be considered as a weather ship: the equipment required for the ordinary observations is not expensive and the training of the crews is a simple job. Conscientiousness in such measurements is quite a different affair but a continuing, vigorous program can develop it.

With buoys the problem is not one of conscientiousness in old techniques but ability to devise, fund and employ new ones. It really seems that we ought to be able to do this.

Hugh McLellan has given us a list of four experiments which might be carried out with present ideas and a better array of data. Each of these, and the various others which might be proposed, require careful consideration. Any experiment of this scale must be judged in advance in terms of the quality of the ideas behind it and exactly what it might tell us, our ability to carry it out with present equipment, and how much scientific manpower and how much money would be expended which might be used in other programs. This sort of judgement is not easy and it should not be made lightly, since failure in one large, expensive effort might inhibit further opportunity to experiment. On the other hand too great a fear of failure can do just as much damage, and with a greater certainty.

But other ocean-wide studies, particularly the biological ones, have endured two major disadvantages. One has been a narrower base of support. A Kansas farmer may care about studying weather but he may not be enthusiastic about spending money to study fish. This disadvantage is slowly being overcome, since the world may really require things from the ocean—such as protein—that the Kansas farmer cannot provide for the same price.

The other disadvantage which the biological studies have endured may stem from the first. With limited support truly oceanic studies of the biomass had not been possible until recently, and the techniques developed by individual researchers for particular areas and biological forms had not produced any acceptable standard for ocean-wide use. Progress has certainly been made upon this. The various attempts to standardize net hauls and productivity measurements made on the last few international cooperative expeditions are steps leading toward a rational solution of this problem.

Tim Parsons has given us some examples of what sort of analysis can be made from the data of Station "P" and the various expeditions into the Northeastern Pacific. He has discussed time-variations of the population and the growth rate of certain organisms in terms of the critical depth (a function of light intensity) and the depth of the mixed layer of the ocean. He and Mr. LeBrasseur have had a considerable success in relating these quantities. The depth of the mixed layer is fairly well known in their area, collections of the organisms have been made in various seasons, and the continuous records at Station "P" have made it possible to handle the light-intensity variations in a rational manner over the rest of the area. Though far from being all they could ask, the data are sufficient for Parsons and LeBrasseur to form a reasonable hypothesis about the different seasonal growth-rates between the Northeastern Pacific and the Northeastern Atlantic oceans.

To improve this sort of study these two investigators require better information about layer depth, radiation, extinction coefficients, and the organisms. Some of this may come from satellites or aircraft, but other parts require, at least at this stage, work by surface vessels.

Dr. Ahlstrom has pointed out once again that many species of fish have very wide distributions in the ocean and that our knowledge of the limits of these distributions can be extended considerably by wide-scale collections of eggs and larvae. In effect this is an indictment of the work of many of our expeditions for not having made net hauls. If such net hauls had been made regularly, there might already exist a collection of plankton that could help to answer some of the questions Dr. Ahlstrom posed. It is a point well worth remembering when any large expedition is being planned.

In the title of this Symposium and in the titles of the various papers there is an implicit question: are oceanographers interested in carrying out wide-scale studies of the ocean? Though all of the speakers have been prudent enough to point out the difficulties of such work, there is no doubt that all of them believe that we do stand to gain tremendously from a wide-scale application of ideas and techniques now at hand.

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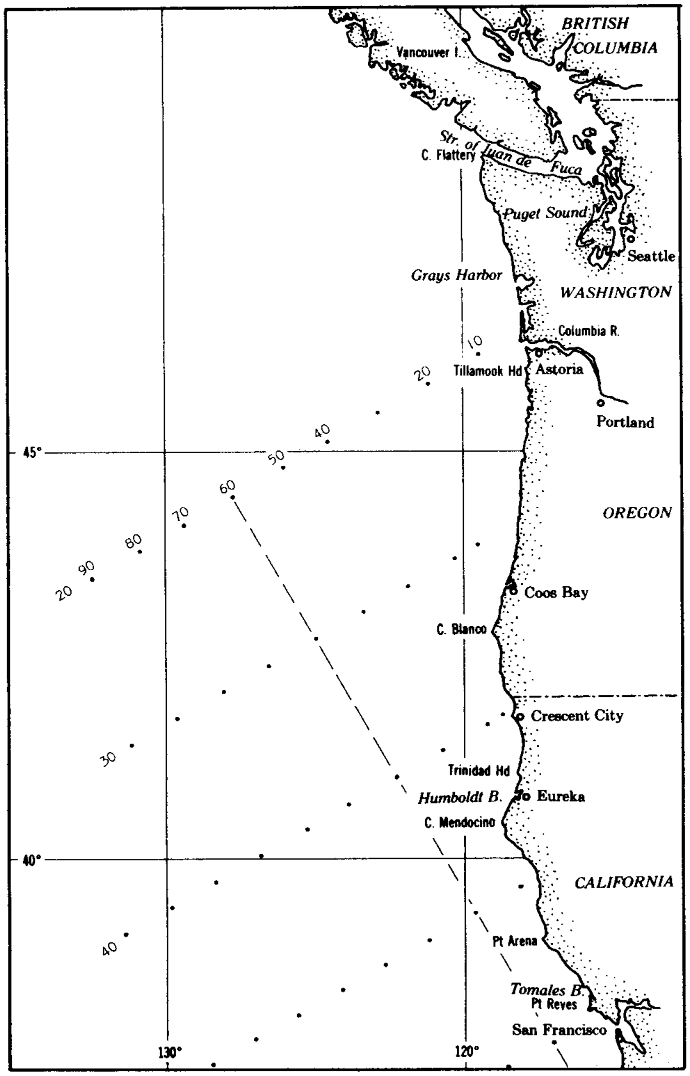

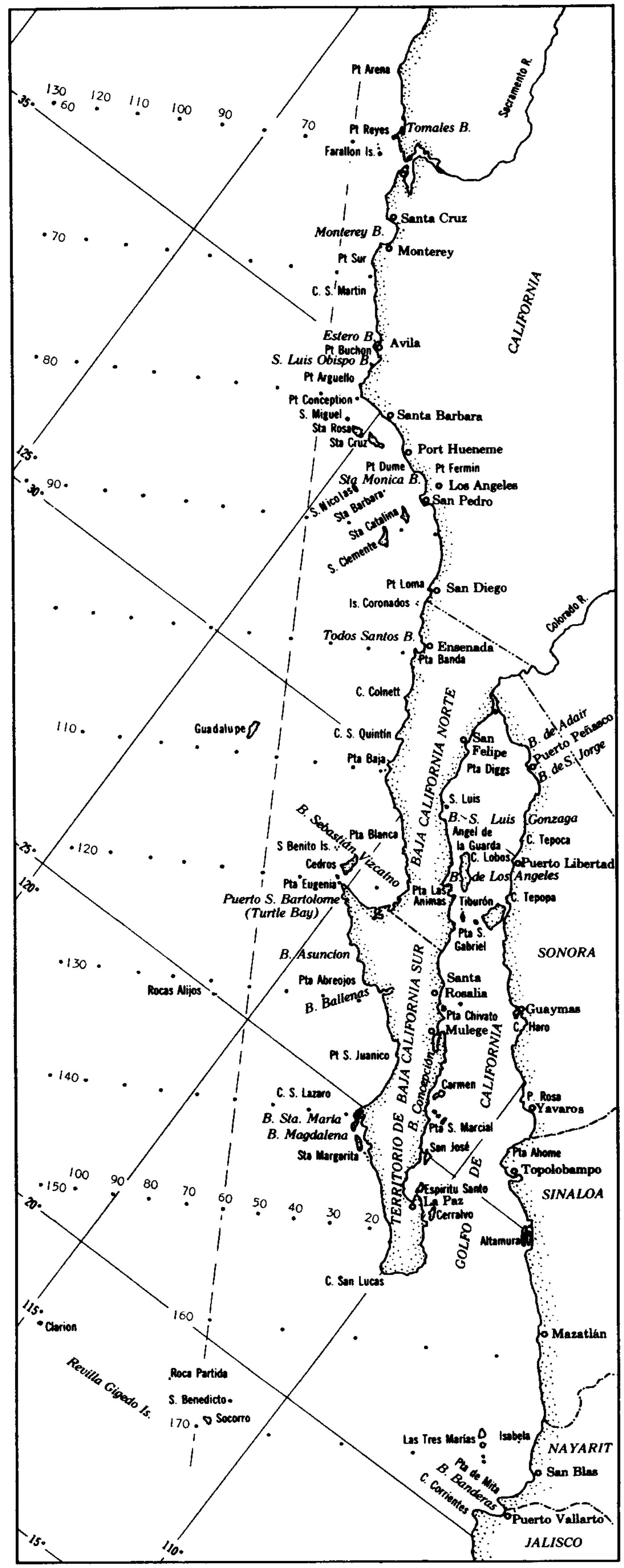

These maps are designed to show essential details of the area most intensively studied by the California Cooperative Oceanic Fisheries Investigations. This is approximately the same area as is shown in red on the front cover. Geographical place names are those most commonly used in the various publications 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.

CONTENTS

Page

I. Review of Activities 1 July 1966–30 June 1967

Report of the CalCOFI Committee 5

Agency Reports 10

Review of the Pelagic Wet Fisheries during the 1966–67 Season 22

II. Symposium on Wide-Scale Studies of the Ocean

Joseph L. Reid, Editor 25

Introductory Statement

Joseph L. Reid 27

Long-Range Forecasting of the Atmosphere and its Oceanic Boundary—An Interdisciplinary Problem

Jerome Namias 29

Ocean-Wide Surveys, both Meteorological and Oceanographic

Harris B. Stewart, Jr. 43

Some Future Trends in Ocean Research as seen by the Geophysics Branch of the Office of Naval Research

Hugh J. McLellan 50

A Discussion of Some Critical Indices of Primary and Secondary Production for Large-Scale Ocean Surveys

T. R. Parsons and *R. J. Le Brasseur* 54

What Might Be Gained from an Ocean-Wide Survey of Fish Eggs and Larvae in Various Seasons

Elbert H. Ahlstrom 64

Summary by the Chairman

Joseph L. Reid 68