PHYSICAL OCEANOGRAPHY, 1947–1987

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The charter of the Marine Life Research Program, as stated in 1958 by Roger Revelle, Director of the Scripps Institution of Oceanography, is as follows:

> The objectives of the Marine Life Research Program at Scripps are to support, foster, stimulate, and carry out coordinated investigations leading to an understanding of the continuity and changes of the nature, environment, ecology, and general biology of the pelagic fishes and associated organisms of the eastern North Pacific . . . .

I would like to review for you something of what has been done, beginning with the earlier work. Most of you were not active in this field when the Marine Life Research Program began, and therefore it is necessary to tell you that techniques and instrumentation were quite primitive at that time. Meteorological data over the ocean were very few, and without computers even these few had to be arrayed by hand.

What was known or proposed about the ocean, and about the California Current in particular? George Davidson had published several papers dealing with the flow, and Richter (1887) had studied coastal temperature. Many of the major concepts about ocean circulation, including the California Current, are of long standing. Early in the century, Thorade (1909) related the temperature to the general circulation; George McEwen (1912) discussed upwelling as a consequence of Ekman transport, and later the effect of the circulation upon climate; and Marmer (1926) presented measurements of coastal currents made from lightships.

Skogsberg (1936) noted that near the coast the water at 50–100-meter depths was warmer from December through February than in summer; he attributed this to summer southward flow and upwelling and the Davidson Current in the winter, which he called the oceanic period.

Sverdrup and Fleming (1941) discussed the circulation of the Southern California Bight, and noted that the conventionally accepted notion of uniform upwelling and offshore flow seemed to be interrupted by eddies at the outside edge of the upwelled water. Tibby (1943) presented maps of geostrophic flow covering the area from northern Washington to Punta Santa Eugenia. McEwen (1948) published a study on eddies in 1948.

These gave the concept of a California Current moving southward, an inshore flow that was southward in summer, above a northward countercurrent that was present during most of the year, and a surface northward flow in winter along the coast. The southward surface flow was geostrophically balanced and was therefore accompanied by coastal upwelling that began in March or April off central Baja California and moved northward, with its maximum effect off northern California in July and August. That the deeper waters in the countercurrent, or undercurrent, had come from farther south had been shown by their higher temperature and salinity, their nutrients, and their lower oxygen. Eddies had been noted and discussed by Sverdrup and Fleming, and in a paper written by McEwen (1948) on their nature, though he emphasized the semipermanent Southern California Bight circulation rather than the smaller-scale and less regular features to the north and south.

This represents what had been learned or conjectured about the California Current at the time CalCOFI began. Sverdrup had had a major part in proposing CalCOFI, but both Sverdrup and Fleming, who had contributed so much, had left Scripps before CalCOFI went to sea for the first time.

Carl Eckart was director of Scripps at the time the field work began, and Roger Revelle was on hand to manage it. Dale Leipper was in charge of the physical oceanography, aided by Bob Reid and later by Paul Horrer. Dave Carritt and Warren Wooster handled the chemistry. An early method of chlorophyll measurement was proposed by Marston Sargent, but it was not satisfactory and was soon dropped. The biological sampling was in the hands of Laurie McHugh at Scripps and Ahlie Ahlstrom of the U.S. Fish and Wildlife Service.

Scripps acquired the two ships *Horizon* and *Crest*, which, with the *E. W. Scripps* and the California Fish and Game vessel *N. B. Scofield*, carried

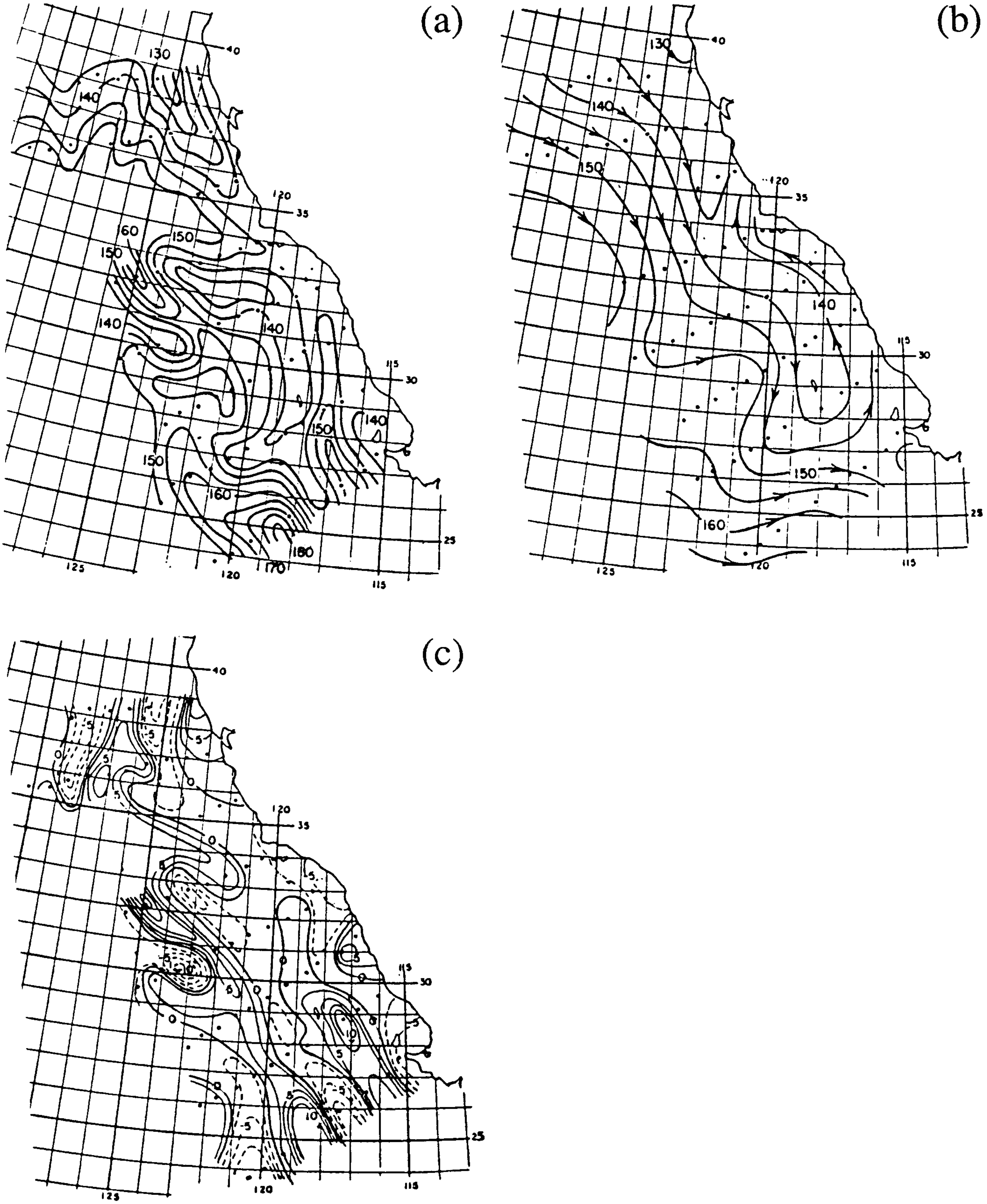

Figure 1. Marine Life Research Cruise 1 (Feburary 28–March 15, 1949): (*a*) dynamic height anomalies (0 over 1000 decibars); (*b*) dynamic height anomalies after elimination of tidal effect; (*c*) deviations in dynamic heights caused by tides. Contour interval 2½ dyn cm. Dashed lines indicate negative values. (Defant 1950a).

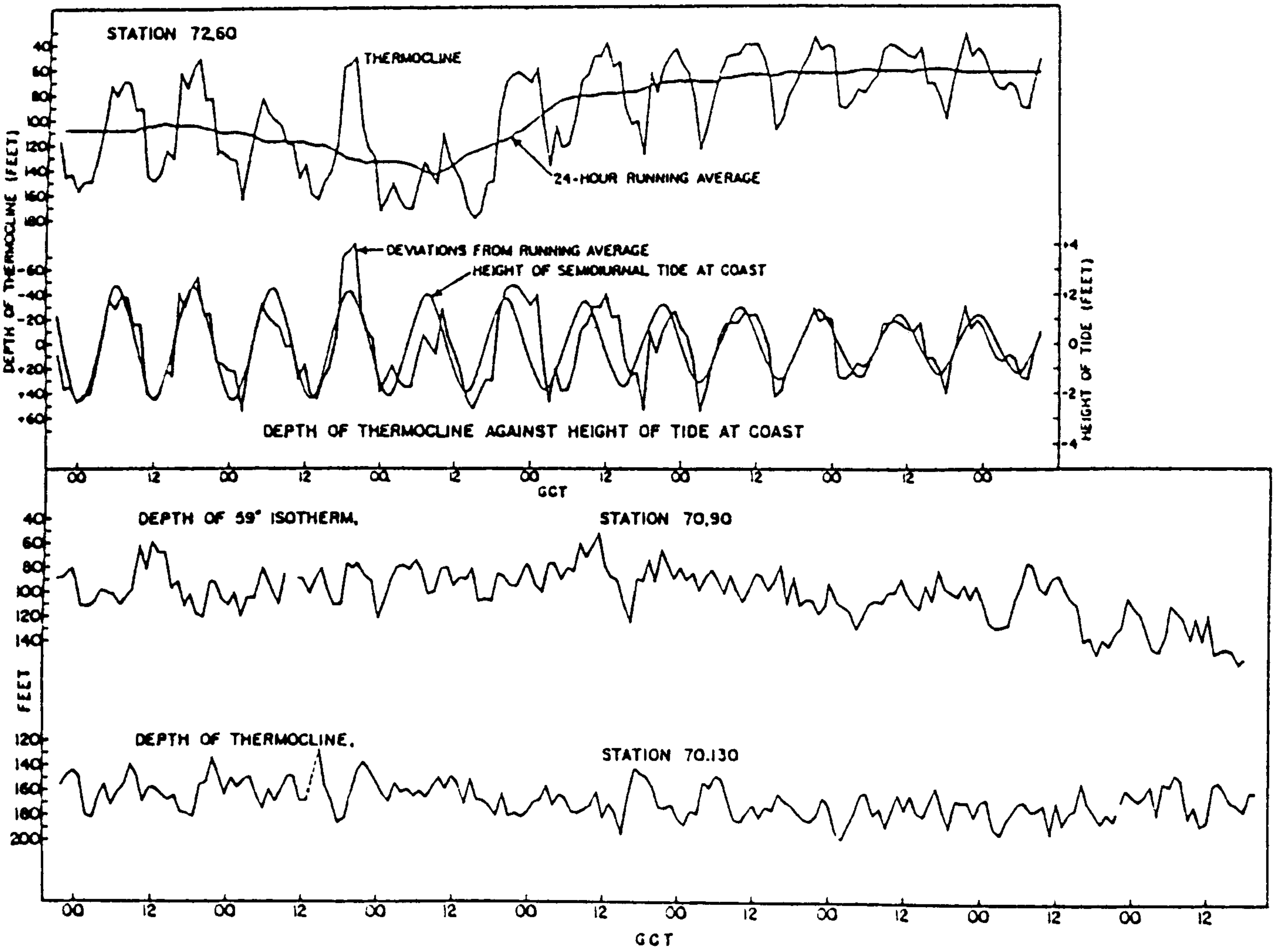

Figure 2. Data from the three anchor stations in October 1950 (Reid 1956).

out the first cruise in March 1949. Setting up the operation for three or four ships at once, in a period of about six months, was not easy. From a core of one experienced technician and two new hires in June of 1948 and enough gear for one ship, they had to expand rather quickly to equip and man three ships by March of 1949. Much of the work on the first cruise was done by students (I was one of those) and a few professors.

Thus began the field work that later became known as the CalCOFI cruises. It was based at first on an extension of work by earlier investigators, and its first pattern had stations widely spaced to permit broad coverage, and to allow the work to be carried out by small technical parties. During the first year it became clear that closer spacing was needed, and intervening lines of stations were added; also the stations were more closely spaced inshore. The requirements of broad-area coverage and small technical parties did place a lower limit, however. Quite often it was not possible to measure both oxygen and phosphate. Vertical spacing and depth range of samples changed from 12 samples in the upper 1000 meters in 1949 to 15 in 1950, to 16 samples in 500 meters in 1953, 17 from 1954 through 1959, and 18 samples from 1960 through 1981; it is now 20 samples in the upper 500 m.

This program, which was to deal with biology as well as physical and chemical oceanography, was planned for a broad coverage of the California Current in order to study the major circulation and its seasonal and year-to-year variations, including upwelling and the countercurrent, and the relation between these and the organisms over a substantial

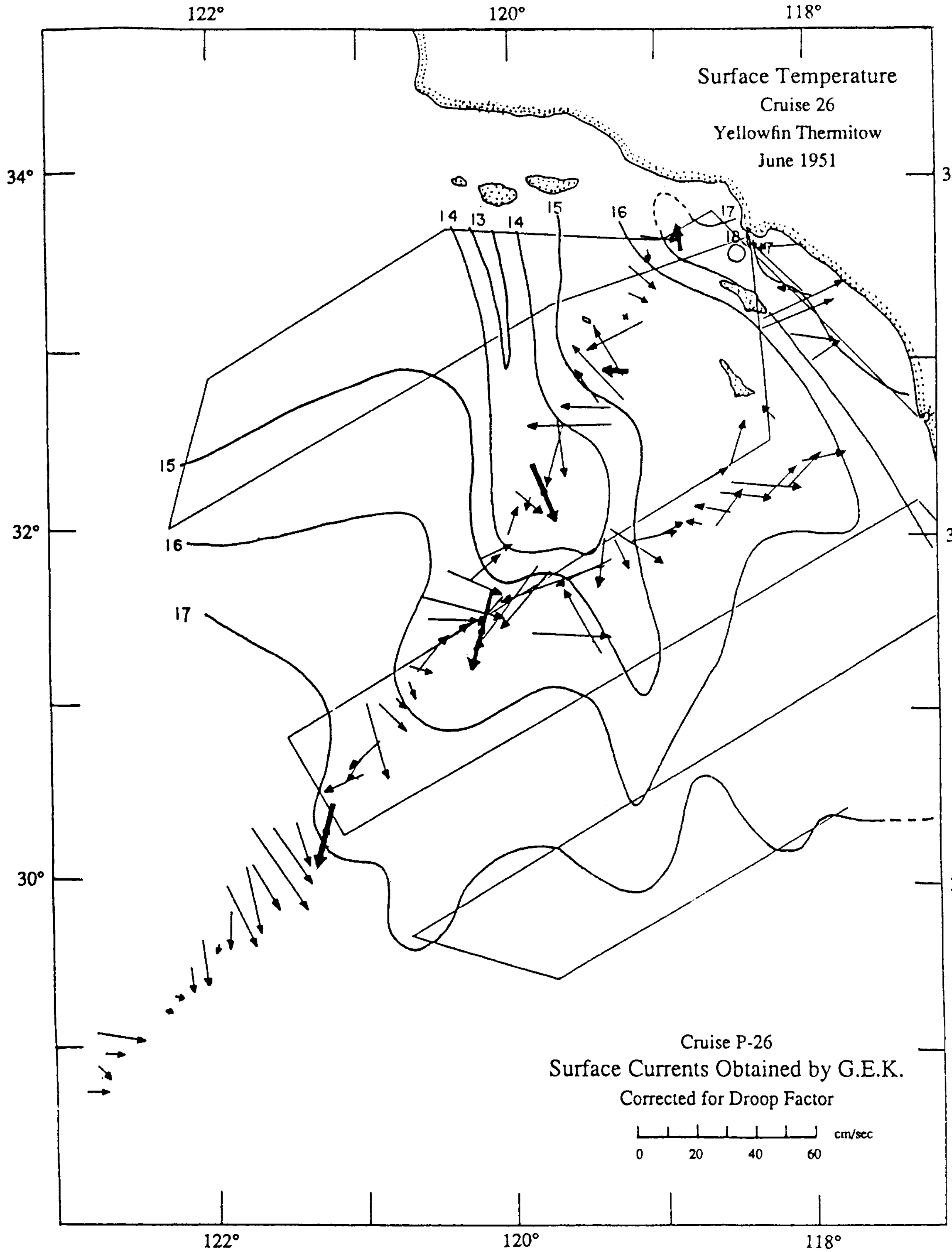

Figure 3. Surface temperature, from the California Department of Fish and Game vessel *Yellowfin* thermitow measurement, in June 1951, and surface currents obtained by GEK on the Scripps vessel *Paolina T* in June 1951. Heavy arrows indicate 24-hour averages (SIO unpublished).

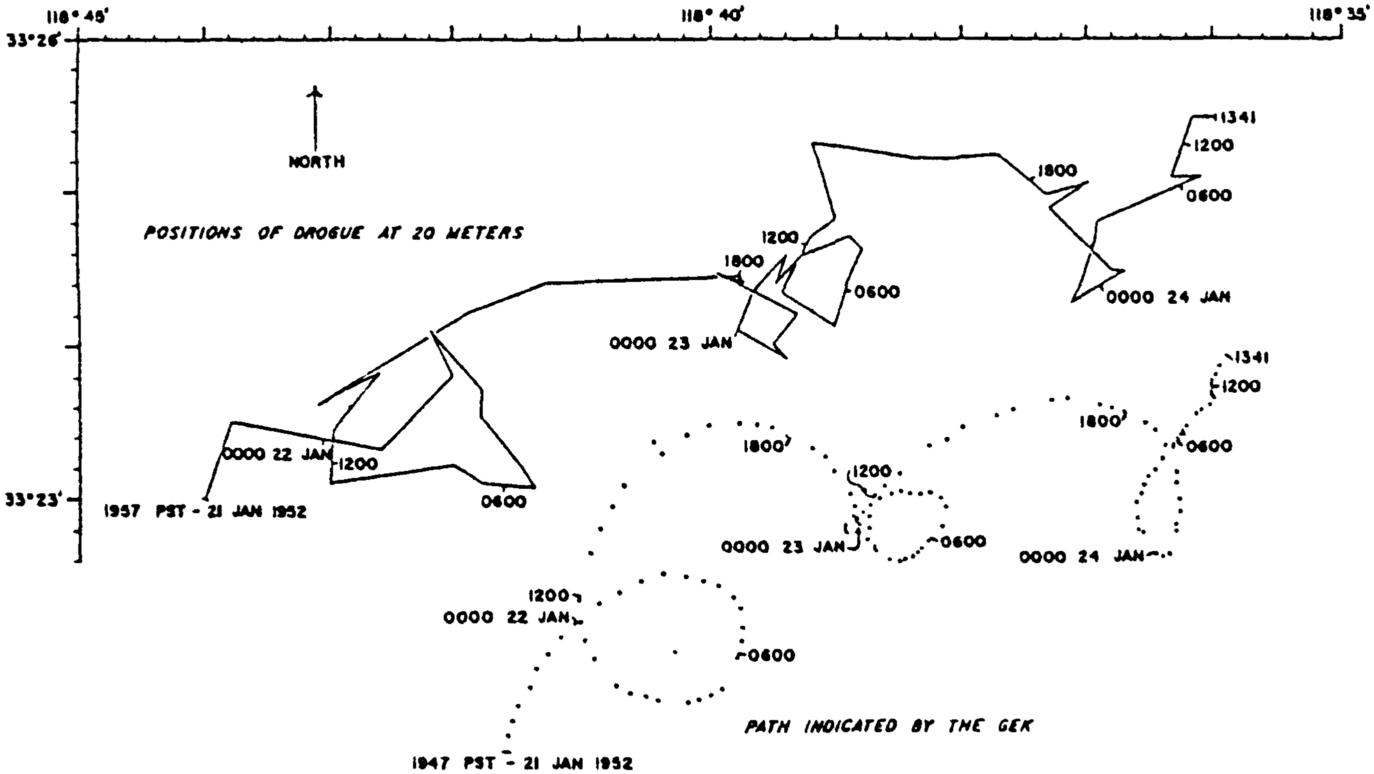

Figure 4. Trajectory of drogue near Santa Catalina Island and path indicated by the integrated GEK velocity measurements (starting points offset to avoid overlap) (Reid 1958).

area. Among the first published papers based upon the 1949 data were studies of upwelling by Yoshida (1955 and 1958) and by Yoshida and Mao (1957). Yoshida continued his interest, and one of his last papers (1980) dealt with coastal upwelling.

The circulation as revealed by the dynamic topography showed various irregularities, and these were at first ascribed to the effect of internal waves of semidiurnal period. Albert Defant (1950a,b) spent six months at Scripps in 1949–50 studying the data, and attempted to develop a method of eliminating the tidal effect. He took part in a brief anchor-station study in January 1950. Data from the first cruise (Figure 1a) showed various bumps and troughs. After Defant had applied his methods to the data, the pattern was much smoother (Figure 1b). The adjustments he made, which he interpreted as internal waves, were as large as 10 dynamic centimeters (Figure 1c).

After Defant left, I was assigned the job of continuing his method. Anchor stations were carried out in October 1950 with three ships (Figure 2). These stations did not show enough amplitude inshore, where the waves were clearly recognizable. Offshore the signal was not clearly present and, in any case, was of even lower amplitude. But it did not seem to account for the amplitude of some of the various smaller-scale features in the data. It was obvious that these features were not simply internal waves, but were something else. Several drogue studies were carried out, but it was not possible with the techniques available in 1952 to follow drogues except by ship, and this could not be done for more than a few days. Positions accurate enough to determine velocity during only a few hours could be determined only by coast piloting, within sight or radar range of the coast or of islands.

In early 1950, a new instrument—the Geomagnetic Electro-Kinetograph (GEK, or jog-log)—became available. In case you don't remember, this instrument allowed measurement of surface current from a ship underway. The measurement required a brief excursion from the ship's course, and we made the measurements hourly underway during many of the cruises. Some results of such measurements made in 1951 are shown in Figure 3. The thin arrows are single measurements made underway, and the wide arrows are the average of 24 hours of measurement in one position. It is clear

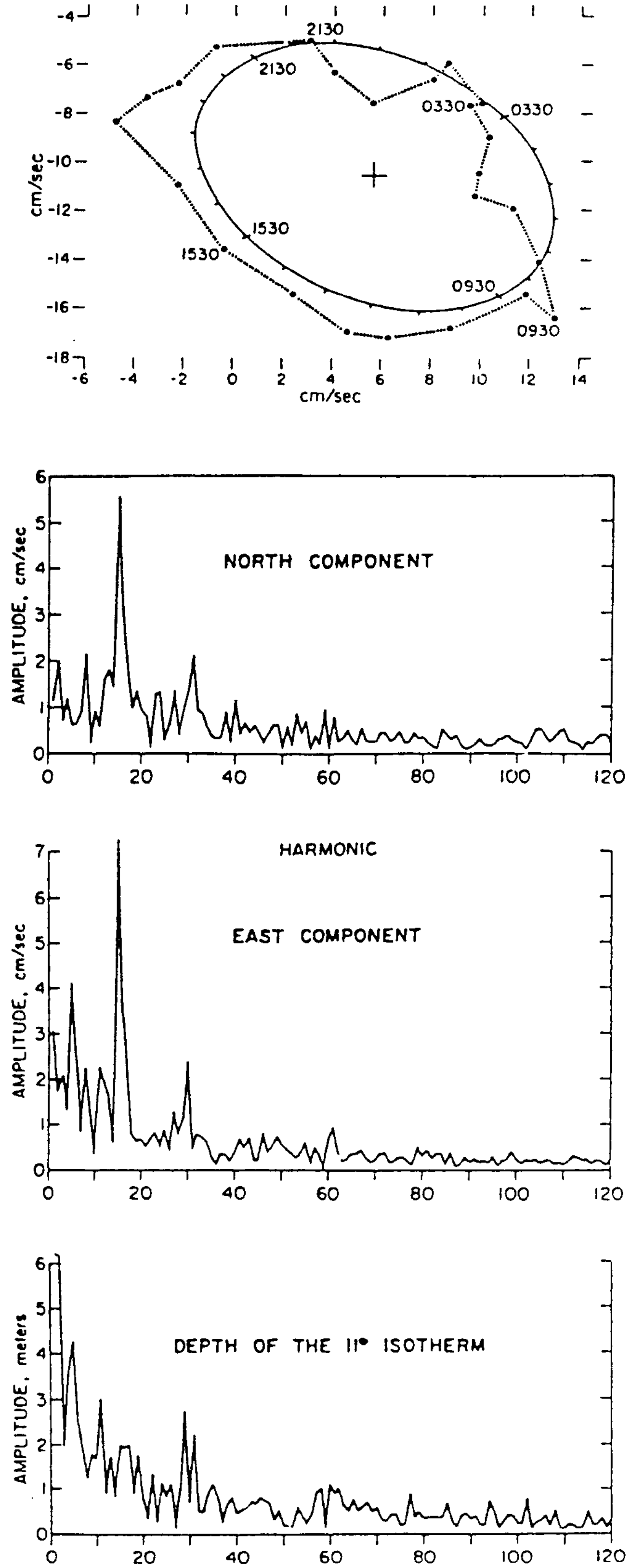

Figure 5. *Above*, measured currents averaged over 15 days for the diurnal cycle (*dots*), and the results of the harmonic analysis (*smooth curve*). *Below*, amplitudes of the first 120 harmonics of the north and east components of the current and of the depth of the 11° isotherms (Reid 1962a).

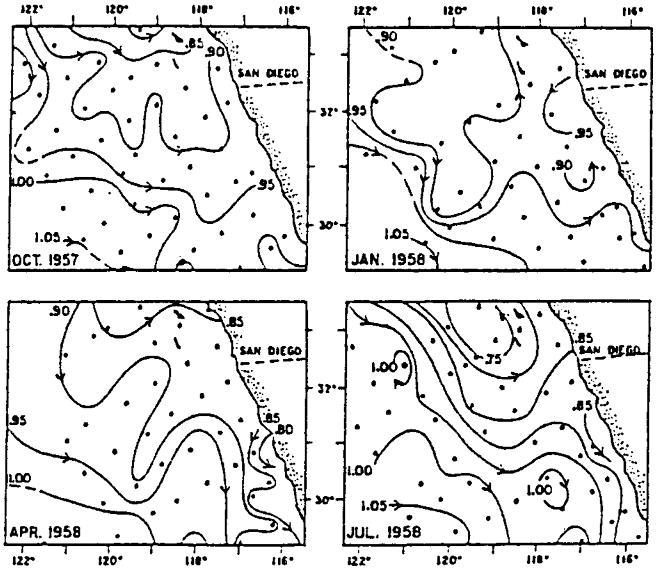

Figure 6. Surface flow (steric height of the sea surface with respect to the 500-decibar surface, in dynamic meters) of the California Current in four seasons (Reid et al. 1963).

that the space and time variations are so large that single measurements made hourly underway do not define the mean large-scale flow.

As a check on the accuracy of the measurements, two series of GEK measurements were made around a drogue (Figure 4). Rather ragged fixes of position were made from bearings taken on Santa Catalina and San Clemente islands, using a magnetic compass on the ship's bridge and a bearing circle on the forecastle. The results seemed to find the GEK a little short, but the diurnal and semidiurnal paths real. GEK measurements were made for 15 days at one position—30°N and well offshore—to investigate the inertial flow, which should have a period of 24 hours there, possibly augmented in amplitude by the tide (Figure 5).

Thus the flow field seemed even more complicated and peculiar than the earlier cruises had indicated. There seemed to be oscillations of semidiurnal and diurnal or inertial periods. Horizontal oscillations were both inertial and semidiurnal inshore but predominantly inertial offshore. Vertical oscillations had already been seen to be predominantly semidiurnal but of lower amplitude offshore than inshore. Wave length could not be established, and series of GEK observations along the tracks of the CalCOFI cruises could not be resolved into a coherent flow pattern. The acoustic Doppler log seems not to have this problem, though it is measuring much the same quantity. I haven't yet worked out why.

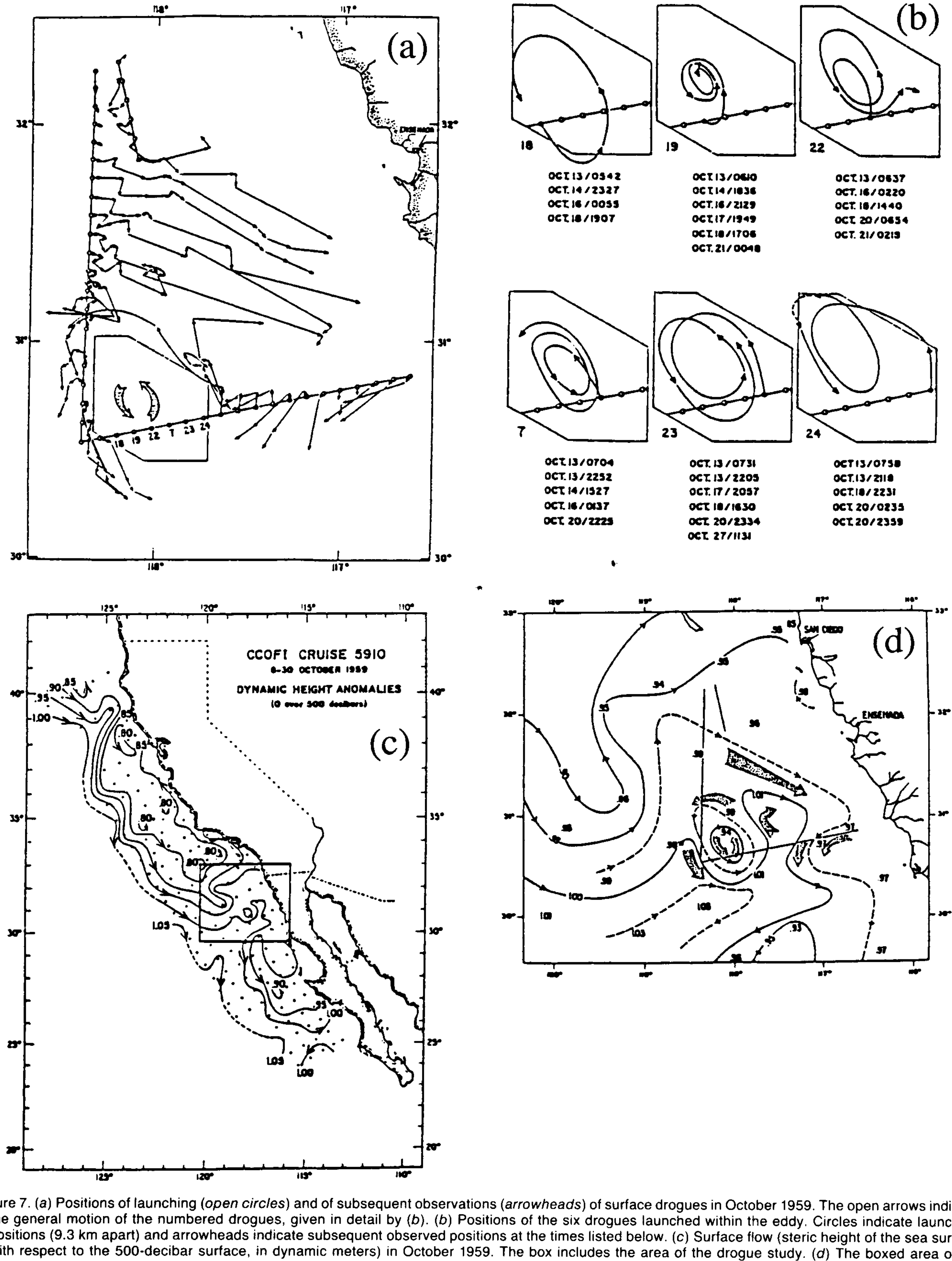

Figure 7. (*a*) Positions of launching (*open circles*) and of subsequent observations (*arrowheads*) of surface drogues in October 1959. The open arrows indicate the general motion of the numbered drogues, given in detail by (*b*). (*b*) Positions of the six drogues launched within the eddy. Circles indicate launching positions (9.3 km apart) and arrowheads indicate subsequent observed positions at the times listed below. (*c*) Surface flow (steric height of the sea surface with respect to the 500-decibar surface, in dynamic meters) in October 1959. The box includes the area of the drogue study. (*d*) The boxed area of (*c*) enlarged, with the drogue movements indicated by open arrows. The value of 0.94 dynamic m within the eddy is at the supplementary station (Reid et al. 1963).

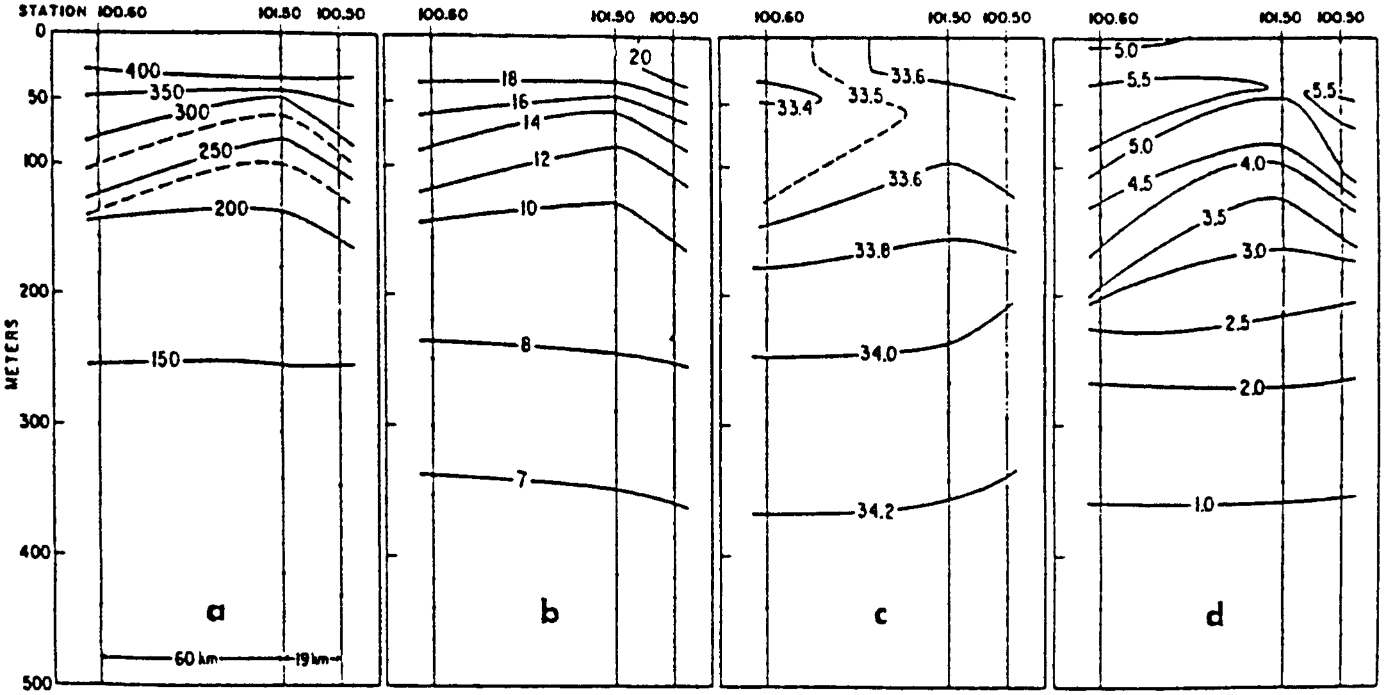

Figure 8. Vertical section across the eddy, including the eddy station and the adjacent inshore and offshore stations, in the upper 500 m. *a*, Thermosteric anomaly in centiliters/ton. *b*, Temperature in degrees Celsius. *c*, Salinity in parts per mile. *d*, Dissolved oxygen content in milliliters per liter (Reid et al. 1963).

To study the eddy features, we carried out quite a number of what would now be called process-oriented cruises, and found the features everywhere we went. In one of our most interesting studies, a set of drogues was laid out and tracked for us by a naval vessel (they were easier to borrow in those happy days). We had noted an inshore turn of the California Current toward Ensenada (Figure 6), as part of the Southern California Eddy, and wished to look at it more closely.

The drogues confirmed the inshore turn, but also found a cyclonic eddy (Figure 7a). Some of the drogues could be followed through several cycles (Figure 7b). We managed to get an extra cast at what seemed to be the center of the eddy (Figure 7c). The geostrophic flow, the general path of the drogues, and the eddy, which had a magnitude of about 5 dynamic centimeters, are shown together in Figure 7d. It is curious that there was no surface manifestation. There was no interruption in mixed-layer depth, and no outcropping of denser, colder water (Figure 8). All the shear associated with the flow of the eddy took place below the mixed layer. Satellite thermal sensors would have found no signal.

In that distant past the gear was primitive and the work both difficult and expensive, but we seemed to find eddylike surface flow wherever we followed drogues (Figure 9).

The first general study of the CalCOFI physical, chemical, and biological data (Reid et al. 1958) began with the best wind data available at the time (Figure 10), and the surface and subsurface flow as given by the relative geostrophic flow, and tried to relate the zooplankton volume to the nutrients (Figure 11), and delineate the boundaries of various species of zooplankton.

One of the purposes of CalCOFI was to observe and account for year-to-year variations. Although there had been very large variations observed in the years before 1950, it was hard to see a strong interannual signal in the early years, which seemed much the same—slightly colder than the long-term mean, with some suggestion of warming in the first half of 1957 (Figure 12). It had been eight soggy years, frustrating to contemplate. While surface temperatures from years before CalCOFI showed some strong anomalies, the 1949 through 1956 differences were small. In the first half of 1957 temperatures were higher, but not enough to make a case for strong interannual variation. Some correspondence between variations of zooplankton abundance and temperature could be detected, though not yet enough to be conclusive (Figure 13).

These cruises began to give us more information about the density field, the geostrophic shear, salinity, phosphate, and zooplankton volume within

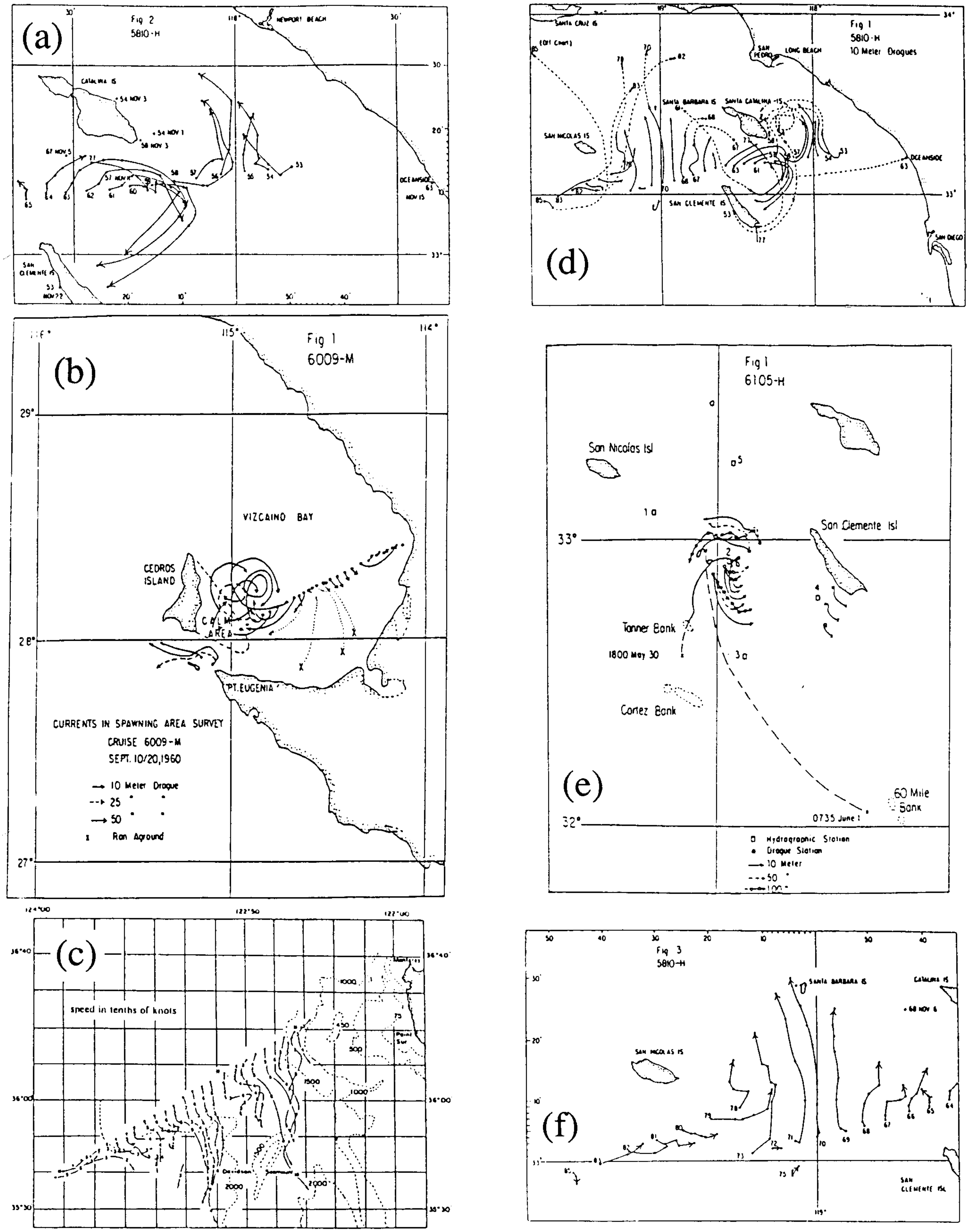

Figure 9. (*a*) Eastern part of the October 23–27, 1958, deployment (SIO 1962). (*b*) Drift of parachute drogues at 10 m on September 10–20, 1960 (SIO 1962). (*c*) Drift of parachute drogues at 10 m, March 23–26, 1958 (Jennings and Schwartzlose 1960). (*d*) Drift of parachute drogues at 10 m, October 23–27, 1958 (SIO 1962). (*e*) Drift of parachute drogues, June 1, 1961 (SIO 1962). (*f*) Western part of the October 23–27, 1958, deployment (SIO 1962).

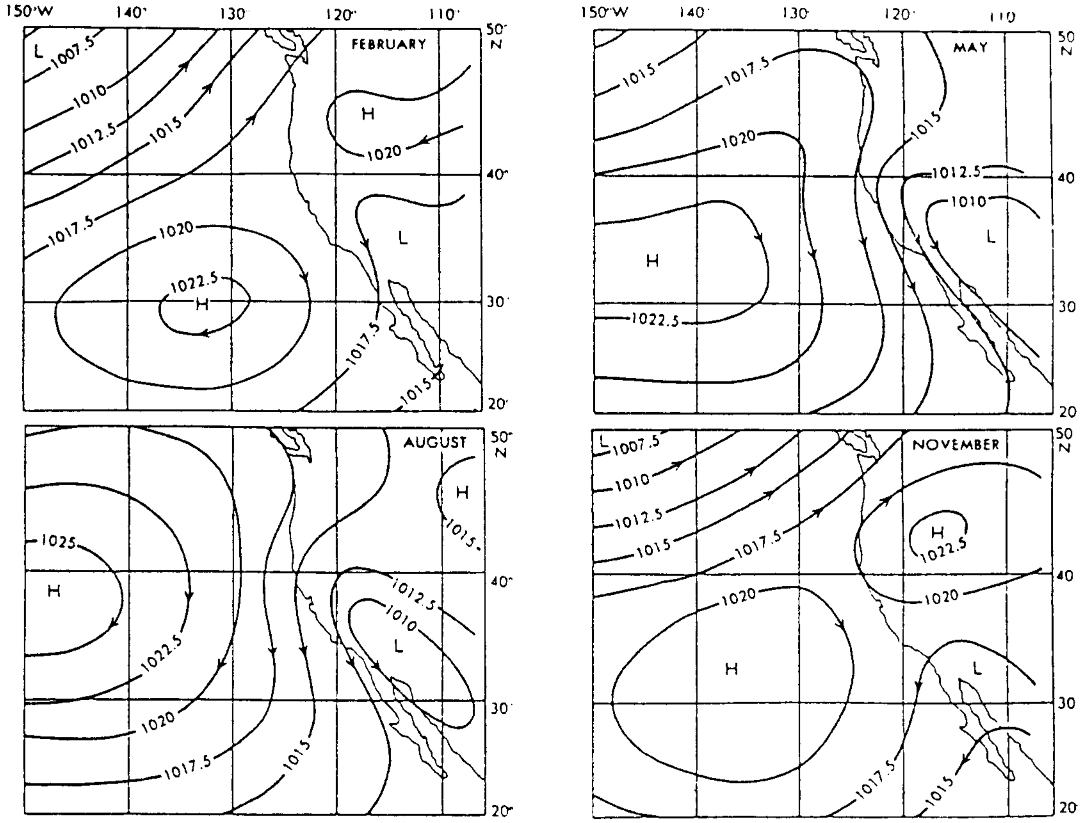

Figure 10. Average monthly atmospheric sea-level pressure (in millibars) over the eastern North Pacific Ocean and the western coast of North America during four months of the year (Reid et al. 1958).

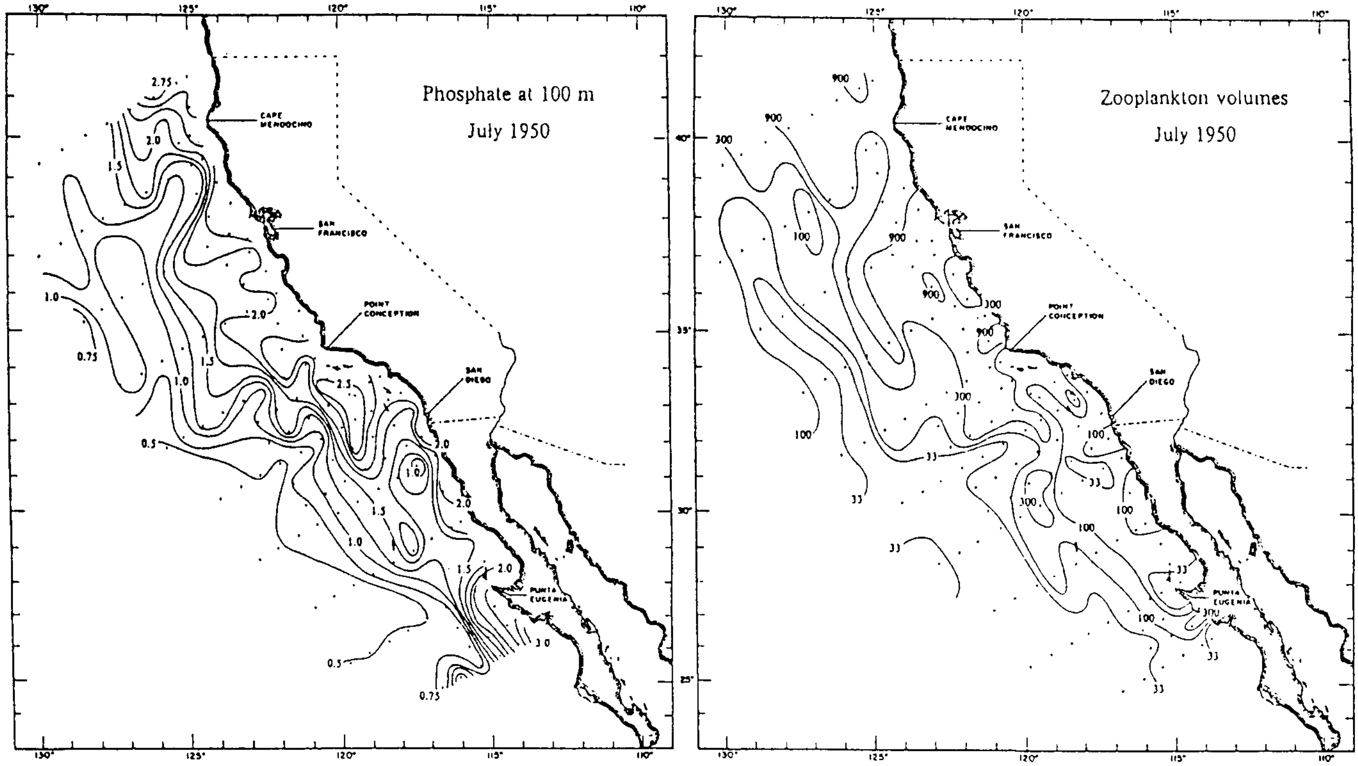

Figure 11. Distribution of phosphate-phosphorus (microgram-atoms per liter at 100 m) and zooplankton volumes (cubic cm per 1000 cubic m) in July 1950 (Reid et al. 1958).

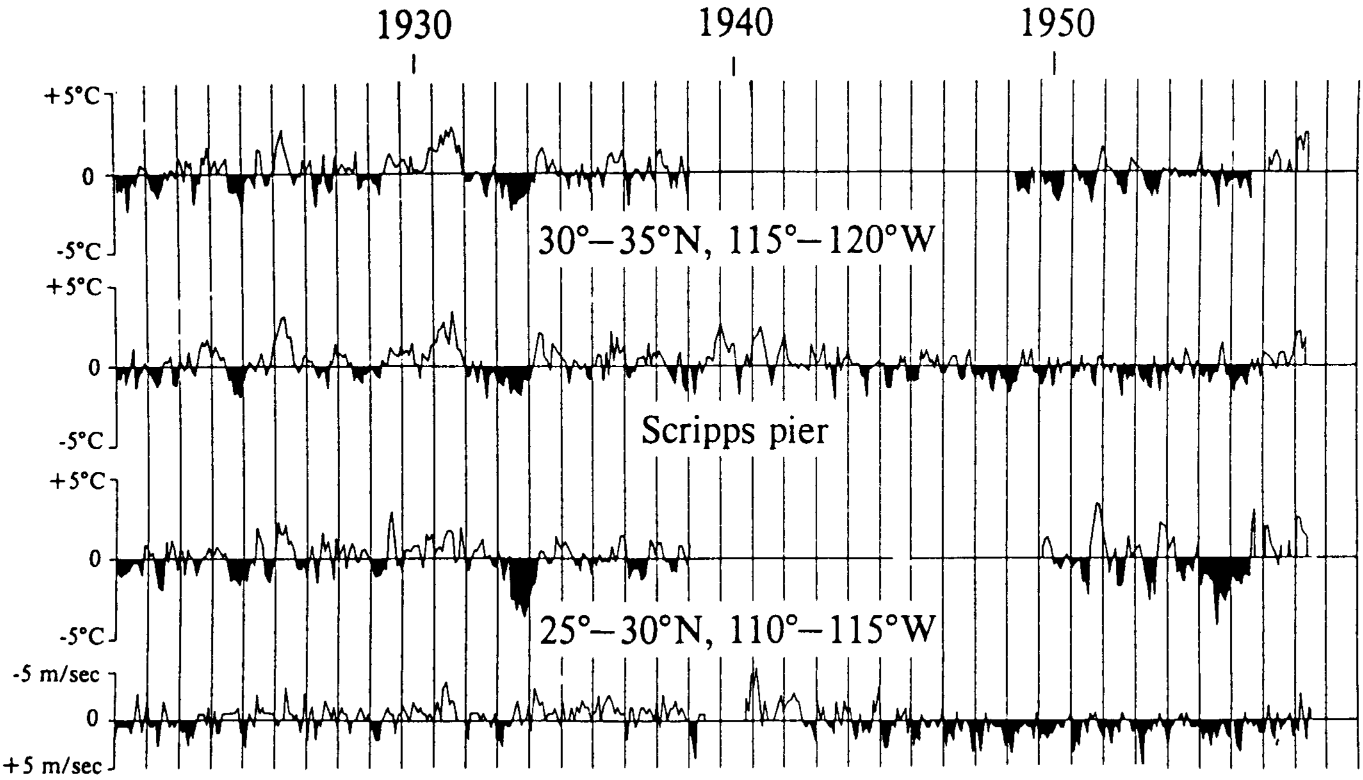

Figure 12. Monthly differences from average sea-surface temperatures (degrees C) at 30°–35°N, 115°–120°W; Scripps Pier; and 25°–30°N, 110°–115°W. *Bottom line*, monthly differences from average northerly wind component at 30°N, 110°–130°W. The period 1921–38 was taken as the average (Reid 1960).

the California Current. All of these were interesting and informative in themselves. But they seemed to suggest relations between these fields that could be examined more effectively if data could be collected over a larger area. We found there was enough interest in the field, and enough resources to carry out, at least once, a coverage of much of the North Pacific Ocean.

With ships and parties from CalCOFI, the University of Washington, the Canadian Pacific Oceanographic Group, the Pacific Oceanographic Fisheries Investigations in Hawaii, and many universities and government agencies from Japan, the entire area north of 20°N was covered in July–September 1955, and the geostrophic flow at the surface relative to 1000 decibars could be mapped for the North Pacific (Figure 14).

Later in the year the Eastropic Expedition added the area east of 140° between 20°N and 20°S, and in the summer of 1956 another expedition involving the United States, Japan, and France extended the tropical work westward to the Philippine Islands. With these background data, many of the relations between nutrients, circulation, and zooplankton seemed much better established throughout the Pacific (Figure 15).

As more data became available, numerous studies were carried out on the flow (Schwartzlose 1963; Wyllie 1966; Brown 1974; Hickey 1979; Chelton 1980; Gomez-Valdez 1984); on the seasonal variation of flow and characteristics (Roden 1961; Anonymous 1963; Lynn 1967; Pavlova 1966; Kindyushev 1970; Wyllie and Lynn 1971; Eber 1977;

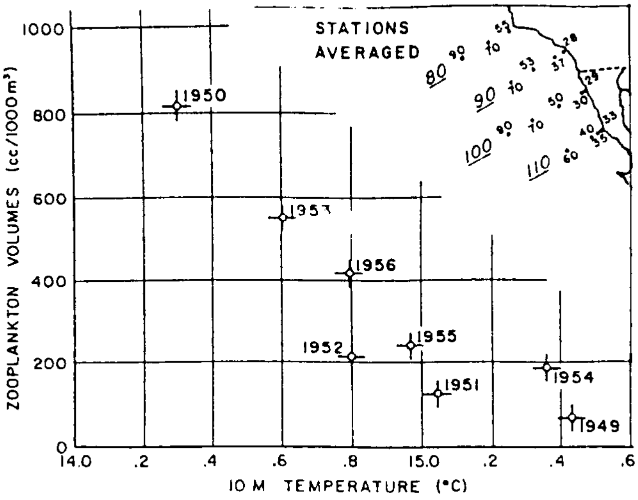

Figure 13. Temperature at 10 m and zooplankton volumes averaged from February through August, 1949–56 (Reid et al. 1958).

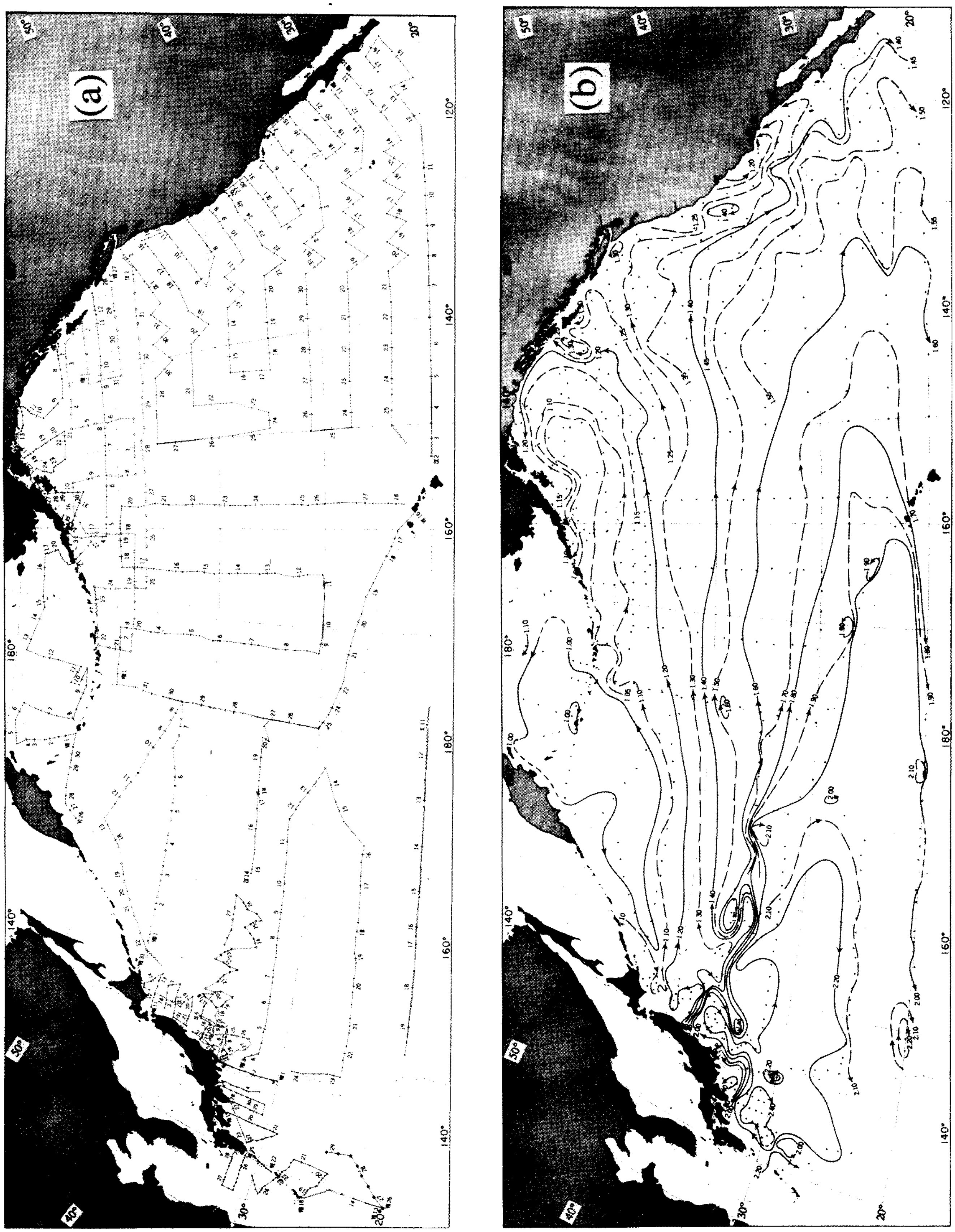

Figure 14. (*a*) NORPAC Expedition track, July–September 1955. (*b*) Steric height at the sea surface relative to 1000 decibars (NORPAC Committee 1960).

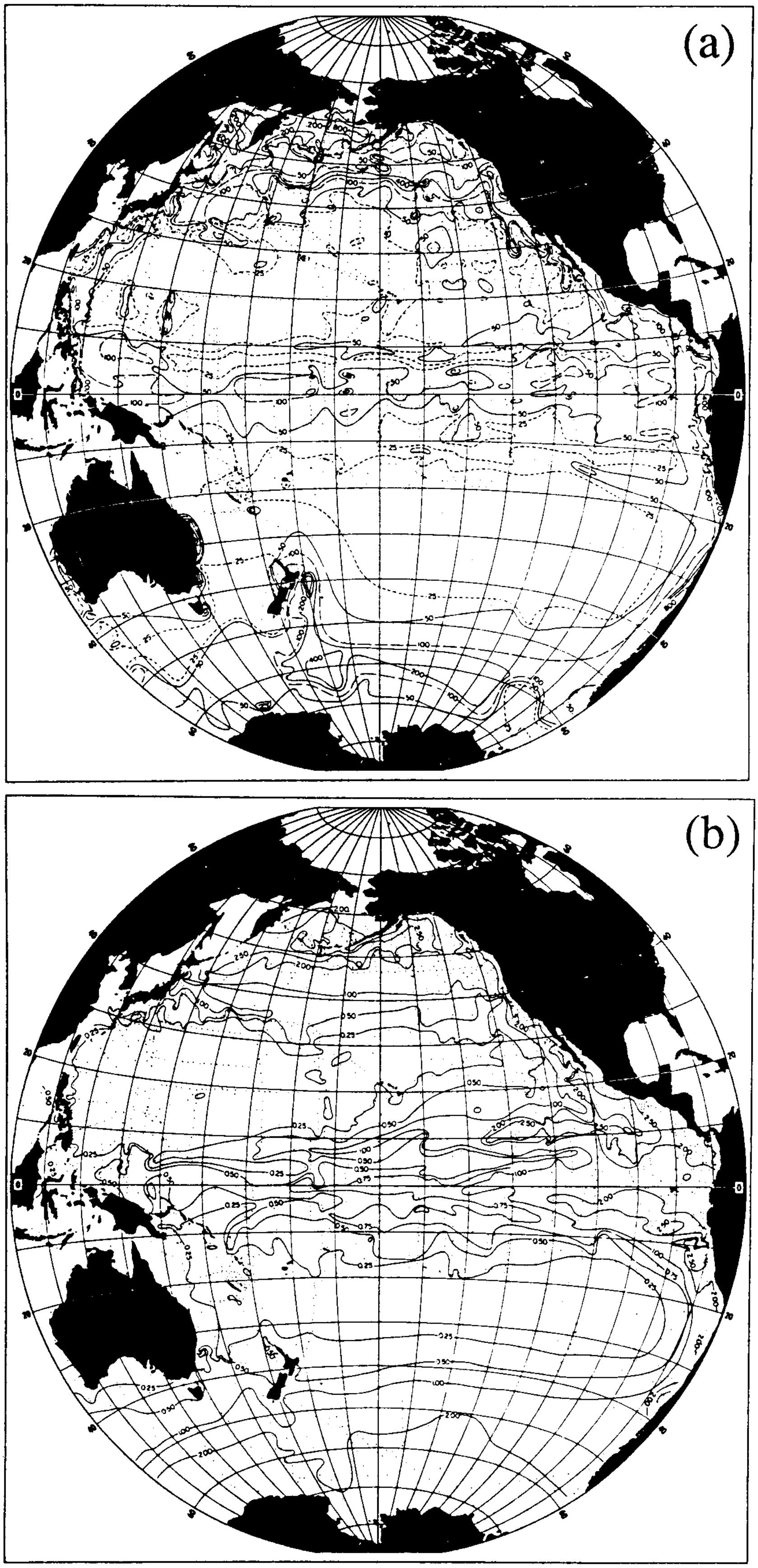

Figure 15. (*a*) Distribution of zooplankton volume (parts per 10^9 by volume) in approximately the upper 150 m of the Pacific Ocean. (*b*) Distribution of PO_4-P at a depth of 100 m in the Pacific Ocean (μg-at./l) (Reid 1962b).

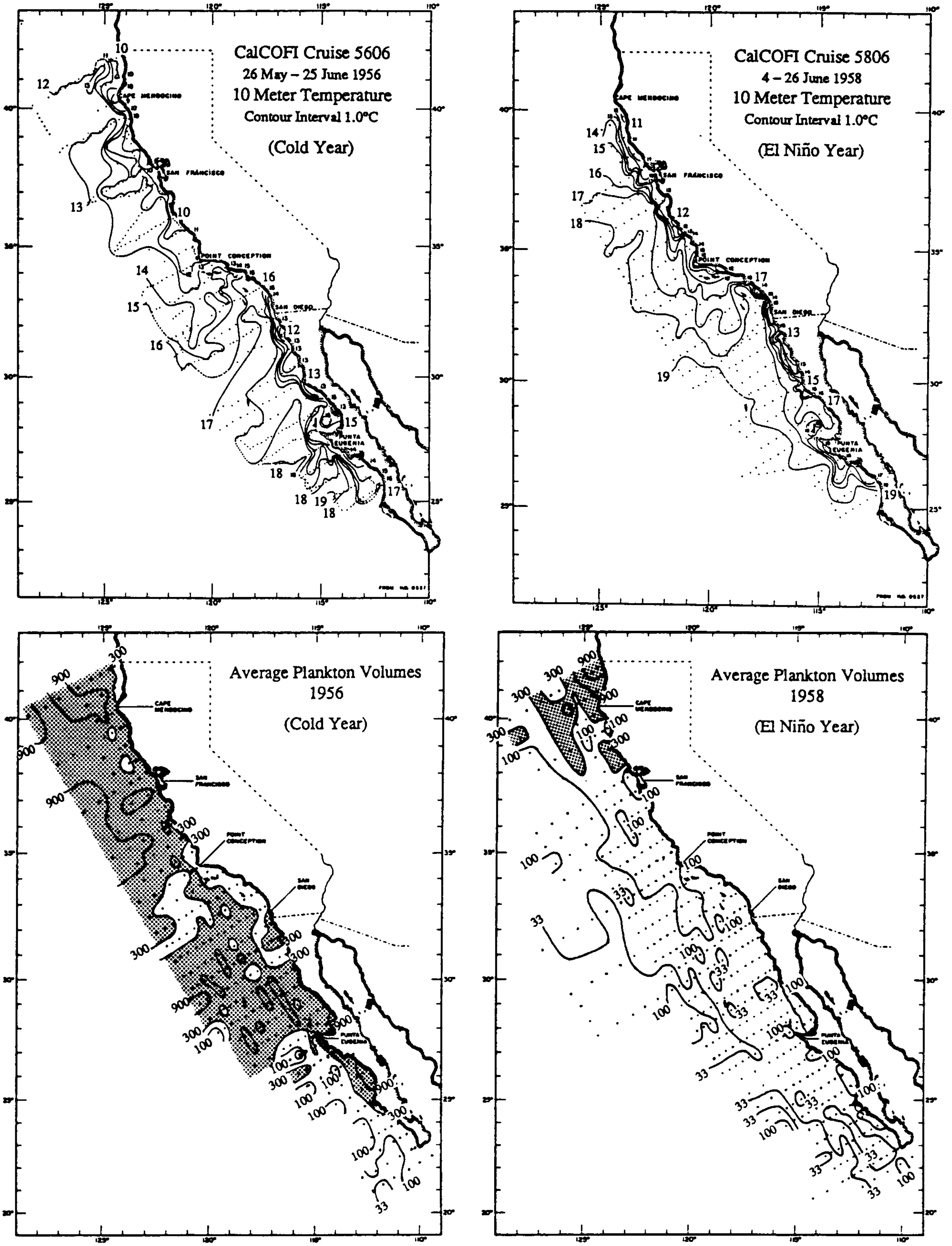

Figure 16. *Top*, Ten-meter temperature in June of a cold year (1956; cruise 5606) and in June of a warm, El Niño year (1958; cruise 5806) (Anonymous 1963). *Bottom*, Zooplankton volumes during a cold year (1956) and the warm, El Niño year (1958) (Reid 1962b).

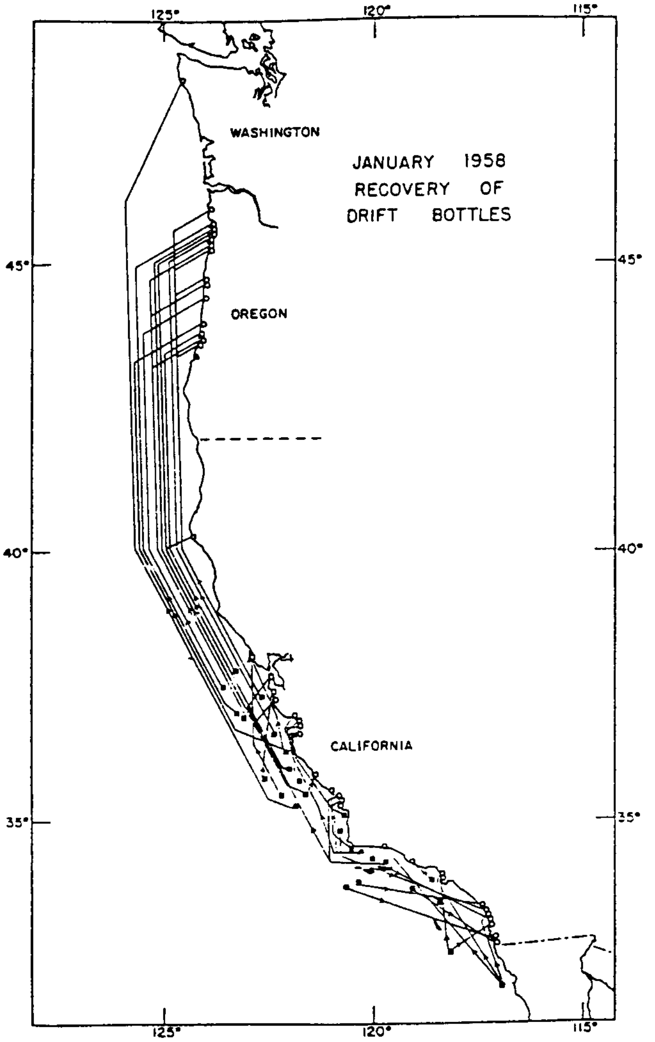

Figure 17. Recoveries of some drift bottles released in January 1958. Black squares show the release points; circles show the recovery points (Crowe and Schwartzlose 1972).

Alvarez-Borrego and Schwartzlose 1979; Lynn et al. 1982); on the heat and salt balance (Roden 1959); and on the deep characteristics (Mantyla 1969). Burkov and Pavlova (1980) described the eddy field. Coastal elevations of the sea surface were discussed by Roden (1960), Sturges (1967), Reid and Mantyla (1976), Enfield and Allen (1983), and Hickey (1984). Early theoretical studies included those by Arthur (1965), Behringer (1972), and Muraki (1974). Larger-area studies were carried out by Wooster and Reid (1963), Wyrtki (1965, 1975), Robinson (1976), and Reid et al. (1978).

EL NIÑO

By the end of 1957, the warming of the California Current had continued, and similar warming signals were being reported from all of the areas of the eastern and equatorial Pacific, wherever measurements were being made.

In particular, in the CalCOFI data, we noted such things as a continued warming of the coastal waters, beyond the slight warming reported in 1957 and extending over the whole area, with a decrease in zooplankton volume (Figure 16). The tentative relation seen through 1956 (Figure 13) held up in the later years. Drift bottles were found much farther north from the January 1958 release (Figure 17). The temperature anomaly was even greater at thermocline depth than at the surface (Figure 18). The ordinary seasonal variation noted in all years (Figure 19) shows values of dynamic height higher in January than July, decreasing monotonically offshore, but the interannual variation in the 1958 El Niño showed higher values inshore and offshore, with a long, narrow trough of low values in the middle (Figures 20a–c). This meant that during El Niño there was more northward flow inshore, as expected, but also more southward flow offshore. Temperature and salinity anomalies were high inshore, dropping offshore.

We tried to account for these variations in terms of the wind system. Previous El Niño events and anti-El Niño events had been accompanied by shifts in the winds, as in 1931, when temperatures were high in the northeastern Pacific, with an anomalously high wind from the south, and in 1933, when low temperatures seemed to follow from a stronger-than-normal wind from the northwest (Figure 21). Gunnar Roden and I tried to pursue this further, by relating temperature off southern California to a wind index across 30°N (Figure 22). But it didn't work out with the wind data we had, or the simple concept we tried.

The El Niño events during the CalCOFI period have been as extreme as any in the longer-term record, but we have not yet found anything as cold as the 1917, 1921, and 1933 record. We still have something to look forward to.

The data were such that a CalCOFI symposium was held in June 1959, attended by—among others—Jule Charny as chairman, and Nick Fofonoff, Elton Sette, Carl Eckart, Fritz Fuglister, John Isaacs, John Marr, Walter Munk, Jerome Namias, Roger Revelle, Benny Schaefer, Henry Stommel, Frances Clark, and Ahlie Ahlstrom.

I quote from John Isaacs's introduction to *CalCOFI Report VII* (1960)

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

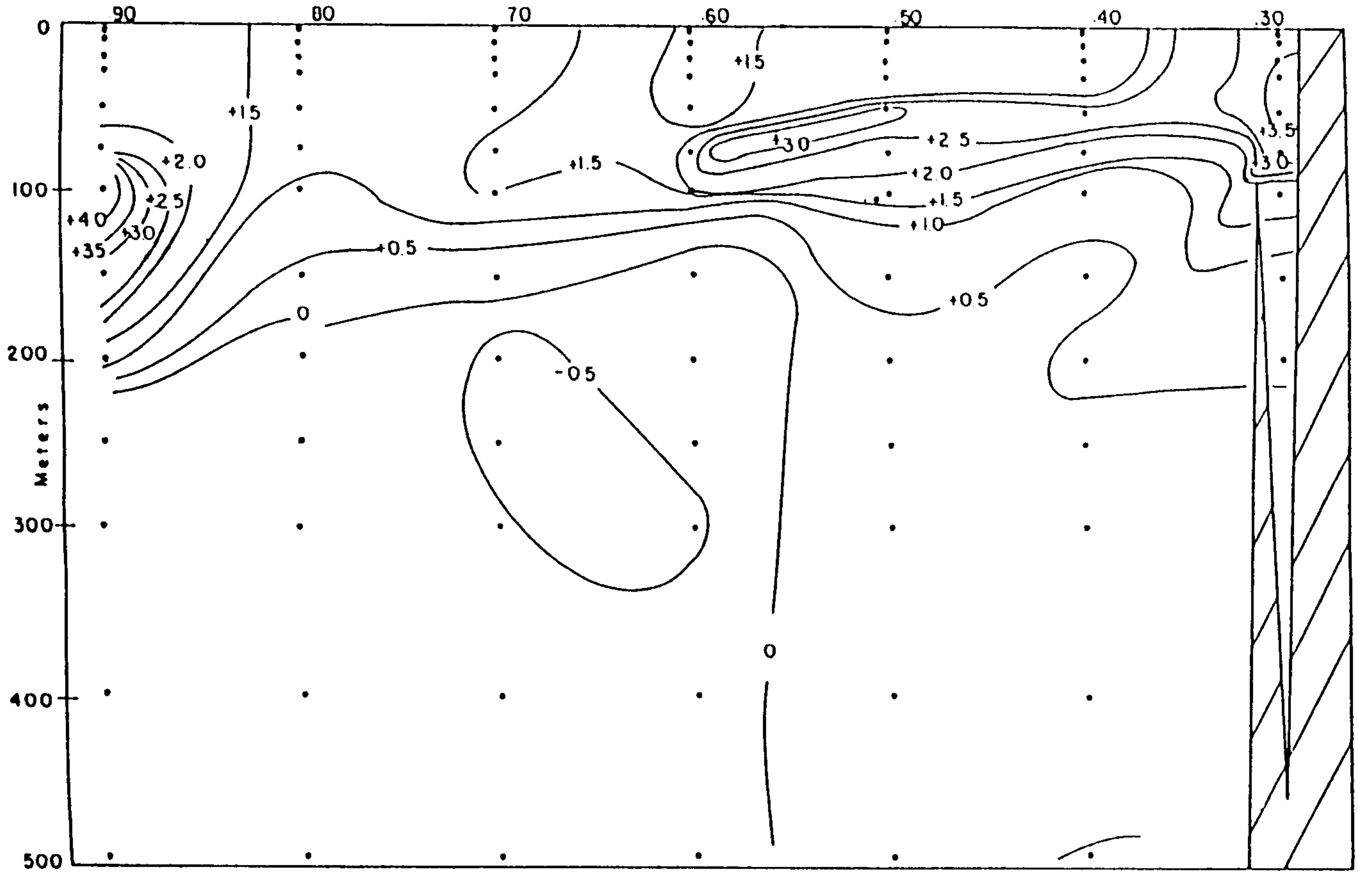

Figure 18. Temperature anomaly on a vertical section extending 250 miles offshore. The values are those measured in January 1958 minus the CalCOFI mean (Reid 1960).

> Two remarkable and unprecedented events gave rise to this transformation, for during 1957 great rafts of sea-borne seeds and heavy rains had visited her barren shores.
>
> One is inclined to select the events of this isolated atoll as epitomizing the year, for even here, on the remote edges of the Pacific, vast concerted shifts in the oceans and atmosphere had wrought dramatic change.
>
> Elsewhere about the Pacific it also was common knowledge that the year had been one of extraordinary climatic events. Hawaii had its first recorded typhoon; the seabird-killing *El Niño* visited the Peruvian coast; the ice went out of Point Barrow at the earliest time in history; and on the Pacific's Western rim, the tropical rainy season lingered six weeks beyond its appointed term.

The 1957–58 El Niño had a tremendous impact on both oceanography and meteorology. The data off California made available by CalCOFI, and the data assembled from other areas made it possible to consider in a useful manner the relation between winds, current, temperature, nutrients, and biomass. In particular, the meteorologists began to

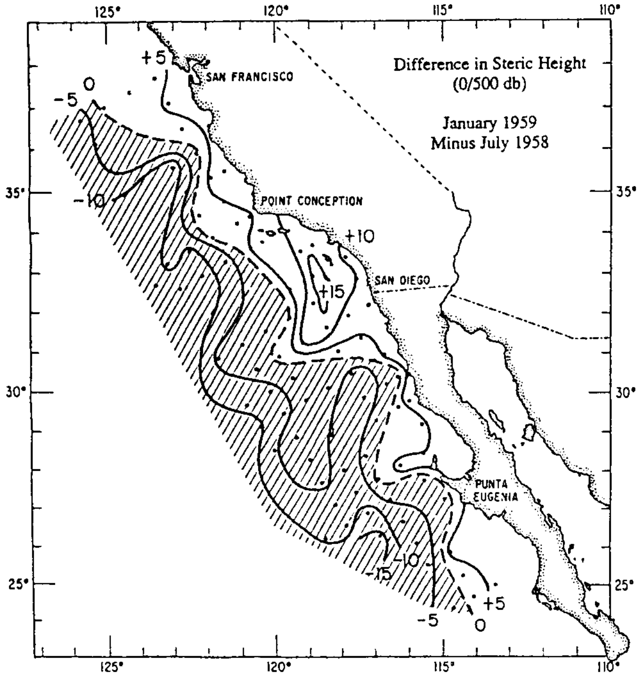

Figure 19. Seasonal difference in steric height, January 1959 minus July 1958. Shaded area indicates negative values (Lynn and Reid 1975).

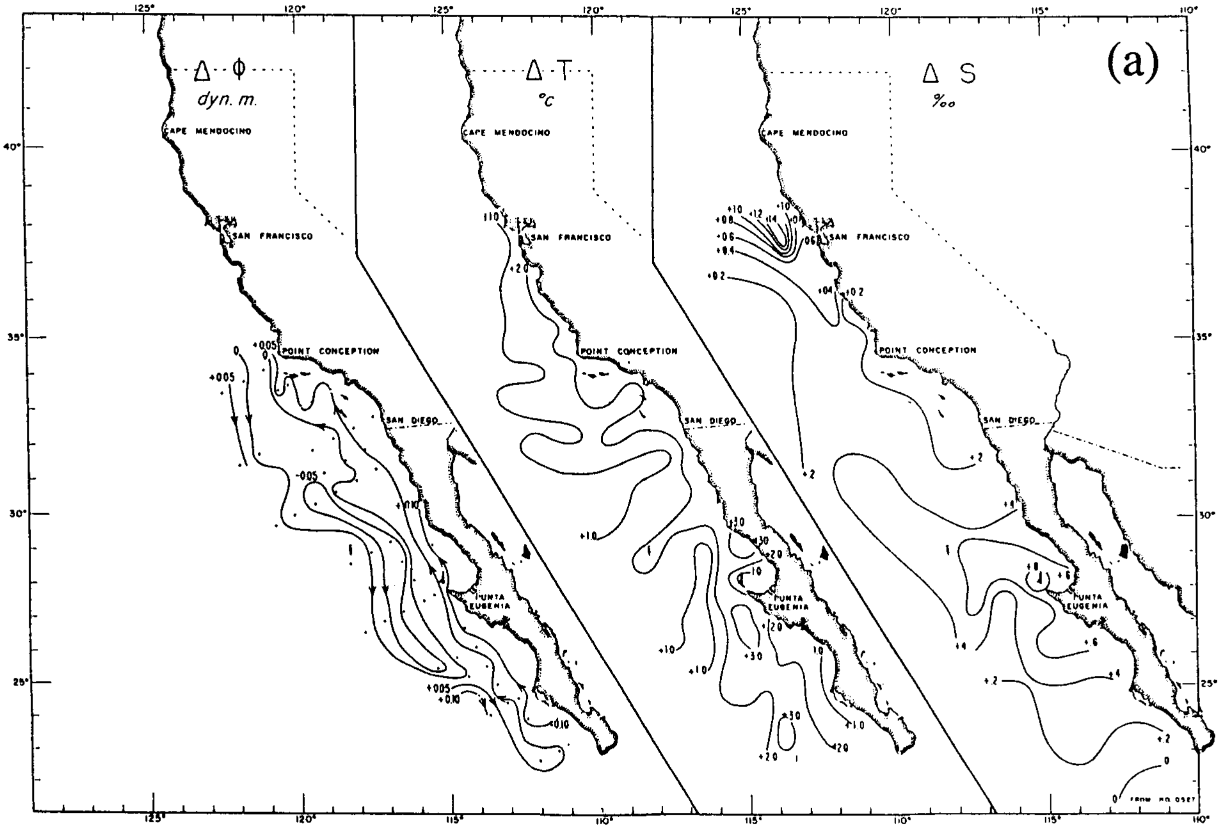

Figure 20a. January 1958 Φ, T, and S anomalies (ΔT and ΔS refer to 1949–54 mean; ΔΦ refers to 1953 only) (Reid 1959).

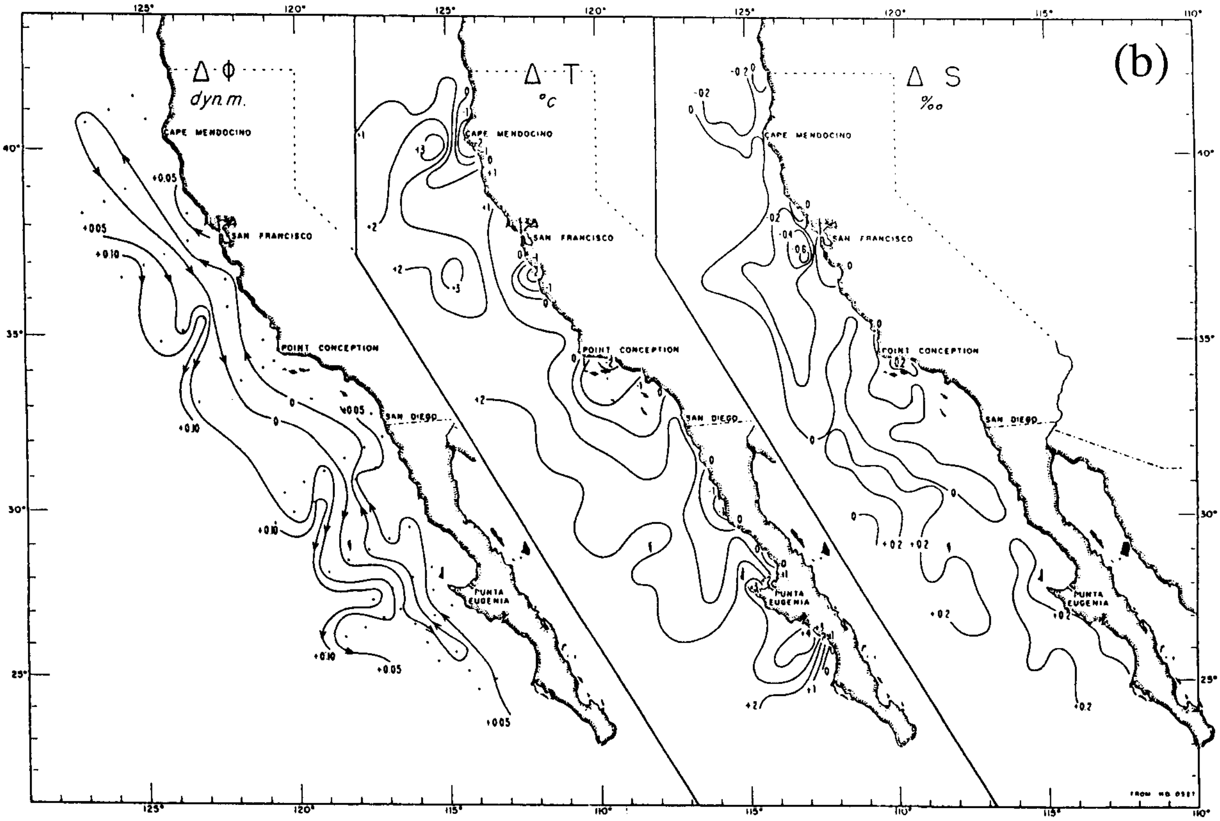

Figure 20b. July 1958 Φ, T, and S anomalies (ΔT and ΔS refer to 1949–54 mean; ΔΦ refers to 1952 only) (Reid 1959).

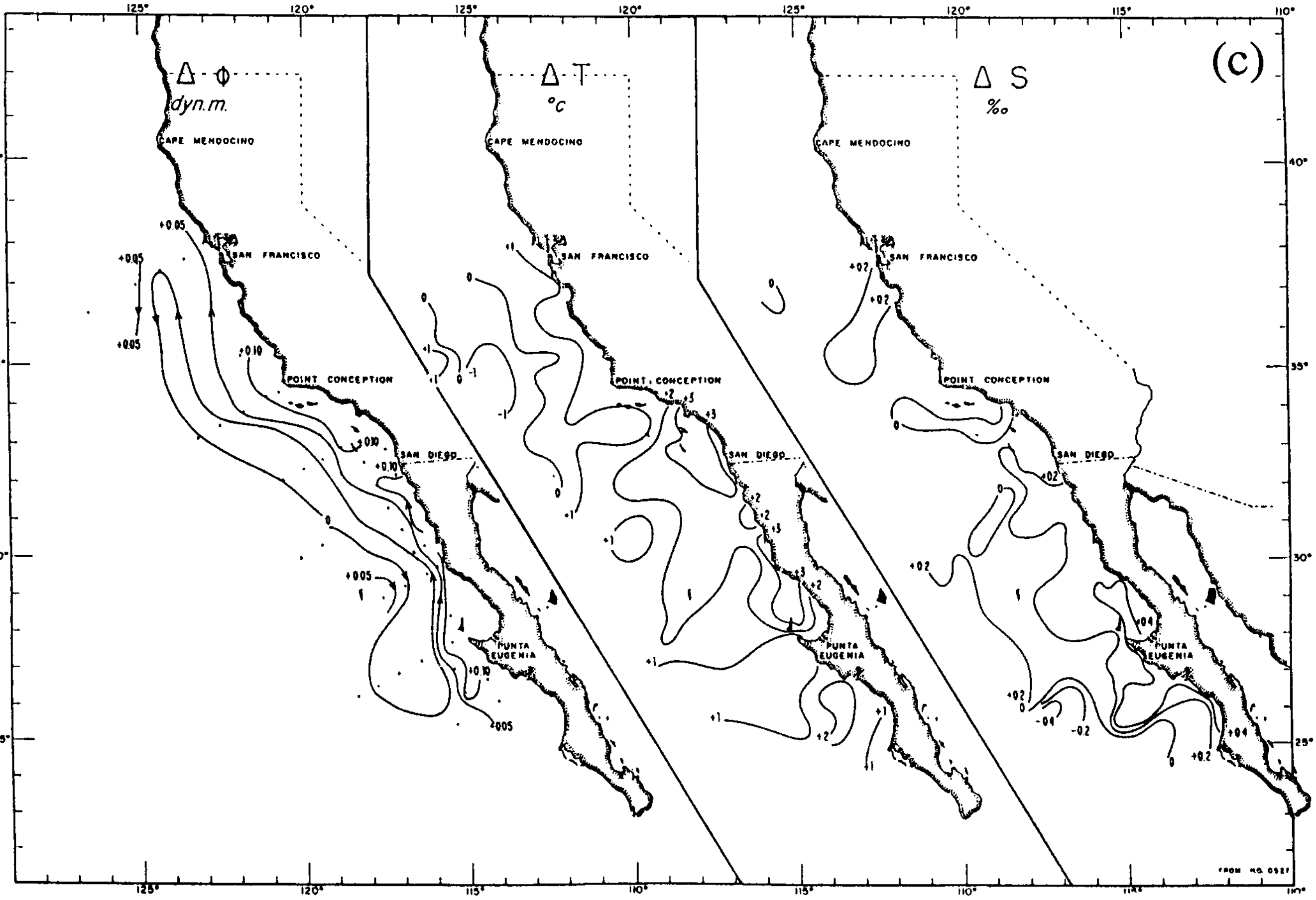

Figure 20c. October 1958 Φ, T, and S anomalies (ΔT and ΔS refer to 1952 only) (Reid 1959).

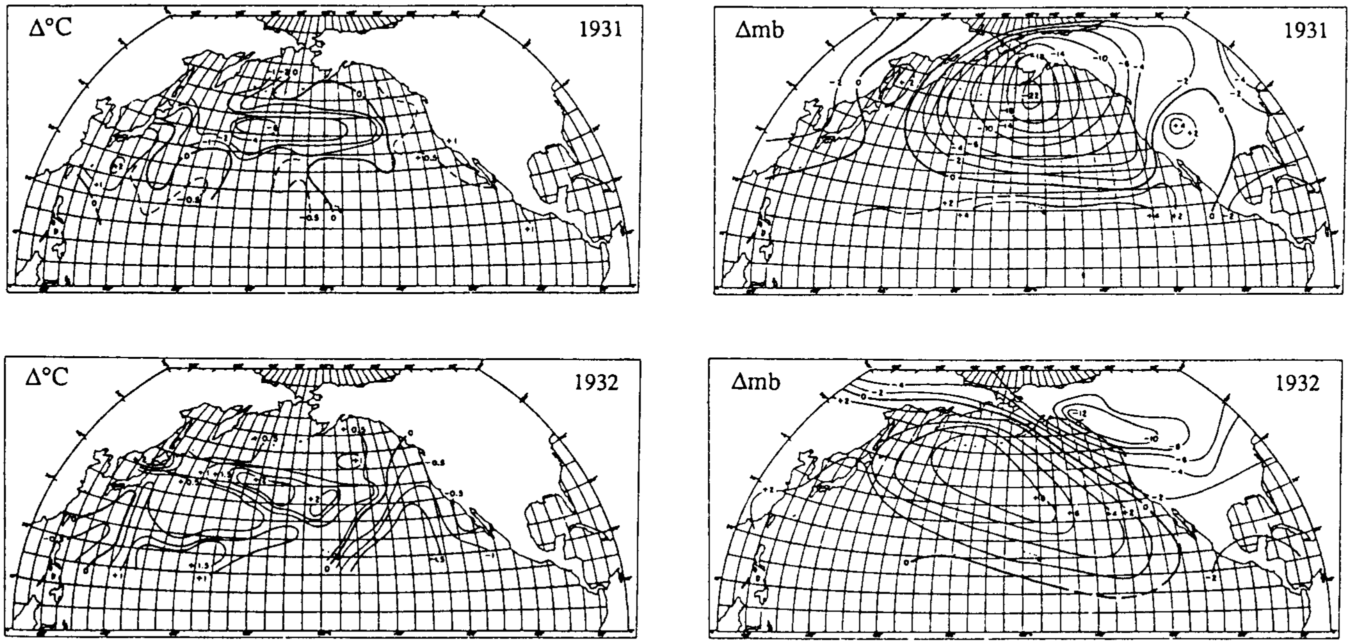

Figure 21. Anomalies of sea-surface temperature (*left*) and atmospheric pressure (*right*) in January of a warm year and a cold year (Reid 1960).

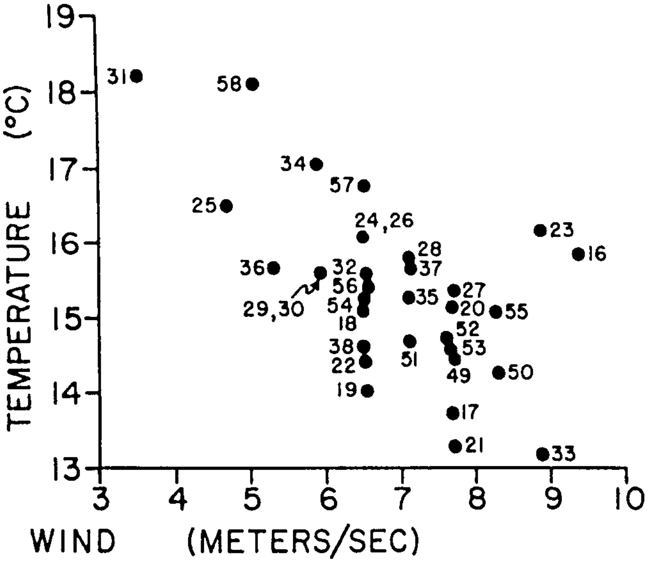

Figure 22. May temperature, 30°–35°N, 115°–120°W, and wind at 30°N, 1916–38, 1949–58 (Reid 1960).

use the increasing information about ocean temperatures in predicting weather and trying to understand climate. I might say that neither oceanography nor long-range weather prediction has ever been the same.

It is also noteworthy that some of the Atlantic people were reluctant to accept these phenomena in the Pacific as real. This was partly because the Atlantic, for reasons we don't know, does not have large-scale year-to-year variations of the magnitude we see in the Pacific. But it may be simply that they didn't like oceans to behave that way, when the large-scale variations were harder to model, any more than they liked our eddies at that time. But 30 years later the large variations in the upper layer are found to be easier to model than the general circulation itself, and we have dozens of modelers, even from the Atlantic, trying to account for Pacific El Niño events.

AFTER EL NIÑO

In the 30 years since the 1957–58 El Niño, a great deal of change and improvement has occurred in instrumentation. Drogues and drifters are no longer tortuously tracked by ships, but by satellites (Figure 23). Analysis of variations in the flow of the California Current can be carried out far more elegantly with the larger series of data and modern computers.

The early studies of seasonal variation (Anonymous 1963; Wyllie 1966; Wyllie and Lynn 1971; Reid 1959) are seen to be statistically justified, and El Niño differences, with a long, narrow trough in the center of the pattern (Figure 20; and Lynn and Reid 1975) are revealed much more clearly and securely by the longer time series (Figure 24).

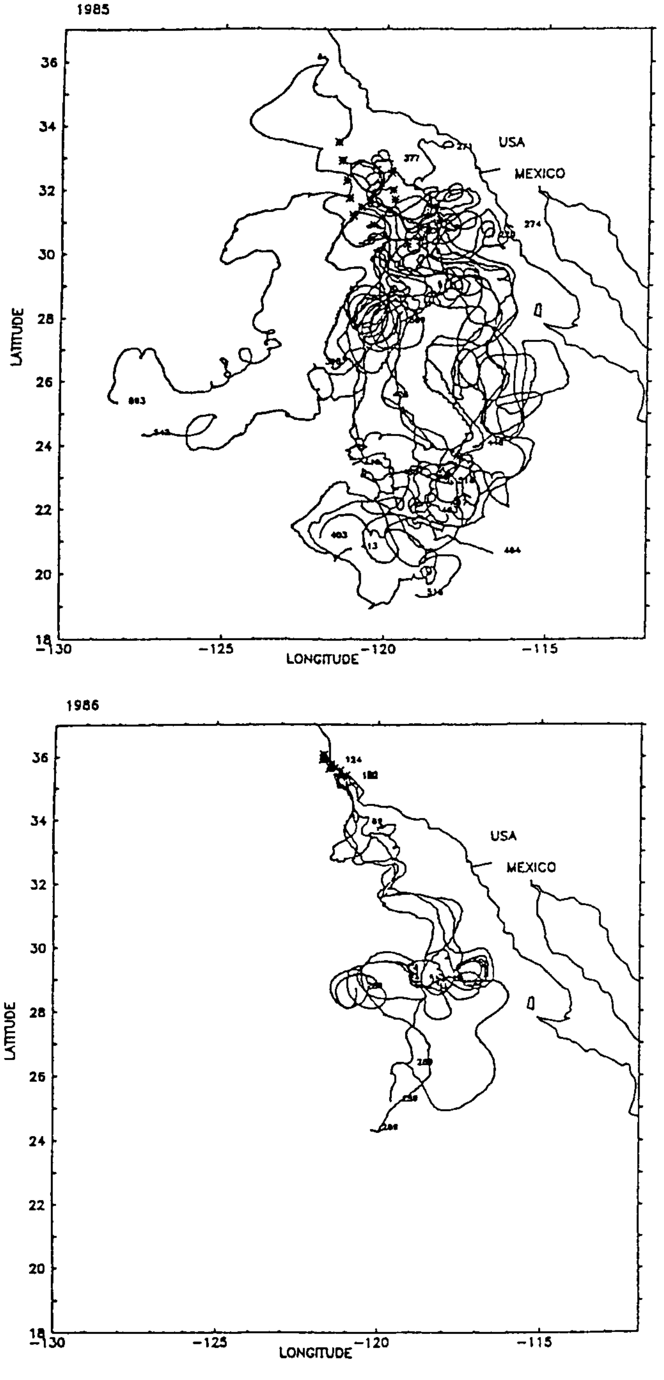

Figure 23. Drogue trajectories, 1985 (*top*), and 1986 (*bottom*) (Poulain et al. 1987).

The zooplankton-temperature relation has been extended and related to circulation (Figure 25). Surface measurements have been improved and can be made continuously from vessels underway

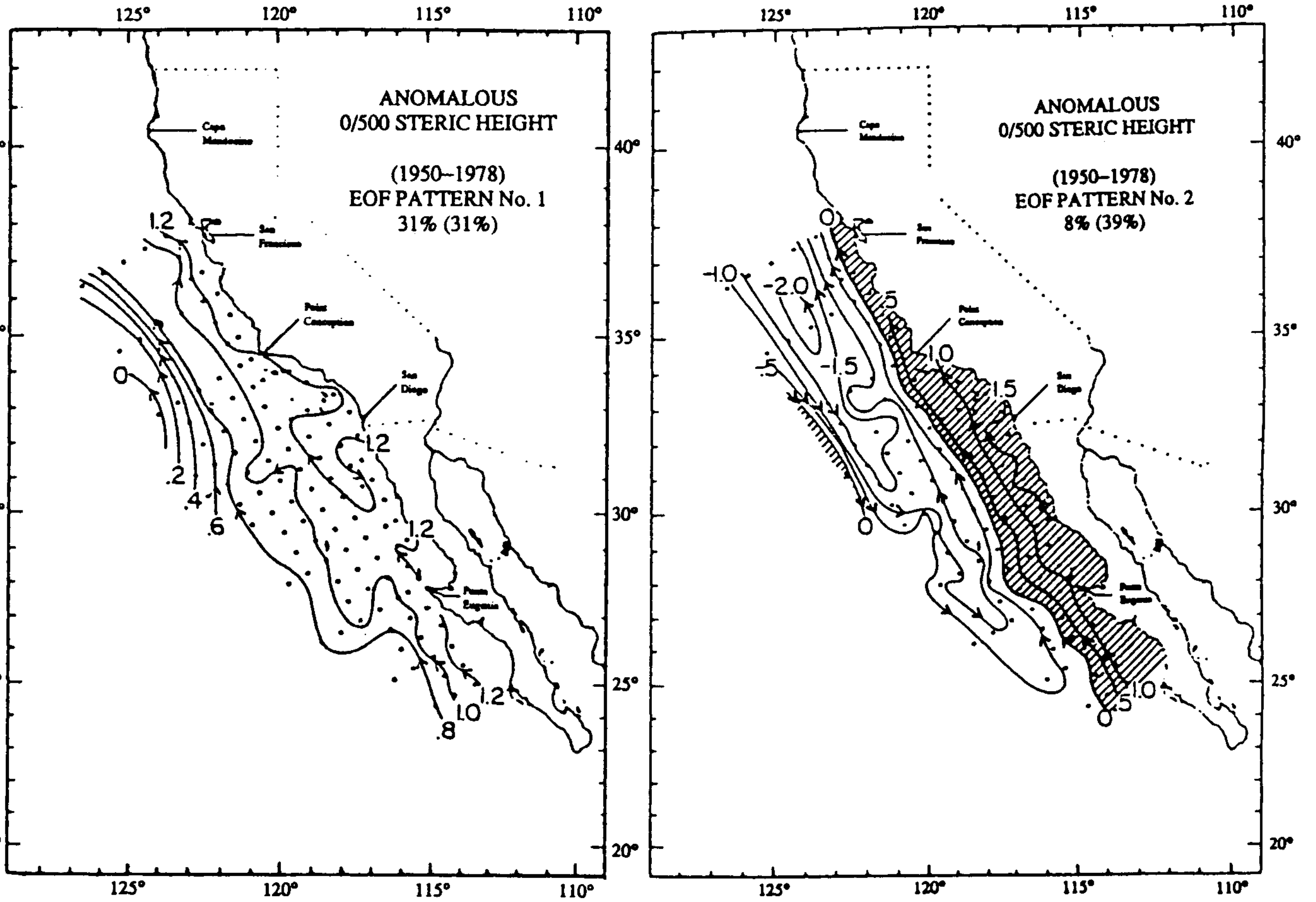

Figure 24. Anomalous 0/500 steric height, 1950–78: *left*, EOF pattern no. 1; *right*, EOF pattern no. 2 (Chelton 1980).

(Figure 26), providing tighter coverage and greater detail, and satellite measurements extend a tight coverage for surface chlorophyll to larger areas where it can be seen to correspond in some cases to the circulation at the surface (Figure 27). In particular, the frontal feature at the latitude of Ensenada that had been seen in the earlier flow pattern (Figure 6) is seen here in chlorophyll.

Variability has been shown to occur over a wide range of time and space scales, and we should not lose track of them. But I do not see how these phenomena can be examined usefully without considering also the background ocean upon which they are imposed. The real question is, How does the whole system operate; how is the biomass sustained in such a varying system?

John Hunter tells me that off southern California alone, each year the mackerel eat as much as 350,000 tons of anchovies, and the anchovies eat about 30 million tons of copepods. How many tons of smaller forms have those 30 million tons of copepods eaten?

And Alec MacCall estimates that 200 years ago, before the millions of seals and sea otters were killed off by the fur hunters, they must have taken an enormous biomass each year to sustain themselves. How were the nekton and plankton arrayed in the great web then?

These are some of the findings from the CalCOFI program. Other programs, such as NORPAX, CUEA, CODE, and various others have followed, pursuing in greater detail and with greater facilities and work force some of the same sorts of studies. But CalCOFI has been, and is still, the only program that has really been interdisciplinary. What have we done, and what have we done wrong? We have the greatest, and the most complete, array of ecological data ever assembled over such a large ocean area.

The most important question is, what should we do now? Should we maintain our present schedule of monitoring cruises and process-oriented cruises? Or divert to an entirely different sort of study? What should be added that can be added to

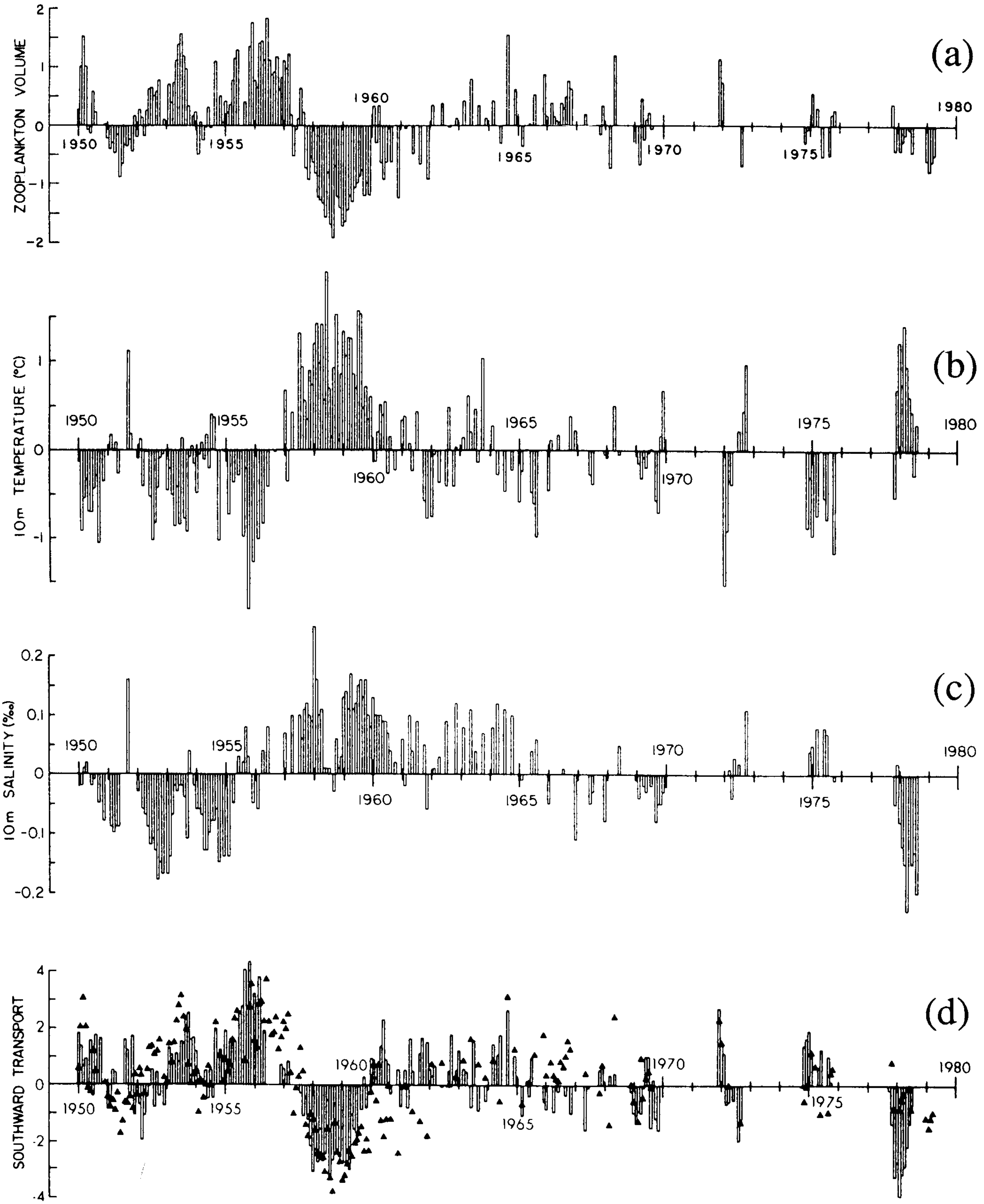

Figure 25. Time series of nonseasonal values of four parameters in the California Current: (*a*) the average of the individual zooplankton time series; (*b*) the average 10-m temperature over 150 hydrographic stations; (*c*) the average 10-m salinity over 150 hydrographic stations; (*d*) the amplitude time series of the principal EOF of 0/500 steric height. Triangles in (*d*) represent the zooplankton time series shown in (*a*) (Chelton et al. 1982).

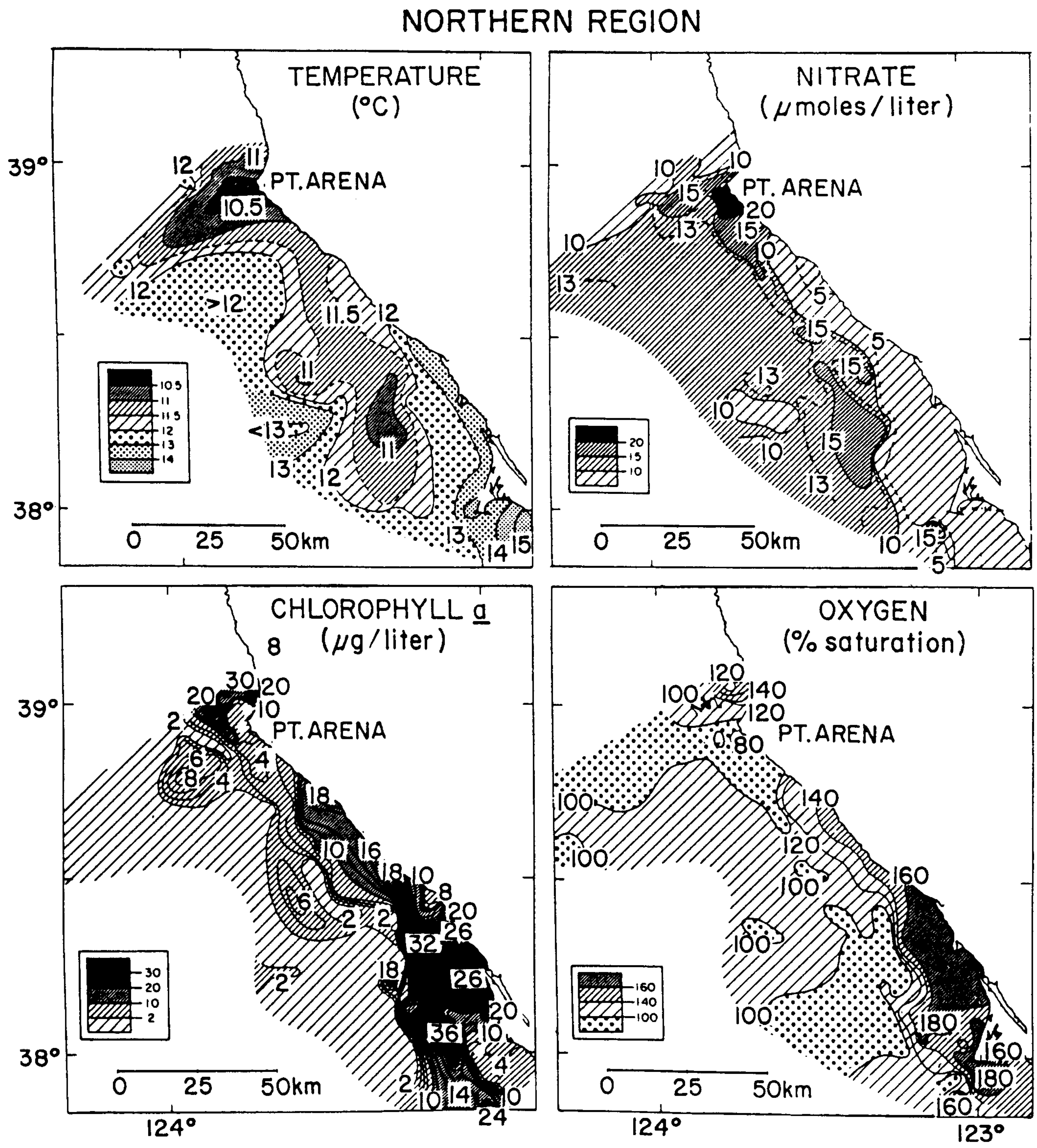

Figure 26. Northern region temperature, nitrate, chlorophyll-a, and oxygen (Simpson 1985).

our investigations? When will the technology be developed to allow us to deal with phytoplankton? Our problems, and these questions, are not unique to the CalCOFI program, but to any investigations of marine ecology. Please give us your thoughts.

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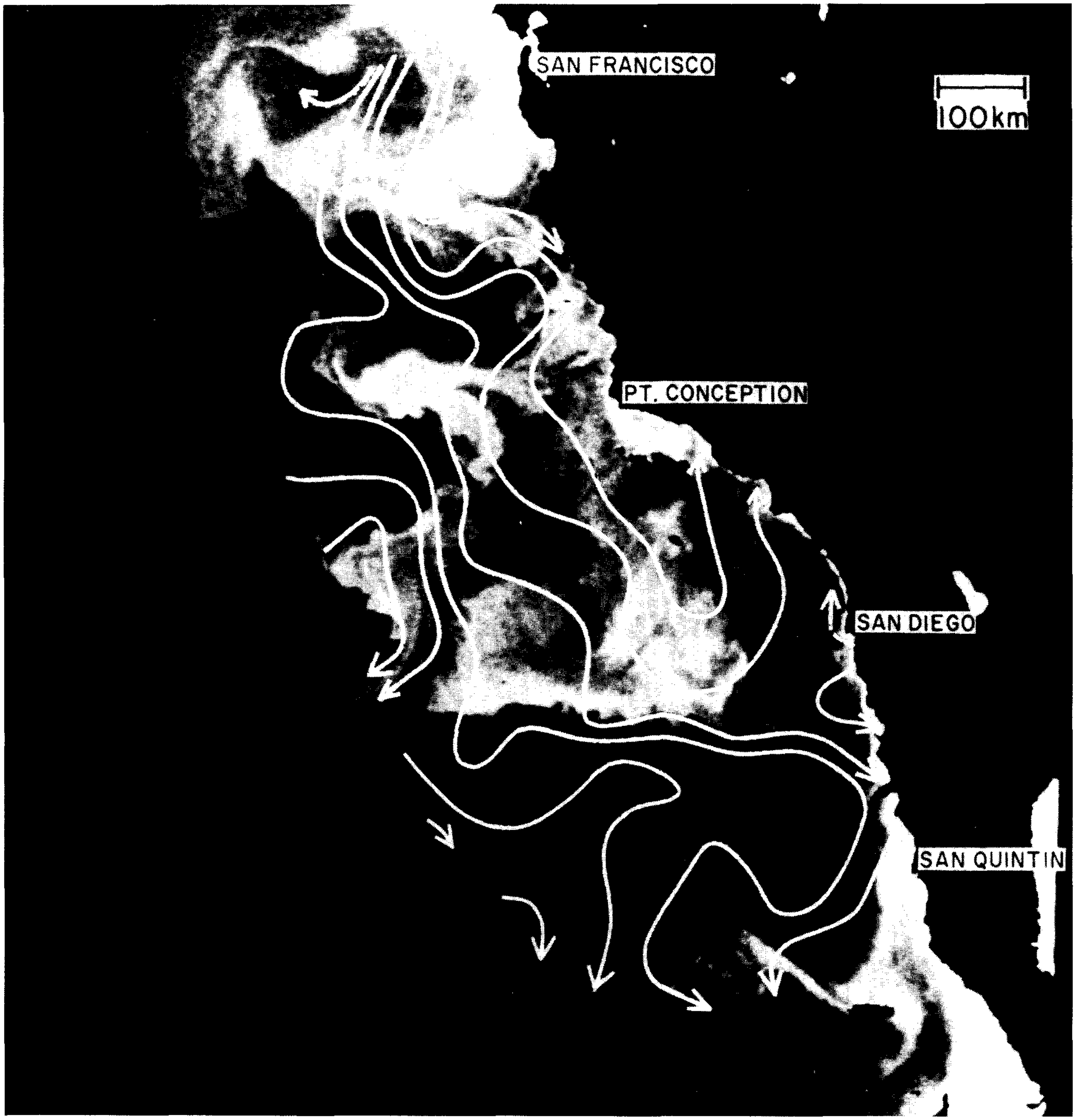

Figure 27. Surface flow and pigment image (Peláez and McGowan 1986).

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