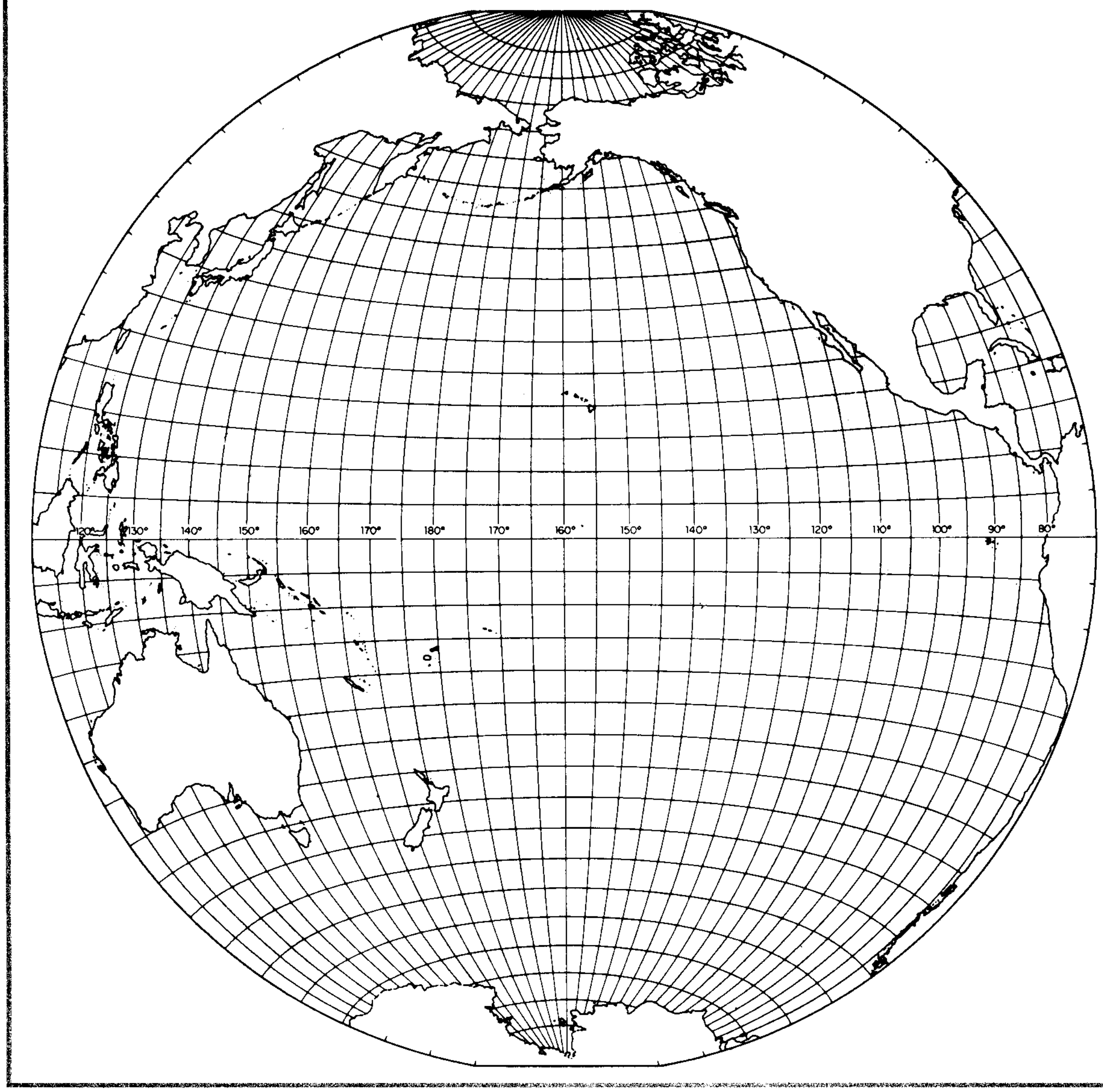


California Cooperative Oceanic Fisheries Investigations

Reports

VOLUME 31 **OCTOBER 1990**

CALIFORNIA
COOPERATIVE
OCEANIC
FISHERIES
INVESTIGATIONS

Reports

VOLUME 31
January 1 to December 31, 1989

Cooperating Agencies:

CALIFORNIA DEPARTMENT OF FISH AND GAME
UNIVERSITY OF CALIFORNIA, SCRIPPS INSTITUTION OF OCEANOGRAPHY
NATIONAL OCEANIC AND ATMOSPHERIC ADMINISTRATION, NATIONAL MARINE FISHERIES SERVICE

CALCOFI COORDINATOR Patricia Wolf

EDITOR Julie Olfe

SPANISH EDITOR Carina Lange

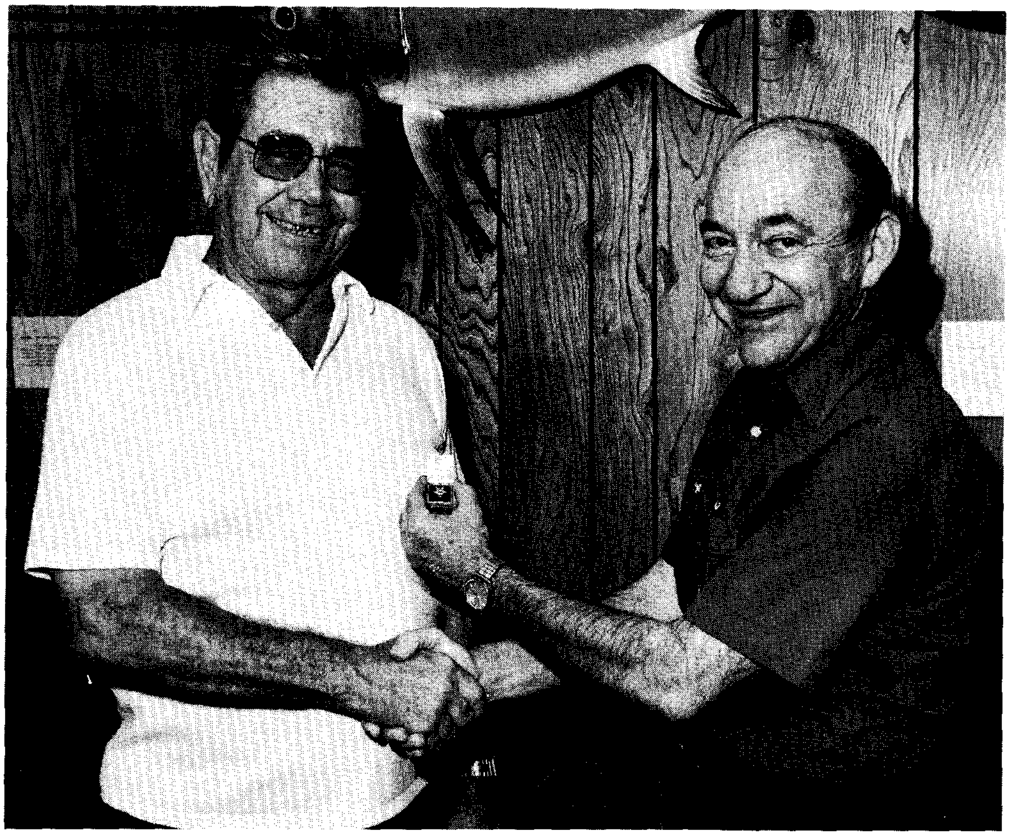
Published October 1990, La Jolla, California
ISSN 0575-3317

CONTENTS

Indexed in *Current Contents*. Abstracted in *Aquatic Sciences and Fisheries Abstracts* and *Oceanic Abstracts*.

IN MEMORIAM

James R. Thrailkill
1921–1990



Jim Thrailkill *(left)* receives his forty-year service pin from Reuben Lasker.

CalCOFI lost a friend when Jim Thrailkill died in Boise, Idaho, after a brief illness at the age of 68. After retirement from the National Marine Fisheries Service in April 1986, Jim returned to his native Boise where he could enjoy the good trout fishing and beautiful surroundings of nearby streams.

At the time of his retirement, Jim was the leader of the Coastal and Pacific Fisheries Investigation within the Coastal Fisheries Resources Division at the Southwest Fisheries Center. He received his early education in Boise schools and graduated from the University of Kentucky in 1944 with a degree in civil engineering. He later earned an M.S. in marine biology from Oregon State University. His government service also included time spent with the army in India during World War II and a stint as a surveyor for the U.S. Geological Survey.

His long career in fisheries began in 1949 when he transferred to the sardine investigation of the then U.S. Fish and Wildlife Service in San Diego. He joined others to begin the federal government's cooperative investigations with Scripps Institution of Oceanography into the reasons for the catastrophic decline in Pacific sardine landings. His first cruise was on the *Black Douglas* (Marine Life Research Group Cruise No. 8) in October 1949, working from Mendocino to the Columbia River. Jim's talent and dedication were immediately evident, and he became responsible for planning cruises, processing plankton samples, and reducing data. In later years he supervised the conversion of field and laboratory data procedures to a computerized system. Jim emphasized accuracy and consistency in all aspects of his CalCOFI work and is largely responsible for the high quality of the time series. He was the author of a series of scientific reports on zooplankton volumes, coauthor of several papers on plankton volume loss with time of preservation, and collaborator in the development of a high-speed plankton sampler.

Jim was a wonderfully warm and cheerful man. He always greeted people with a smile and friendly conversation. He never hesitated to offer his help when it was needed — and it usually was. Those of us who were fortunate enough to know Jim will always remember his unbounded loyalty, generosity, and kindness.

Part I

REPORTS, REVIEW, AND PUBLICATIONS

REPORT OF THE CALCOFI COMMITTEE

The CalCOFI family of member agencies observed its fortieth anniversary in 1989 by acknowledging past accomplishments and evaluating present strengths in preparation for the global challenges of the years to come. The 40-year CalCOFI collection of physical, chemical, biological, and meteorological data from the California Current is the most complete ocean time series in the world, and has led to an understanding of the pelagic ecosystem that is unmatched in any comparable marine region. CalCOFI has also served as a model of successful collaboration among diverse agencies. An article in this volume chronicles the early history of CalCOFI and the roles of the visionary scientists and managers who partnered in its development.

The sardine resource continues to recover, and the California Department of Fish and Game (CDFG) permitted the fifth 1,000-ton quota fishery in as many years. For the second year, the quota, which opened on January 1, 1990, was allocated between northern (200 tons) and southern (800 tons) California. The 800-ton quota was landed in one week, completing the shortest season to date.

Fishery-related cruises included a 50-day groundfish survey to collect sablefish eggs and evaluate the use of the egg production method for determining spawning biomass; two cruises to collect sardine eggs off southern California and northern Baja California for biomass assessment; two night-lighting cruises to assess recruitment of juvenile Pacific mackerel; and one midwater trawl survey to collect young anchovy, mackerel, and sardines. The quarterly CalCOFI cruises surveying the southern California sector of the California Current were completed. In addition, a rapid but intensive hydrographic survey of the periphery of the station grid was conducted just after the summer cruise, and analysis of data from the 1988 biological/physical survey of the Ensenada Front continued. The purchase of a CTD will permit a gradual change in the techniques used on the CalCOFI surveys, improving the vertical resolution of data without impairing the continuity of the time series.

We used 1989 CalCOFI collections of anchovy eggs and larvae to estimate daily egg production, which was incorporated into a stock synthesis estimate of anchovy spawning biomass. Unusually low water temperatures may have inhibited sexual maturity, reduced spawning activity, and resulted in a low estimate of spawning biomass, since the 1988 year class appeared large, and total biomass was judged to be high. National Marine Fisheries Service (NMFS) and CDFG scientists prepared an amendment to the anchovy Fishery Management Plan to allow a small reduction fishery under circumstances when the total biomass is high but the spawning biomass is below the cutoff level for fishing.

At the third annual meeting of MEXUS-Pacifico, it was suggested that the cooperative scope of this fisheries research agreement between the United States and Mexico be broadened beyond coastal pelagic species to include sea lions, sea turtles, and remote sensing. We continued our routine exchange of fisheries and biological data, and conducted two cruises in Mexican waters to estimate anchovy egg production. For 1990, we planned a port sampling workshop, joint egg production cruises to assess anchovy and sardine biomass, a stock synthesis workshop, and a workshop on fisheries applications of satellite technology. A workshop on aging pelagic fishes was held in Ensenada.

CalCOFI continued to support the Spanish-Portugese Sardine Anchovy Recruitment Program (SARP). We hosted a meeting to review work accomplished over the last three years by SARP participants. A planning meeting sponsored by the Intergovernmental Oceanographic Commission (IOC) of the United Nations Education, Scientific and Cultural Organization followed immediately, and included an *ad hoc* expert consultation session on SARP.

In a break with tradition, the 1989 CalCOFI Conference was held at the Scripps Institution of Oceanography. The symposium, which consisted of invited addresses and a panel discussion, was organized to honor the fortieth anniversary of the CalCOFI program, and to consider what society and its policymakers can reasonably expect in terms of scientific advice concerning large-scale changes in the ocean. Other facets of the anniversary celebration included the preparation of a brochure and a videotape (for which E. Venrick deserves special thanks) describing CalCOFI and some of its notable achievements; the construction of two new permanent exhibits at the Scripps aquarium-museum; a presentation to Roger Revelle honoring his role in CalCOFI's early years; and some unusually elaborate wining and dining. A special CalCOFI exhibit and a continuous showing of the CalCOFI video were also part of the NMFS Southwest Fisheries Center twenty-fifth anniversary rededication and open house.

Many thanks to the officers and crews who assist us in our work on the University of California RV *New Horizon*, the National Oceanic and Atmospheric Administration ship *David Starr Jordan*, the Southern California Ocean Studies Consortium RV *Yellowfin*, the RV *Shana Rae*, the RV *Westwind*, and the FV *Jonathan Michael*. The Committee also wishes to thank everyone who contributed to volume 31 of *CalCOFI Reports:* editor Julie Olfe for her professional, thorough work and patient assistance; Spanish editor Carina Lange; past CalCOFI Coordinator George Hemingway and current Coordinator Patricia Wolf; and the many peer reviewers for their time, effort, and suggested improvements to the scientific contributions. The reviewers for this volume were Alice Alldredge, John Butler, Dan Cohen, Dudley Chelton, John Cullen, Deborah Day, Thomas Hayward, Dennis Hedgecock, Daniel Huppert, Sharon Kramer, John Marr, Milton Love, Alec MacCall, Marc Mangel, Douglas McLain, Geoffrey Moser, Michael Mullin, Tim Parsons, Elizabeth Venrick, Robin Waples, and James Waters.

The CalCOFI Committee:
Izadore Barrett
Richard Klingbeil
Michael Mullin

REVIEW OF SOME CALIFORNIA FISHERIES FOR 1989

California Department of Fish and Game
Marine Resources Division
330 Golden Shore, Suite 50
Long Beach, California 90802

Total landings of fishes, crustaceans, echinoderms, and mollusks decreased slightly (3%) this year, after three years of increase. The 1989 landings remained well below the ten-year average, by approximately 20%. However, they exceeded the 1985 low by nearly 32%.

Pelagic wetfish landings continued the upward trend that began in 1985, with a 2% gain over last year (table 1). Jack mackerel landings nearly doubled from last year, and market squid landings continued to be high. Pacific herring landings increased to the highest level since 1982, and the take of northern anchovy and Pacific sardine also increased. Pacific mackerel landings decreased by 16%.

Groundfish landings increased slightly, but differed markedly in species composition from last year. California halibut landings increased slightly but have remained fairly constant over the last nine years.

Landings of swordfish and the common thresher shark increased in 1989. However, landings for both the shortfin mako shark and the Pacific angel shark continued to decline.

Dungeness crab landings showed a slight increase, and Pacific Ocean shrimp landings continued to rise for the sixth straight year. Landings of the southern California spiny lobster were the highest since the mid-1950s.

Landings of the California red sea urchin decreased slightly, and the fishery is likely to come under increasingly restrictive management measures in 1990.

Albacore landings declined for the fourth consecutive year and reached an all-time low in 1989. Only 10% of the previous 25-year average was landed.

The sport catch increased, and rockfish retained the first-rank position.

PACIFIC SARDINE

The California Department of Fish and Game (CDFG) conducted sea surveys in May of 1988 to assess the spawning biomass of the Pacific sardine

TABLE 1
Landings of Pelagic Wetfishes in California (Short Tons)

Year	Pacific sardine	Northern anchovy	Pacific mackerel	Jack mackerel	Pacific herring	Market squid	Total
1966	439	31,140	2,315	20,431	121	9,513	63,959
1967	74	34,805	583	19,090	136	9,801	64,489
1968	62	15,538	1,567	27,834	179	12,466	57,646
1969	53	67,639	1,179	26,961	85	10,390	106,307
1970	221	96,243	311	23,873	158	12,295	133,101
1971	149	44,853	78	29,941	120	15,579	90,900
1972	186	69,101	54	25,559	63	10,080	105,043
1973	76	132,636	28	10,308	1,410	6,031	150,489
1974	7	82,691	67	12,729	2,630	14,453	112,577
1975	3	158,510	144	18,390	1,217	11,811	190,075
1976	27	124,919	328	22,274	2,410	10,153	160,111
1977	6	111,477	5,975	50,163	5,827	14,122	187,570
1978	5	12,607	12,540	34,456	4,930	18,899	83,437
1979	18	53,881	30,471	18,300	4,693	22,026	129,389
1980	38	47,339	32,645	22,428	8,886	16,957	128,293
1981	31	57,659	42,913	15,673	6,571	25,915	148,762
1982	145	46,364	31,275	29,110	11,322	17,951	136,167
1983	388	4,740	35,882	20,272	8,829	2,001	72,112
1984	259	3,258	46,531	11,768	4,241	622	66,679
1985	653	1,792	38,150	10,318	8,801	11,326	71,040
1986	1,283	2,105	45,503	12,209	8,405	23,454	92,959
1987	2,309	1,595	45,890	13,055	9,258	22,028	94,135
1988	4,172	1,618	47,278	11,379	9,721	41,040	115,208
1989*	4,308	2,700	39,725	21,820	10,134	38,288	116,975

*Preliminary

(*Sardinops sagax*). The egg production area method (EPAM) was used to determine if the observed spawning area (based on the occurrence of sardine eggs) exceeded the minimum critical spawning area of 2,300 nautical miles^2 (n.mi.^{2}), which is considered to indicate a 20,000-ton spawning biomass. A 20,000-ton spawning biomass is needed before a directed fishery for sardine can be permitted. Results indicated spawning activity over an area of about 2,508 n.mi.^{2} As a result, on January 1, 1989, a 1,000-ton directed fishery for sardines was opened for the fourth consecutive year.

Of the 1,000-ton quota, 800 tons were allocated for landings south of Point Buchon, and 200 tons were allocated for landings to the north. Also established were a 350-ton quota (beginning on January 1) for live bait, and a 250-ton quota (beginning on March 1) for use as dead bait.

The southern allocation of the 1989 directed fishery closed on January 12, three days earlier than the previous year, with total landings of 924 tons. Of these landings, 34% were pure loads and another 32% contained at least 70% sardines. The fish were caught by the southern California mackerel purse seine fleet and were almost exclusively canned for human consumption.

The northern California directed fishery saw no landings until late February, and remained open until early April. The 23 landings totaled 258 tons, and almost all were pure loads of sardines. The fish were all purchased by a single processor and marketed for human consumption.

The 250-ton dead bait quota proved difficult to monitor. Processors were not required to specifically declare the intended use of purchased sardines. Unless a landing exceeded the allowable incidental tolerance limit (35% sardines by weight, mixed with other fish), sardines within that load were generally not declared as dead bait. The quota was reached on March 20, when an estimated 250 tons had been landed. Live bait landings reported by fishermen amounted to 111 tons, with an additional 194 tons estimated by Department observers on sportfishing partyboats. As in 1988, young-of-the-year sardines did not make a strong showing in the 1989 live bait fishery.

Incidental landings of sardines in the mackerel fishery totaled 2,876 tons, down 7% from 1988 and reflecting the overall decrease of activity within the mackerel fishery. Sardines composed just under 6% of the mackerel landings, up slightly from the 5% observed in 1988. Fishermen continued to complain that the abundance of sardines interfered with mackerel fishing, and that sardines are displacing the mackerel from traditional fishing grounds. The tolerance limit for incidentally landed sardines mixed with other fish remained at 35% by weight.

Landings from all sources, excluding live bait, totaled 4,308 tons in 1989, as compared to 4,172 tons in 1988 and 2,309 tons in 1987 (table 1). Of this year's catch, 93% can be attributed to southern California landings, and just under 7% to the northern California allocation of the directed fishery. Sardine sold for approximately $100 per ton.

AB 2351, which became effective in September 1989, allocates the 250-ton dead bait quota among three geographic regions: 125 tons are reserved for landings south of Point Buchon, 50 tons for landings north of Point Buchon and south of Pescadero Point, and 75 tons for landings north of Pescadero Point. In addition, all sardine fishing for dead bait purposes must be accompanied by a written order from a processor; all fish landed must be kept in a whole condition; and the receipts must be labeled "For Dead Bait Only." This is to ensure that no fish allocated for dead bait will be used for other purposes, and to facilitate monitoring of landings against the quota.

Biomass estimation cruises using the EPAM were again conducted in 1989, and were expanded to include, for the first time, areas off northern Baja California as far south as Bahía de San Quintín. The area off central California north of Point Conception was not sampled in 1989 because no spawning activity was observed in this area during the 1988 cruise.

During the 1989 cruise, evidence of spawning was observed along the California coast out to the Channel Islands, from Santa Barbara south to Dana Point, and offshore in a large area west of Tanner and Cortez banks (figure 1), over a total area of 3,280 n.mi.^{2} Evidence of spawning off Baja California was limited to a small area totaling 400 n.mi.^{2} As in 1988, the total area over which spawning was observed exceeded what is considered to indicate a 20,000-ton spawning biomass; as a result, a 1,000-ton directed fishery for sardines was scheduled for 1990.

On December 7, the CDFG held a meeting for members of the sardine industry. The occasion provided an opportunity for the CDFG to present the methods and results of the fishery research that have led to the current and proposed management directives. The meeting also provided a forum for the industry to express its concerns and intentions regarding the sardine resource.

MARKET SQUID

Market squid (*Loligo opalescens*) landings in 1989 were 38,288 short tons (table 1): 31,011 tons (81%)

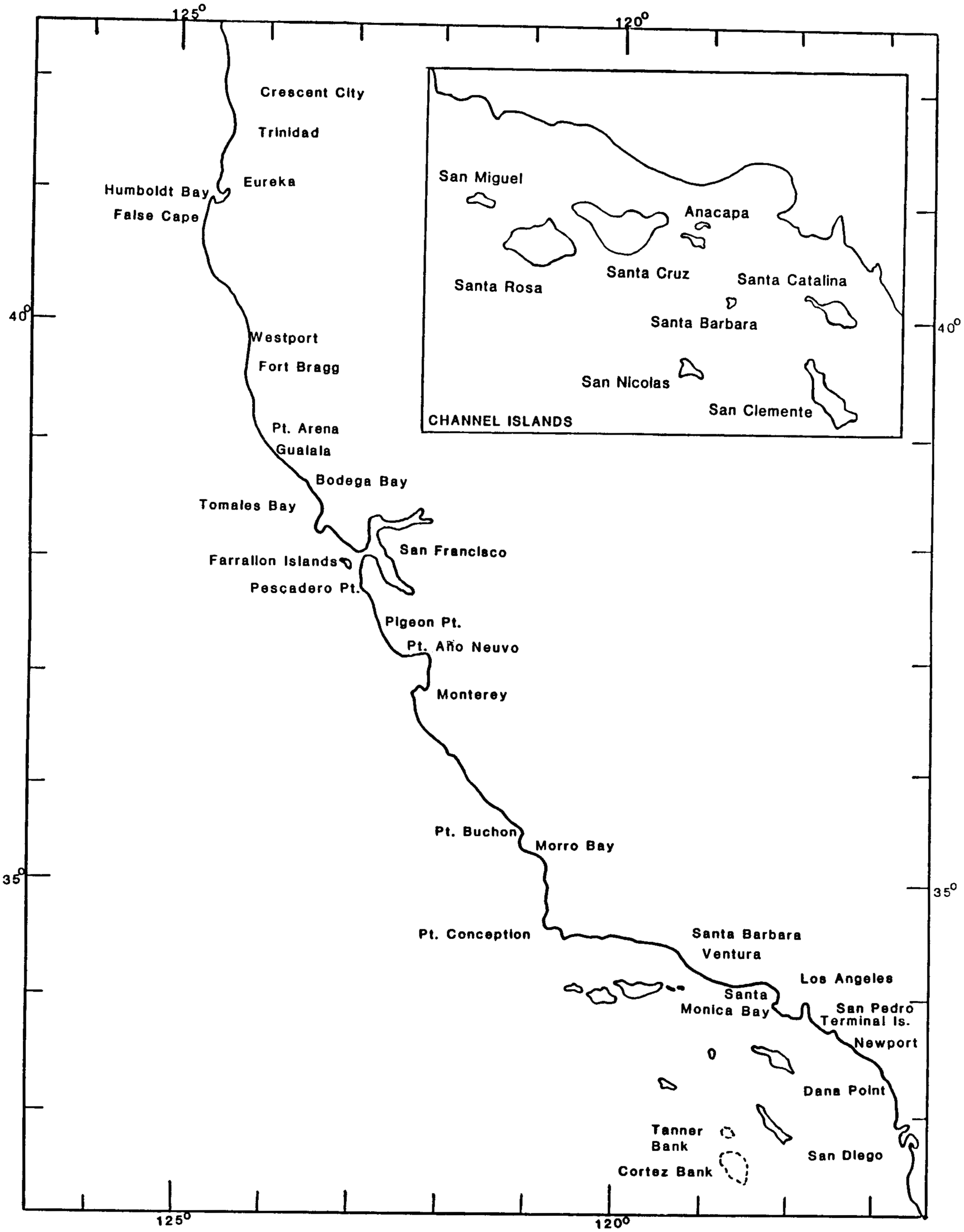


Figure 1. California ports and fishing areas.

were from the southern California fall-winter fishery; 7,274 tons (19%) from northern California (Monterey Bay area) spring-summer fishery; and 3 tons from other areas. The total ex-vessel value was approximately $5.9 million. Ex-vessel prices typically fluctuate from year to year and during the year. The ex-vessel price paid in southern California continued to be lower than that paid in the Monterey area. Southern California fishermen were paid from $120 to $200 per ton, while Monterey's ex-vessel price started at $200 per ton and increased to $260 per ton after a mid-April strike. Within the past 20 years, the ex-vessel price has been as high as $600 per ton.

Most squid were frozen for human consumption; some were sold fresh; some were used for dead bait; and some were used for live bait. Exported squid accounted for a large percentage of the total processed.

Annual squid landings during the last 20 years averaged 16,740 tons. Landings during years characterized by El Niño Southern Oscillation (ENSO), such as 1973, 1983, and 1984, were unusually low. However, large increases in landings in southern California and increased fishing effort in other areas in recent years suggest that the resource has been underutilized.

Before 1986, annual southern California landings averaged less than one-half of total statewide landings; since 1986, southern California landings have risen dramatically, averaging nearly 80% of the total. One reason for southern California's dramatic rebound following the 1983–84 ENSO is an increase in squid fishing effort around the Channel Islands off the coast of Santa Barbara (figure 1). During the 20 years before the 1983–84 ENSO, Santa Barbara area landings averaged about 1,500 tons per year; since then, landings have averaged about 11,700 tons per year. Several large Monterey-based boats now regularly participate in this fishery and deliver to Santa Barbara area ports. These squid are trucked to Monterey for processing.

During the last 20 years, Monterey Bay area annual landings averaged 6,060 tons. Since the 1983–84 ENSO, annual landings have averaged approximately 5,600 tons, slightly below the 20-year average and well below the annual average of 11,600 tons per year during 1978–82 (table 2). Landings of 7,274 tons in 1989 made this an above-average year. Of this total, 975 tons were landed at the port of Santa Cruz. These squid were caught north of Santa Cruz near Año Nuevo Island, an area fished only sporadically during post-ENSO years.

The Monterey Bay area fishery experienced major changes in 1989. Attracting lights were allowed for the first time in many years in the southern bight of Monterey Bay. Also, purse seiners were allowed to fish in this area, which before had been restricted to the use of lampara nets. By the end of the season, the Monterey Bay area round haul fleet had all converted to purse seine gear.

Despite the widespread use of attracting lights by nearly all the Monterey Bay fleet, the fishing community appears to be split on the issue of using lights. Many fishermen contend that lights disrupt squid spawning, which could adversely affect the fishery in the future. Others point out that southern California fishermen have used lights for many years without any apparent effects on spawning. They also claim that lights allow the fishermen to use shallower nets, which can be fished off the bottom, thus protecting squid egg clusters attached there.

In 1987 and 1988, the CDFG authorized three Monterey boats to experiment with purse seines in southern Monterey Bay to test their effectiveness relative to lampara nets, and their effects on squid egg clusters and bottom habitat. As a result of this study, all round haul nets were authorized in this area in 1989. Many fishermen were especially interested in the half-purse drum seine, which allows a smaller crew to work the gear. Continuing low ex-vessel squid prices and difficulties in finding enough crew have heightened interest in drum seines. Few boats,

TABLE 2
California Market Squid Landings (Short Tons)

Year	Monterey	Southern California	Other	State total
1970	4,314	7,982	0	12,295
1971	8,323	7,435	trace	15,759
1972	6,129	3,950	0	10,080
1973	620	5,140	0	6,031
1974	7,248	7,205	0	14,453
1975	2,495	9,316	trace	11,811
1976	2,511	7,642	0	10,153
1977	2,234	11,887	1	14,122
1978	10,328	8,571	trace	18,899
1979	14,183	7,842	1	22,026
1980	7,856	9,100	1	16,957
1981	14,134	11,779	2	25,915
1982	11,670	6,276	5	17,951
1983	542	950	509	2,001
1984	431	84	107	622
1985	4,202	7,039	85	11,326
1986	6,049	16,488	917	23,454
1987	5,269	16,381	378	22,028
1988	5,330	35,348	363	41,040
1989*	7,274	31,011	3	38,288

*Preliminary

however, switched to drum seines this year, primarily because a major capital outlay this year was for powerful (and expensive) attracting light systems that typically cost from $16,000 to $20,000. More boats will probably convert to drum seines in the future.

PACIFIC MACKEREL

The year began with 19,120 tons of Pacific mackerel (*Scomber japonicus*) already landed through the first half of the 1988–89 season (July 1 through June 30). Current law allows an open fishery when the biomass exceeds 150,000 tons. Since the biomass was estimated to be 220,000 tons, no quota restrictions were in effect.

During the first three months of the year, mackerel landings were high, even though the sardine fishery in early January and poor weather throughout the period threatened to interfere with mackerel effort. Large Pacific mackerel, which had been relatively uncommon since the previous October, became more available in March. Landings declined during April, May, and June, as fishermen were occasionally hampered by rough weather and sometimes turned their attention to Pacific bonito (*Sarda chiliensis*). Catches of small and large fish were made, although large fish were reportedly difficult to find.

The 1988–89 season closed on June 30, 1989, with a total catch of 43,398 tons of Pacific mackerel. This is only slightly below the previous five-year season average. Although mackerel landings were lower than in the last two years, revenues for the San Pedro purse seine fleet from all fish species increased over last year, primarily because of higher landings of bluefin tuna (*Thunnus thynnus*) and record landings of squid. Pacific mackerel contributed 79% to statewide landings of mackerel, and nearly 99% of all Pacific mackerel landings were made in southern California.

The 1989–90 season opened on July 1, 1989, with no quota restrictions, based on a biomass estimate of 263,000 tons. Landings during the third quarter, which were only fair, were comparable to the previous quarter. Effort was often directed toward bluefin tuna, and in September, jack mackerel (*Trachurus symmetricus*) began to dominate landings. Fourth-quarter landings of Pacific mackerel were low. Jack mackerel dominated landings in October and November, and price disputes between fishermen and United Food Processors (UFP), a Terminal Island cannery, inhibited fishing and persisted through the quarter. UFP reportedly wanted to reduce the price paid to fishermen from $135 to $80 per ton for fish used for pet food. The purse seine fleet has seen the mackerel price decline steadily: in the early 1980s the canneries paid $200 per ton, and over the last few years the price declined to $155 per ton.

By the end of the year, only 15,447 tons of Pacific mackerel had been landed toward the 1989–90 season total. Landings of Pacific mackerel for the year totaled 39,725 tons (table 1). These are the lowest annual landings since 1985 and are 11% lower than the previous five-year average. Northern California landings contributed less than 1% to the year's total. This continues a steady decline in the proportion of the catch occurring in Monterey.

In general, market conditions were stable through the year. Processors continued to impose landing limits, which sometimes were as low as 20 tons per boat, per day, to limit landings of small fish. Two changes in southern California mackerel processing occurred in 1989. Coast Cannery, a pet food canning facility operated by Pan Pacific, ceased operation in October. In September, Starkist Seafoods' pet food production operation was renamed Heinz Pet Products, after the parent company.

The CDFG continued to conduct "night-light" surveys in which mackerel were sampled by hook and line, in an effort to develop an early, fishery-independent index of year class strength. Two cruises, one in April and another in May, were conducted this year. Results suggest that the 1988 year class is at least similar in strength to the 1986 year class. Fishery data support this conclusion: the 1988 year class — which as yearlings contributed nearly 50% by weight of the catch this year, and which appears to be very strong — and the 1986 year class — which contributed 13% — together made up most of the landings.

NORTHERN ANCHOVY

Landings of northern anchovy (*Engraulis mordax*) for reduction purposes in 1989 were limited primarily by poor market conditions. As in the 1987–88 reduction season, high fish meal prices during the 1988–89 season were not reflected in the price offered to local fishermen. California processors thought that an increase in price to $35 per ton would attract some fishermen away from other species to anchovy, but few fishermen considered the price to be fair. For this reason, northern processors issued no orders during the latter half of the 1988–89 season. Although processors in the southern region issued orders for anchovy, local purse seine fishermen continued to concentrate on more lucra-

TABLE 3
Anchovy Landings (Short Tons) for Reduction

Season	Southern area	Northern area	Total
1967–68	852	5,651	6,503
1968–69	25,314	2,736	28,050
1969–70	81,453	2,020	83,473
1970–71	80,095	657	80,752
1971–72	52,052	1,314	53,366
1972–73	73,167	2,352	75,519
1973–74	109,207	11,380	120,587
1974–75	109,918	6,669	116,587
1975–76	135,619	5,291	140,910
1976–77	101,434	5,007	106,441
1977–78	68,467	7,212	75,679
1978–79	52,696	1,174	53,870
1979–80	33,383	2,365	35,748
1980–81	62,161	4,736	66,897
1981–82	45,149	4,953	50,102
1982–83	4,925	1,270	6,195
1983–84	70	1,765	1,835
1984–85	78	0	78
1985–86	0	1,595	1,595
1986–87	0	42	42
1987–88	0	122	122
1988–89*	0	258	258

*Preliminary

tive mackerel and squid. Consequently, no reduction landings took place in either the northern or southern regions after December. The 1988–89 season closed on June 30, with six landings totaling 258 tons (234 MT; table 3).

National Marine Fisheries Service biologists estimated the 1989 spawning biomass of northern anchovy to be 235,892 tons (214,000 MT) and the total biomass to be 1,111,118 tons (1,008,000 MT). Normally when the spawning biomass is less than 300,000 MT, the Anchovy Fisheries Management Plan allows for zero take of northern anchovy for reduction purposes. However, spawning biomass was low because cold sea-surface temperatures caused one-year-old fish to mature more slowly and not be actively spawning when the surveys were conducted. Because of this anomaly, the Pacific Fisheries Management Council (PFMC) determined that an emergency existed in the northern anchovy fishery and requested the secretary of commerce to approve regulations that would allow a 5,000-MT reduction fishery during the 1989–90 season. The secretary approved the emergency rule, which went into effect on September 25, 1989.

For the first time since the 1984–85 season, reduction landings were recorded in southern California. Two landings totaling 120 tons (109 MT) were delivered to a Terminal Island cannery in December 1989. The price paid was $40 per ton. However, southern California processors stated that there were no plans to reduce anchovies for the rest of the 1989–90 season. No landings were made in the northern area through December 1989, and orders are not anticipated before the season's end.

Total anchovy landings during 1989 included 120 tons for reduction, 2,580 tons for nonreduction purposes (table 1), and 5,064 tons for live bait. Although live bait landings increased from 1988, most live bait fishermen rated 1989 as only slightly better than average.

PACIFIC HERRING

The 1989 annual roe herring catch (*Clupea harengus pallasi*) increased 4%, to 10,134 tons (table 1), and the 1988–89 seasonal (December–March) roe herring catch also increased 4%, to 10,022 tons. California's 1988–89 roe herring quota of 9,990 tons was taken, because of a quota overrun of 236 tons in San Francisco Bay. Even though the 1988–89 Tomales Bay roe herring quota was reduced from 750 to 400 tons, there was a quota shortfall of 187 tons in Tomales Bay. In Crescent City Harbor, the 30-ton quota was taken; in Humboldt Bay there was a 16-ton shortfall on the 60-ton quota.

The 1988–89 San Francisco Bay herring spawning biomass estimate was 66,000 tons; hydroacoustic and spawn survey estimates agreed. The population declined about 5% from the 1988 estimate because average recruitment of the 1987 year class was not strong enough to maintain the increasing trend in abundance apparent since the 1982–83 El Niño.

In Tomales Bay the herring biomass continues to decline. The 1988–89 spawning-ground surveys estimated spawning escapement of only 167 tons. Spawning biomass, which includes the catch, was only 380 tons. Both are historic lows for Tomales Bay. The average structure of the Tomales Bay herring catch — primarily age 4- through 7-year-olds — appears normal. This does not support the decline in abundance of the Tomales Bay population. It is believed that most of the Tomales Bay population is avoiding the bay because of the recent drought and lack of freshwater runoff near historic spawning areas.

Based on 1988–89 spawning biomass estimates, the 1989–90 San Francisco Bay roe herring quota remained at 9,500 tons. However, because of the lack of spawning escapement in Tomales Bay, further restrictions were placed on the 1989–90 season. A spawning threshold of 2,000 tons was established for the 1989–90 Tomales Bay season, with no fishing allowed until 2,000 tons of herring had spawned. If 2,000 tons escapement was reached before January 31, the fishery would open with a 400-ton quota.

The 1989–90 season began quickly in San Francisco Bay, and the December quota of 1,999 tons was taken easily. In the Tomales Bay area no herring were caught in December, and there was no spawning activity.

At the beginning of the 1989–90 season herring buyers were offering $1,000 per ton for gill net herring, the same as last season. However, the price offered for round haul herring dropped to $400 per ton. Japanese buyers are willing to pay more for the larger, better-quality herring caught by gill nets.

GROUNDFISH

California's 1989 commercial groundfish harvest was 40,510 MT, with an ex-vessel value of approximately $28,879,000. All-species 1989 landings increased only 3%, or 1,090 MT, from 1988 but differed markedly in species composition (table 4). Setnet landings continued their recent decline, contributing less than 5% of all 1989 groundfish landings. The general historical pattern of landings by gear continued during 1989. Bottom and midwater trawl landings accounted for 86.2% of total landings, followed by line gear (6.6%), setnets (4.8%), and traps (2.4%). The size and composition of the trawl, trap, and longline fleet did not differ markedly from recent years. Rockfish (*Sebastes* spp.), Dover sole (*Microstomus pacificus*), Pacific whiting (*Merluccius productus*), and thornyheads (*Sebastolobus* spp.) were the principal species harvested in 1989.

The domestic shore-based Pacific whiting fishery in California achieved record landings during its six-month season. This midwater-trawl fishery, located off Eureka and Crescent City, has grown from approximately 3,000 MT in 1987 to 7,300 MT in 1989. In the 1989 Pacific whiting fishery a conflict occurred between shore-based and joint venture (JV) operations; the shore-based trawlers testified before the PFMC that the large and highly efficient JV fleet dissipated whiting schools within the small operating radius of the shore-based fleet. The JV fishery involves U.S. trawl vessels delivering whiting to foreign processing vessels at sea. The fleet concentrated its effort off northern California early in the season. Although CDFG scientists and others could not verify this alleged JV impact, it was cited by processors as the cause for failure to meet a domestic 1989 production goal of 15,000 MT. A reduced whiting optimum yield (OY) and an increased JV demand in 1990 are expected to intensify conflicts within the industry.

TABLE 4
California Groundfish Landings (Metric Tons)

Species	1988	1989	Percent change
Dover sole	8,176	7,713	−6
English sole	1,062	1,015	−4
Petrale sole	785	840	7
Rex sole	840	735	−13
Thornyheads	4,524	5,319	17
Widow rockfish	1,847	1,566	−15
Other rockfish	9,846	9,978	1
Lingcod	873	1,262	45
Sablefish	3,784	3,583	−5
Pacific whiting	6,541	7,302	12
Other groundfish	1,142	1,197	5
Total	39,420	40,510	3

Despite limits on trip poundage and frequency for deepwater-assemblage landings of sablefish (*Anoplopoma fimbria*), Dover sole, arrowtooth flounder (*Atheresthes stomias*), and thornyheads, a robust Asian market drove thornyhead landings to a record high of 5,319 MT. Much of the 70% increase in thornyhead landings since 1987 is due to the development of a market for headed-and-gutted longspine thornyheads (*Sebastolobus altivelis*), which are typically too small to fillet for domestic consumption. Additional regulations and reduced demand apparently contributed to a 6% drop from 1988 in Dover sole landings.

Federal and state regulations for 1989 affected the harvest of sablefish, Dover sole, thornyheads, and widow rockfish (*Sebastes entomelas*). A coastwide, Washington-Oregon-California (WOC), widow rockfish OY of 12,400 MT (300 MT greater than in 1988), with a 30,000 pound-per-week trip limit was imposed. Excellent fishing conditions in the first quarter of 1989 accelerated widow rockfish landings. As a result, the PFMC's Groundfish Management Team projected that a 51% reduction in rate of landings would be required to extend the fishery to year's end. Consequently, the PFMC reduced the trip limit to 10,000 pounds per week, or 20,000 pounds biweekly, effective April 26, 1989. In early October a by-catch-only trip limit of 3,000 pounds was imposed to further slow the fishery. The widow rockfish quota was eventually filled, and the fishery was closed in mid-December. The midseason restrictions undoubtedly contributed to the 15% reduction in California's widow rockfish catch; fishermen reported that fishable aggregations were available and quite vulnerable to trawl gear during most of the year off northern California.

Regulation of the WOC sablefish fishery increased in complexity during 1989. The year began with an OY range of 10,400 to 11,000 MT. The intent was to manage toward 10,400 MT, with a 600-MT reserve to accommodate by-catch and gear other than trawl if OY was attained before year's end. Initial allocations, after a set-aside of 22 MT for Native Americans, were 5,397 MT (52%) for trawl and 4,981 MT (48%) for other gear. To maintain a year-round trawl fishery, a trip limit was imposed of 1,000 pounds, or 45% of the deepwater assemblage of sablefish, Dover sole, thornyheads, and arrowtooth flounder — whichever was greater. The intent was to discourage targeting on sablefish, while allowing sablefish landings from the deepwater assemblage fishery. However, this trip limit did not slow landings sufficiently. At its April 1989 meeting, the PFMC modified the trip limit and transferred the 600-MT reserve and 400-MT other-gear quota to the trawl fishery in order to minimize trawl discards late in the year. The revised trip limit restricted the deepwater assemblage to one landing per week of not more than 30,000 pounds total. Of this total, sablefish could constitute 1,000 pounds or 25% by weight, whichever was greater. On October 4, PFMC removed the assemblage restrictions but retained the sablefish limits until year's end. Preliminary analysis revealed that the assemblage trip-frequency and percentage restrictions effectively prevented premature quota attainment. California trawl sablefish landings of 2,200 MT accounted for approximately 40% of WOC landings.

Directed nontrawl sablefish fishing was terminated on July 17, when only 200 MT of quota remained for incidental catches. A 100-pound trip limit subsequently was imposed and remained in effect until early October. In response to testimony from the Newport, California, dory fleet and northern California rockfish longline representatives, PFMC increased the limit to 2,000 pounds per trip, or 20% of all groundfish aboard. California accounted for 1,383 MT, or 31%, of the 4,500 MT landed by nontrawl gears.

DUNGENESS CRAB

California Dungeness crab (*Cancer magister*) landings during the 1988–89 season totaled 9.2 million pounds, only slightly more than the 1987–88 landings of 8.7 million pounds.

Landings for the northern California ports of Crescent City, Trinidad, Eureka, and Fort Bragg (figure 1) were 5.42, 0.90, 1.28, and 0.24 million pounds, respectively. Production in Crescent City almost doubled, and total northcoast landings exceeded 1987–88 seasonal landings by 2.22 million pounds. A total of 318 vessels made 5,436 trips and averaged 1,399 pounds per trip. The price paid to the fisherman on December 1, opening day, was $1.25 per pound.

The 1988–89 season in the San Francisco/Bodega Bay area ended with a landing total of 1.44 million pounds. This is slightly less than half of the total for the previous season. Approximately 215 boats made a total of 2,866 trips for an average of 501 pounds per trip.

PACIFIC OCEAN SHRIMP

Statewide landings of Pacific Ocean shrimp (*Pandalus jordani*) in 1989 increased to 13.3 million pounds, from 11.1 million pounds landed in 1988. This was the second largest total ever and the sixth consecutive rise in statewide landings. Areas of production were Area A (Oregon border to False Cape) Area B-1 (False Cape to Point Arena), and Area C (Pigeon Point to the Mexican border; figure 1).

Shrimp landings at Area A ports totaled 12.5 million pounds — a 2.2 million pound increase over 1988 deliveries. These landings tie with 1978 as the second largest ever. The total landings comprised 11.74 million pounds from Area A waters, 250,000 pounds from Area B-1, and 465,000 pounds from Oregon waters. The season opened on April 1, with fishermen receiving a split price of $0.40 per pound for shrimp at or below 140 per pound and $0.25 per pound for smaller shrimp. Shrimpers were on strike for 26 days in July over a price disagreement, which was finally settled at $0.35 per pound for the larger size group (140 count or better), with no guarantee of any payment for smaller shrimp.

A total of 56 boats (36 single-rigged and 20 double-rigged) delivered shrimp to Area A ports during 1989, down one boat from 1988. Single-rigged vessels had an average seasonal catch rate of 543 pounds per hour, an increase of 55 pounds per hour over 1988. Double-riggers averaged 842 pounds per hour, up from 758 pounds per hour in 1988.

One-year-old shrimp made up 65% of the catch in April and 85% in October during 1989. This was approximately a 10% decrease in one-year-olds from 1988. There were no incoming year-class (zero-aged) shrimp found in the sampled catch, the first year-class failure since the total fishery failure in 1983.

Landings in area B-1 were 833,000 pounds this season, compared to 379,000 pounds in 1988. Four local single-rigged boats, along with one double-rigged and two single-rigged vessels from Crescent City and one single-rigged boat from Santa Cruz, fished the B-1 area this year. Ninety-one percent of the catch was landed at Noyo Harbor in Fort Bragg. Forty percent of the Fort Bragg landings occurred in April and 73% in April, May, and June combined. One local boat accounted for 54% of the Fort Bragg landings.

Catch-per-unit-of-effort (CPUE), measured as pounds landed per delivery, started at 14,600 pounds per delivery in April. CPUE declined to 9,000 pounds in May and June, climbed back to 14,200 pounds in August, and declined again to 4,000 pounds in October. One-year-olds made up 27% of the sampled catch in April, when ovigerous females were also noted. In May, one-year-olds composed 73% of the sampled catch and ranged from 40% to 52% of the catch through the end of the season. There were no zero-aged shrimp noted during the season. The count per pound increased in May, causing several fishers to return to groundfish trawling until the count improved.

The total shrimp catch for Area C in 1989 was 24,000 pounds, down considerably from the 380,000 pounds landed in 1988. This was the least productive year since 1978, when no shrimp were landed in Area C. Four single-rigged vessels made five trips and averaged 314 pounds per hour.

SWORDFISH AND SHARK

Landings of swordfish (*Xiphias gladius*) for 1989 rose to 2.8 million pounds, a 15% increase from 1988 (table 5). Harpoon fishermen reported landing only 422 fish, making 1989 one the poorest harpoon years on record. Drift gillnetters reported landing 11,190 fish, nearly the same number reported in 1988.

TABLE 5
Landings of Selected Shark Species and Swordfish (Pounds)

Year	Shortfin mako shark	Swordfish	Common thresher shark	Pacific angel shark
1977	19,911	511,388	129,522	366
1978	26,765	2,604,233	302,054	82,383
1979	35,079	586,529	735,726	128,295
1980	154,529	1,197,187	1,805,978	110,037
1981	274,217	1,142,897	1,973,411	268,640
1982	527,006	1,677,020	2,396,960	317,953
1983	322,854	2,601,600	1,722,056	351,344
1984	239,687	4,429,540	1,662,587	632,937
1985	225,535	5,196,685	1,540,770	1,237,810
1986	473,608	3,845,932	606,583	1,241,130
1987	602,718	2,741,015	525,076	940,187
1988	488,136	2,484,428	549,516	487,278
1989*	388,312	2,850,734	647,865	267,577

*Preliminary

Fish taken early in the season (August–September) were much larger (averaging 150 pounds) than fish caught later in the year (averaging 125 pounds). Catch locations were centered off San Francisco, Morro Bay, and San Diego (figure 1).

CPUE for gill net boats remained nearly identical to 1988. Both years had a catch rate of two fish per day, per boat. The CPUE for harpoon gear was 0.33 fish per day, per boat in 1989 and 0.40 fish per day, per boat in 1988.

Common thresher shark (*Alopias vulpinus*) landings in California during 1989 reached 647,865 pounds, 17% more than the 550,000 pounds in 1988. No thresher sharks were landed in Oregon or Washington during 1989 because these states ended their permit fishery, mainly because of decreased interest by local fishermen, higher incidental catch of marine mammals and leatherback turtles, and concern for the status of the resource. Currently, the Pacific States Marine Fisheries Commission is coordinating the development of a coastwide management plan.

Shortfin mako shark (*Isurus oxyrinchus*) landings decreased 20% from 1988. Of the 388,312 pounds taken, 46% was caught by the experimental drift longline fleet and the remainder by drift gill net boats. Most of the fish were taken off central and southern California during the summer and fall. The California Fish and Game Commission reauthorized the use of drift longline gear for 1990 under the conditions of a 175,000-pound quota and the development of a market for blue shark (*Prionace glauca*), specifying that 40,000 pounds be marketed for human consumption.

Pacific angel shark (*Squatina californica*) landings in 1989 continued their decline for the third straight year, reaching only 267,577 pounds. A management plan establishing a minimum size limit was enacted in 1989 to protect juveniles and a portion of the spawning stock. Reduced availability, decreased market demand, and the size limit appear responsible for the lower landings. The fishery continued to be centered off Santa Barbara and Ventura counties, although some landings occurred in San Diego and San Pedro (figure 1).

CALIFORNIA HALIBUT

California halibut (*Paralichthys californicus*) landings for 1989 were 550 MT, 9% more than the 505 MT recorded in 1988 (table 6). Catches over the last five years have remained fairly constant, averaging 542 MT, with landings for 1989 exceeding the 13-year average of 448 MT. During 1989, 55% of the halibut landings occurred north of Point Conception. Landings south of Point Conception accounted for the remaining 45%, a 7% decrease from 1988.

The highest landings occurred during winter and fall, with peak catches in February (58 MT) and October (60 MT). Entangling nets (trammel and set gill nets) accounted for 51% of all halibut taken, followed by trawl (20%), unknown gear (16%), hook and line (11%), and other gears (2%). Most of the trawl-caught (91%) and hook-and-line-caught (93%) halibut were taken off central California. The southern California area accounted for nearly 61% of all halibut taken by entangling nets; 39% came from north of Point Conception. Ex-vessel prices for California halibut ranged from $0.45 per pound in San Francisco to $5.35 per pound in Ventura, and averaged $2.25 per pound statewide.

CALIFORNIA SPINY LOBSTER

The southern California spiny lobster (*Panulirus interruptus*) fishery landed 650,000 pounds during the 1988–89 season (first Wednesday in October to first Wednesday after March 15), making it the best season since 1955–56, when 790,000 pounds were landed.

Historically, landings of lobster from California waters peaked at 1.1 million pounds in the 1949–50 season. Seasonal landings generally declined over the next 25 years, reaching a low of 152,000 pounds in 1974–75. Since then, there has been a general upward trend.

The 1988–89 season's total is an increase of 173,000 pounds (36%) from the 1987–88 season, in spite of a slight decrease in participating fishermen. Only 303 permits were issued, down 5.6% from the 1987–88 level of 321. The number of permittees has decreased each season since 1984–85.

The remaining fishermen, however, may be fishing a greater number of traps, keeping effort high. Most of the catch was taken early in the season: 50% in October, 19% in November, and 12% in December. The remaining 19% was caught from January to March.

Ex-vessel price ranged from $5.00 to $7.00 per pound, averaging about $5.50. With landings at 650,000 pounds, the fishery was worth $3.6 million to the fishermen, a $1.1 million (43%) increase over the previous season.

TABLE 6

California Halibut Landings (Metric Tons)

Year	North of Pt. Conception	South of Pt. Conception	Total
1977	25	186	211
1978	34	165	199
1979	54	205	259
1980	90	231	321
1981	163	409	572
1982	206	339	545
1983	256	248	504
1984	153	345	498
1985	144	429	573
1986	240	312	552
1987	192	347	530
1988	229	276	505
1989*	305	245	550

*Preliminary

ALBACORE

In 1989, albacore (*Thunnus alalunga*) landings in California reached an all-time low. Only 914 tons of albacore were brought in; this is approximately 10% of the previous 25-year average. For the past five years, commercial catches of albacore have declined substantially. From 1984 to 1987, season totals decreased by 50% each year. In 1988 and 1989, the rate of decline slowed to 20% per year (figure 2). Fishing effort this year was moderate, with 225 boats participating in the fishery. Of these, 78 landed over a ton of albacore.

The season had a promising start in July. Catches were reported at Rosa Bank and Geronimo Island, off southern Baja California, as well as off the central California coast. In addition, toward the end of July, a good sport fishery developed off southern California. As the season progressed, however, it became apparent that the center of fishing activity was once again off the Washington coast. Seventy percent of California's albacore fleet spent August through September in the north. A few vessels even followed the albacore as far north as the Queen

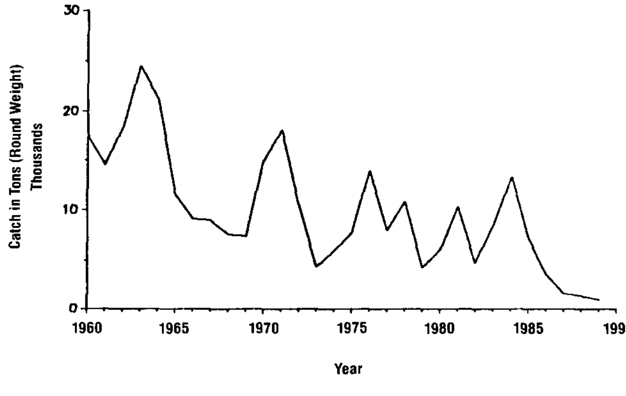


Figure 2. Annual albacore landings for California, 1960–89

Charlotte Islands, Canada, before returning home in October. Drift gill net boats fishing primarily off central California had incidental catches of four albacore per day throughout the season. The fish caught off California ranged between 11 and 15 pounds on the average. Very few large fish (greater than 25 pounds) were seen.

Price agreements between the Western Fishboat Owners Association and Pan Pacific and Starkist canneries began at $1,500 per ton for fish over 9 pounds and $1,000 per ton for fish 9 pounds and under. This was a drop of $200 per ton from the 1988 price and was due mainly to the large quantities of albacore coming in from Japan. In addition, vessels landing at ports other than Terminal Island were charged a shipping fee, which reduced the price paid by another $200 per ton. By the end of August, the shipping fee was eliminated to encourage fishermen to sell their loads to the canneries instead of directly to the public. Overall, dockside sales increased 3% in 1989.

Oceanic and market conditions did not appear to influence the poor season in California this year. A strong corridor (warm water/cold water front) developed in June and should have led albacore into southern California waters and then along the coast. The salmon season was poor, so albacore fishermen were not drawn away to that fishery. The poor albacore season in California can generally be attributed to a lack of large albacore off southern California and an above-average season off Washington. Fishermen had a different opinion: many considered foreign high-seas drift gillnetting to be the cause.

RIDGEBACK AND SPOT PRAWN

Ridgeback prawn (*Sicyonia ingentis*) are fished commercially, primarily by otter trawl. They may be trawled by permit from October 1 through May 31. During the restricted period an incidental catch of 50 pounds is allowed. Landings for 1989 were approximately 176,104 pounds, 23% greater than the previous year's catch (table 7). Most of the catch came from the Santa Barbara Channel (figure 1). Log data showed a CPUE of 66 pounds per hour, virtually unchanged from last year. The average ex-vessel price in the Santa Barbara region was $1.25 per pound.

Spot prawn (*Pandalus platyceros*) landings increased to 179,718 pounds in 1989, about 8% more than in 1988 (table 7). The spot prawn is a larger shrimp and brings a higher price than the ridgeback. Originally caught in traps, spot prawns were predominantly caught by trawl by the mid-1970s. With the recent increasing demand for live products, trapping is on the increase. For the second year, log data indicate that just over half the catch is taken by traps.

TABLE 7
Ridgeback Prawn and Spot Prawn Landings (1,000s of Pounds)

Year	Ridgeback prawn	Spot prawn
1980	276	69
1981	193	369
1982	141	300
1983	157	109
1984	623	49
1985	905	64
1986	672	102
1987	242	88
1988	143	167
1989*	176	179

*Preliminary

Spot prawns may be trawled by permit from February 1 through October 31. During the restricted period, an incidental catch of 50 pounds is allowed. Spot prawns can be harvested by trap year-round. Log data from 11 boats showed a healthy CPUE of 75 pounds per hour. Trawling took place in the Santa Barbara Channel, Santa Monica Bay, and off Santa Catalina Island (figure 1). Trapping occurred in the same locations and also off San Diego. Ex-vessel price in the Santa Barbara region was $3.50 to $5.00 per pound.

SEA URCHIN

In 1989 the red sea urchin (*Strongylocentrotus franciscanus*) fishery continued to be one of the major fisheries in the state. Landings for 1989 are estimated to be 50.9 million pounds, a 2.1% decrease from 1988 (table 8). Northern California landings are down 12.4% from 1988, whereas those from southern California increased 12.6%. Once again, Fort Bragg led all ports, with 30% of the statewide total. The southern California ports of Santa Barbara, Ventura-Oxnard, and San Pedro-Los Angeles had 11%, 16%, and 15% of the statewide total. The reduction in northern California landings is also reflected by a 20% drop in average pounds per landing from 1988. These decreases are attributed to the continued reduction of high-density, virgin stocks in northern California.

Divers, using surface-supplied air, harvest sea urchins by raking them into mesh bags, which are then air lifted to the surface and winched aboard the vessel. CPUE is measured as pounds harvested per

TABLE 8
Sea Urchin Landings (1,000s of Pounds)

Year	Northern California	Southern California	Total
1971	0	<1	<1
1972	<1	76	76
1973	18	3,594	3,612
1974	51	7,056	7,107
1975	3	7,323	7,326
1976	95	11,012	11,107
1977	386	16,208	16,594
1978	34	14,394	14,428
1979	237	20,307	20,544
1980	103	21,196	21,299
1981	194	24,720	24,914
1982	92	19,347	19,439
1983	61	17,207	17,268
1984	59	14,920	14,979
1985	1,921	18,074	19,995
1986	10,174	23,957	34,131
1987	23,600	22,500	46,100
1988	30,525	21,463	51,988
1989*	26,745	24,168	50,913

*Preliminary

diving hour. The northern California average was 570 pounds per hour in 1989, compared to 505 pounds per hour in 1988. In southern California the 1989 average CPUE was 323 pounds per hour, ranging from 166 at the Palos Verdes Peninsula to 516 at San Nicolas Island; in 1988, the average was 286 pounds per hour and ranged from 160 to 393.

Logbook data show that the majority of harvesting effort in northern California occurred between Fort Bragg and Gualala (figure 1), but increased effort also took place in the Westport area to the north and at the Farallon Islands to the south. Over 60% of the effort in southern California was expended at the Channel Islands, with 37% at the four northern islands and 17% at three of the southern islands. The San Diego coastal area was the highest mainland zone, receiving 15% of the effort.

Size distributions of sea urchins landed in northern California have changed slightly, with a mean size of 103 mm (108 mm in 1988). Only 3.6% of the samples were smaller than the 76-mm minimum size, which was adopted in March 1989. This new size regulation appears to have affected harvesting practices in southern California. The mean size of sampled sea urchins was 94 mm (91 mm in 1988), and the overall percentage of sea urchins below the minimum size dropped from 17% in 1988 to 10% in 1989. In coastal areas such as Santa Barbara and the Palos Verdes Peninsula, percentages of undersize sea urchins decreased from as high as 38% in 1988 to as low as 10% in 1989.

The sea urchin fishery is likely to come under increasingly restrictive management measures in 1990. The objective of these new measures will be to further reduce harvesting pressure, especially in northern California. Resource surveys and fishery monitoring programs will continue and will be increasingly important for evaluating management changes.

RECREATIONAL FISHERY

Catches from the California commercial passenger fishing vessel (CPFV, or partyboat) fleet can generally be considered indicative of nearshore and offshore sport angler success. The CPFV fleet can locate and catch any species available within the fishing area. Catches can vary widely for latitudinally migratory species, such as barracuda (*Sphyraena argentea*) and yellowtail (*Seriola lalandei*), and for highly migratory transoceanic species like albacore. Catches of resident species in nearshore areas may also show fluctuations associated with warmer oceanic regimes.

Partyboat landings for 1989—4.4 million fish—were slightly higher than in 1988. Rockfish maintained its first-rank position, with 2.1 million fish caught; this is about a 15% increase over 1988 (table 9).

Sand bass (*Paralabrax nebulifer*) landings again exceeded kelp bass (*Paralabrax clathratus*) landings, which increased 16% over 1988. Sculpin (*Scorpaena guttata*) maintained its sixth-place ranking: 25% more fish were caught than in 1988. The barracuda

TABLE 9
1989 Commercial Passenger Fishing Vessel Catch

Species/species group	Thousands of fish	Rank
Rockfish	2,135	1
Sandbass	415	2
Kelp bass	373	3
Pacific mackerel	350	4
Bonito	322	5
Sculpin	161	6
Barracuda	133	7
Salmon	110	8
Lingcod	75	9
Halfmoon	67	10
Yellowtail	61	11
Ocean whitefish	44	12
Albacore	29	13
Flatfish (misc.)	28	14
Sheephead	22	15
Skipjack tuna	20	16
Yellowfin tuna	17	17
White croaker	15	18
California halibut	9	19
Bluefin tuna	6	20
Others	61	—
Total	4,453	

catch was still well above the lower catches in the 15 years before 1987. Salmon (*Oncorhynchus* sp.) also had a good year. The lingcod (*Ophiodon elongatus*) take increased 19%. Albacore finally made the top 20, although mostly small fish in the eight-pound range were caught. The highly desirable California halibut retained nineteenth place with only 9,000 fish, a 25% decrease from 1988. Striped bass (*Roccus saxatilis*) had a poor year: a little over 2,000 fish were caught, as opposed to over 10,000 in 1988.

Contributors:
Kristine Barsky, ridgeback and spot prawns
Patrick Collier, Pacific Ocean shrimp
Cedric Cooney, northern anchovy
Gary Galovich, Pacific sardine
Frank Henry, groundfish
Mary Larson, albacore
Robert Leos, market squid
Malcolm Oliphant, recreational fishery
David Parker, sea urchin
Jerome Spratt, Pacific herring
John Sunada, swordfish and shark
Phillip Swartzell, California spiny lobster
Patricia Velez, California halibut
Ronald Warner, Dungeness crab
Patricia Wolf, Pacific mackerel

Compiled by Terri Dickerson

PUBLICATIONS

January 1 through December 31, 1989

Balch, W., R. E. Eppley, M. R. Abbott, and F. M. H. Reid. Bias in satellite-derived pigment measurements due to coccolithophores and dinoflagellates. J. Plankton Res. 11(3):575–581.

Baumgartner, T. R., J. Michaelsen, L. Thompson, G. Shen, A. Soutar, and R. Casey. The recording of interannual climatic coral bands, ice cores and varved sediments. *In* Aspects of climatic change over the eastern Pacific and western Americas, D. Peterson, ed. Amer. Geophys. Union, Geophys. Monogr. 55:1–14.

Beers, J. R., and D. C. Brownlee. Laboratory observations on the reproduction and feeding of a marine pelagic strombidiid ciliate. University of California, Institute of Marine Resources, Reference 88–4, 25 pp.

Boveng, P. L., A. A. Hohn, D. P. DeMaster, A. E. Dizon, D. A. Hanan, and J. P. Scholl. Tracking harbor seals in Southern California. *In* Proc. 1989 North Amer. Argos users conf. and exhibit. Service Argos, Inc., 1801 McCormick Dr., Suite 10, Landover, MD., 20785, pp. 205–217.

Butler, J. L. Growth during the larval and juvenile stages of the northern anchovy, *Engraulis mordax*, in the California Current during 1980–84. Fish. Bull. 87(3):645–652.

Butler, J. L., C. A. Kimbrell, W. C. Flerx, and R. D. Methot. The 1987–88 demersal fish surveys off central California (34°30′N). U.S. Dept. Commer., NOAA Tech. Memo., NMFS, SWFC, No. 133, 44 pp.

Chereskin, T. K., E. Firing, and J. A. Gast. On identifying and screening filter skew and noise bias in acoustic Doppler current profiler measurements. J. Atmos. Oceanic Tech. 6:1040–1054.

Chereskin, T. K., M. D. Levine, A. J. Harding, and L. A. Regier. Observations of near-inertial waves in acoustic Doppler current profiler measurements made during MILDEX. J. Geophys. Res. 94:8135–8145.

Chereskin, T. K., P. P. Niiler, and P. M. Poulain. A numerical study of the effect of upper ocean shear on flexible drogued drifters. J. Atmos. Oceanic Tech. 6:243–253.

Craven, D. B., and A. F. Carlucci. Spring and fall microheterotrophic utilization of dissolved free amino acids in a California borderland basin. Mar. Ecol. Prog. Ser. 51:229–235.

Dayton, P. K. Interdecadal variation in an antarctic sponge and its predators from oceanographic climate shifts. Science 245:1484–1486.

Dayton, P. K., J. H. Carleton, A. G. Mackley, and P. W. Sammarco. Patterns of settlement, survival and growth of oysters across the Great Barrier Reef. Mar. Ecol. Prog. Ser. 54:75–90.

Dayton, P. K., R. J. Seymour, P. E. Parnell, and M. J. Tegner. Unusual marine erosion in San Diego County from a single storm. Estuarine Coastal Shelf Sci. 29:151–160.

Dayton, P. K., and M. J. Tegner. Bottoms below troubled waters: benthic impacts of the 1982–1984 El Niño in the temperate zone. *In* Ecological consequences of the 1982–1983 El Niño to marine life, P. W. Glynn, ed. Elsevier Oceanography Series.

Dillon, T. M., J. N. Moum, T. K. Chereskin, and D. R. Caldwell. On the zonal momentum balance at the equator. J. Phys. Oceanogr. 19:561–570.

Ebert, E., and J. L. Houk. Abalone cultivation methods used at the California Department of Fish and Game's Marine Resources Laboratory. *In* Handbook of culture of abalone and other marine gastropods, K. O. Hahn, ed. CRC Press, pp. 239–254.

Forney, K. A., D. A. Hanan, and J. Barlow. Detecting trends in harbor porpoise abundance using aerial surveys: a preliminary report based on three years. August 1989. SWFC Admin. Rep. #LJ-89-20.

Graham, J. B., W. R. Lowell, N. C. Lai, and R. M. Laurs. O_2 tension, swimming-velocity, and thermal effects on the metabolic rate of the Pacific albacore, *Thunnus alalunga*. Exp. Biol. 48:89–94.

Hanan, D. A. Harbor porpoise, *Phocoena phocoena*, aerial surveys, September 1988 to November 1988. Final Rep. Coop. Agreement NA-86-ABH-00018. NOAA/NMFS SWR, March 1989, 30 pp.

Hanan, D. A., and S. L. Diamond. Estimates of sea lion, harbor seal, and harbor porpoise mortalities in California set net fisheries for the 1986–87 fishing year. Final Rep. Coop. Agreement NA-86-ABH-00018. NOAA/NMFS SWR, January 1989, 10 pp.

Hanan, D. A., L. M. Jones, and R. B. Read. California sea lion interaction and depredation rates with the commercial passenger fishing vessel fleet near San Diego. Calif. Coop. Oceanic Fish. Invest. Rep. 30:122–126.

Hanan, D. A., J. P. Scholl, and S. L. Diamond. Harbor seal, *Phoca vitulina richardsi*, census in California, May–June 1988. January 1989. SWFC Admin. Rep. #LJ-89-13, 53 pp.

Hunter, J. R., B. J. Macewicz, and C. A. Kimbrell. Fecundity and other aspects of the reproduction of sablefish, *Anoplopoma fimbria*, in central California waters. Calif. Coop. Oceanic Fish. Invest. Rep. 30:61–72.

Jackson, G. A., F. Azam, A. F. Carlucci, R. W. Eppley, B. Finney, D. S. Gorsline, B. Hickey, C. Huh, R. A. Jahnke, I. R. Kaplan, M. L. Landry, L. F. Small, M. I. Venkatesan, P. M. Williams, and K. M. Wong. Elemental cycling and fluxes off the coast of southern California. EOS 70:146–149, 154–155.

Jacobson, L. D., and N. C. H. Lo. Spawning biomass of the northern anchovy in 1989. SWFC Admin. Rep. #LJ-89-17.

Jacobson, L. D., and C. J. Thomson. Evaluation of options for managing northern anchovy — a simulation model. SWFC Admin. Rep. #LJ-89-26.

Kalvass, P. Characteristics of red sea urchin, *Strongylocentrotus franciscanus*, populations in the vicinity of Fort Bragg, California. Calif. Dept. Fish. Game, Mar. Res. Admin. Rep. 89-1.

Kimor, B., and F. M. H. Reid. Large dinoflagellates of the orders *Gymnodiniales, Noctilucales* and *Pyrocystales* from the west coast of North America. IMR Ref. 89–1.

Lea, R. N., J. M. Duffy, and K. E. Wilson. The Cortez angelfish, *Pomacanthus zonipectus*, recorded from southern California. Calif. Dept. Fish Game 75(1):45–47.

Lea, R. N., K. A. Karpov, and L. F. Quirollo. Record of roughscale sole, *Clidoderma asperrimum*, from northern California with a note on the Pacific lined sole, *Achirus mezatlanus*. Calif. Dept. Fish Game 75(4):239–241.

Leong, R. J. H. Sperm concentrations and egg fertilization rates during spawning of captive anchovy, *Engraulis mordax*. Calif. Coop. Oceanic Fish. Invest. Rep. 30:136–139.

Lo, N. C. H. Evaluation of the port sampling of the wet fish fishery. SWFC Admin. Rep. #LJ-89-10.

Lo, N. C. H., J. R. Hunter, and R. P. Hewitt. Precision and bias of estimates of larval mortality. Fish. Bull. 87(3):399–416.

Lo, N. C. H., and R. D. Methot. Spawning biomass of the northern anchovy in 1988. Calif. Coop. Oceanic Fish. Invest. Rep. 30:18–31.

McGowan, J. A. Pelagic ecology and Pacific climate. *In* Aspects of climate variability in the Pacific and the western Americas, D. H. Peterson, ed. Amer. Geophys. Union, Geophys. Monogr. 55:141–150.

Methot, R. D. Synthetic estimates of historical abundance and mortality for northern anchovy. *In* Mathematical analysis of fish stock dynamics, E. F. Edwards and B. A. Megrey, eds. Am. Fish. Soc. Symp. Ser. No. 6, pp. 66–82.

Mullin, M. M., E. R. Brooks, and E. F. Stewart. Nearshore surface-dwelling zooplanktonic assemblages off southern California during the anomalous winters of 1983 and 1984. Cont. Shelf Res. 9:19–36.

Mullin, M. M., and A. Conversi. Biomasses of euphausiids and smaller zooplankton in the California Current — geographic and interannual comparisons relative to the Pacific whiting, *Merluccius productus*, fishery. Fish. Bull. 87:633–644.

Niiler, P. P., M. R. G. Poulain, and L. R. Haury. Synoptic three-dimensional circulation in an onshore flowing filament of the California Current. Deep-Sea Res. 36:385–405.

Oda, K. T. The Pacific herring, *Clupea harengus pallasi*, open pound roe-on-kelp experimental fishery in San Francisco Bay, 1988 to 1989. Calif. Dept. Fish Game, Mar. Res. Admin. Rep. 89-7.

Ohman, M. D., J. M. Bradford, and J. B. Jillett. Seasonal growth and

lipid storage of the circumglobal, subantarctic copepod, *Neocalanus tonsus* (Brady). Deep-Sea Res. 36:1309–1326.

Ohman, M. D., and J. R. Wilkinson. Comparative standing stocks of mesozooplankton and macrozooplankton in the southern sector of the California Current system. Fish. Bull. 87(4):967–976.

Owen, R. W. Microscale and finescale variations of small plankton in coastal and pelagic environments. J. Mar. Res. 47:197–240.

Owen, R. W., N. C. H. Lo, J. L. Butler, G. H. Theilacker, A. Alvariño, J. R. Hunter, and Y. Watanabe. Spawning and survival patterns of larval northern anchovy, *Engraulis mordax*, in contrasting environments — a site-intensive study. Fish. Bull. 87(3):673–688.

Parks, W. W., S. F. Herrick, Jr., and P. J. Donley. U.S. trade in tuna for canning, 1987. Southwest Region Admin. Rep. #SWR-89-01.

Parrish, R. H., N. W. Bartoo, S. F. Herrick, Jr., P. M. Kleiber, R. M. Laurs, and J. A. Wetherall. Albacore management information document. U.S. Dept. Commer., NOAA Tech. Memo., NMFS, SWFC, No. 126, 56 pp.

Porter, R. G., B. Culver, L. H. Hreha, and M. L. Larson. Pacific coast fishery review reports, albacore fishery in 1988 — Appendix 2. *In* 41st Ann. Rep. Pac. Mar. Fish. Comm., R. G. Porter, ed. Pp. 9–10.

Reid, F. M. H., and E. Stewart. Nearshore microplanktonic assemblages off southern California in February 1983 during the El Niño event. Cont. Shelf Res. 9(1):37–50.

Reid, J. L. Circumpolar water in the North Atlantic. EOS 70(43):1132 (abstract).

———. On the total geostrophic circulation of the South Atlantic Ocean: flow patterns, tracers and transports. Prog. Oceanogr. 23(3):149–244.

Reilly, P. N., K. T. Oda, and F. E. Wendell. Pacific herring, *Clupea harengus pallasi*, studies in San Francisco and Tomales bays, April 1988 to March 1989. Calif. Dept. Fish Game, Mar. Res. Admin. Rep. 89-4.

Riddle, M. J., D. M. Alongi, P. K. Dayton, J. A. Hansen, and D. W. Klumpp. Detrital pathways in a coral reef lagoon. Mar. Biol. 104:109–118.

Roemmich, D. Mean transport of mass, heat, salt and nutrients in southern California coastal waters: implications for primary production and nutrient cycling. Deep-Sea Res. 36(9):1359–1378.

Roemmich, D., and T. McCallister. Large scale circulation of the North Pacific Ocean. Prog. Oceanogr. 22:171–204.

Seymour, R. J., M. J. Tegner, P. K. Dayton, and P. E. Parnell. Storm wave induced mortality of giant kelp, *Macrocystis pyrifera*, in southern California. Estuarine Coastal Shelf Sci. 28:277–292.

Simpson, J. J. An integrated study of offshore mesoscale eddy dynamics in the California Current system. IEEE/Oceans '89 Conference Proceedings, 18–21 September 1989, Seattle, Wash.

———. Remote sensing in fisheries: a tool for better management in the utilization of a renewable resource. Symposium abstracts, International Symposium on Operational Fisheries Oceanography (ISOFO), St. John's, Newfoundland, Canada, 23–27 October 1989.

Small, L. W., M. R. Landry, R. W. Eppley, F. Azam, and A. F. Carlucci. Role of plankton in the carbon budget of Santa Monica Basin. Mar. Ecol. Prog. Ser. 53:57–74.

Smith, P. E., M. D. Ohman, and L. E. Eber. Analysis of the patterns of distribution of zooplankton aggregations from an acoustic Doppler current profiler. Calif. Coop. Oceanic Fish. Invest. Rep. 30:88–103.

Spratt, J. D. Biological characteristics of the gillnet catch from the 1988–89 Pacific herring, *Clupea harengus pallasi*, roe fishery in California. Calif. Dept. Fish Game, Mar. Res. Admin. Rep. 89-5.

———. Biomass estimates of Pacific herring, *Clupea harengus pallasi*, in California from the 1988–89 spawning-ground surveys. Calif. Dept. Fish Game, Mar. Res. Admin. Rep. 89-6.

———. The distribution and density of eelgrass, *Zostera marina*, in Tomales Bay, California. Calif. Dept. Fish Game 75(4):204–212.

Squires, D., S. F. Herrick, Jr., and J. Hastie. Integration of Japanese and United States sablefish markets. Fish. Bull. 87(2): 341–351.

Stevens, E. G., W. Watson, and H. G. Moser. Development and distribution of larvae and pelagic juveniles of three kyphosid fishes (*Girella nigricans, Medialuna californiensis,* and *Hermosilla azurea*) off California and Baja California. Fish. Bull. 87(3):745–768.

Tegner, M. J. The California abalone fishery: production, ecological interactions and prospects for the future. *In* Scientific approaches to management of invertebrate stocks, J. F. Caddy, ed. New York: John Wiley and Sons. Pp. 401–420.

Tegner, M. J., P. A. Breen, and C. E. Lennert. Population dynamics of red abalones (*Haliotis rufescens*) in southern California and management of the red and pink (*H. corrugata*) abalone fisheries. Fish. Bull. 87(2):313–339.

Tegner, M. J., and R. A. Butler. Abalone seeding. *In* Handbook of culture of abalone and other gastropods, K. Hahn ed. Boca Raton, Florida: CRC Press. Pp. 157–182.

Theilacker, G. H., and Y. Watanabe. Midgut cell height defines nutritional status of laboratory raised larval northern anchovy, *Engraulis mordax*. Fish. Bull. 87(3):457–469.

Thomson, C., C. Cooney, and J. Morgan. Status of the California coastal pelagic fisheries in 1988. SWFC Admin. Rep. #LJ-89-14, 25 pp.

Venrick, E. L. Behind a front: an overview of the Ensenada Front Project. EOS 71(2).

Wahl, D. D., and J. J. Simpson. Velocity estimates from space: sensitivity to computational methods and near-surface physical processes. IEEE/Oceans '89 Conference Proceedings, 18–21 September 1989, Seattle, Wash.

Watson, W., and R. L. Davis, Jr. Larval fish diets in shallow coastal waters off San Onofre, California. Fish. Bull. 87(3):569–591.

Part II

FORTIETH ANNIVERSARY SYMPOSIUM OF THE CALCOFI CONFERENCE

La Jolla, California

October 27, 1989

OCEAN OUTLOOK: GLOBAL CHANGE AND THE MARINE ENVIRONMENT

The fortieth anniversary symposium was held on the final day of the 1989 CalCOFI Conference. The morning session featured an award to Roger Revelle and talks by Lieutenant Governor Leo McCarthy and Under Secretary of Commerce for Oceans and Atmosphere John Knauss. The afternoon session was devoted to a panel discussion of the question "What does society need from ocean scientists in preparing for global change?" Panelists were seven distinguished representatives of scientific, governmental, and environmental agencies.

Proceedings for both sessions were recorded, transcribed, edited for clarity, and reviewed by the speakers. We trust that the transcribed talks will be as informative and enjoyable to read as they were to hear.

The CalCOFI Committee

Edward A. Frieman: I'd like to welcome all of you on behalf of the faculty, staff, and students of the Scripps Institution of Oceanography of the University of California, San Diego. I'd like to express our appreciation for CalCOFI's selecting our campus for the location of this fortieth anniversary celebration. It gives an opportunity for many of us to hear and participate in what you are doing and, of course, to allow members of the San Diego community to join in.

I'd also like to give special greetings to a few people. I really appreciate the presence of the Honorable Leo McCarthy, lieutenant governor of California, and of the Honorable John Knauss, who is the under secretary of commerce for oceans and atmosphere, and, as it's been pointed out many times, one of us — a Scripps graduate. John still has a home here, so I think we can claim some special attention on his time.

There are many others I would like to welcome. I would like to give a special welcome to Roy Brophy, who is the chairman of the Board of Regents of the University of California. We are clearly delighted that he has taken the time to come down and listen to what we're doing here. I'd also like to welcome Claire Dedrick, executive director of the State Lands Commission; Mike McCollum, chief deputy director of the California Department of Fish and Game; and Mario Martinez, director of our sister university system in Mexico, CICESE. We have strong and ongoing relations across the border; we try very hard to maintain them and keep up our end of the bargain. I think it's extremely important that we do this. I'd also like to welcome Howard Ness, who is the U.S. regional fisheries attaché for the American embassy in Mexico City; Craig Denisoff, who is representing California Senator Barry Keene; Alicia Wenbourne, representing Assemblyman Gerald Felando; and representatives of the office of City Councilman Bruce Henderson. There are probably others here whom I should have recognized and didn't; my apologies.

Of course a very special welcome to — I guess I'd have to call him a Renaissance man — Roger Revelle.

This conference comes at a difficult time in our history, when humankind is being stressed by severe environmental change. And clearly the combined efforts of science and society will be required to meet this challenge. It seems to me that there's a growing appreciation of the strong linkages that exist between environmental policy, energy policymaking, and economic policymaking throughout many sectors of society. There's a growing realization in the international arena that action is needed. We see Maggie Thatcher and President Mitterand proposing major conferences and international action in this regard. And there's a slower-growing realization that a strong science base is needed, much more so than in the past, to support this kind of policymaking. So it seems very fitting that CalCOFI sponsor a symposium on global change, bringing together scientists, government leaders, and those with environmental concerns to focus attention on these matters of science and public policy.

It's also noteworthy that part of our understanding of the impact that the oceans have on climate came from CalCOFI, whose primary goal is understanding the marine life of the California Current. CalCOFI has led in many areas of research on air-sea interaction, the dynamics of ocean currents, the El Niño phenomenon, and other insights vital to our understanding of both short-lived and long-lived, short-range and long-range weather predictions. I'm told that there was a CalCOFI meeting in the late 1950s that brought together some of the nation's leaders in ocean and atmospheric science, including many from Scripps, to discuss the role of the oceans in determining climate.

It's my contention that those in the political arena, worrying about the issues of global warming, tend to think of it purely as the province of the atmospheric sciences. The major role of the oceans, which perhaps will be discussed today in later sessions, is often disregarded, poorly understood, swept aside. It is not part of the national agenda in this debate, and I think one of the outcomes of meetings like this will be to help us focus on this issue, which is a very serious issue for the future.

An outgrowth of the work of CalCOFI was the establishment of the Scripps Center for Climate Research. Later on, the nation's first experimental long-range forecasting center was established at Scripps in the 1970s. CalCOFI has assembled an enormous data base on ocean temperature, salinity, and circulation patterns, along with comprehensive marine life collections from larvae to adult fishes and including marine mammals and birds. This is all invaluable to our research in global change. We must have data of high quality and continuity for projecting changes in climate and species.

It has been noted that CalCOFI began in the 1940s. It is really amazing in the world of science for such a cooperative program, which involves federal and state governments, universities, and support from the private sector, to be able to function for four decades. The relaxation time for political phenomena tends to be four years, yet we have functioned and stayed together over many, many political decades. I cannot think of another major scientific program that has this characteristic. From the scientific point of view, one might say it provides evidence that it can be done. I think it's important for policymakers at high levels to be aware of that.

CalCOFI, as you know, was a response to a decline in the catch of the sardine fishery, which was at that time the largest single fishery in the world. I'm told the sardine fishery is back, at least to the degree that fish have been canned again in 1988. Clearly the program would like to take credit for that, and though you are great, I think you don't yet walk on water. But you did provide the understanding that led to much of this.

An issue then is the role of CalCOFI in the future, and I look to CalCOFI for major leadership. I think there is a growing understanding throughout the oceanographic community of a new kind of unification of a number of disciplines — chemical, biological, physical, marine biological — to attack the global problems we must deal with in the future. We can no longer maintain the stance in the scientific literature, in our research, that these areas are distinct and separate. For example, there is talk in the National Science Foundation of a new initiative in global ecosystems dynamics, called GLOBEC. I would hope that the Marine Life Research Group and CalCOFI will play a major role in that new endeavor when and if it gets started.

Lastly, I would like to commend the CalCOFI Committee for their really tireless efforts over the past year to provide this forum. There are many people on the planning committee — Izadore Barrett, who is director of the Southwest Fisheries Center of NOAA's National Marine Fisheries Service; Richard Klingbeil of the California Department of Fish and Game; the current CalCOFI coordinator, Patricia Wolf, from the California Department of Fish and Game; and last, but certainly not least, Mike Mullin, director of our Scripps Marine Life Research Group; and his assistant George Hemingway. I know, from Mike's first talking to me about this, many months ago, that he has been a superb leader, tireless in his efforts on your behalf. I wish to thank you all.

Finally, on behalf of the institution, I would like to welcome you to what promises to be an exciting day of talk and debate as we explore the new roles of science and public policy in meeting this challenge.

Michael M. Mullin: I want to add my welcome to that of Professor Frieman, and I stress that this is a triple welcome, because, as you know, one of the strengths of the California Cooperative Oceanic Fisheries Investigations is the enduring partnership between the Marine Life Research Group here at Scripps; the Southwest Fisheries Center of the National Marine Fisheries Service, National Oceanic and Atmospheric Administration; and the California Department of Fish and Game. Therefore, welcome, welcome, and welcome.

This symposium takes place on the third day of the annual scientific meeting of this consortium; together with other interested scientists, we've been sharing our developing, hard-won knowledge and speculations. The symposium today is really about the future — about the problems that may arise for society from environmental change, and the roles of marine scientists in addressing these problems.

We will hear from distinguished and diverse scientists and policymakers as they discuss and debate these issues from several perspectives, and we are very grateful for their participation. I particularly want to thank our participants from the Bay Area and Sacramento — Lieutenant Governor McCarthy, Assemblyman Sher, and Professor Scheiber — who are with us in spite of the recent earthquake and its major disruption of normal life.

Yet the symposium also honors the past — forty years of a collaborative effort to develop an understanding of the sea off California and its living resources. Put quite simply, we believe that CalCOFI exemplifies the three bases for grappling with future environmental problems. These are the fund of factual knowledge that has been accumulated; the conceptual and technical tools that have been developed (and which, incidentally, have been adopted in many parts of the world); and the model of cooperation and mutual support between organizations whose internal politics are often quite divergent. We believe, to paraphrase Santayana's oft-quoted remark about history, that those who do not understand the environmental change of the past are condemned to misinterpret what is happening to them in the present.

As proud as we are of the record of this program — and we all are proud of it — we organized this symposium for the future, so I would like to leave you with some visual images representing the CalCOFI program up to this point.

First, to demonstrate that the program has been solidly based in seagoing, though by no means limited to that, Roger Hewitt[1] put together maps of the coverage of the California Current by the CalCOFI cruises since 1949. I show these to you for an overall visual impact, not for details. The first slide (Hewitt's figure 5) shows the coverage from 1949 through 1960, during which much of the California Current was sampled each month. The second slide (Hewitt's figure 6) represents the years from 1961 through 1965. Coverage was reduced to approximately quarterly, but was still spatially extensive. The third slide (Hewitt's figure 7) is for 1966 through 1978, when extensive coverage was reduced to every third year. The final slide (Hewitt's figure 9) represents 1979 through 1987. The weakness of the triennial system was revealed in 1983, when a major El Niño had the impudence to occur in a non-CalCOFI year. Resources were begged, borrowed, and stolen to set up a single line off Del Mar, California, and in 1984 the sampling plan was changed to quarterly coverage of the segment from San Diego to San Luis Obispo. And that is the pattern that continues today. There are sound scientific reasons for reducing the area of coverage, as well as financial pressures, but I'll spare you the arguments.

I don't want to leave you with the impression, though, that all of the seagoing science has been confined to the California Current. The basic methods have been used by CalCOFI scientists and others to map most of the North Pacific. Furthermore, not shown on these maps are extensive and intensive cruises within the current, specifically designed to assess the abundance of eggs and larvae of commercial species of fish.

The second visual image is a videotape that Chuck Colgan and Bill Call of Scripps prepared for this anniversary celebration, starring one of our own researchers. We plan to distribute this widely as a source of information about CalCOFI and about how large-scale issues in the marine environment have been tackled. *(videotape)*

As you can see, we are very proud of the cooperative aspect of this program, and although it's certainly evident to those of us in it, I should point out to newcomers that there has also been a very longstanding and fruitful cooperation with marine researchers and fisheries biologists in Mexico. They are regular participants in the CalCOFI conference, and we welcome their continuing involvement, because the problems that we face simply don't recognize national borders.

Finally, to remind you again of the reason for this symposium in words from outside CalCOFI, I quote from *Our Changing Planet, An Executive Summary of the U.S. Global Change Research Program*:

> Although human activities may have the potential to alter the Earth system, it is clear that variations occur naturally over a wide range. For many of these changes, current knowledge is insufficient to reliably predict the

---

[1] Historical review of the oceanographic approach to fishery research, Calif. Coop. Oceanic Fish. Invest. Rep. 29:27–41.

likely debate, rate, or timing of these changes. To understand and ultimately predict the impact of both natural processes and human activities on these changes, it is necessary to improve our understanding of the underlying physical, geological, chemical, biological, and social processes that control the earth's environment. . . . An effective and well-coordinated national and international research program will be required to dramatically improve our knowledge of these complex earth processes — to provide the basis to discriminate between natural and man-influenced changes and ultimately to predict global change.

So much for background. My distinguished predecessor as director of the Marine Life Research Group was Professor Joseph Reid, who led the university's part of the CalCOFI program for many years. Joe is, as most of you know, a physical oceanographer, but he has published on chemistry and biology as well, and represents an integrative, large-scale view of the ocean. I have asked Joe to make a special presentation to Roger Revelle, one of the great men in CalCOFI's history and in American environmental science.

Joseph Reid: Hello, Roger. At the time the CalCOFI program was first conceived, Roger wasn't here. That was in the middle forties; I believe he was off as a sailor in Washington at that time. But a little later he did manage to get two ships — stealing ships from the navy in 1944 would have been awkward, but in 1947 or 1948 it was a little easier; there was a surplus. And he did this so quickly that the university president at that time, Gordon Sproul, complained to Harald Sverdrup that he'd only learned by memorandum after the event that he was now responsible for a fleet.

Well, getting ships out of the navy at that time was the easiest part of the job. When Roger came to Scripps as associate director in 1948, he had to use them. One of his many responsibilities was to see that these ships were equipped and manned with people who knew how to carry out the work at sea. This was much the hardest part of the job.

In the earlier part of the Scripps career, the faculty had done much of their own work at sea. Because most of the cruises were fairly short, this had worked out well. But monthly cruises of three ships were a different order, of course. And they would need more trained people. Starting, I believe, with a core of one experienced marine technician in 1948, and enough gear for one ship, Roger somehow managed to find enough instruments and trained people for three ships. So that in March of 1949 the Scripps vessels *Horizon* and *Crest* and the Cal Fish and Game vessel *N.B. Scofield* were able to go to sea. Much of the work on the first few cruises was done by students and at least one professor, Norris Rakestraw (he was on cruise 1 when I was).

Well, Roger supported this Marine Life Research Program of the Scripps Institution, a component of CalCOFI and its sardine study, arguing with his characteristic vigor for a type of environmental approach now called fisheries oceanography. He pursued his aims with so much vigor that some of the non-Scripps scientists of the time complained that, lacking the mandates that the federal and state agencies had, Roger was sometimes insensitive to their concerns and interagency rivalries.

Roger became director in 1950, and he was, with Carl Hubbs, the consistent Scripps presence on the Marine Research Committee, which managed the CalCOFI program. He remained actively involved in that committee until 1959. Without losing sight of the concern for the economically important, but failing, sardine industry, Roger was a vocal and eloquent proponent of the central idea of modern ecology: that any particular species is part of a physical and biological environment and must be studied from the broad perspective by experts in a variety of fields. He combined broad vision with great energy and persuasive powers. He was certainly one of the first marine scientists to become interested in the possibility that adding carbon dioxide to the atmosphere from the burning of fossil fuels might cause global warming — the greenhouse effect.

This is the fortieth anniversary of CalCOFI, but I was reminded by Saul Alvarez-Borrego yesterday that it's also the fiftieth anniversary of your trip, Roger, on the *E.W. Scripps* to the Gulf of California in 1939. And that merits some mention too.

I have heard, and it's only a rumor, that while the ship was down there and you were engaged on your work on the sediments in some of the basins, the captain had to leave, and you brought the *E.W. Scripps* back to San Diego. I don't know whether this is true or not, but it's rather frightening to think of the scientific leader of a cruise all of a sudden becoming the navigator. Those of you who have been to sea know what sort of scientists we've got. All are very competent in their own particular fields, which may be deep, but not necessarily very wide. I'm afraid that there are some of us who, given the problem of navigating a ship, once we've gotten offshore far enough not to see North America, couldn't find it again. . . . And there are some of us who can do anything. I don't know whether Roger really brought that ship back by himself, but in his case, I don't think there's any doubt that if he'd put his hand to it, he could have done that, as well.

Today, as a symbol of our recognition of what he's done for this program, we present Roger with a scale model of the Scripps vessel *Ellen B. Scripps*, which has been used for research in the California waters. This model was made by George Snyder, who has cared for the plankton collections at Scripps for many years. He has received, cataloged, and preserved plankton samples collected by Scripps vessels from all the world's oceans. And he certainly knows what goes on the fantail of a Scripps vessel. This is not the largest or the newest of the Scripps vessels. It's a workhorse for much of the research Scripps has carried out in the inshore waters. A small token, but given sincerely. Thank you, Roger.

Roger Revelle: Thank you very much, Joe. And thank you all.

This occasion brings back a flood of memories of the early days of this remarkable program. It is quite right that we went to the Gulf of California in 1938. Harald Sverdrup led that expedition. In 1939 Francis Shepard and Charlie Anderson and I led a geological expedition on the old *E. W. Scripps*. On the way back, the engine broke down. To get out of the gulf and to get along the coast of Baja California, we had to sail. We sailed a very long tack to the westward—about 800 miles, as I remember—and then sailed back again, a total distance of about 1600 miles, and we made 20 miles good. *(laughter)* This was a very difficult way to get from Baja California to San Diego. Fortunately we did have some professional sailors on board (we were not all amateurs), and the sailors could see that unless we got that engine fixed we'd never get home. So they doubled and redoubled and quadrupled their efforts, and pretty soon the engine was working again. So the last part of the trip was a lot easier.

In those days, the scientists literally were the sailors—not the only sailors, there were also a few pros on board—but we all had to stand watches. We stood six hours on and six off. That's really a very difficult kind of watch to stand. It's awfully boring about 5 or 6 o'clock in the morning, particularly after you've been up since 12. Nothing ever really happened, of course, but just staying awake was a serious problem.

This occasion makes me think of some people who aren't here, particularly Harald Sverdrup and John Isaacs. Harald was the man who really conceived this CalCOFI program more any other person. And although Joe gives me the credit for thinking about the problem from an ecological point of view, Harald, in spite of the fact that he was primarily a geophysicist, also had ecological ideas, very much so.

The other man who pushed CalCOFI very hard and had many original insights was John Isaacs, who was director of the Marine Life Research Group for several years. I think that was perhaps the happiest time of his life. He was a biologist manqué. He was trained as an engineer and a physicist, but all his life he wanted to be a biologist. And to a considerable extent he *was* a biologist. One of the most interesting things he did was to start a program of coring in the Santa Cruz Basin, south and west of Santa Barbara, where there are varved sediments deposited under anaerobic conditions. The study of the fish remains in those sediments showed that long before there were any fishermen, any Cannery Row, any John Steinbeck, any collection of Portuguese and Italian fishermen in California, the sardines fluctuated just about as much over several hundred years as they have since 1930.. And there was an alternation between the populations of sardines and of anchovies long before any human activities affected the fishery.

Another man who was very much involved in this program is still alive. He is Jack Marr, who's sitting right here in this room. He played a major role in the early days of the program. John McGowan, who is also here, was another one.

What's remarkable to me is that the CalCOFI program is still being enthusiastically pursued after more than forty years. People still think about it hard and work hard on it, and obtain interesting new results. It's quite a remarkable scientific operation to have gone on so long and so effectively. I'm very proud to have been involved with it in the early days. Thank you very much. *(applause)*

•••••

Mullin: It was a great pleasure for us to be able to choose a gift so close to Roger's seagoing abilities. I hope he can navigate this one home as well as he navigated the *E. W. Scripps*.

Next, it's my honor and pleasure to introduce the Honorable Leo McCarthy, lieutenant governor of California. His most notable environmental role is on the State Lands Commission, but of course he has been connected with lands in California much more intensively during the last two weeks, and we're very happy that he could join us today.

REMARKS BY THE HONORABLE LEO MCCARTHY, LIEUTENANT GOVERNOR OF CALIFORNIA

Director Frieman, Director Mullin, Under Secretary Knauss, Regent Chairman Brophy, Professor Revelle, and ladies and gentlemen. On the program it says that I have 45 minutes. This will not be a 6-hour watch, and it won't be a 45-minute watch. I once spoke for 32 minutes at a gathering, and have never forgiven myself for that. So we'll do it in a somewhat shorter period of time.

Professor Revelle is remarkable for many reasons. I was told before we came into this auditorium that just a couple of weeks ago he had his pacemaker checked out, and that he watched as they were doing it. Professor Revelle, when I was in the legislature, especially as speaker, I had many incisions in my heart, but they were never voluntary, and I never enjoyed watching them.

It struck me as I was sitting there in the front row that I was watching a man being honored, Roger Revelle, who is one of the eminent scientists in the world and has achieved so much. One of the things from my childhood that I cling to as a happy memory is the one B that I got in a science course. *(laughter)* We don't have to mention what the other grades were. But that doesn't diminish my feeling in any way that today's symposium is critical, not only to celebrate this remarkable fortieth anniversary of CalCOFI, but in attempting to fuse the magnificent research that has been going on here — it's one of the premier science programs in the world — with the making of public policy.

Being here in La Jolla today, I'm reminded of John Steinbeck's *Cannery Row* and of the journeys that Doc Ricketts used to make from his biological lab in Monterey down the coast to La Jolla, looking for specimens of marine life. I wonder what Doc Ricketts would think if he made such a journey today as he passed mile after mile of vital, beautiful coastline pocked by a number of offshore oil platforms, as he watched a young lifeguard post another "no swimming" sign on the beach in Santa Monica, as he saw the La Jolla tidepools he prized littered with plastic six-pack rings, some lying free, some entwined around the beaks and throats of seabirds. I believe Doc Ricketts would see what we see — that the forces of human ignorance, arrogance, and greed have the power to turn the majesty of our environment into a memory found only in fiction and photographs.

It's up to the policymakers, in concert with the scientific community, the environmental community, a number of enlightened business leaders in the state, and concerned citizens to challenge those forces and to reverse the damage they have caused in the past and would continue to cause.

Of course oceans make up only a part of all of our resources that are in jeopardy. Rain forests, home to half the species of the world, are being eradicated. Acid rain threatens many of our most beautiful, pristine areas. Chlorofluorocarbons are eating a hole in our atmosphere. And overdevelopment is taking a toll on open space, particularly on crucial wetlands areas.

The scientists in this audience, better than anyone, understand the environmental implications of these phenomena. Your arguments are compelling for reducing the use of chlorofluorocarbons, for banning the use of carcinogenic pesticides, for doing everything possible to prevent oil spills, and for recycling.

And hearing about the very real damage being done to our environment, it is hard for me to believe that we — we in a broad, public sense — don't respond more vigorously. But we allow the damage to continue with only moderate change. Acid rain wasn't discovered yesterday. The *Valdez* disaster last March wasn't our first exposure to the devastating effects of a massive oil spill. And although global warming has gained considerable attention in the last year, scientists have known about this phenomenon for quite some time. Scientists are doing research and giving us much new knowledge.

So why the lack of progress? Because time and again we lose vital battles in the political arena. Because every time scientists, and those enlightened businesspeople, and community leaders issue warnings about an environmental hazard, someone inevitably clouds the issue by hiding behind specious arguments, usually disguised as economics, but often cloaking somewhat narrow economic self-interest. The partnership between science, policy, and community action must disperse those foggy arguments.

We work to require oil companies to take steps to prevent spills. They say that they can't make those requirements fit, because they would raise the cost of oil and gasoline, thereby raising costs for thousands of businesses and forcing layoffs. We hear that argument even after the Exxon *Valdez* accident.

We want to ban carcinogenic pesticides. The public hears that we shouldn't because crop yields would plummet and food prices would skyrocket. Or that pesticides really aren't all that dangerous when properly applied. I've been visiting with groups of farmers all over this state who on their own initiative have been reducing the use of pesticides and using alternatives — predators and parasites, good bugs to kill the bad bugs.

Let's say we put together a stringent plan to cut back on chlorofluorocarbons. Before the ink on the plan was dry, chlorofluorocarbon apologists would be contending that using substitutes would raise prices and that thousands of people would lose their jobs.

We support mandating higher deposits on beverage bottles to increase recycling. Our opponents claim store owners would have to hire extra people to deal with the returns, and would have to reduce shelf space to create storage room for all the bottles.

Over and over again we lose because many people, especially those in my line of work, accept the opposition's economic arguments over the scientific and environmental arguments. The only exception to this trend seems to be the brief periods following high-visibility disasters or scares. The *Valdez* tragedy created a small window of opportunity for doing something to prevent oil spills. The alar scare may have made it easier to address the question of pesticides. But those windows may already be closing. The best long-term solution is for us to increase our use of strong economic arguments to counter the weaker ones of our opponents.

For some of us, simply protecting the environment for our children while preventing the extinction of our fellow creatures makes for a good enough argument. But others — and we must recognize this — are moved more by economic considerations, by the immediate obligation of rearing a family, fulfilling the economic obligations to dependents. Those are important obligations. So we need to add those considerations to the environmental and scientific arguments in terms a broader constituency can understand. The *Valdez* spill cost Alaska billions of dollars in damages: lost fishing industry, maybe for a long time; lost tourism; and losses to other sectors of their economy as well. Those are tangible adverse circumstances that diminish the ability of many families to earn a livelihood.

The California State Lands Commission, which I chair, has proposed legislation, which will be acted upon in January, that would require oil companies running 2,500 tanker trips up and down the California coast to maintain a $500 million oil spill prevention and cleanup fund. I hope we never have to spend a dime from it — for cleanup — once that legislation is enacted. It's not intended to be punitive; it's intended to be preventive. It will use an economic tool to prevent disaster. And compared to the multibillion-dollar damages experienced in Alaska by both the private and the public sector, it makes good sense economically as well as environmentally.

Together, the scientific and the environmental communities must work with supportive elected officials and those business leaders who are taking initiatives, to try to harmonize economic growth and environmental sense. We must unify those elements and present evidence to prove that many suffer economically because of a polluting company's indifference or mismanagement. When a chemical refinery opposes tougher standards on toxic emissions because they say it will raise consumer prices and put people out of work, let's respond by talking about the medical costs to workers and nearby residents, and about those who have to pay the medical costs. And let's talk about the economic losses from damage done to buildings and cars and other kinds of property. Or let's raise the specter of billions of dollars in costs and lawsuits that companies and their stockholders will encounter if a Bhopal occurs in California. And no one can say it's impossible, it cannot happen. We know differently. When manufacturers resist banning chlorofluorocarbons for cost reasons, or belittle the threat of global warming, let's respond with costs of the fresh water and cropland we could lose. Or the $27 billion construction bill we'd get for new power plants to meet the increased demand for electricity.

Our key will be credibility. Our numbers have to be at least as believable as theirs. And our stories as clear. That's where the scientific community must help us again. I have suggested a fusion in the University of California and other institutions, not only the scientific community, but other departments as well, that will look at the consequences to this state's future economy and to the nation's future economy. You must help us translate your scientific findings into conclusions that will move the public and the politicians, and you must then speak loudly to those conclusions. No one will have more credibility. Nobody can, when motivated to do so, speak more clearly.

I understand that having to make this type of argument can be somewhat frustrating to scientists. After all, isn't it enough to know that chlorofluorocarbons are raising the earth's temperature? Isn't it enough to know that an oil spill will jeopardize the majesty of the coastline and the health of marine life? Isn't it enough to know that automobile emissions are polluting the air we breathe and shortening the lives of many of our fellow human beings living in smoggy areas? It should be, but by themselves, these facts have not been enough.

We have to recognize which combination of arguments has power, and we have to make those arguments. Combining your data, your conclusions and credibility with economic projections and the deep emotional chords that environmental issues can strike will make the best armaments for policymakers to take with us into the political arena.

Here in California, home to so many environmental treasures and so many environmental threats, you also have a few hard-core elected officials eager to work with you: Byron Sher, from whom you will hear this afternoon, is one of those. Several representatives of state legislators among that group are here this morning. Together, I know we can work to win the kind of victories that ensure we do not pass on to our children and our grandchildren a hopelessly, needlessly damaged and dying planet.

•••••

Mullin: Lieutenant Governor McCarthy may or may not have gotten a B in a science course, but he's obviously been studying since then.

Our next speaker is, in a very real sense, one of our own—John Knauss, who is presently the under secretary of commerce for oceans and atmosphere and the administrator of NOAA. He received his Ph.D. here at Scripps. He then became the distinguished dean of the School of Oceanography at the University of Rhode Island. In addition to continuing his interest in science, he's been very active in policy issues such as the law of the sea negotiations. It's a great pleasure to introduce John Knauss to those few of you in the audience who don't already know him.

REMARKS BY THE HONORABLE JOHN KNAUSS, UNDER SECRETARY OF COMMERCE FOR OCEANS AND ATMOSPHERE

It is indeed a pleasure to come back to Scripps; I always enjoy returning. It's a particular pleasure to be invited here to celebrate an extraordinary event — CalCOFI's fortieth birthday — forty years of almost continuous, systematic observations of the ocean. These are not simple observations of tides or sea-surface temperature at a few spots along the coast, but a complex set of physical, chemical, and biological observations at many locations off the Pacific shore that require the skill, the care, and the dedication of many. There's nothing else like this program on this scale in the United States, and with the possible exception of the North Sea, I'm not aware of anything like it anywhere else in the world.

We are also here to celebrate forty years of cooperative work between the state and federal governments and a major public university. I might note in passing that I believe this alone makes CalCOFI a remarkable and unusual program, particularly the fact that the University of California is still involved.

Universities have many wonderful attributes, but the care and feeding of long data sets is not one of them. Universities tend to go where the action is. Long-term, systematic monitoring programs generally have a life span that corresponds to that of a professor. A professor retires, and suddenly the set of observations that he or she so carefully nurtured ceases to be a high priority of the department. Too often the collection ends, or its quality is allowed to erode.

I watched that almost happen here at Scripps, when I was a young graduate student and Professor McEwen retired. The question arose as to whether Scripps would maintain his collection of surface temperatures and tide gage records taken from the end of the pier. There was a time when even that simple set of observations was in danger of being abandoned because no member of the faculty was prepared to take responsibility.

But CalCOFI has outlasted its original participants, although a few like Joe Reid and Roger Revelle continue to be actively involved in oceanography. I'm told that none of those involved in the early years of CalCOFI are still part of the program.

Even I can claim some indirect part of the action during that first year of CalCOFI. One of my first tasks as a new employee in the Office of Naval Research in late 1949 was to answer a request from Revelle to find navy loran sets that could be used on the research vessels involved in the program (at no cost to Scripps, of course). It turned out not to be all that difficult to fill Roger's request, in return for which he bought me dinner at the Cosmos Club on his next trip to Washington.

As Roger indicated, CalCOFI was probably the original idea of Harald Sverdrup. CalCOFI was conceived in the grand tradition of ICES — the International Council for the Exploration of the Sea, a multinational effort designed to explore the relationship between fisheries and the ocean environment in the North Sea.

I had an opportunity, while on sabbatical two years ago, to spend some time as an amateur historian of marine policy and to look at how British fishery policy developed during its period of explosive growth more than a century ago. In the process I learned a bit about the early days of ICES, which, by the way, is celebrating its eighty-fifth birthday this year.

Although Great Britain had by far the biggest fisheries of the North Sea, ICES was essentially a Scandinavian idea. It was developed from the ideas of such scientists as Otto Petterson and Johan Hjort. Their goal was to explain the fluctuations of such resources as the herring fishery, the cod fishery, and the great bottom fisheries, mostly plaice, by studying the life histories of the fish, their environmental requirements, and relating those requirements to the ever-changing physical and chemical environment of the North Sea. And that was and is, of course, the goal of CalCOFI. Only in CalCOFI, the fish was originally the sardine, and the environment was the California Current.

ICES was a great success from the beginning, but — and this is part of my story — not in its original mission. In retrospect, its greatest success in its first few years was in establishing the standards of physical oceanography. Through its short-lived International Hydrographic Bureau in Oslo, under the leadership of Fridtjof Nansen — that remarkable explorer, scientist, and statesman — it laid the groundwork for modern physical oceanography. It was there that Knudsen invented "standard seawater," making it possible for technicians to measure salinity to five significant places in the cramped laboratories of small research vessels. It improved on the development of deep-sea reversing thermometers, so they could be trusted to give accurate readings of *in situ* temperature to a few hundredths of a degree. It was through the bureau that Ekman developed his equation of state of seawater, so if one knew the temperature, salinity, and the depth at which the water was taken, one could measure density to a few parts in a million. This in turn made it possible to calculate geostrophic currents. And of course there was the Nansen bottle, which allowed not only the capture of uncontaminated water at depth, but also the ability to string a dozen or more such bottles on a wire and thus collect many samples simultaneously.

All of this was developed by ICES in its first few years, mostly before 1910, and these were the techniques of CalCOFI when it began operation after World War II; these were the techniques of physical oceanography up until about twenty or twenty-five years ago. However, ICES had less success in its original goals, particularly in relating the physical environment to the abundance and distribution of fisheries.

ICES was originally conceived as a five-year experiment. It is still going on. In 1909, at the end of its first five years, when the original budget was up for renewal, and many, including the fishing industry, were questioning its success, Johan Hjort wrote: "We hope, consequently, that the results now obtained will facilitate to a substantial extent further investigation in this difficult but yet so important a field of inquiry."

Although written eighty years ago, this has a familiar ring to those of us, and I expect there are a few in this room, who at one time or another have had a large experiment not quite live up to our hopes and have had to go hat-in-hand to the National Science Foundation or to the director and admit that we might have been overly optimistic in our original proposal. ("But we are getting close, and you don't want to cut off our funding now, do you?")

But the problem of relating fisheries to hydrography was and continues to be an extraordinarily difficult one. Listen to one of the best fishery biologists of the last generation, Michael Graham, in his excellent book *Sea Fisheries*, almost fifty years after Hjort's statement: "Future editions of this book would certainly include more on the relations between fisheries and hydrographic conditions, but at the present time they are imperfectly understood." That was in 1956. So it was when CalCOFI started some forty years ago, and so it was and has been for much of its existence.

These relationships are slowly yielding to analysis. For example, one now can do a reasonable job of predicting next year's anchovy population from this year's temperature at the time of spawning. But, and I expect that those of you who are professionals in this room know it, understanding the detailed relationship of fisheries and hydrography continues to be a challenge.

I would like to celebrate another aspect of CalCOFI. Just as laying the foundation of modern physical oceanography was one of the unanticipated achievements of ICES, so is the magnificent, unparalleled forty-year time series of biological, chemical, and physical observations of the oceanographic conditions off the California coast a major achievement of this program. It has produced a data set that finds an increasing number of uses not related to its original purpose.

There are two parts to this success story, and they are not unrelated. The first is the quality of the data. It is not easy to maintain a tradition of quality control in a complex observational program that grinds on month after month, year after year, with an ever-changing cast of technical observers. But it is a tradition that is absolutely essential in a program such as this. It is the tradition embodied in that wonderful Teutonic taskmaster, the late Hans Klein, who oversaw the data collection and processing program of CalCOFI for so many years. He accepted nothing less than the most precise, most careful observations and the most rigorous analyses.

If I may be forgiven a personal note, it was a tradition and a program that I inherited when I began my work on the equatorial waters, and one I had reason to recall a few years ago when I retired as dean at the University of Rhode Island and began clearing out some old files. Among the artifacts, I found an old CalCOFI form 4.5, one of those marvelously complex plotting sheets designed by Hans that allowed one to plot not only the traditional temperature-salinity relationship, but a number of others as well, such as silicate versus thermosteric anomaly, and temperature versus oxygen, and all on at least two scales.

Time, as it does for events of one's youth, has a way of eliminating the rough edges. I can only dimly recall working over a light table on the equator, in the days before either air conditioning or CTDs, sweat dripping over those damn forms (Hans had them printed on high-quality paper so that they would not disintegrate when sweated upon), trying to manipulate a French curve between observational points. I can still remember the Hans Klein instructions to lay the form 4.5 from the preceding as well as the succeeding stations underneath the one being worked on and use the observational points from those stations as a guide for interpolating. This was an early form of what we now call objective analysis, but the computer does the work for us these days. I hope that somewhere on this campus a few form 4.5's still exist, along with the detailed Klein instructions, including, as I recall, a shift in the hardness of the pencil depending on the quantity being plotted.

In addition to all that physical and chemical data carefully observed and recorded, there is the wonderful biological collection of CalCOFI—the fish eggs and larvae, phytoplankton, zooplankton, and the fish, all carefully collected, carefully preserved, and carefully archived.

At a time of increasing concern about global change, at a time when the prospects are increasing that we will soon be able to predict yearly and decadal changes in our atmospheric and oceanic climate, and at a time when we are looking for biological and chemical trends over time, many are scrambling to find long-term data sets upon which to test ideas, to look for small signals hidden in a large noise level of what must still be treated as random variability. The CalCOFI data set is almost unique. Certainly there is little like it for the Pacific, where our ideas about atmospheric ocean coupling are more advanced. And there are few, if any, long sets of biological samples where one can test trends in pesticides, for example—ideas not dreamed of when CalCOFI began, using techniques not invented at the time CalCOFI began. Like Fridtjof Nansen's International Hydrographic Bureau, which in retrospect was one of ICES's greatest contributions to science during its first decade, so the magnificent records of observations of the California Current may be one of CalCOFI's greatest contributions to future science during its first forty years.

The care that Hans Klein and those who succeeded him have given to the collection and analysis of data ensures their quality, something that one cannot always guarantee with some other long-term data sets when attempting to use them for attacking problems not originally contemplated in the experimental design. CalCOFI is a wonderful program; it has a unique set of records, which can only become more valuable as they are extended in time. The data and the samples are to be trusted, and they can be used with confidence.

This afternoon there will be a seminar dealing with certain aspects of global change. We've all known for some time that the key to the interannual and decadal changes in our weather is the oceans. We've known that in an abstract sense, and more recently we think we are beginning to understand the relationships. Certainly the success that we are now having in relating the so-called Southern Oscillation to El Niño events on the west coasts of both North and South America gives us hope that this complex interaction can be unraveled further.

We also are beginning to understand the role of the oceans in global warming, triggered by the increase in such greenhouse gases as atmospheric carbon dioxide. One can perhaps best see the ocean role by examining two relatively recent models of how and how much the earth warms as the greenhouse gases increase. One model, out of the United Kingdom, treats the ocean more or less as a boundary condition. It shows the temperature of the earth warming up less in the tropics, but increasingly as one moves poleward. The heating is more or less symmetrical about the equator. The second model uses a three-dimensional ocean and is generated by Suko Manabe and colleagues at NOAA's Geophysical Fluid Dynamics Laboratory in Princeton. Like the U.K. model, it indicates limited heating in the tropics, and increased heating with latitude. But unlike the U.K. model, it indicates that almost all the heating is in the Northern Hemisphere; the Antarctic Circumpolar Current absorbs the heat from the Southern Hemisphere, with the result that there is almost no atmospheric heating at high latitudes in the south.

I'm not certain that anyone, including the modelers, believes their predictions in any great detail, but they do point to the importance of our understanding the circulation of the ocean and its heat budget much better than we presently do.

We can certainly learn something about the grand circulation patterns of the ocean from satellite observations. From the pattern recognition that one can observe by looking at the surface temperatures of the ocean, and from satellite altimeters, one can measure the shape of the sea surface and in turn derive the equivalent of surface pressure maps of the ocean. But we cannot "sound" the ocean from satellites as we can the atmosphere. We will need *in situ* measurements of one kind or another if we are truly to understand the circulation and the heat budget. The CalCOFI data set provides us with forty years of monitoring one small but essential piece of the ocean circulation. I expect that studying that data set will play an important role as we design our total ocean monitoring system. Whether that system follows the CalCOFI form is not so important, but I expect that the interannual and decadal variations one can find in the California Current as indicated by the CalCOFI data can be used as a guide.

California, this nation, and the world are fortunate that those who began this program have continued it. Having been party to a number of joint efforts, I can only assume that the cooperative arrangements between this university, the state, and the federal government have not always been easy. That you have succeeded as you have is a tribute to the patience, the tolerance, and the statesmanship of many.

Congratulations, and happy birthday.

PANEL DISCUSSION
WHAT SOCIETY NEEDS FROM OCEAN SCIENTISTS IN PREPARING FOR GLOBAL CHANGE

Michael Mullin: We have arranged the afternoon session as a discussion in which the panelists will have an opportunity to make opening statements from their individual perspectives about science and policy and the issues that will be facing society in global change.

The moderator of the discussion is Robert Sulnick, executive director of American Oceans Campaign. In addition to giving his own opinion, he will pick out points of agreement or contrast and guide the discussion after each member has had a chance to speak. We will, at that point, take written questions from the floor.

Robert Sulnick: First let me thank Scripps for giving me the honor of being the moderator for this session. I am truly impressed by the quality of the panel and feel quite humbled by it.

I want to briefly introduce this wonderful group. On my immediate right is the Honorable Byron Sher, assemblyman for the Palo Alto district in the state legislature. Next to Byron is the director of Scripps, Professor Ed Frieman. Next to Ed is Bert Larkins, who is a fisheries biologist and now the executive director of the Alaska Factory Trawlers Association. On my immediate left is Professor John McGowan, professor of oceanography at Scripps. Next to John is Boyce Thorne Miller, a marine scientist from the Oceanic Society, recently merged with Friends of the Earth. On my far left is Professor Harry Scheiber, UC Berkeley law professor and historian.

We will hear from Byron Sher first.

Byron Sher: Thank you very much. It's a great pleasure to be here. I appreciate the opportunity to participate in this important panel. Our topic is, what society needs from ocean scientists in preparing for global change. My special perspective is, what politicians need from science, although some would argue that politicians should not be considered a part of society, or at least civilized society.

A few months ago the Smithsonian Institution held a two-day conference in Washington on global environmental problems. Participants included some of the country's most eminent scientists, several prominent senators and congresspeople, and some of President Bush's key environmental advisors. The scientists from various disciplines painted a very bleak picture of the effects of rapid urbanization: deteriorating air quality, ever-increasing water pollution, rapidly accelerating deforestation. Among other depressing statistics, they underscored that society is destroying forest land at the alarming rate of one acre per second. The U.S. production of synthetic organic chemicals has gone from zero to over 225 billion pounds per year in the last 75 years. And of course the world's fossil fuel use has soared. Chlorofluorocarbon emissions, which were almost nonexistent before World War II, are doubling every decade.

The scientists agreed that we could attack some of these problems if we imposed a two-dollar additional tax on gasoline. That would reduce the use of fossil fuel and thereby reduce these harmful emissions. But it's interesting to note that not one of the members of Congress who were present — and they included some of the best environmentalists in Congress — stepped forward and volunteered to introduce that legislation. Politicians simply do not want to tell their constituents that solutions to serious environmental problems require increases in taxes or dramatic changes in lifestyle.

That is not to say, however, that the voters cannot be aroused to approve dramatic programs to attack environmental problems. As you will remember, in 1986, sensing that the voters were deeply concerned about toxic contamination of the drinking water, and believing that the state legislature and governor would not adequately address the problem, several environmental groups qualified Proposition 65 for the ballot. In the November election the voters, while returning a very conservative, probusiness governor to office, also overwhelmingly approved Proposition 65, which, among other things, established strict new prohibitions on toxic discharges into the state's groundwater and surface waters.

Interestingly enough, for the first time ever, the scientific community was drawn directly into that election campaign. Experts in toxicology were used both by supporters and opponents of the initiative to bolster their respective cases. One prominent UC Berkeley professor, whom I shouldn't name but will — Dr. Bruce Ames — was pressed into service by the opponents of Proposition 65, which he stated was environmental overkill. He appeared at public forums and urged voters to defeat the initiative. And indeed his so-called peanut butter argument (eating peanut butter poses a greater cancer risk than ingesting drinking water containing minute levels of toxic substances) became legendary during the campaign and was really the cornerstone of the business community's opposition.

Now that Proposition 65 has become law, scientists continue to be involved in implementing it. Under its provisions a scientific review panel has been set up and is required by law to determine which chemicals should be listed as carcinogens or teratogens. This panel has routinely been at the center of heated controversies between elected officials on both sides, and its determinations not to list certain chemicals have even been challenged successfully in court.

My point is that science and scientists are being drawn more and more into the political arena. Elected officials and society in general are demanding hard-and-fast answers to complex problems. But science often cannot provide such answers. And indeed, in those instances when science does propose a set of solutions, such as at the Smithsonian conference, we are often told that those solutions are politically impractical.

Global warming is one of the most vivid examples of the dilemma created as science and political decision making come together. The tremendous impacts that scientists have predicted from global climatic change demand responses. But so far California is doing little either to reduce its contribution to global warming or to prepare for the effects that are predicted.

For example, the Department of General Services, the agency that oversees new construction of state buildings, is not considering global warming effects when siting or designing new state facilities. The Water Resources Control Board, which has jurisdiction over the groundwater and surface waters of the state, has told us that it does not intend to address changes in runoff patterns resulting from global warming in its current hearings to determine the allocation of waters that flow into the Sacramento River Delta and San Francisco Bay. On the basis of this experience, we can justifiably conclude that these agencies, and indeed state government as a whole, are ignoring science.

There are several reasons why scientific warnings about the effects of global climate change have not generated much response from political bodies, or a demand for action by the public at large. First, these warnings are predictive. Scientists are not absolutely certain that these changes will occur, and many, or some, profess not to know whether the proposed solutions will work.

Secondly, the public does not yet perceive the physical effects of global warming, and consequently science has not convinced the public of the need for action.

Third, many of the solutions offered by science are perceived to be extreme or impractical. Reducing greenhouse gas emissions seems to require enormous sacrifice on the part of the public.

And finally, other immediate concerns such as housing, AIDS, health care, and education are absorbing all of the state's limited resources. The public is unwilling to divert funds from these important programs to attack an uncertain global warming.

To return to the question before the panel: What answers does society need from science in preparing for global change? Well, here's the challenge. Science needs to provide the public and elected officials with precise, accurate information on the nature of the problem, and a range of realistic solutions that can be implemented within a reasonable time. State and federal governments will most likely not be moved to action on global climate change in any meaningful way until proposals of this nature are made.

I believe there is little chance that significant amounts of California state revenues will be devoted to broad-based programs to address the threat of global climate change unless we get something that's the political equivalent of the recent earthquake. In fact, in this past session of the legislature, the governor vetoed a couple of modest bills that would have inventoried greenhouse gas emissions in California and would have made a beginning on reducing those emissions.

So science must become part of the education process. Unless the public is well informed and demands action from its politicians, there will be no political will to address problems like global warming.

In summary, there is a gap between the answers to problems offered by science and the solutions acceptable to the public and therefore to politicians. That gap needs to be narrowed. Conferences like this one and the media attention that is paid to it are an important part of the process.

Edward Frieman: I'm here under false pretenses; you'll have to pretend that I'm Bill Frazer, senior vice president of academic affairs, who was supposed to be on this panel and is otherwise occupied with earthquake activities. I'll base my remarks on the paper that he sent down.

He said, "It's a great personal pleasure for me to extend congratulations on the fortieth anniversary of the CalCOFI program."

He had been asked to provide an overview of the university's activities related to global change. The Global Change Advisory Group held its first meeting here at SIO just two days ago. The group consists of scientists from the campuses and the three Department of Energy laboratories — Lawrence Berkeley, Livermore, and Los Alamos — that are run by the University of California. The group was convened to advise the office of the president on how to build upon the already significant research being conducted throughout the university on this very important topic.

The group's specific charge is to suggest means by which the university administration can most effectively support the design and functioning of a coordinated systemwide research effort on global change, and to recommend elements that might constitute such a research effort.

Five options were presented to the group for discussion. These came out of a previous small rump meeting we had over two months ago. The options included establishing a research center focusing on one or more of the nationally identified high-priority research fields; creating a research center without walls to address one or more high-priority research fields; expanding the INCOR effort to develop a coupled ocean-atmosphere model, and you heard something about that this morning from John Knauss (I'll come back to this in a little bit); creating a global change minigrant program; and establishing an international clearing house for information on research activities and data about global change.

Presently there is a special project organized under the UC Institutional Collaborative Research Program (INCOR), which was started in October of 1988. We at Scripps, along with researchers at Lawrence Livermore and Los Alamos, won one competitive program with a project to understand exactly how pollution and other factors are changing the world's climate. All three of us have supercomputer facilities and programs in climate research. The project focuses on developing a coupled ocean-atmosphere model for global change. The Scripps researchers bring their expertise on ocean modeling, Los Alamos its expertise in areas of atmospheric science, and Lawrence Livermore the results of a decade of research on modeling the global climate. The first report is due this December. The total cost of this four-year project will be $800,000.

A second initiative already under way is a joint UC Davis–Lawrence Livermore–Lawrence Berkeley Lab effort, funded partially by INCOR and partially by the Department of Energy, to conduct a series of workshops on global greenhouse effect. These workshops were designed to elicit the views of international and national global warming experts on problems of climate change. The first conference took place in July, and was aimed at identifying information needed to improve climate models and climate projections. The second conference, held in September, focused on options for reducing emissions of CO_2. The third workshop, just a few days ago, brought together a small group of policy planners and researchers from fourteen Pacific Rim countries to examine the causes of climate change and its implications.

There is a third, very major, initiative under way, in which the university will participate. It's called the National Institute on Global Environmental Change and is just in its formative stages. This is a multiuniversity effort involving the University of California, Tulane, Indiana, and Harvard. It is written into legislation introduced by Congressman Fazio and calls for $6 million in this first year and $10 million thereafter. (This has caused some raised eyebrows.)

Those are the three major activities under way. The charge to our group was clear. Two days ago we examined these various issues, and I guess it's fair to say that we are in the very formative stages. One of the first things we did was to try to get an inventory of what's going on throughout all the laboratories and campuses in the University of California. We tried to map that into the Committee of Earth Sciences' famous document on U.S. global change research categories, which lists things in seven major chunks: climate and hydrological systems, biogeochemical dynamics, ecological systems and dynamics, system history, human interactions, solid earth processes, and solar influences.

The first result, which perhaps wasn't surprising, was that there isn't a one-to-one mapping; there is much more going on in the University of California that can be labeled under the rubric of global change, perhaps with a small g and a small c. In particular, this White House document doesn't really address issues of mitigation.

In our deliberations we examined three possible ways to move ahead. One was to expand the collaboration that now exists between Scripps, Livermore, and Los Alamos to other campuses. Two of them have major programs that would be a natural match: the atmospheric group at UCLA, and Sherry Row-land's outstanding group at UC Irvine. Perhaps there will be others in the future.

The second point that was made was that it would be a good expenditure of a small amount of money to have a number of us talking to each other. We really don't know from one campus to another what's going on. The totality of the research going on throughout the UC system, as addressed to this particular problem, is just enormous. Some people made the statement — it's hard to prove that it's true — that it's perhaps the major powerhouse in the United States.

The last thing we examined was the issue of whether it makes sense at this point to somehow join forces across all the campuses for a new major initiative. We're still in the process of exploring this.

Bert Larkins: Even though I spent the first twenty years of my career as a fishery biologist in the Northwest Fisheries Center of the National Marine Fisheries Service in Seattle, I did get directly involved in some of the information that CalCOFI was collecting.

Many will remember that about 1965 the Soviet fleet came sailing around the corner and parked right off the middle of Washington and Oregon and began taking away 200,000 or 300,000 tons of hake a year. In its infinite wisdom our government thought that we ought to know something about these critters that the Soviets seemed to know so much about. So we instituted a groundfish research program at the Seattle lab, and I was its first director.

The Northwest rather prided itself in being way out in front of fishery research. It had the University of Washington and Oregon State University and the University of British Columbia and two or three others. But when we looked around for information that might indicate something about what went on off our coast, there was zip.

So we got on a mailing list for CalCOFI information. We did know enough about hake to at least surmise that they spawned down here somewhere. In those days the National Marine Fisheries Service was like a university; we had our little turf battles — Northwest came down so far and Southwest went up so far, and never the twain would meet. I have kiddingly said that those were the days when Paul Smith and I would talk to each other from phone booths — we didn't dare do it from the office. But nevertheless, we found out something about what they knew about hake.

So twenty years ago — halfway through your history — we were able to learn something about these animals, something about their migration paths, something about their reproduction. In fact we bought the *Miller Freeman*, brand new, the first U.S. factory trawler research vessel. I think that it was on its second cruise when we came down here to try to find spawning hake.

We knew from your information that they spawned somewhere around here, probably from San Francisco to the southern end of Baja California in some years, sometimes out as far as anchovies spawn — probably 150–200 miles. They were very patchy some years. We had some new trawl gear, and we wanted to get our hands on fecund hake. We arranged through Paul to have help from one or two of the ships that were out doing the CalCOFI survey. Overnight they sorted their plankton samples from the previous day and looked for hake larvae or eggs. We started getting nightly reports that "Yes, we're getting more of them and they're getting younger, and we're heading west on such and such a trackline." We'd run another forty miles west of them and drag our net around and look at our echo sounder. It took us about one week to find our first spawning concentration of hake. They were not where most people thought they would have been; they were farther offshore and farther south that year, as I recall. We would probably have spent our entire month looking for the first one if it hadn't been for this very simple colleague-to-colleague type of arrangement.

One thing I want to mention about what we're going to need from scientists in the future is to at least talk to each other and make sure that the information any of us has gets into the hands of our colleagues and of the public.

The organization I represent, as of two weeks ago, is the Alaska Factory Trawlers Association. It is made up of about forty-five vessels, and has become unbelievably powerful in the last three or four years. There were three or four small factory trawlers in 1980; now there are forty-five. Some of these are small, headed and gutted boats; they have a modest need for high-value fish during the year. Other boats represent $30 million and $40 million investments; they have not just the capacity, but also the financial requirement for about 60,000 tons of pollock a year. They can't make their payments without 60,000 tons; if they catch more than that they start making some money. Fifteen more of these big ships, which make surimi and fillets, are on the ways around the world and will be in the fleet in another year.

Right now we have a capacity — a necessity — for about a million tons of groundfish a year. Most of this is pollock. Next year, or two years from now, we'll probably need two million tons. The scientists tell us that pollock is probably the biggest single groundfish resource in the world now. It has an allowable biological catch about 1.2 or 1.3 million metric tons in the Bering Sea and another 60,000 to 150,000 metric tons in the Gulf of Alaska. This fleet now has the capacity to take all of that. (There are no foreigners fishing this any more.) In another year we will have one and a half times the capacity for taking it.

We had our sardine situation ten years ago in Alaska. The big fishery at that time was king crab — weight 10 pounds each. The ex-vessel price in Alaska is now about $5 a pound. In 1980 the record landings that had been peaking every year before that were something like 80 million pounds from the Bering Sea. The next year the landings dropped in half, the next in half, the next year in half. Now the landings are maybe between 10 and 20 million pounds. And this is after several years of some rebuilding.

Nobody quite knows why it happened. Was it a natural occurrence? Was it overfishing? I think the best judgment is that it was neither; it was probably both, very much like some people would guess about your sardine situation. King crabs were in a sort of natural decline; the fishery was very intense; and before people could get a handle on the decline, that very powerful fishery drove it a little further down than nature might have taken it, and got it down there a little faster than nature might have. These are long-lived animals; they don't mature until they are 6, 7, or 8 years old, when they enter the commercial fishery. They live to be 15 or so. So they are not like shrimp.

At the same time, one species of Tanner crabs, which was not being terribly heavily exploited, also took a downturn. Most of the shrimp in both the Bering Sea and the Gulf of Alaska virtually disappeared.

Also about that time, and it took us two or three years to figure this out, most of the finfish — like pollock and cod — expanded to historic highs and are still there. These highs even exceed those of some of the anecdotal reports from the turn of the century, when the groundfish fishery was pursued by sailing schooners and dories. I've heard some people say that what happened in the Bering Sea, probably starting in the late 1970s, might have had some sort of relationship, maybe an inverse one, to an earlier El Niño down here that had some effect, not really well understood, up in the Bering Sea. Others have said that for some reason a lens of cold water formed over the bottom of the Bering Sea and was detrimental to shellfish but was just wonderful for finfish. Again, I don't think anybody knows for sure.

What's happening now? I'm jumping around a bit, but I'm trying to relate some of the man-made problems that are going to occur in the fishing industry, particularly in the North Pacific. How they get mixed up with natural events is anybody's guess. It's probably not going to be for the better when we hit a natural downturn combined with all of the pressure that's out there now.

The fleet that I represent has now a billion-dollar asset value. As I mentioned, some of these boats are $30 million investments, and there are others coming on line that are 40, 45, and $50 million investments. Most of these boats have twenty-year mortgages on them; no longer is it a six- or seven-year mortgage.

As your experience down here would indicate, I'm sure, fishermen are generally very good theoretical conservationists. They understand. And they mean it when they say, "I want my son to be able to do this. I don't want to overexploit. I want to make a living ten years from now, and I want my son and grandson to do it." But then the bank says, "You owe me $100,000." Well, suddenly they have to catch as many fish as they can, very quickly, to make this year's payments. This is in spades now, in the industry I represent. There are debt services that are unbelievable — $10,000 a day on some of these boats. Their pro formas count on 270 days fishing.

The allowable catch is rather conservative now, because up to this point we had foreigners to boot out if there was any concern. But now the foreigners are all gone, and we're competing among ourselves. Not only is there concern on the part of vessel owners that they need more fish to make their payments — which is the case, and it's going to get even worse as more of these boats come on line. What this is going to do is put political pressure on management councils, on the government. John Knauss is going to see a lot of this as the politics overflows from the councils into Washington.

What we're going to need is scientists who understand what's going on in a multispecies, very complex, physical-chemical environment. They need to understand the relationships of both man-made and naturally induced changes in relative abundance of competitors, predators, etc. We don't know very much about this now, certainly not up north, where we are. Is it good to catch cod, because they probably eat king crab, even though you catch some king crab while you're trawling?

What we are going to particularly need from folks like you are scientists who are willing to get up, work in the fishbowl, and learn to speak lay talk so councilmembers who have their hearts in the right place but don't understand the jargon can understand you. And then we need you to be willing to defend your conclusions. My experience has been that out of 130 people at the Seattle laboratory, there were 6 of us who wanted to do this kind of thing; we wanted to be management biologists rather than research biologists. It's been very rewarding for some of us. We need a lot more people like that.

John McGowan: Since I've had long experience with the CalCOFI program, I assume my role on this panel is to serve as some sort of a sea-truth referee, or perhaps a dull scientific answer man. While I'm willing, and may even be capable of serving in that role, I have something to say as a private citizen.

It seems to me that there have been some very important changes taking place in the world, especially lately, and there is really much cause for optimism. The threat of nuclear war or even large-scale conventional war is greatly diminished; human rights and even human welfare are now being seriously considered in many places; there have been relatively peaceful, popular, revolutions, major ones, as in Poland, Hungary, and other places. Democracy has become a fashionable word once again. We have now institutionalized the prevention of the great killer diseases such as smallpox and cholera. We can grow enough food to feed the world, although we don't distribute it very well as yet, and even the fine arts are flourishing. It's beginning to look as though humankind has learned how to deal with some of the great traditional adversities and afflictions that have been with us for so long.

But while our leaders and policymakers and politicians have been making this remarkable progress, an entirely new and unprecedented challenge has arisen, one with which we have not had any substantial, collective experience. I'm talking about our relationship with nature. This has become a critical issue, and it may become acute. The problem is so large and so complex that it can hardly be stated coherently. In the next twenty years or so we'll hear much about it.

It differs from previous problems due to our disruption of the natural order of things in that its scale is much, much larger, even global, and it is, therefore, much more complicated. Although we are short of much factual data, we know for sure that we have managed to change the atmosphere itself. What is not so clear is what the consequences of that change will be, but almost everyone agrees that they will not be benign.

We don't know much about the rest of the global conditions or what we've done to them, but I suspect that maybe other such large-scale changes have gone on. But since we simply haven't monitored them the way we've monitored the CO_2 question, we are unaware of them. We can be certain, however, that these, over time, will become more and more evident.

So all of the marvelous progress we've made since World War II is now threatened in a new way. We must understand the magnitude of this threat. We absolutely must better understand what's happening to us.

Because the oceans are so large and such a big part of the world, and because they serve as a sink for many pollutants, especially CO_2, it's crucial that we understand how they function and how their biota responds to changes. We must understand how to measure change and its direction and magnitude. We must understand its consequences.

The CalCOFI program — with all of its faults, and I understand many of them — was designed, in the first place, for the purpose of studying the magnitude and scale of environmental change. Although we've made a lot of mistakes, many of which only those of us who work in the program fully understand, we've made a lot of correct decisions as well, and have learned much. We understand now how to go about studying the large-scale problem of change, and it is the CalCOFI data that will help us do so. We're very fortunate to have this marvelous data set, for it can serve as a basis from which to proceed and as a template for further study. To quote someone from outside the system: "That data set, right now, is a national treasure." There is, in my view of things, room for optimism; we in fact started to study the problem of change forty years ago, and we now know how to go about it. This is an enormous advantage.

Boyce Thorne Miller: Global environmental change, as we are discussing it today, begins with humans and ends with humans. In between that beginning and end there will continue to be significant impact on the land, the oceans, the atmosphere, and all the life that knows this planet as home. Bill McKibben, in his powerful new book, has noted that this era of human-induced global change marks the "end of nature" — the title of his book.

No longer are humans responding to the forces of nature; now nature is responding to the forces of human culture. We are dealing with a nature molded by human beings. Because of this, science is also changing. Scientists studying natural phenomena are now more often measuring and predicting nature's response to human manipulation of the environment. And they are called upon to distinguish natural variations from human-caused variations, though the two are often inextricably meshed.

There are two human factors driving this overwhelming influence we have on the natural world: explosive population growth and explosive economic development. Unless these are curtailed, we cannot hope to slow the rate of global change. And it is that unprecedented rate that has led many scientists to worry about the ability of species and ecosystems to adapt. There are many ways our individual lives and the structure of our societies will have to change if we are to succeed in slowing global change. But that is a discussion for another forum.

Today we have been asked to speak about ocean scientists and what they can do to help prepare for global change — and, I would like to add to that, to help moderate global change. I would suggest to scientists three contributions that you can make.

You can do basic and applied research relevant to global change; you can aggressively involve yourselves in environmental policy and management decisions; and you can take a strong public stand on environmental issues. Now, how does this apply specifically to ocean scientists?

I have no doubt that research is the easiest for me to convince you to do. We need more information about the ocean's role in climate change, in the oxygen and carbon cycles, and in other geochemical cycles. We need to learn more about how the ocean may influence and respond to global warming. We need to know more about the diversity of life forms and biological processes in ocean ecosystems — how they compare to terrestrial ecosystems, how they will respond to global environmental change, and on what time and space scales these responses will occur. Fisheries biologists need to learn more about the effects of pollution and harvesting on the health and abundance of fish and shellfish populations. We also see a need for more well-designed, long-term environmental monitoring programs in coastal and ocean ecosystems worldwide, similar to the CalCOFI program. But ocean scientists are eager to do this work. I don't have to convince you to do basic and applied research and monitoring.

Better that I sit here as a representative of the environmental community and tell you that we are working hard to try to get Congress and government agencies to allocate more funds for such work, possibly even from that seemingly untouchable ocean of funds now set aside for defense. The environment, after all, is an issue of global defense.

I may need to work a little harder to convince some of you scientists to participate in environmental and management decisions at local, state, federal, and international levels. Ocean scientists are particularly important because the coastal zone is quickly becoming the front line — the place where the environmental battles will be first and biggest, because most of the pressures from population and economic development are focused on the coastal zone. Ever-increasing stresses are placed on the coastal environment as a result of industrialization, urbanization, residential and tourism development, waste discharge, dredging, poison runoff, and overfishing. Also, agriculture, forestry, and mining in interior as well as coastal regions create runoff that eventually enters coastal waters.

The threats to the coastal zone from all this human activity include pollution from overenrichment, toxics, and debris; habitat destruction, particularly on the coastline; and overfishing. Many coastal ecosystems are already impoverished. And even deep ocean waters are not immune from the impact. Our living marine resources are being jeopardized on a global scale.

Problems created by multiple usage of a fluid environment that cannot be compartmentalized and does not honor political boundaries are complex. The solutions are also complex and must be innovative. Sound decisions about coastal policy and management and how best to regulate human activities in the coastal zone require scientific and technical expertise. Decision makers often don't have this expertise. So it is imperative that marine scientists get deeply involved in the process.

I am asking you all to participate — locally, nationally, and internationally. One word of caution, however: don't expect this involvement to be a particularly rewarding experience. Don't expect to come away each time feeling that politicians and environmental managers will act on what you said, even if they listened. It is not an ego trip, and it requires perseverance. Keep going back with your message.

And you will have to learn to simplify your messages as much as possible. Those making the decisions often find it difficult to convert very complex scientific information into policy or regulation or management practice. So it is up to you to bridge that gap. And we of the environmental community can help also.

Let me give you some examples of what you can do. A very simple thing you can do if you are at Scripps is to march down to your aquarium and tell them to stop selling shells plundered from Philippine reefs. Another thing you can do, on a larger scale, is to involve yourself in the EPA decision about sites for dredge disposal off the coast of California. The EPA seems to feel that there are no data. Knock on their door, or make a telephone call and tell them about the CalCOFI program.

The secondary treatment issue is a big one, a controversial one. Many scientists have said that off the coast of California you don't have to have secondary treatment, or at least that's not the best use of funds. Let's look at it in a slightly more innovative way. It may be that pretreatment will have more of an effect than secondary treatment. However, both of these together would be even more useful. We cannot afford to waste our wastes by throwing them into the ocean. They should be put back on land and used. Get the toxics out, yes, and then use the organic matter to fertilize our fields.

Finally you can work on an international level. International cooperation of scientists, as you have here in the CalCOFI program, is admirable and should be encouraged. The regional sea programs offer another opportunity for that. International lending agencies need some advice. They believe that economic development is what all developing countries should be aiming toward. So the scientists in those countries — in Mexico and in other developing countries — need to get the message across to their governments and to the lending agencies that there is now a new goal — sustainable use of resources.

The last suggestion I have is that you scientists take a strong public stand on environmental issues. This is not for everyone. You have to tread a fine line between credibility and effectiveness. So I ask those of you who don't choose to go public to be tolerant of those who do. Don't criticize them for being what may seem to you too simplistic. Remember that the public and policymakers do not quite know how to deal with all the words of caution and conditions that you put into your scientific conclusions when presenting them to your peers. And they often use the uncertainty to further personal, institutional, and political goals.

For example, the World Bank recently declared global warming a nonissue in its lending policies because of the scientific arguments over the magnitude of what the impact will be. For the purposes of solid scientific research, it is important to be aware of the weak links and the possible sources of error. But for environmental decision making, it is important to take strong action on strong scientific likelihoods. We can't wait for absolute proof before we act, because the proof comes after the damage is done.

Harry Scheiber: As a person trained in economic history and legal history, doing a considerable amount of teaching on law and technology and on the law of the sea, I found this a daunting assignment. I think that in some ways we who are trying to see the proper linkages between matters of policy and law must be more responsive to environmental needs and crises.

We're in much the same state as the scientists of CalCOFI were forty years ago — a state of great perplexity. If you were to draw a historical time line, as I've been seriously playing with in the last few years, of the relationship of changes in what we today call fisheries oceanography and international policy and law, I think there would be, from World War II to the present, working backwards, the following moments of really fundamental change and innovation — turning points.

One, I think, will turn out to be the current discussion of global change, which really started in a serious way two years ago.

Going back a considerable period, we'd go to the 1972 Stockholm Conference, which brought together worldwide United Nations representatives to discuss the problem of environmental crisis. This was certainly another of those watershed moments when our thinking was fundamentally changed around a really significant problem.

Going back again — a leap of fourteen years — we come to 1958, when there were conferences on the law of the sea in Geneva and then Rome. They were the beginning of the law of the sea movement, which has culminated thirty years later. At those meetings we began to discuss the possibility of some kind of convention that would bring the nations of the world to agreement on the regulation and conservation of the sea's living resources. There was a fundamental transforming effect, not only on those who tried to create a law of the sea — a long, difficult, somewhat frustrating and not altogether happy process, which has culminated in the current law of the sea convention. At the time, the introduction of the sustained yield concept, of the idea that the goal should be maximum sustained yield of ocean resources over time, crystallized thinking in a way that had not been done before.

In 1952, six years earlier, came the first of the postwar treaties of the Pacific, which Japan signed after its sovereignty was restored. This treaty included for the first time in the postwar era the concept of maximum sustained yield. Japan, the United States, and Canada agreed with respect to salmon, halibut, and herring that they would maintain the goal of maximum sustained yield. From that came the very controversial abstention concept.

These, I think, are the major turning points — the watershed events when our thinking really crystallized in new ways.

But first, on this time line, really has to be CalCOFI. You may be startled to think that your being out on the edge of the continent worrying about the sardine and the California Current is such an event, but I don't think we exaggerate. In this venture — now forty years old, an extraordinary length of time for such a venture to have survived and be still so vital — we have some lessons to be learned. Not just about why things developed as they did later, because there are lines of continuity here, but also about how things can be done in the future. I'm not going to be able to talk about all of these; I just want to suggest that this is another in the series of important events in which thinking was crystallized on new lines.

Some of the consequences were not at all unanticipated. There are two interesting elements that I'd like to single out. One is the collaborative element from the very beginning. The other is something that was discussed as a longer-term consequence and outgrowth of the beginnings, and that is ecological vision — the ecosystemic approach to fishery problems.

The Marine Research Committee was approved by the legislature and funded from the beginning by a rather sizable tax on sardine landings. CalCOFI was a response to the sardine crisis, at the beginning. There is a little two-year prehistory here, which hasn't been mentioned. It was the formation of the Marine Research Committee, authorized by the legislature of California in the winter of 1947. The committee was financed by an industry tax on the landing of sardines and was really the progenitor of CalCOFI. It was the response to a crisis.

From the outset of the project there was a collaborative intent, with the argument made that it would bring together for the first time the resources of the Scripps Institution of Oceanography and the University of California generally; a federal agency — the Fish and Wildlife Service; and the California Department of Fish and Game, in which Frances Clark had done many years of very important sardine research. From the beginning there was the vision that the sum would be much greater than its parts if they could be brought together. Something that hasn't been mentioned, but that I think is worth underlining, is that industry supported it from the start. The industry had a specific problem and went to the scientists for the solution.

This brings me to the second point, which is that the scientists' response was extremely creative and, in a fundamental way, "subversive" from the beginning of CalCOFI — California Cooperative Oceanic Fisheries Investigations. When this group first formed, the Marine Research Committee, which granted money for collaborative research, said, "We have to have a name for it." So John Marr of the federal service immediately provided a name that would fit nicely into the federal hierarchy and table of organization — the California Sardine Investigation. Frances Clark of the state agency responded and said, "Let's call it the Marine *Resources* Investigations." Roger Revelle came up with the California Cooperative Fisheries Investigations as a compromise. But of course it wasn't a compromise at all. From the outset it announced that CalCOFI wasn't just about sardines — it was also about fisheries in general.

From the very beginning there was an ecological vision as well. We think of it as something that evolved and was produced over time, and we associate it with the late sixties. But in fact, in the very first documents that were circulated — from Scripps, from Frances Clark, and from Oscar Sette of the federal agency — the idea was inherent that (with the new equipment and the new ships that Roger Revelle had produced as a gift from the navy, to President Sproul's shock, and with the new funding) for the first time they could get answers to questions that had been plaguing them for twenty years, and which they *already understood* were the important questions. So in many ways the scientists' vision prevailed here.

From the very beginning this vision was set forth; the scientists had an agenda right from the outset. The lesson was not lost on the industry, which was a little dismayed, but which continued to support the investigations. Over time, people in the industry and in the outside world supported the research, in considerable measure because of the efforts of a scientist who was also a great organizer — Wilbert M. Chapman, a very important force in the beginning, first working out of the California Academy, then in the State Department, and later in the industry. This project was watched closely from the beginning by ocean scientists in all nations. As John Knauss said this morning, it could be that its role as a prototype was understood from the beginning.

To wind up where I began, one of the really interesting things about the documentation of the meetings of the scientists who set up the protocol for the 1952 Japanese-American-Canadian convention and also many of the scientific papers that were presented at the Geneva UN meeting, was that they talked about the research done in the California Current as exemplary of what had to be done in order to make a concept like maximum sustained yield consistent with the best science. So the influence has been very great over the years.

The lesson, if you want to draw a lesson from it with regard to the role of science, is that a project well founded organizationally and well conceived scientifically is going to have an integrity of its own and a vision of its own. And room has to be left for that vision to be nurtured. And that has been done remarkably well indeed in the forty-year record of CalCOFI.

Robert Sulnick: Let me begin by saying that it's obvious to me, an environmental lawyer for over twenty years, that global change is inevitable and that we will all have to be involved in it. If we are not involved the change could be so disastrous that the human species will no longer thrive or possibly even survive.

So my approach to this discussion is going to be somewhat practical. My assumption is that I'm addressing an audience made up mostly of scientists. I myself am an environmentalist and an activist. I would like to give you an insight into how the people that I work with — my side of the fence — think about these problems. Because my premise is that unless we come together, unite, and go forward, the planet is in very serious trouble.

When I think about the oceans, and when I talk to people about the oceans, as I do all over the country, I say this: "The oceans are the lungs of this planet. They provide us with 70 percent of our oxygen." I don't know that that's absolutely true, but I've done enough reading to know that I'm in the neighborhood. And I also know that it grabs everybody's attention, from people in the White House to people on Smith Island in the middle of Chesapeake Bay. So I will continue to say it, unless people like you tell me, "You really can't say that, because it's absolutely not true."

I have been to Smith Island in Chesapeake Bay and spent days with the crab fishery there. They have shown me that from their point of view that fishery is disappearing. And when you look at the habitat through their eyes, it is disappearing. Their explanation has to do with all of the non-point source pollution that flows in from all the rivers that enter into the bay.

I have been up on Puget Sound, where I've seen liver cancers in sole. And I've heard the explanation that they come from the toxins that the industrial complex puts into Puget Sound.

And I've been in Boston Harbor and Deer Island, where I've seen raw sewage in an overwhelming display — emptied daily into Boston Harbor. That's a very serious insult to the integrity of that ecosystem.

I've been around the country and have heard horror stories time and time again. In addition, I'm given intellectual input. By next year 75 percent of us will live within 50 miles of the coast, and we are toxic-dependent as a society. We humans, gathering on the coast, are dependent upon toxics that inevitably, as a waste stream, make their way into that coastal zone, which includes estuaries, bays, and coastal wetlands, and then kills the vitality of that coastal zone.

I am trained to be an advocate, and I now instinctively take all of that information and begin to campaign with it. I do this without even thinking about it, as do all my colleagues. The campaign is aimed at the the public at large, the general population, to seek a critical mass, and then at the decision makers in Congress. Because we want that critical mass to be translated into public policy.

Because I've been a lawyer for twenty-five years, I have learned that decisions will be made irrespective of who has the input, when a problem becomes large enough to be perceived by the political body. It's a reactive body. So we in the environmental community are going about our business of trying to create public opinion — critical mass (i.e., pressure) — and then we are trying to translate that into public policy. We will do that irrespective of the scientific input.

The danger is that much of what we do, by definition and not by any conscious intent, is polemic. It's not meant to be polemic, but we work in such a rampant atmosphere of no resources, no money, and no support system that we can only do the best we can. I don't have five days to research anything, normally. I'm either on the phone with the press or I'm in the field or I'm at a hearing. My schedule is jammed. And again, I'm not talking about me as an individual but me the species. So we work with what we have.

The danger is this: we move very quickly. And we will win our political battles because the public now wants us to. But in winning the battle, if the solution is ineffective, we may lose the ultimate struggle.

Your danger, it seems to me, since I have a background in social science and methodology, is that you can be restricted by two things: your methods, which obviously you need, but which can restrict you because of the time involved; and the money to finance your methods, which is probably the root of the issue if we're honest about it. Because money is not being given to science, much less ocean science. And the next generation of ocean scientists is not getting in line, which is quite alarming. The money is obviously going to military spending and has been for quite some time.

However, we need to slow down, and you need to speed up. Somewhere in the middle there's a balance to be made. A dialogue must be created on how best to go forward. It's no longer a question of should we go forward together. The question is, how do we do it? Because we communicate in much different terms. We're out there in the spotlight all the time articulating, and you generally are not. I would not expect that to be natural for you. You're very cautious and methodical and precise in what you say. We do not have the time or, in our heads, the luxury to do that. So the question is, how do we blend those two things?

It seems to me that's what this panel has been talking about and what we need to continue the dialogue about.

Frieman: A number of issues have been raised that are of vital interest to the scientific community, and I'd like to try to address a few of them wearing my director's hat.

Let me start with the philosophical point of view. I see a mini intellectual movement going on. I've seen the essay by Francis Fukuyama earlier this summer from the State Department policy planning staff concerning the end of history. Fukuyama tells us that there is a triumph of capitalism and western liberal democratic thought, and we're condemned to a boring and static future.

In the last week or two we've seen reports of conferences on the end of science — that somehow we have lost our methods of dealing with objective reality, followed by a report (in probably the same newspaper) on exciting new discoveries from SLAC at Stanford and CERN in Europe on new particles that are the foundation of our universe.

And I read Bill McKibben's *The End of Nature* when it was first published in *The New Yorker*. He says that humankind has come to dominate the planet, and we've carved our initials so deeply into the biosphere that we can no longer consider nature to be separate and pristine.

So I see some sort of a mini intellectual trend. I'm no historian, but my academic colleagues tell me that this is a phenomenon that has been seen before as the end of a millenium approaches.

But rather than accept these pronouncements by these intellectuals and philosophers, I prefer my own favorite philosopher — Yogi Berra, who said 95 percent of the experts in a certain field agree that such and such is the case, and the other half believe the opposite. It seems to me that is, in fact, the situation we are in with global warming.

We do not, I'm afraid, have the unique footprint, the scientific evidence to move ahead. You heard from John Knauss this morning about major global models, one of which says the Southern Hemisphere does not change, another of which says it does change. You will hear, if you choose to talk to Tim Barnett, who has analyzed the data of Jim Hansen (who testified in Congress last year that the global warming signal is upon us) and has printouts from Jim Hansen's monstrous computer code which indicate that even if you add no CO_2 the temperature goes up for fifty years. So it seems to me we are, unfortunately, left in the position of having both feet firmly planted in midair.

I guess I feel, as the director of a scientific institution, that we as an institution are responding to the University of California's motto, which is "Research and Teaching and Public Service," and we can serve the public by somehow trying to get at this vague notion of objective truth.

I agree with my colleagues here. We have a very serious societal issue on our hands. And I have absolutely no problem at all with Scripps scientists speaking out on these issues as members of the public. But I do think we have a responsibility not to do it as an institution. We have an issue of credibility on our hands, and we can debate that for a long time.

Let me turn to the second issue that has been raised. I look at the 1990 research plan for the United States to tackle the fundamental issues we're talking about. I look at the total expenditures for fiscal 1990: \$190 million. This is across all the agencies of the U.S. government. And I look at the fact that our total GNP across the world is \$14 trillion, and I try to argue, well, suppose the U.S. contribution of \$200 million is just one-fifth of the total. I have no idea whether that's true or not; I suspect that maybe it's wrong. But maybe the total amount of research we are expending throughout the world, trying to cope with this problem is $1 billion out of a $14 trillion GNP. It is just ridiculous to assume that somehow we now have the science base to make industrial decisions that will make a major change in a $14 trillion GNP.

I don't know how to cope with this. It's clearly a major issue for all of us to somehow get the major research engine moving. We must do that, and we must convince our elected representatives to do it. At the moment it seems to me that we are in a very serious situation in which we are trying to make vast global decisions on the basis of an extremely poor data base.

Sulnick: I'd like to establish some ground rules for what I hope will be a dialogue among all of us. I'll synthesize what I heard everyone say. Then I would like to direct some individual questions to the panelists, questions that I have been writing down as they were speaking. Then I would like you, the audience, to involve yourselves in their responses. And then we'll move on to questions from the audience.

As I listened to everybody talk, this is what I heard. (If I misquote anybody, by all means let that be part of our dialogue when I'm finished.) First, I heard Assemblyman Sher say that it is important for scientists to make themselves more accessible to the political process, so that the political process can make use of the science.

Professor Frieman, when you gave your initial remarks, I understood that there is a large need now to study things on a global, systemic basis, maybe even instead of on an individual scientific basis. From your last set of remarks I understood that this institution as an institution ought not involve itself in the political debate, but that its individuals are free to do so.

What I heard Bert say was that it's crucial to practitioners who make their livings from the ocean resources to have correct data so that their institutional approach to making a living from the ocean will be effective.

Professor McGowan, when I listened to your eloquent remarks, I heard you say that the things we have achieved since World War II are now being threatened and that we must approach the global problem or lose all of the benefits that we've gained in the last fifty to sixty years.

I heard Boyce say that it's important for scientists to get involved in the solutions to environmental problems.

Professor Scheiber talked about watershed events in history, which, in my mind, is like reaching a critical mass toward moving consciousness forward in a given area.

All of these points raise initial questions that I would like to direct to each of you.

To you first, Assemblyman Sher: What if, in fact, science cannot make its interpretations and its findings more accessible to the political process? How do you in the political arena then reach out to the scientists so that we can still have the input?

Sher: First of all, let me say that as politicians we don't need scientists who are captured by people who resist making changes, who are impacted by some of the regulations that we establish in reaction to problems. Unfortunately there are a lot of scientists, as there are a lot of medical people and a lot of lawyers and other specialists, who are out there and available. I remember a debate we had about so-called noncriteria air pollutants — not the things that cause smog but some other bad stuff. And we were told that our solution to a problem that we knew existed was based on bad science. There were expert witnesses for the industries that would have been impacted by the regulation who told us that. The answer was, don't do anything.

I think we're going to see the same thing in the global warming area. Only this week we saw President Bush's proposal for a new approach to pesticides, and it contains some good recommendations to help the EPA respond more quickly. But at the same time there is a big debate about whether the federal government should preempt efforts by the states to impose more stringent standards. The EPA representative I heard debate this said, "We set our standards based on good science, and we don't want the states to [complicate the matter]."

I don't think I'm answering your question, but I wanted to get all these things off my chest anyway.

Sulnick: Let me ask you this question: Let's assume that science could not give a precise, accurate solution to a political problem but would give a solution that is imprecise from a political point of view and not readily accessible to a political body. The way I view it, there will be one side in favor of social change and one side opposed to social change, at its most simplistic level, in a legislative debate — those who are going to push toxics reform and those who are going to favor the status quo. What I'm hearing you say is that whenever you have scientific input that is not in favor of pushing toxics reform, the opponents seize on it and say, "Well, see, there's really no reason to reform, and our industry shouldn't have to spend millions of dollars on an imprecise solution."

Is there any way that you see, from your years of experience in the legislature, to still involve science in that debate and not give up the fight? Because what I'm hearing you say is that when science comes in and says, "Look, we really don't know what the effects of global warming are going to be," then everybody says, "Well, why should we spend billions of dollars on global warming?" Even though intuitively people believe and know that there is going to be a horrible effect of global warming. How do you deal with that?

Sher: I don't think there is a way to do it. When someone has convinced a member of the legislature or even the governor that we have a serious problem that we ought to be addressing, and that issue comes up and we start to debate it . . . if there is this difference of opinion (and I hear some scientists tell us we have a new ice age coming), people who would be adversely affected by the proposed regulations will seize on that difference of opinion as a reason to take no action at all.

And what frequently happens is that the process is long and complicated, with a scientific advisory panel. And nothing can be implemented until the proposal works its way through the deliberations of the scientists. Meanwhile the problem is overtaking us. Let me give an example of one that I worked on this last year.

As an environmental activist in the state legislature, I've been trying to do something to protect what's left of the ancient forests in California, particularly the redwood forests, where there used to be 2 million acres and now we're down to 100,000 acres. (And almost 20,000 are owned by one company that's cutting them down in order to pay the debt on the junk bonds that were issued when they were taken over by a conglomerate.) What the industry has proposed, and what has now passed, is a three- or four-year study of the ancient forest to determine whether there really are ancient-forest-dependent species. But in the meantime I tried to get some constraints put on cutting down the forest while we're studying whether we need it to preserve the spotted owl and other ancient-forest-dependent critters. But we were unable to do that. The industry supported the study, which they will use to say, "We don't need to do anything now because we're making the study."

We can't have a in-house panel of scientists that we depend on who will inevitably direct us to the right thing to do. But I do think we have to be concerned about preserving what we're trying to protect while the science is going on. I think the state has an obligation to use tax money in significant amounts to promote the scientific study of these problems. But what do we do in the meantime?

We're told that tremendous impacts of global warming are coming. I would say we ought to at least make a start on trying to respond. What are the agencies that have jurisdiction over different areas, like the Department of Water Resources, doing to plan for the potential impact of global warming?

Whatever we do, we should do it in the context of existing programs, so that we're promoting the underlying policies of those programs. For example, there's a good reason to cut down on carbon dioxide for reasons other than global warming. So if we can bring the global change considerations in to help us do what we are already trying to do for other reasons, we may have more success.

Sulnick: Would anybody on the panel like to comment on this issue of science in the political process? From the moderator's point of view, it's a big issue, because those of us who get involved in the political process do so as advocates. We rarely get involved objectively. And we're looking for people to support our positions. That's the contest. And of course that's not a scientist's approach. And yet scientists have an enormous impact on that process.

Larkins: I started to touch on this in my presentation. A year or two ago at the University of Washington I participated in a symposium that had to do with science and fisheries. I made a lot of my old colleagues a little miffed — and one or two of them very pleased (the one or two who happened to share the same view). My training was in fisheries biology. I worked my first ten years as a researcher. I became very interested, on my own, in the application of science to fishery management. This was back in the days before the National Marine Fisheries Service had any management authority. We were a research institution.

We saw the Magnuson Fishery Conservation and Management Act coming along in the seventies; it looked as though NOAA, which was fairly new at that time, was going to have some management responsibilities. There were a very small number of us who just jumped at this opportunity. I guess we felt a little frustration. We had been doing science; we had some conclusions; and there was nobody to give them to, nobody who was going to do anything with them. No one had the wherewithal to do it.

So there were a few of us who found ourselves in a different frame of mind. And first of all, as we've heard from many of the speakers today, it was obvious that we had to learn to speak human, rather than science. I don't know how many of you are familiar with the council system that the Magnuson Act set up, but it's the fishery management councils that advise the secretary of commerce, through Under Secretary Knauss, about fishery regulation. They've become a little bit more than advisory. In effect, what they say goes, unless they've done something blatantly illegal, and then the secretary of commerce may override them.

The folks who are appointed by the political process to these fishery management councils quite often come from the industry. This is unique. A lot of them have vested interests in the fishing industry that they have been charged to give advice on managing. They're not scientists, and they say, "Well, if the scientists can't give us 100 percent assurance, don't shut us down."

People in my association (I'll probably get fired if there are any of them here) do that. Part of my job, as I see it, is to advise them when to back off, so that they have something to be operating on five or ten years from now.

In any case, this is a long way of saying that I see the federal service — the National Marine Fisheries Service — and the state agencies as being applied science agencies. This doesn't mean they can't do some very fundamental research; I think they have to. But on the other hand, I think their job is to get their conclusions out to the public where they can be used. And by God, go out and do it as advocates to make the policymakers understand what the scientific ground truths are, to the extent they can be articulated.

A lot of us in fishery biology like to tell ourselves that we're worse off than most other scientists because it's such an inexact science; it's almost an art. You put a little net down in the middle of an ocean and get only a very small sample. I'm not sure we're unique in that. But what I've tried to tell some of the young fellows that were working for me when I was still a fed is: You guys have to have the courage of your convictions; you're the experts on the biology of these animals. The Magnuson Act requires use of the "best available scientific information" and that is what you must provide. No "insufficient data" copouts, but the best you can do with the data at hand and your best scientific judgment. I'm not asking you to be biostitutes. But put the outside parameters around it; tell them what your advice is as an expert. Because if you don't they are going to decide the "science" by themselves. And when policymakers, even those who mean well, don't get the kind of strongly presented science that might be available, they usually make the very worst choice.

What all of this boils down to is part of what upset some of my colleagues a little: I see a need, way down in the science education programs, to start having people who are going to be in science understand that there may be two ways for their careers to progress. At least make them aware of these two avenues that they might follow as they get into their careers. One is as a researcher; that's finding knowledge for the sake of knowledge. The other is applying knowledge. Somewhere, probably at the graduate level, certainly when one gets into a governmental agency, there ought to be two distinct career tracks. Those who have the talent and the wherewithal should be encouraged to get out, live in the fishbowl, translate science into lay talk, and get the word across to the policymakers.

McGowan: This issue of advocacy on the part of scientists is a serious one. I've thought about it a lot because it has come up many times before. I'm very leery of having scientists advise policymakers and politicians on solutions to some of our environmental problems — not on the nature of the problems, but on solutions. Because scientists', or anyone's, advice is always value laden somehow. There's a certain amount of entrepreneurship involved, and self-promotion. Scientists, for better or for worse, often have a rather narrow view of the world. They come from a part of society that is remote from real world problems and concerns. I don't think that their notions about solutions to environmental problems or other serious problems are any better than anyone else's. In many cases they're worse because of the narrow value judgments they put on things.

I think the judgments about solutions should come from society, from people who have the information. It's our job to present them with the information. And the decision then is made at some other level.

Sulnick: Given that the decision will inevitably be based on value judgments, which are often subjective, why should a scientist not offer his or hers?

McGowan: Because they represent a very small segment of society, and a rather privileged segment of society at that. Their educational background is rather specialized, and they often lack even a rudimentary idea of human conditions. I'm not at all certain that they've got the best interests of society in general at heart. *(laughter)*

Scheiber: Just a footnote to that. At each of those watershed moments in the history of fisheries oceanography over the last fifty years, the scientists did take a very strong position on what needed to be done. For example, in the case of CalCOFI, there really was an agenda. A few scientists had done some brilliant work at the University of Washington under William F. Thompson, here at Scripps Institution under Harald Sverdrup, and at other places including the California Department of Fish and Game under Frances Clark and Richard Croker. Those people knew what had to be done, and they said what had to be done.

The same was true of maximum sustained yield at the United Nations. Richard Van Cleve gave a very influential presentation that was based heavily on the work that had been done by CalCOFI scientists, who said, "This is what we need to do." So there have been moments when scientists have spoken out and have spoken out constructively.

Listening to Dr. Frieman, I think there's a difference between those cases and this one, because there is — as he said so eloquently — a lack of an agreed-upon agenda today on this larger question of global warming.

McGowan: The kind of issue I'm talking about can maybe be illustrated by an example well removed from CalCOFI and the oceans and us here. It's been said, and I believe it's true, that there are ten million homeless farmers in southern Brazil. They represent a very serious problem to the Brazilian government — a social problem and a potential political problem. The policy of the Brazilian government has been to cut down Amazonia to provide farms for homesteaders. Whether the farms will work or not, I don't know.

Most scientists, of course, are horrified by the idea of destroying Amazonia: look at all those species; look at all those beautiful trees; look at the diminishment of diversity. And after all, the jungle provides oxygen . . . and on and on and on. But what about those ten million peasants? That's the kind of advice that I'm very dubious about. How do you make a judgment about what it is we want to preserve? Which do you choose — those poor bastards who are trying to live and survive, or a bunch of parrots? *(laughter)*

Sulnick: I would like to make a response to that, although I've never represented parrots before. *(more laughter)*

I think that that is not a good argument. Clearly — and Boyce's remarks, I thought, were perfect on this — the problem is overpopulation. We rarely talk about it because it's verboten — then you get birth control, and that's a big deal. It's hard enough to raise money to run a 501(c)(3), much less tell your audience they've got to deal with birth control. But clearly that's the problem. And clearly, the way we live, considering how many of us there are, exacerbates the problem.

Obviously the ten million peasants have to be factored into the solution. But you can still create a solution that preserves the rain forest, that promotes the health of the planet that we're all dependent upon. And you don't pit them against one another. That's a huge mistake, because no government can turn its back on its people and still be in power. So the people are part of the solution and need to be factored in, and need to be part of the dialogue.

Frieman: I have spent far too many hours and years advising the government on one issue after another to feel very sanguine about the prospect. We saw what happened to the President's Science Advisory Committee when they recommended against developing the supersonic transport because of environmental concerns. PSAC was then abolished, and it didn't exist through the rest of the Ford Administration or the Carter Administration; then something was put back together under the Reagan Administration.

At Scripps you will find many scientists who serve on all sorts of government advisory panels. Their advice is sought. I can't guarantee that their advice is often listened to. But at least there are recognized avenues. We have a political process — as some people say, we have the best Congress money can buy.

Nevertheless, it is important that tensions exist in our society, partly because of the environmental movement, which does a spectacular job in raising these issues and bringing them forward and pushing on them. You must continue. There are other sides who are also pushing forward. Somehow, in the tension, we work out a political process that is, unfortunately or fortunately, the best one we have.

Scientists do have a voice. There are many ways to get our views known, both to the federal government and to the state government. Because we are a public instution, all of our information is paid for by the state of California or the federal government, and is available to everybody.

And it is up to you, the environmental movement, to interpret it one way; it is up to other people to interpret it another way; it is up to the federal government to interpret it a third way, etc. I agree with John McGowan: we scientists do not have the necessary right to say that this particular scientific result has that particular effect on society. There is no reason why we should be any wiser than anybody else in that regard. I think we have to have a certain amount of humility about what our results mean. We should make them uniformly available. We should take our teaching responsibilities seriously and get those messages out to as many people as possible. It just does not work in any other fashion that I can see at the moment.

Thorne Miller: I want to say that where scientists may represent only one viewpoint, it is an important one. And if you back off from the decisions, then the decisions will be made with an unbalanced viewpoint, because other people will not be afraid to come forth and offer solutions.

So I think scientists do need to take that extra step to suggest solutions and make sure that the information that they have provided is used. And the solutions finally chosen may not be the ones that the scientists suggest, but these must be considered as part of the equation.

Sulnick: I want to ask you, Boyce, a question, but first I'd like to say to Professor Frieman: the information is available, but it's really not accessible, and it's a mistake to believe that it is accessible. And I want to explain why that is.

When I took my job at American Oceans, my salary was cut more than half—dramatically more than half of what I made as a lawyer. And that's neither good nor bad. I just want to point out that environmentalists don't have a lot of money. And when you don't have a lot of money, you don't have a lot of support. So I don't have an associate, as I had in the law practice, and two secretaries, and a whole support system, to whom I can say, "Would you analyze this for me, please. I need it by tomorrow," and then have it.

Instead I would have to spend a lot of time on my own—which is not a bad thing: I certainly am capable of doing it—gathering that information and making it accessible, first to me and then to my audience. I don't have that process built in, because I can't pay for it. And I can't do it myself and still run the organization and do what else I have to do in life. So while it is clearly available, it's important for you to understand that it's not really accessible. And until it becomes so, it will only be used by those who can pay to access it, which is of course industry. That's not bad; it's not unfair. But that is the way it is.

So what environmentalists always have to do is work overtime, which is not a problem, but it's still very hard to compete with the opposition that will spend millions of dollars in accessing the information and then interpreting it. And in order to rebut the opposition's interpretation, you need to have an expert witness or you really lose credibility. I just want to make it clear that although the information may be available, if it is not easily accessible, it is going to be used by the side that can afford to access it, which is the side of the status quo. Again, this is not necessarily bad, but without question has its consequences.

Let me ask Boyce a question: You clearly said that scientists need to become involved in the environmental dialogue. How do we environmentalists meet them halfway and bring them in? Because many times I hear from scientists, "You environmentalists really don't want to hear from science, and you reject our information if you don't agree with it."

Thorne Miller: We do want to hear from scientists. And it's not just I as a scientist who wants to hear what scientists have to say. The Oceanic Society has a service called a technical assistance program, and we hear from small groups around the country who say, "We need technical information. We're working in a vacuum. We feel that this particular coastal issue is an important one, but we don't have the expertise to fight this battle."

A good example is the proposed Monterey Bay Marine Sanctuary off the coast of northern California. A group came to us and said, "We want the sanctuary to be as large as possible. In fact, we think it should go up to the Farallons, but the Marine Sanctuaries Office [of NOAA] is resisting that. We don't have the scientific information." We contacted some scientists at Santa Cruz and found that, in fact, they believed the same thing that the environmental group did. So we can facilitate that interaction, get those people together, and get both the environmentalists and the scientists talking to the decision makers.

Sulnick: Would anybody who is a scientist like to respond to that?

Frieman: Just one quick point. Jackie Parker is the head of our Public Affairs Office. She will respond to phone calls from anybody, and will try as hard as she can to put people from the public in touch with Scripps scientists. If you want information in terms of reports, she will facilitate getting them. That's one of the reasons we have this office. I admit that it has a very small staff, in terms of the issues you're raising. But this is an important point: we are a public institution. We have a responsibility to get our information out. We have such an office to try to help, and when anybody calls Jackie, to the extent that she can (she works terribly hard and has an overworked staff, as we all do), she will endeavor to get the information to you. This resource is available to you.

Sulnick: Professor Scheiber, your remarks about the watershed events and the turning points were wonderful, I thought. My question is, do you see one coming up in the near future over the global crisis that is perceived among us, and is the role of science in that to help initiate it, or simply to respond to what comes out of the grass roots?

Scheiber: I agree with Dr. Frieman's remarks entirely. I think we're not in a good position now, either as scientists or as people who study science in the policy process, to advance this enterprise. There's not agreement on what has to be done. There isn't the kind of agenda that existed in these turning point situations: one reason that they were turning points is that there was an agenda. Scientists were able to guide policy in a given direction, faced with a specific problem.

In the case of global change, there is enormous variation in how the dangers are perceived. Obviously, from the discussion in this room, there is enormous variation in the degree of confidence with which scientists from the various specialities or those seeking an ecological view approach the problem. Contrary to the moments in time that I mentioned in the post-1945 history of fisheries oceanography, the issue isn't really being presented to scientists in a coherent way today. They are being asked to give it coherence. So as I say, I share Dr. Frieman's rather gloomy assessment of how well equipped we are at this moment to deal with it. I think we're floundering, not in this room alone, but in the professions.

Sher: Does that lead you, then, to the conclusion that the government should not be taking any steps? Or if that's not your conclusion, what steps should government be taking, given this diversity of opinion about the problem?

Scheiber: One possibility would be to accept that, remote as we are from a coherent conception on which a great majority can agree, continuing support of the still-fragmented approaches is warranted. I'm not preaching standing back and doing nothing at all.

Sher: Do you think there should be more money for research, or should there be some. . . .

Scheiber: Yes. More money for research on lines that are directed toward a better and more coherent definition of the problem.

Sher: But how about the argument that the problem is overtaking us and that if we wait for the results of the research and don't take the preliminary steps it will be too late to deal with the problem?

Scheiber: Yes, I agree with that. You'll be happy to know I support you on the forests; I think there should be a moratorium and not just a study.

Sher: I wish you were a member of the legislature.

Sulnick: My last question is for Professor Frieman. If one assumes — which you may not, so part of my question is to ask you to clarify this — that the house is burning down around us, so to speak, meaning that the problems are very serious on the global level, is it not possible that the role for an institution like Scripps in the twenty-first century should be different than it was in the twentieth and the nineteenth, and that it should be dedicating itself to setting this agenda rather than just doing objective research?

Frieman: I believe that my role here is to try to put Scripps on a course that I call Scripps 2000. I and my colleagues, as you can see by the color of our hair and the lines in our faces, are ready to depart this system. There are a huge number of young people out there who have to be trained to take over positions of leadership.

I regard the agenda of global change as really the future of Scripps in the next decade and beyond the year 2000. I've made that clear to our faculty. We are hiring new faculty, and we've organized new research divisions along those lines, and so on. That's our internal business, but nevertheless it is part of a major agenda. As a leading institution in the United States, we must take a scientific leadership role.

I then ask the other question: What else can we do as scientists? Suppose that this ecological disaster really does creep up on us. I think that we are in the very early stages of understanding mitigation. We understand global mitigation — reduce fossil fuel use, switch to alternate fuels, switch to nuclear, switch to solar, switch to fusion when it comes along; eliminate chlorofluorocarbons; we're doing that. We believe that methane is a major greenhouse gas; we have no idea what to do to control methane. Methane might be a much larger piece of this whole program. We can reforest. As I go further on in this list we find ideas that are more speculative: dispose of CO_2 by burying it in the oceans; ferry huge amounts of sulfur dioxide into the atmosphere to reflect sunlight; put satellites up like venetian blinds to control the sunlight. We as human beings have enormous intellectual power, and we haven't thought through the mitigation problem at all. We have not really encouraged ourselves or let ourselves do that. I think we have to get on with thinking about mitigation quickly, because it's a neglected part of the agenda, and it may be one way to cope with the problem if it comes on us rapidly.

Scheiber: I'd like to ask Assemblyman Sher, just to clarify his question to me: What would you put a moratorium on in this area? It's clear what you do about cutting ancient trees, but would you put a moratorium on all economic activity?

Sher: That's the kind of proposal, obviously, that I resist, and it's not going to be acted on. But what would the scientific community do in terms of trying to promote a decrease in chlorofluorocarbons? What kind of drastic actions? Is that important enough? A number of bills were introduced this year. There was a perception that this is a very persistent problem, affecting greenhouse gas as well as causing a problem with the ozone layer. Is science ready to mobilize and say in a unified voice that this is something we must do now? We know it's important. We don't know how serious the global change problem is, but we know this will help, and there are other reasons to do it.

That's what we're looking for: we need help when we take these initiatives — there are people in the legislature who respond to this problem, who've been contacted about it, who read about it, and so they drop bills into the legislative hopper. That's what you elect us and pay our minimal salaries to do *(laughter)* — introduce legislation. So even though we don't know whether the problem is really going to overwhelm us, there are good reasons to take some steps anyway, and this is one of the strategies. I want to see that kind of thing put together with the help of the scientific community.

So often in the legislature, as the moderator was saying, when proposals are made, well-financed industry and business groups who will be impacted by the proposals use science in the other way. They find scientists who will say, "We don't know the answers, or we're not sure of this. It's not good science you're doing here." So in the legislation we set up scientific advisory panels. That's why there are a lot of them in federal government; they are not put there in order to collect the information to lead to action. A lot of them were put there to prevent action. And that's what's a little frustrating for me.

Sulnick: We have received a lot of written questions from the audience, some of them general and some addressed to individual panel members. I'll present the general questions first.

The first one reads: "Much scientific information and opinion is housed in institutions with political constraints, i.e., agencies and other governmental bodies. How can that expertise found in individuals be released into the public arena in the same way that Scripps allows its scientists to speak as individuals, while taking no position as an organization?"

This is directed to anybody on the panel, but first I'd like to comment. I read this as a two-part question. One part has to do with the myriad data that exist in different agencies like EPA or NOAA, data that are really astounding to try to tackle. I've tried, and I'm sure Boyce has tried. The way I handle it is that I call up my local congressperson and say, "Look, I need your staff to do this," and some do and some don't. How do you handle it?

Thorne Miller: I try to call NOAA.

Sulnick: As I said, in my mind this is a matter of economics, because all of those data are available, but access is clearly not, from a practical point of view. Obviously the information is public; anybody can go and look at it, but one needs time to go and look at it, to write an analysis of it.

Scheiber: Actually, some of the federal data bases have been privatized; they are not public. A lot of the data that researchers want have to be purchased today. It's a very serious problem.

Sher: Let me tell you how we do it in government. We use moles. . . . There are a lot of agencies, and not necessarily for scientific information, particularly when the executive branch is in the hands of one political party and you're in the other party, or just generally in another branch of government. There are individuals in the various departments and agencies who know a lot of things, but they are under constraints. And yet they have friends on the outside, and they feel strongly about an issue. It's not just whistle blowing, but it's a lot of other things. They make knowledge available, and then the politicians on the other side can start digging without revealing their sources.

I suppose there are people in the scientific community who are under similar constraints, but who can furnish what they think is revealing information that ought to be known and acted on.

Sulnick: The second part of this question is rather interesting, the way I'm reading it. If a member of the faculty at Scripps makes a public statement, is that attributable to the institution, and how does the institution respond to that?

Frieman: As I said, as far as I'm concerned, any member of the Scripps faculty can make any statement in public that he or she would like. That is their right as citizens of this country. They cannot, however, make the statement in the name of the institution, because there are 1,200 people here, and maybe 1,199 disagree with them. So the individual simply should not speak for the institution. That's all we're saying.

McGowan: I can confirm what Ed has been saying. In my thirty years here at Scripps Institution, I've never felt any constraints against getting up and speaking my mind in public. *(laughter)* As a consequence, I'm not very often invited to do so. But it is a serious matter. One should not claim special consensus expertise or give the impression that one is speaking for the institution in general.

Thorne Miller: I see a little more to the question. I think it also is asking about the highly competent individuals in government agencies who perhaps cannot speak out. I think that this is not so much a question of cannot. I think Jim Hansen, for instance, showed us that they can speak out; they can express an opinion that is not necessarily representative of the agency, in his case NASA. But it's complicated. When they give testimony in Congress, very often that testimony is reviewed by members of the administration who say, "No, you can't say this; you have to say that." I think that the individuals in the agencies who feel strongly about this need to speak out and say, "Give us our chance."

Sulnick: The author of the next question begins by stating the function of this meeting — to talk about what society needs from science in preparation for global change — and then asks, "Ought not the question be rephrased to the following: Because humans must exploit the environment to some extent to survive, how can science help society to continue exploitation responsibly in the face of global change?"

A follow-up question is addressed to Bert Larkins: "Since the Alaskan factory tanker fleet will need to overfish the resource in the next two years to make mortgage payments, what do you feel is the first step needed to implement effective management to allow maximum sustained yield of the resource?"

I think the general flavor of these questions is good, because we are a society based upon the consumption and exploitation of resources. Some of our resources are nonrenewable, and some take a long time to renew. But all of them, once used, show a consequence in the global scheme of things. So the question becomes for all of us, in one way or another, How do we begin to to retrofit our thinking so that we can sustain maximum yield?

Larkins: The members of my association are thinking about this themselves. They are sort of the last of the great pioneers. They've generally been opposed to limited entry; the ocean is the last place that's a common property resource; we can all go out and compete — "The cream floats to the surface," we hear daily. So government, stay out of our hair; just give us the biological maximum. If the quota's a million tons, okay, that's all we want. Some of us will make it, some of us won't.

That's a bit simplistic; some of the viewpoints are starting to change, particularly among those with $30 million mortgages. They are starting to wish they had bought a 100,000-ton share five years ago.

Part of what I tried to say earlier is that none of these folks will argue that it's not important to live within the bounds of conservation, whether you describe it as the allowable biological catch, which is a term now in the fishery management area, or as the maximum sustainable yield (which, by the way, is a term I abhor, but I'll argue that some other time). But nevertheless, the pressures are strong.

Scientists, don't you start taking a conservative view of what these numbers are because you're concerned about other things. If you really think that the system can support a 1,200,000-ton removal of pollock for the next year, that's the number you should give to the policymakers. And you ought to do everything you can to support it and to stand behind it. Other people, for whatever reasons, will try to increase or decrease that. What we want from you folks is the ground truth. We want it stated very succinctly, very clearly. And we want you to stand behind it. And then the political system will start to work.

Other than that, I don't know how to answer this question. There are groups of bioeconomists and there are social scientists coming into the realm. So far, at least in my experience, the mix of biologists, economists, and sociologists has not really worked very well. There are social and economic aspects to almost all of the actions that my members and anybody else fishing for a living have to face up to. We're starting to hear such things now as, our fleet, because of its bigness, is responsible for increasing the suicide, abortion, and divorce rates of Kodiak. That's pretty powerful in a political arena. And how do you defend against it? Maybe it's right; I don't know.

Sulnick: Isn't it true that this debate about exploitation is really directed at each one of us? Because all of us are living a lifestyle dependent upon exploitation. We all have mortgages; mine isn't $30 million, but I have to make the payment, and I assume everybody else has an equivalent. So isn't this the question: How do we no longer exploit and still manage to carry on with the work of living? This is not a question directed just to fisheries. It's a question directed to all of us. How do we change our lifestyles so that we no longer dump oil into the gutter that runs into the coastal zone, or so that we conserve miles per gallon? It's a generic question.

Larkins: I'd like to make one more comment about this. For a long time, even when I was working in Seattle in the sixties, I had some interface with the folks down here in the tuna industry. The superseiners were just coming on line; overcapitalization was being talked about. But people always knew there was another opportunity. They weren't sure what it was, but "Okay, if we've overcapitalized this one and we can't make it in the tuna industry, sardines will come back, mackerel will come back." We, and even folks on the East Coast, and in the tuna fleets, looked to the Bering Sea. There was a 2 million ton-plus potential resource up there that was already marketable if you knew how to market in Asia, because there were Asian fleets up there exploiting it. I know of factory trawlers that were built in New England when things were rather depressed there (they have since gotten worse), and some people said, "Well, we may end up in the Bering Sea." That was their out.

There isn't any out any more. The whole commercial fishing industry — at least the groundfish, coldwater fishery — has suddenly, for the first time, met itself coming around the corner. And we are just now starting to face up to this. The political-scientific system that's been working within this community for thirty or forty years has suddenly come to the same conclusion. And I think the shock value is such that we really haven't sorted it all out collectively or independently. There is no place else to go.

We have tuna boats in the North Pacific fleet. We have some of the converted 200-foot seiners: they took the seine table off and put trawl winches on. They don't do very well, but they are probably doing better than if they'd stayed in the tuna industry. Many of the vessels that entered the Alaskan groundfish fishery in the last ten years were designed to do something else when they were built. King crab boats had stern ramps installed and have become draggers. I suppose there are rattails and sauries and perhaps one or two other species out in the middle of the ocean, but even FAO now says that there are no great untapped resources left.

McGowan: I'd like to address a couple of points, one that you just raised. And that is, How do we continue in the style to which we are accustomed and not exploit nature? I think this has been treated many times before. One of my favorite philosophers, Eric Hoffer, wrote an essay about that subject — a very good one. I think the answer is, we can't. We can't do it. We're going to have to exploit nature in order to live well.

It's the same question, or a very closely related question, as the one about the ten million Brazilian peasants. Amazonia has to be exploited in order to provide those people with the minimum adequate life. That at least is the argument. I don't know what we do about that.

Sulnick: I want to tell you. We were walking along on the same path, step for step, and then you went that way and I went this way. I said, "That's right," and I assumed your next sentence was going to be, "We're going to have to change the way we live." To me that's obvious.

McGowan: I don't want to. *(laughter)* It took me too long to get here.

Sulnick: That's the way it goes. To my mind, you don't cut down the rain forest to maintain your lifestyle. You can't kill all the dolphins to maintain your lifestyle. You're not going to have drift nets to maintain the lifestyle. To me, that is insane. Nothing personal. *(laughter)*

I think that this is the real debate. And we, each of us, will obviously determine the outcome. Because to the degree that you say you're not involved, you're teasing yourself — to put it mildly.

Sher: Conservation is not going to come about voluntarily, by individuals. It's going to be done by regulation. The best example of that is what's happening in the South Coast Air Quality Management District. We've been reading about the new air basin plan and the 120 or 130 regulations that have just come out in order to comply with ambient air quality standards.

These regulations — such things as not being able to barbeque in your back yard, and what kinds of paints you can apply to your structures — are going to affect everyone's life. A tremendous range of activities will be controlled. There will be very serious attempts to cut down the number of vehicle miles traveled, because all of the gains we've made in cleaning up the internal combustion engine — and we've made a lot of gain on points of basic science — have been wiped out by the increased number of vehicles on the road and the number of miles that they are traveling. So there are going to be very stringent restrictions on how you can use your personal automobile in the future. This will affect our lifestyle, and it will affect all of us. But it won't come about voluntarily. If we had to rely on its being done voluntarily we wouldn't make any gains.

Scheiber: Following along this line, the Bering Sea is an interesting case in which there are also conflicting uses, and terrible controversies that arise. Not only is it the place of last resort in the dreams of fishermen who are depleting stocks elsewhere, it has also been an arena for enormous international tension and a huge investment of American prestige over the years — first to keep the Japanese out, then a leading motivation for the 200-mile zone. There is a vast history there, and virtually overnight we might see it all go to a point of extreme danger with oil rigs and tankers out there (a statement that doesn't sound as paranoid as it might have sounded before *Valdez*; we all understand that a little better now).

So that's the kind of controversy in which—in answer to you again, Byron—choices are clear. We can put off the day of involuntary gas rationing for another week by opening up all those wells and producing a week's supply for the nation out of the Bering Sea. That's a very discrete kind of situation. The global change problem is not that kind of a discrete situation in which you can bring either scientific or other kinds of expertise to bear as intelligently and as rapidly. So that's where the perplexity is.

Thorne Miller: I followed along your path, Bob, and made that same divergence with John McGowan. But I also want to remark on the comment about voluntary change. I think that there can be some impact from voluntary changes. A good example of this is the recycling issue. That is something that we have had to push policymakers hard on, and we've done it by people voluntarily showing that they can recycle. The local government refuses to legislate recycling requirements, saying residents won't go along with them. So the citizens take the matter into their own hands and establish voluntary recycling programs. The programs prove successful, so the local government decides, "Okay, then we'll put in recycling regulations."

Sulnick: I've got four more questions that are pretty good, and I'd like to get one or two of them in. The first one is to you, Assemblyman Sher.

"Environmental fads come and go, yet global change develops over a long time—decades, years. What mechanism can be implemented to ensure long-term commitment of federal and state agencies to document and understand environmental change?" That same idea was raised in the *New York Times* two days ago. . . .

Sher: Let me see if I understand: is that money for research again? *(laughter)*

Sulnick: You've got it. Next question.

Sher: I'm for it. I always support money for research.

Sulnick: This is an interesting question to me because I don't personally believe this is going to turn out to be a fad. When one gets sick, there are symptoms, and the earth is showing a lot of symptoms. If you don't see them, come to L.A. for a day and breathe the air. That habitat is so stressed out that it really is not supporting the quality of life for those of us who live there. And we know it, even if not intellectually.

So I don't think the issue of global change is a fad. But in this question, the word *fad* is being used to mean politically popular. It's politically viable for the moment. The problem doesn't go away, but its attractiveness goes away. And then do we stop paying attention to it? That's the question. And how do we ensure that that doesn't happen? Even if it's not in a politician's best interest to promote x, but x needs to be promoted, how do we continue to promote it, especially when we're talking about global change?

Sher: I think there are a lot of fads that politicians respond to, one of which, for example, is whether we ought to amend the Constitution to make the burning of the American flag illegal. There are certain powerful emotions, and people read the polls. But in the environmental area, I don't believe that's the case. I think that where there are serious problems and there are people working on them seriously, we don't get that kind of political mileage out of working on them. There are too many organized forces against us. . . .

That may not be entirely true; you're going to find an environmental initiative on the ballot next year. One of the gubernatorial candidates is supporting it, and it deals with three major areas. One is offshore oil activity; a second is pesticides; and a third relates to various aspects of the global warming problem. It's true, people are using that because they detect a public awareness and involvement, and they think it will benefit them politically. But those issues are not fad issues; they are all serious. And we'll talk about them for a while, and the initiative may or may not pass, and if it passes it may not be implemented very well to deal with the problems. It isn't that the politicians will walk away from it because it's no longer fashionable.

McGowan: I'd like to make a comment in my role as a scientific answer man. I think there is absolutely no question among reputable scientists that concentration of CO_2 in the atmosphere has been increasing. We know that for sure, as well as we know anything. And so it is for other gases that affect radiative transfer of heat and energy from the sun. It is a virtual certainty that the increase in these gases is going to cause a warming of the global atmosphere. Almost every reputable scientist I know of believes the theoretical physics behind that argument.

What is uncertain is whether or not we have detected that warming as yet, because there are, after all, cycles in climate, and this recent warming may be just part of a larger cycle.

What is even more uncertain, and in my opinion, virtually unpredictable now, are the consequences of that warming. We don't know what the magnitude is going to be; we don't know where it's going to take place; and we don't know how environmental systems, especially biological systems, are going to respond. That's where the real questions are. And that's where the uncertainty is. But it is a phenomenon — there's no question — that's going to happen. It's not a fad.

Sulnick: In your view, is it a phenomenon we should actively seek to stop?

McGowan: I wrote something down here earlier: Is our present knowledge sufficient for major political action, or are there uncertainties enough to delay action? Isn't that the question?

Sulnick: Precisely.

McGowan: Yes and no. There is sufficient knowledge right now to say that we absolutely must do a better job of understanding what the consequences of change might be. We must tune up and become much more elegant and sophisticated in studying change. Change as compared to what? What kind of a baseline do we have? We must know the magnitude and direction of change, and it must be compared to what the ordinary state of the system is, on a global basis. We really have very little of that information. And yes, we need action in terms of, if you'll excuse me, more research.

Sher: But you wouldn't take people's cars away from them, based on what you know?

McGowan: Not yet.

Sher: You wouldn't make them travel fewer miles in order to cut down the carbon dioxide yet?

McGowan: Oh, sure.

Sher: You would support a law that says you can only drive your car every other day?

McGowan: But there is already a lot of carbon dioxide in the atmosphere.

Sher: What I have to respond to is proposals that are made to at least try to stabilize how much CO_2 is going into the air. And what I want to know is whether you are going to support me.

McGowan: The economic consequences of some of the suggestions might be very severe. The cure might kill the patient, and that's what I worry about.

Sulnick: Let me add one other point of view. It seems to me that while we study things we should do so from a safe, or relatively safe, perspective. And if the environmental changes that are taking place are as potentially destructive as they appear to be, we should put the brakes on as quickly as possible. I don't think that means that we should drive our cars every other day; I think that means we should drive cars that don't add to global warming, which, my information tells me, are technologically feasible. But not yet politically acceptable. But that debate would just go on and on. So, using the prerogative of the chair. . . .

Question from the floor: When we get down to the specifics, what are they really going to be, and are we prepared to advocate moving to nuclear power?

Sulnick: As moderator, I don't know that that ought to be what we are debating, because everyone has a point of view. As a quick response, I think there are alternatives that would promote an energy policy based on conservation and renewable fuels, and would not jeopardize the nation with the potential devastation of nuclear energy.

If I can avoid having a debate on this I would like to, simply because it's four o'clock and I have one more question I would like to present to the group before I thank you for your participation.

The question is: "How much money is the United States government giving CalCOFI to investigate the amount of contaminants the United States sends to Mexican territorial seas through the California Current?"

Mullin: I should point out that this question was translated from Spanish. Obviously there is a legitimate concern on the part of our Mexican colleagues that there is a flow of water from the coast of California to the coast of Mexico. I don't know that it will be possible for the panel to come up with a number, but it's certainly worth thinking about.

McGowan: The other part of that question is how much does the federal government support CalCOFI studies relevant to this? There is no support from the National Science Foundation or the Environmental Protection Agency; only NOAA supports the program. Most of the support comes from the state of California.

Sulnick: I have one final statement: I would like to thank the audience. You've been really attentive, and obviously involved, and that made it a much better discussion than it otherwise could have been.

Mullin: In closing, I suppose it's obvious to point out that a very large number of people have contributed, both to the symposium today and to the CalCOFI meeting for the last three days. I'd particularly like to thank Mary Olivarria, Sadie Gonzalez, Lari Maczko, Debbie High, and Kitty Haak, who have probably walked a hundred miles between here and our office to try to get all of the details straightened out.

Again I'd like to say that I'm grateful to our panel, particularly those from the Bay Area and Sacramento, who have probably had enough disruption in their lives in the last two weeks to keep them occupied for quite a while. We very much appreciate their coming here.

And finally, again, thank you to the audience for your participation and support.

Part III

SCIENTIFIC CONTRIBUTIONS

CALIFORNIA MARINE RESEARCH AND THE FOUNDING OF MODERN FISHERIES OCEANOGRAPHY: CALCOFI'S EARLY YEARS, 1947–1964

HARRY N. SCHEIBER
School of Law (Boalt Hall)
University of California
Berkeley, California 94720

INTRODUCTION

"Throughout the 1950s and into the 1960s," as McHugh (1970) asserted in a study surveying trends and accomplishments of U.S. fishery research, there arose

> a growing realization that the simple prewar concepts of scientific fishery management are not very useful in practice and that successful fishery management must be based on scientific understanding of the resource as it interacts with all the physical and biological variables in its environment.

The shift in concept of which McHugh wrote was truly profound, not only in its impact upon the scientific approach to fisheries management but also in its transformation of the ocean sciences. What occurred during the decade and a half following World War II was nothing short of a methodological revolution. Marine biology research was reunified with work in physical and chemical oceanography and meteorology, and a new holistic approach to the study of ocean environments emerged; researchers sought to analyze the processes of change in complex biotic communities rather than to study segmented processes or small geographic units of the deep seas (Scheiber 1986; McEvoy 1986).

The coordinated marine fisheries and oceanographic studies that would become known as CalCOFI (California Cooperative Oceanic Fisheries Investigations) played a crucially important role in this transformation of marine studies, participating centrally in the great methodological advances. The contributions associated with CalCOFI research cover virtually the whole spectrum of techniques and subject areas of research in what has become known as fisheries oceanography in the post-1945 era, from improved trawls and a new approach to comprehensive egg and larval studies in the late forties to the modern-day applications of remote sensing. It is a remarkable thing, moreover, that despite some rough spots along the way, CalCOFI has evolved successfully and survived in full vigor, continuing to make important contributions to an advancing oceanography even after four decades of corporate existence — in the face of all the odds associated with the modal "life cycle" of such scientific and other academic enterprises (Knauss 1990).

This historical perspective on CalCOFI in its early years (1947 to 1964) will focus on two important aspects. The first concerns how the scope and design of CalCOFI research on the California Current, and on the Pacific Ocean more generally, were originally formulated — that is, how the marine scientists and fisheries management specialists, industry leadership, and state and federal policy officials defined their research strategies and future needs in 1947–49. The state of American ocean research when the project was first designed will also be discussed. The second aspect concerns the dramatic development of the range and modes of scientific inquiry in the early years of CalCOFI research. The focus will be especially upon how a great conceptual divide in ocean science was perceived and then dramatically breached, opening the way for modern ecosystemic studies of the oceans.

DEFINITIONS AT THE FOUNDING: A COMPREHENSIVE DESIGN FOR "THE PACIFIC RESEARCH FRONTIER"

The enterprise that became CalCOFI was set in motion in 1947, when, as the successful culmination of efforts by a small group of scientists, government officials, and industry leaders, the California legislature approved a special tax on commercial sardine landings (McEvoy and Scheiber 1984). On the industry's initiative, the tax revenues were designated specifically for research on the causes of the sardine decline — commonly also termed the sardine depletion — which was then troubling the state's important sardine fishing industry and the processing plants and canneries that it supplied.

Landings of pilchard (California sardine) by the state's commercial fishing fleet had slumped from their phenomenal levels of the 1930s, when the sardine fishery in the California Current was said to be one of the world's most intensively exploited marine

---

[Manuscript received February 27, 1990.]

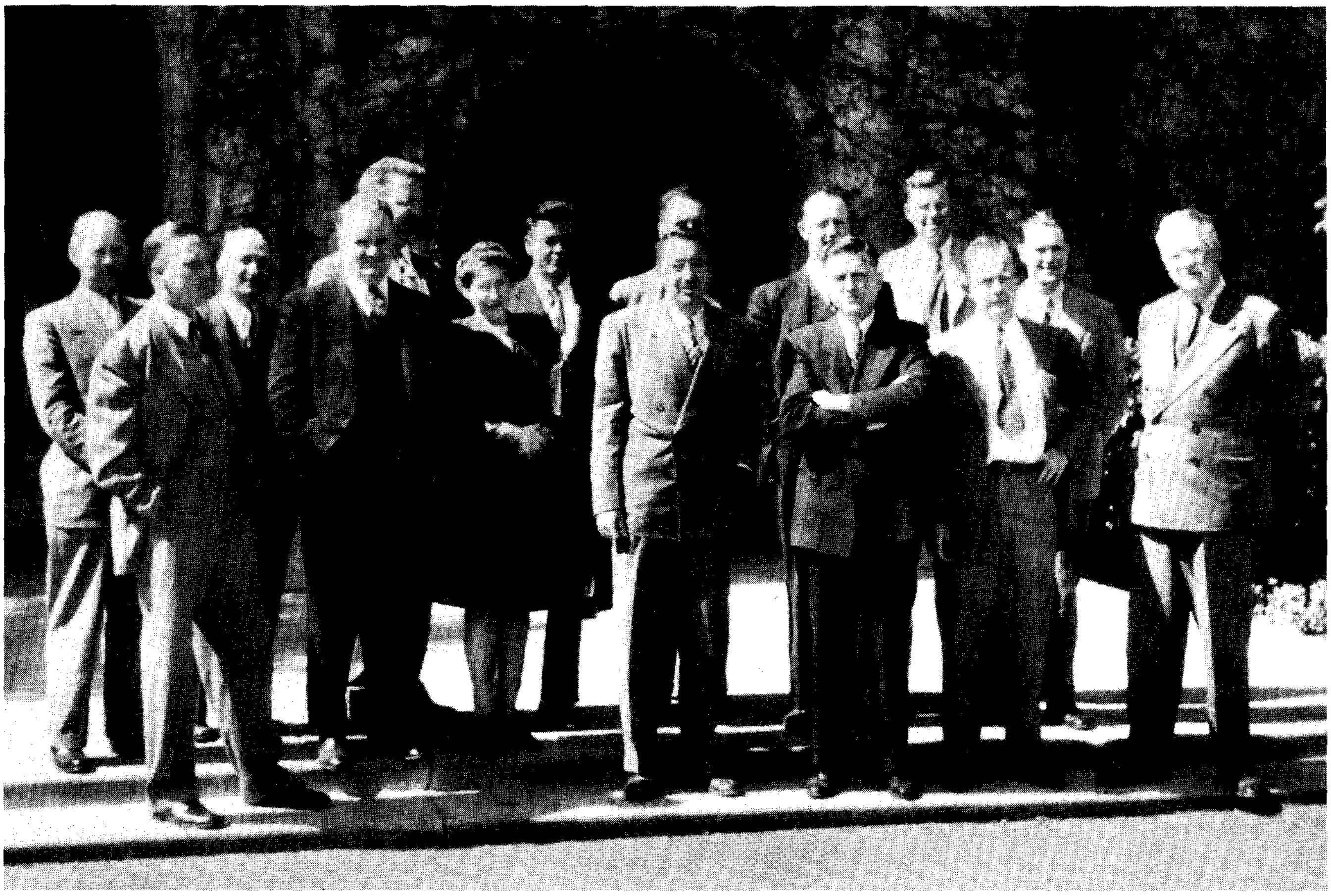


Figure 1. Photograph taken on March 14, 1947, at Stanford University at a meeting of representatives from the California Department of Fish and Game (1), the California Academy of Sciences (2), the South Pacific Investigations of the U.S. Fish and Wildlife Service (3), and the Scripps Institution of Oceanography (4). *Front row:* Milner B. Schaefer (3), John L. Kask (2), Frances N. Clark (1), John F. Janssen (1), Julius B. Phillips (1), Osgood R. Smith (3), and Donald H. Fry (1). *Back row:* Harald U. Sverdrup (4), Oscar E. Sette (3), Wilbert M. Chapman (2), Carl L. Hubbs (4), Robert C. Miller (2), Elbert H. Alhstrom (3), Richard S. Croker (1), and Kenneth M. Moser (3).

fisheries, and had then fallen disastrously in only a few years following the war. Thus the sardine harvest dropped from the peak of nearly 800,000 tons in the 1936–37 season to only half that level in 1945–46, then fell again to only 130,000 tons in 1947–48 (Radovich 1981).

The special tax funds were turned over to a joint industry-science committee, called the Marine Research Committee. It was this group which, in the course of administering the sardine research funds, would some years later formally establish the CalCOFI enterprise as the coordinating body for the research it was helping to sponsor for the U.S. Fish and Wildlife Service (USFWS), the Scripps Institution of Oceanography (SIO), the California Fish & Game Commission, and, on a much smaller scale, Stanford University and the California Academy of Sciences (McEvoy and Scheiber 1984).

As a political achievement, the background story of CalCOFI is one of intrigue and rare skill in the arts of persuasion, coalition engineering, and insider political trading in response to alarm about the sardine crisis. A small cabal of industry leaders, SIO and state fisheries laboratory scientists, USFWS scientists, and political leaders in the legislature put together the sardine research program idea in a series of meetings in the winter of 1946–47 (figure 1). The key players, at first, were Wilbert (Wib) M. Chapman, curator of fishes at the California Academy of Sciences; Montgomery Phister of the Van packing corporation; Carl Hubbs of SIO; and, soon coming onto the scene in a major way, Harald Sverdrup as director of SIO; Richard Croker and Frances Clark of the state Fish & Game Commission scientific staff; and Roger Revelle, then in naval service in Washington but soon to return to SIO as associate director.[1] [Numbered notes begin on page 79.] It was an elaborate political dance. They first lined up the reluctant support of the notoriously individualistic fishing boat owners and corporate executives in the sardine canning firms — men who were, as Phister called them, "captains and individualists," never prone to join ranks with one another or anyone else, suspicious of the ivy-towered scientists and the resource managers, such as Clark, who spoke the language of regulation and restraint.[2]

The organizing committee — which one insider late termed "the proto-MRC," because it was predecessor to the Marine Research Committee (MRC), the body established by the California legislature to administer the new program — forged an uneasy alliance between Scripps Institution's scientists, who were committed to pure research in physical and chemical oceanography and in marine biology, and the fisheries scientists in the state and federal agencies who were interested in "mere" applied management concepts.[3] The leadership also somehow overcame much of the long-standing mutual mistrust between the state scientists and the federal agency. ("The Federals," Chapman wrote early in the course of politicking for the project, "are actually wondering whether or not they wish to get any further involved. . . ."[4])

Not least of the unlikely achievements of a winter's whirlwind lobbying and alliance-building, the committee obtained not only industry's consent to a special tax on sardine landings, earmarked for the research project, but also the legislature's agreement to pump large new appropriations into the SIO budget to support the work, especially to operate three new large-scale research vessels. These ships were donated to the University of California, for use by SIO, by act of Congress and cooperation of the U.S. Navy. But they were also the special gift of Comdr. Revelle, who from his post in the Bureau of Ships managed to get the service's approval to transfer these newly decommissioned warships to the university, as well as congressional appropriations for their reoutfitting for research.[5]

It was the sardine crisis that set all this under way, but from the outset what Chapman and some of the others had in mind was a much more comprehensive push for what he termed "high seas research on a scale far beyond anything that the United States has undertaken or thought about in the past."[6] Chapman's language referred not only to research throughout the Pacific basin: for underlying all the early deliberations of the MRC scientists, as I will seek to show here, was a vision that foresaw the *transformation of the scope and content of scientific method in ocean science* — that is, the basic concepts of marine studies, and not merely a dramatic expansion of the geographic scope of studies in the eastern Pacific.

One instrumentality of this vision was to be structural, invoking the coordination of agencies and the collaboration of multiple disciplines. The sum of the enterprise (funded by the MRC and the cooperating agencies) would be made far greater than its parts by coordinating the skills, ships, equipment, and knowledge of the state marine fisheries laboratory, SIO, Stanford University's marine laboratory (the Hopkins Marine Station), the California Academy of Sciences, and the federal agency. Beyond that, the sardine project would share data and plan its work jointly with the ocean scientists based in the fishery agencies of the other West Coast states and British Columbia.[7] Not least important, the sardine project could complement — and in fact from the outset it was coordinated closely with — the oceanographic and fisheries work being started in 1948 under Oscar Elton Sette's leadership, in a Hawaii-based federal tropical tuna project.[8]

The State of Marine Research in the Pacific to 1947

There has been vast growth in the last forty years, since the California cooperative project on the sardine began, in knowledge of the Pacific Ocean in all its aspects — marine biology, ocean chemistry, geophysics, and meteorology. It is astonishing to consider how little, by contrast, was known in 1947 of what California marine scientists at that time liked to call the "Pacific Ocean fisheries frontier" (Pacific Fisherman 1947). The sardine crisis was only one segment, albeit a dramatic one with enormous economic impact, of a vast congeries of interrelated mysteries about the Pacific.

The archival records and some scattered scientific publications of the era reveal that to a remarkable extent the small community of West Coast ocean scientists had a keen understanding that this larger and more comprehensive web of unsolved mysteries had to be attacked if ever the resources for adequate research came to hand. They understood, in other words, how little was known about this ocean system and the precise nature of its dynamics. The patterns of the currents, the basic bathythermography, the ocean floor in the deep-sea areas, meteorological phenomena in relation to biological systems and hydrography — all these were scarcely known, despite the brilliant formulating of what we may term the "right questions" by pioneering figures such as Sverdrup and his associates at SIO, William F.

Thompson at the University of Washington in Seattle, Clark and other California state scientists in fisheries work (mainly on the sardine), Albert Herre on tropical fisheries in the western and South Pacific, Sette of the USFWS research office in California, and a few other notables.[9]

What of fisheries research more narrowly? A startling limitation was that West Coast research had long been confined largely to the inshore areas. Even the most basic questions remained unanswered for some of the great pelagic and anadromous species. No one knew, for example, how many distinctive populations of tuna inhabited the Pacific, where they spawned, or, even at the grossest level, how abundant they were. There was uncertainty even about whether the warmer Pacific waters and inshore areas of the western and South Pacific region had stocks enough to support fisheries on a sustained commercial basis.[10] Similarly, in the North Pacific, apparently no one, at least in Canada and the United States, had the slightest idea whether or where Asian and North American salmon intermingled in the high seas (the Japanese did have some fragmentary data that they kept secret), or knew what events in the high seas most affected the stock during the life cycle (Herrington 1989; Scheiber 1989).[11]

Identifying the relationships between the condition of fishery populations and their ocean environments (including such aspects as nutrients, food chains, chemical properties of host waters, currents and weather, patterns of predation and interspecific predation, etc.) had been in the minds of fishery scientists since well before the end of the nineteenth century. When the first of the great Scots coastal fisheries surveys was established in the 1880s, for example, even before the *Challenger* reports were published, the stated objective was to understand the relative impacts of human activity and environmental conditions on fisheries (Deacon 1990). The importance of such ecologically framed study had also been recognized in the coastal and seabed fisheries research in Scandinavia and northern Germany at the turn of the century, best exemplified in the work of Johan Hjort. Without question the environment's relationship to commercial fishing and its impact on marine resources had motivated the formation of ICES at that time (Idyll 1969; Dymond 1948). But research on these lines had generally been frustrated by the limitations of technology, gear, and funding: the oceans were too vast and impenetrable.

As a result, in the interwar years, 1918–39, the focus of commercial fisheries research had shifted radically. Led by William Thompson, whose theoretical and applied work on sardine, halibut, and salmon was most important in providing the direction and intellectual framework of Pacific studies, the fisheries management scientists resorted to an emphasis on harvest theory and the concept of maximum sustainable yield, indicated by harvest volume (output) in relation to inputs ("fishing effort") (Russell 1942; McHugh 1970). This almost exclusive emphasis, responsive to the needs of the fishery industries and becoming the basis for some successful management programs (most notably, the halibut effort undertaken in 1931 by Canada and the United States, with Thompson in charge), meant a loss of momentum for the more problematic and difficult work of dealing with ecosystemic relationships.

Marine scientists did not lose the vision of ecosystem study, to be sure; fishery experts trained under Thompson himself, for example, later recalled reading in their journal groups at Seattle the studies by Hjort and other pioneers in the ecological style. But during the interwar years in the United States and elsewhere, the requisite money, gear, technology, instrumentation, ships, and personnel were entirely lacking for work in this mode (Herrington 1988; Scheiber 1988).

Given greater resources in scientific personnel and funding, even within the existing limitations of research technology, much more could have been learned, but research on environmental relationships to fisheries remained fragmented, small in scale, lacking in spatial scope or intensity. Pacific Ocean studies on the West Coast of this country were, in sum, almost unbelievably impoverished. The brilliant but scattered achievements of an era that stretched from the Wilkes Expedition in the early nineteenth century to the *Albatross* and *Carnegie* voyages of 1900 to 1931 had been followed by a decade in which only one American-flag research vessel (the *E. W. Scripps*) was dedicated to basic oceanographic research in the Pacific. Only a handful of scientists did offshore research, and many of that small number were in agencies whose funding was based solely on their mission of conducting applied research on coastal fisheries management. The languishing of this American research effort, because the resources were not there, compounded the very real difficulties associated with the state of available technology for deepwater study (Shor 1978; Scheiber 1986).

In retrospect, however, it seems evident that at the important West Coast centers of study — SIO for the chemical and physical sciences, and secondarily for biology; USFWS and the California state agency for commercial fisheries research; the University of Washington for salmon and halibut research and oceanography; and Berkeley and Stanford for zoology and biology — the small cadre of ocean scientists, numbering perhaps thirty at most, understood with remarkable insight what were the most important gaps in empirical knowledge and methodology. Precisely for this reason, as we will see, they were able to reach broad agreement as to what an agenda for expanded study ought to look like, and which kinds of inquiry would be likely to yield the most knowledge of ramifying ecosystemic relations.[12]

In this context, the decision to launch the California sardine project constituted a remarkable departure in the history of Pacific Ocean research — a landmark in the reestablishment of a major American research presence in Pacific science. It put funds in the hands of Pacific marine scientists at levels that were ten and more times the revenues for research that they had enjoyed in the previous two decades, and it made possible the inauguration of deepwater research by several new ships, well equipped with the latest gear, that extended by an enormous magnitude the capabilities for ocean research. At about the same time as the California legislature authorized formation of the MRC and the cooperative sardine project, the U.S. Congress moved to correct what had become a scandalously embarrassing deficiency in support for Pacific oceanography by establishing the Hawaii-based tuna research project (POFI), the scientific work of which was initially under the direction of Sette, with Schaefer in charge of biological studies.

Although the crisis that galvanized California was the sardine's critical decline, the national motivating force was a larger geopolitical concern expressed in congressional debates: the concept of Pax Americana and more specifically the intermeshing ambitions of the U.S. Navy, the Pacific fishing fleets, and the fish-canning industry to establish the American presence in Pacific deep-sea waters before other nations, friendly or otherwise, had recovered enough from the devastation of war to stake out claims that would preempt U.S. interests (Scheiber 1990a).

Understanding "One Ocean As a Whole": The Pacific Vision

A striking feature of the California effort in this surge of new activity in Pacific research is the fact that the community of West Coast ocean scientists — however fragmented in other respects — had their agendas fairly ready in hand when the political moment for action arrived. This is not to say that there was a "Pacific Oceanographer's Manifesto," or the equivalent of some priorities handbook, to which any and all ocean scientists might subscribe. Rather, there was a shared awareness of what needed to be done to get the work started in the Pacific.[13] The best of the fisheries management scientists in 1947–48 were already keenly aware of what they needed to learn in areas where they had little or no data — on problems such as interspecies competition, or the relationship of nutriment levels to juvenile survival rates, or the role of upwelling, which had been explored from a meteorological perspective in the brilliant early Pacific studies of Sverdrup and his associates at SIO.[14] Once the prospect of new funding, gear, and ships was at hand, the scientists quickly produced their wish lists.

Perhaps this is in itself unremarkable; all good professionals have some kind of wish lists ready at hand, in the happy event that funds should suddenly become available. The historian will find, however, much more than random or disparate priority lists in the archival records of the CalCOFI project and of its progenitor the Marine Research Committee, or in the personal correspondence of scientists such as those who masterminded the California push for research funding: Chapman, of the California Academy of Sciences, who principally orchestrated the political moves, put his intellectual imprint on the research proposals, and, rather miraculously, recruited the fisheries industry to the cause; Carl Hubbs, Sverdrup, and Revelle of SIO; Sette of the federal agency, joined in 1948 by John Marr, who would succeed him in charge of the U.S. Fish & Wildlife Service South Pacific Fishery Investigations; and Frances Clark, that remarkable woman who, by her studies over many years in the California state agency, had established herself in the front ranks of fisheries science and was a pioneering advocate of stronger management constraints.[15] Many of these scientists were also associated to varying degrees with the overlapping effort to obtain congressional action to establish the POFI project in Hawaii. The published sources and surviving personal correspondence that express scientific thinking in the West Coast community of fisheries specialists reveal important common themes and a core of common objectives.

There were also some important cleavages, to be sure, within the scientific community — the divergent interests of the biologists versus the physical and chemical oceanographers, and a very clear demarcation between applied and pure scientists. There were also important differences of view among the fisheries-management scientists as to how heavily to rely upon landings data for evaluating the condition of the stocks. Considerable perplexity was also evident as to how, if it could be done at all, to build on the insights of early-day fisheries ecologists — led by Johan Hjort and followed up by Michael Graham and others who had sought to relate environmental conditions to fishery dynamics — as a way of getting beyond Thompson's harvest-yield approach that was so dominant at the time (McHugh 1970; McEvoy and Scheiber 1984).

But a key element of shared understanding, evident in the various agenda ideas that were sent back and forth among the Pacific Coast scientists and that ended up as working policy documents for the direction of new projects, was the sense that *the scope of research ultimately must be the Pacific Ocean* and not merely discrete geographic regions and segments in which one species or another dwelt. This was a vision that went beyond solving even a crisis so ominous and disturbing as the sardine decline that was then occurring.

In October 1946, for example, scientists from the various state fisheries management agencies of the West Coast, together with a representative of the federal agency, had formally proposed research "to establish the relationship between oceanographic fluctuations and the concomitant fishery phenomena . . . [requiring] a continuous record of conditions in both fields: physical oceanography and fisheries."[16] The U.S. Navy Hydrographic Office quickly endorsed this view of a need for "extensive synoptic oceanographic information about the waters off the Pacific coast of North America," and especially "expanded investigations of the departures from normal oceanic circulation" — an endorsement that well reflects the direction of thinking that prevailed among West Coast oceanographers at that time.[17] For this was precisely the view that Chapman, the SIO group, the federal scientists (Sette, Marr, Walford, and Ahlstrom), Robert Miller of the California Academy of Sciences, and others expressed constantly during the hectic period of planning for the sardine studies in 1947–48, preliminary to the forming of CalCOFI.

This large strategy for research was recognized eloquently by one of oceanography's leading figures, Columbus Iselin of Woods Hole, in a conference address at SIO in 1951. Appraising the importance of the California group's sardine studies and other new research, Iselin commended the West Coast scientists for giving substance and hope to the idea that it might be possible "to understand at least one ocean as a whole."[18] Similarly, Roger Revelle and others at SIO often voiced the view after 1948 that their new capabilities — reflected in the ships, gear, funds, and technology that were then at their disposal — permitted their institution's scientists to move far off the California coast and to "make the entire Pacific our oyster."[19]

Looking back on this element of "original intent," as one may term the vision that animated CalCOFI and other Pacific projects in the late 1940s, we can see how it became a permanent part of the program design for the next four decades. This widely shared understanding that the Pacific Ocean required study in its entire scope — that fishery problems could with great profit be intensively studied in relatively small regions, but that the natural variables affecting abundance and condition of such regions might be located only through study of vast areas — has given impetus to the elaborate coordination of far-flung projects, both American and international, that produced the vastly more complete empirical portrait of the Pacific Ocean system that has been achieved in the last forty years (see Miles et al. 1982).

The Ecological Vision

The more timeless element of the new vision associated with the sardine project's design related, however, to the fundamental conception of the ocean science enterprise: it was an ecological vision, and it departed radically from the prevailing mode of twentieth-century ocean fisheries research, and indeed ocean science generally, especially in America. It was a return, in effect, to the older tradition of studies exemplified by Hjort and others who had sought to integrate fisheries management and marine biology with broadly conceived environmental research.

Again, the archival records reveal a scientific vision set forth with remarkable clarity and prescience. An exemplary document in these records, though by no means the only one that might be singled out for citation, is a statement of the research design first prepared by Roger Revelle in late 1947. He contended for a new conceptual framework of biological study in relation to ecosystems — "to make dynamic analyses . . . of the processes in the sea, that is, the cause and effect relationships which affect sardine production. . . ."[20]

"In the past," Revelle continued,[21]

> oceanographic research has been concerned primarily with the description of *average* conditions prevailing in the sea. The investigation upon which we are about to embark poses a new and more difficult problem, that is, of studying the nature and causes of *variations* from the average conditions.[22] The present is a good time to start such an investigation, because obviously we are in a pe-

> riod of major departure from the average conditions, at least insofar as the distribution of the sardine population is concerned.
>
> In attacking a problem of such magnitude all possible scientific tools and methods will have to be employed. It will be necessary first to describe as completely as possible the existing oceanographic and biological situations; second to establish empirical statistical correlations between the various environmental and biological factors; and third and most important, to make dynamic analyses where possible of the processes in the sea, that is, the cause and effect relationships which affect sardine production. Wherever such a dynamical analysis of a particular aspect of the problem can be made, a great saving in time required for a solution will be effected over the "brute force" method of statistical correlation which requires a long series of observations for validity. . . .
>
> The sardines cannot be treated as isolated organisms living in a vacuum. The investigation must be an integrated one in which proper weight is given not only to the currents and other aspects of the physical environment but also to the entire organic assemblage including the plants and animals which form the food chain of the sardines, their competitors for the food supply, and the predators, including man. . . .

The vision that Revelle set forth entailed, in sum, interdisciplinary research in a holistic mode: its focus was to be the ecosystem. As had already been learned from experience in the earth sciences, he wrote, "far more productive results were obtained by complete analysis of all the factors which exist in a particular situation than by a statistical treatment of a few factors in many situations."[23] Similarly, Sette of the USFWS had written that to study the sardine dynamics properly in relation to ecosystemic change, it would be necessary "to set up a program on a basis that will cover much more of the sea area along the Pacific Coast [than had previously been studied] and will run through enough years to establish the average conditions and discover what effect the deviations from the average condition have on the recruitment and availability."[24]

Thus Revelle's presentation in 1947 set forth a precise and unambiguous agenda for research in a "particular situation," the California Current. (And in this respect it described exactly the mode of research that would actually be pursued by MRC and CalCOFI for four decades.) But the ecological vision that he expressed also had a "subversive" side, as good science and interpretive theory in other fields of study usually do: this subversiveness was to be found in the implication that the ecosystem, and not merely the sardine dynamics as one part of that system, was the truly interesting and enduringly important subject of inquiry.

That message was not lost on the sardine industry cosponsors of CalCOFI, who became painfully aware that "their" problem was becoming part of an ever-ramifying scientific enterprise that was coming to focus upon ecological systems. The fishing and cannery interests had to be reassured, on many occasions, that basic research on a broad conceptual basis would eventually produce important practical results.[25]

It was vital that the sardine project succeed, Chapman declared, because it had been "log-rolled through by a small group of far-sighted men in the industry [who were] far ahead of the main body of the sardine industry in their thinking," and if the work succeeded in producing results, the whole industry would fall into line; if it failed, the "diehards" would prevail and "our work is very apt to be set back for a generation."[26]

The subversive content of the new vision was not lost, either, on the applied fisheries management scientists — especially Frances Clark, who at times expressed deep frustration with the way that analysis of complex systems could divert attention from the intense commercial fishing effort that she believed to be the real culprit of the sardine-depletion piece, whatever the other variables and their subsidiary effects (McEvoy and Scheiber 1984; McEvoy 1986).

The subversive side of the ecological vision notwithstanding, its constructive side would have a profound influence upon the direction — and the most renowned achievements — of ocean science in the ensuing decades. Beyond that, however, this holistic or ecosystemic approach that was encapsulated by Revelle in 1947 and eloquently endorsed and later imaginatively pursued by others — Chapman, Schaefer, Marr, Clark, Walford, Ahlstrom, Murphy, and other intellectual leaders of the CalCOFI enterprise and related Pacific studies of that era — was the precursor of the more comprehensive movement in modern science toward ecosystemic research designs. The major shift toward such holistic analysis of systems would occur only in the 1960s, amid the new political concern for "environmentalism," when it was also reflected in the reconceptualization of the public policy approach to environmental monitoring, regulation, and risk assessment (see Fleming 1971).

Fully fifteen years earlier than that, however, in the late forties, the ecosystemic concept and the research designs it inspired became one of the truly glittering achievements of the CalCOFI program — essential to its foundations from the outset, and manifest thereafter in the pursuit of California Current studies.

CALCOFI'S EARLY PROJECTS AND THE EMERGENCE OF A NEW FISHERIES OCEANOGRAPHY

CalCOFI celebrated its fortieth anniversary in 1989, but strictly speaking the project dates from the formation in 1948 of the Marine Research Committee (MRC), under terms of the legislation of the previous year. The actual research supervised by the MRC was set in motion by the Technical Committee (composed of four scientists charged by MRC to oversee the work at sea) in the early weeks of 1948.[27] The research was inaugurated in February with a hastily organized voyage into waters south from the SIO pier, to make some quick visual observations of sardine movements and (it was hoped) pick up a few samples in the offshore waters from San Diego south to Punta Abreojos. This mission was conducted by the *E. W. Scripps*, the heroic little wooden research schooner that had been the main reliance of the Scripps Institution scientists in their upwelling and other high-seas studies in the 1930s.[28] The portrait of ocean phenomena that we can now obtain from space satellites and the advanced gear of modern oceanographic vessels reminds us how far the study of the oceans has moved, conceptually and technologically, in forty years. That little ship beating down the coast, in its 1948 quest to locate some sardine runs, was — in its conception, in its gear and instrumentation, and in the limits of what it might hope to accomplish — much more akin in many respects to the exploration and science associated with Captain Cook's voyages than to the oceanographic studies of our own day.

The California Fish and Game research ship *N. B. Scofield* followed soon after, with an April voyage in quest of evidence of sardine stocks off the Baja California coast. Meanwhile the SIO scientists and navy personnel worked at a frantic pace to modify and outfit the two former war vessels that had been turned over to the University of California for SIO's research at sea.[29]

A formal agenda was set out at the April 1948 meeting of the MRC, at which Dr. Robert Miller, chairman of the Technical Committee, presented a six-part program that included the following lines of research:

> 1. Physical-chemical conditions in the sea [assigned to SIO]. 2. Organic productivity of the sea and its utilization [also to SIO]. 3. Spawning, survival, and recruitment of sardines [assigned to the federal Bureau of Commercial Fisheries]. 4. Availability of the stock to the fishermen (behavior of the fish as it affects the catch) — abundance, distribution, migration, behavior [assigned to the California Fish & Game Division]. 5. Fishing methods in relation to availability [also to California F&G]. 6. Dynamics of the sardine population and fishery [a shared research area, for all participating agencies].[30]

The scope of the plans, and also the way in which they reflected a comprehensive view of the sardine population's dynamics, indicated a substantial increase in personnel and gear as well as ships. In the months that followed, John Marr of the federal agency coordinated planning with SIO and the state fishing management scientists to deploy the new SIO ships, a refitted federal vessel (*Black Douglas*), and the *N. B. Scofield* (also, later, another California state vessel, *Yellowfin*, a refitted naval ship). The plan that emerged from the talks called for observation stations across a grid that went 400 miles off the coast, with probes for collecting nutrients and other materials at depths of nearly 3,000 feet. Expenditures by MRC, from the sardine tax revenues, included $50,000 to the U.S. agency for oceanographic work, $25,000 for its egg and larvae studies, and additional sums for gear and personnel for the other agencies.[31]

The SIO leadership sought out additional laboratory personnel to process samples as they came in from the research vessels, and new oceanographic gear (bathythermographs, barographs, plankton nets, flowmeters, high-speed collectors, sonar devices, etc.) was purchased with the funds from the sardine tax.[32] There soon emerged a lively competition for trained personnel among POFI in Hawaii, the new MRC projects, and other Pacific research centers (especially the salmon research center at Seattle); SIO began training people for specific positions available on several of the newly expanded projects (Scheiber 1986).

Thus was set in place the extraordinary station plan of the sardine research program under MRC. The station plan was the heart of the continuing CalCOFI studies and their extended longitudinal data series, covering an area of some 670,000 square miles (figure 2). The program provided for detailed sampling and testing of ocean water to determine hydrographic conditions. Samples were collected to analyze chemical and physical properties of the waters; the volume and composition of nutriments; and larvae, juveniles, and adult fish (Ahlstrom 1950). The oceanographic sampling program and also the larval and egg sampling cruises were initially conducted monthly at 122 stations over the vast California Current research area; they continued on this basis for over a decade, until they were reduced to a pattern of quarterly cruises, albeit over a more extended ocean area (Wooster 1949; Murphy 1960).

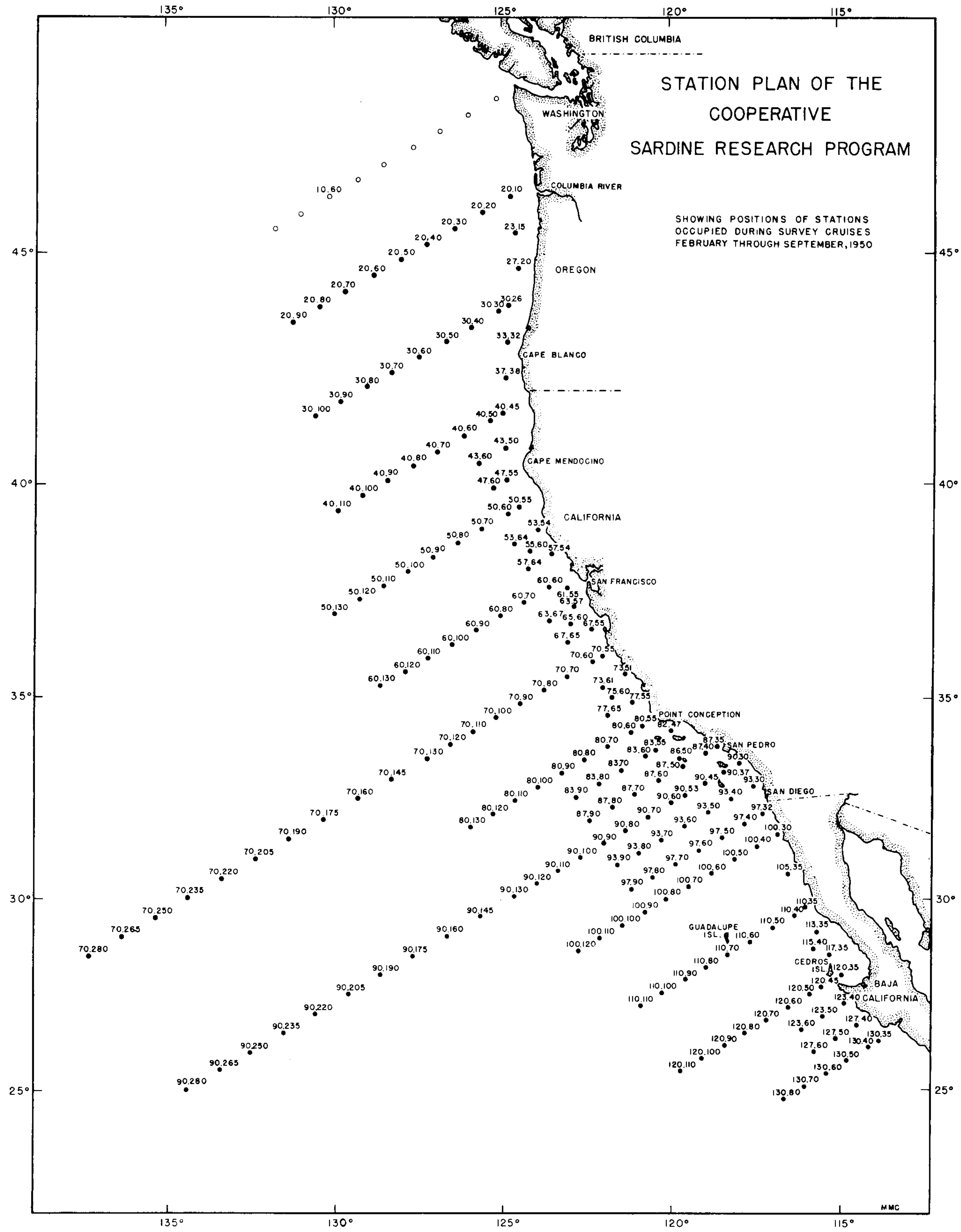


Figure 2. The CalCOFI station plan used for cruises in 1950. The numbering system was planned so that the station lines were 120 miles apart, and individual stations were 40 miles apart. Extra stations were added in regions of particular interest for sardine work. (See inside back cover for basic plan.)

The extent of startling changes in scope, complexity, and scale in California marine research doubtless helped to prompt the June 1948 reflections of Carl Eckart, who succeeded Sverdrup as SIO director that year: "The individual scientist, working in seclusion," Eckart declared, "is apparently a thing of the past." Although he was uncertain that this was "going to be good for science," Eckhart thought it was necessary (if it were to work at all) that the new research projects "be led by people who have a comprehension of the past."[33]

Given the deep and continuing involvement, and leadership, as the sardine project ramified, of distinguished fisheries scholars and oceanographers who had already excelled in highly individual research, it appears in retrospect that the success of this experiment in Big Science-mode organization and coordination was built on exactly the critical foundation that Eckart prescribed. There is no gainsaying, however, that "seclusion" as a way of scientific life had been discarded. Indeed, nearly every great accomplishment in MRC-CalCOFI research in this era reflected the intricate collaborations of agencies and institutions, the crossing of disciplinary lines, and the cooperative relationships that developed between the scientists at sea and the laboratories to which their findings were sent for analysis. It was, quintessentially, what Eckart termed the new-style "organized scientific effort."

An Expanded Design and a New Name

With the cooperative deployment in 1948 of ships and scientific personnel, financed by a mingling of agency and University of California funds, the Marine Research Committee's sardine project was fully under way. The project moved forward, however, under the dark clouds of continuing crisis in the sardine industry, as the decline in catch continued. Indeed, the state marine fisheries scientists, while lending the full weight of their efforts to the new research, were at the same time pushing hard for authority to strictly limit commercial fishing for sardine and to close several ocean areas.[34]

Aware of the political controversies about management decisions underlying the sardine project's start-up, Revelle (then returned from the navy and serving as associate director of SIO) opposed delaying the grid cruises for any reason. When Marr, the USFWS representative, suggested that more time was needed to outfit the ships they had assigned to cruise the northern part of the grid, Revelle replied that "financial and political reasons" alike made it "almost essential that we should start the first cruises with our own [SIO] ships as soon as they are ready for sea."[35] This latter view prevailed, so in late February, *Crest, Horizon,* and the state vessel *N. B. Scofield* began the work, with the *N. B. Scofield* newly equipped with hydrographic and plankton collection gear, bathythermographs, echo sounding equipment, and other instrumentation from SIO, as well as high-speed collectors, standard plankton nets, and flowmeters from the federal agency.[36]

It is worth noting that within the year the state sardine tax funds administered by MRC were also being used for some gear and operating expenses of the USFWS cruises that were augmenting the California project with surveys off the coasts of Oregon and Washington — perhaps the first such instance in American governmental history of significant grant-in-aid money flowing against the established currents, i.e., from the state to the national government, rather than from Washington to the state.[37]

The sardine project initiated by the MRC, according to a Los Angeles *Times* account based on a publicity release from SIO in June 1949, had quickly become the "biggest fish hunt in history." Revelle termed the project, in the same report, a "fine-tooth combing of coastal waters," asserting that it indicated that "the State has decided that it is almost as important to develop and conserve our sea food resources as it is to develop agriculture." Symptomatic of the larger agenda that by then was explicitly emerging, however, was Revelle's further observation concerning the longer-run objectives that could be realized through the new project: "The outcome of the all-out sardine research," he declared, "is vital to thousands, but *our ultimate aim is to obtain scientific data without which we can't hope to assure a maximum sustained yield of food from the ocean*" (Los Angeles Times, 1949, italics added).

By mid-1949 the larger contours of the project were fully etched: they included the more comprehensive dimensions of the research in the realm of pure science, subsuming and in some respects beginning to overshadow the applied fisheries management concern. The state agency, whose scientists throughout the entire early history of the MRC effort were embroiled in controversies over whether to halt sardine fishing as a way of stemming the precipitous decline of the resource, expanded their traditional agenda of studies that were based on standard harvest-yield and input data.[38] Croker, Clark, and the agency scientific staff had long pursued recruitment research, including work on distribution and methods of conducting census surveys of the nursery grounds, and they had continued to measure abundance by using bait-fishery statistics. Aided by the MRC initiative, the agency had also mapped out additional exploratory cruises on the fishing grounds, designed to produce data on "correlation of physical and biological oceanographic conditions with sardine distribution" and to improve methods of locating the stocks, studying school habits and other behavior, and studying "relations to other species (mixing of schools)."[39]

In the case of the federal agency—reflecting the approach to larvae and egg research pioneered by Ahlstrom and Sette before the war and then continued under Sette, Ahlstrom, Walford, and Marr during the late 1940s—the initial cruises in the northern range for the MRC sardine project demonstrated even more immediately how the scientists' agenda went well beyond the narrow issues of sardine management. Thus the 1949 progress report of the federal scientists on the sardine project (which the agency subsumed under the title Expanded Pilchard Research Program, within the framework of its South Pacific Fishery Investigations then headquartered at Stanford University) highlighted the issue of possible interspecific competition on the sardine grounds. Analysis of the plankton and egg samples, the agency reported, indicated that anchovy larvae were distributed in roughly the same areas as sardine, and that an abundance of hake and jack mackerel had also been found in areas where their presence had not previously been recognized.[40] Predation on the sardines was suggested as a factor limiting their population, and in any event, the report continued, "the other fishes may be competitors for food and space" with the sardine. If any single theme was hammered home, it was that of the ramifying scope of the research inquiry:

> In addition, valuable data are being gathered on fishes which are of great importance in the economy of the sea, although not of direct commercial importance. It is becoming increasingly self-evident that the biological, chemical, and physical studies being carried out with immediate reference to the sardine problem will be of tremendous value to the study of many other fishes.[41]

Similarly, in forwarding a report by SIO on its cooperative role in the MRC project during the 1949 cruises, Revelle scarcely mentioned the sardine.[42] Again, he emphasized instead the ramifications of the early research findings for basic oceanography and marine biology, and not the pressing applied (and highly politicized) issue of sardine fishery regulation. The findings in the past year's work, Revelle declared, indicated the desirability of exploring the hypothesis that "to a large extent the ocean is the slave of the wind and that if we gain an understanding of the dynamics of the atmosphere off the West Coast of North America we will learn much about the regime of ocean currents and temperatures." Other major issues that were suggested by the studies completed to that date, Revelle stated, were

> The use of zooplankton as indicators of water masses and diffusion.
>
> The development of methods of collecting post-larval stages of a variety of pelagic fishes.
>
> Studies of the role of oceanic birds as pelagic fish predators.
>
> The unexpectedly large populations of many species of pelagic fish other than sardines in off-shore sub-surface waters.
>
> The discovery that electrical signals can be sent up or down uninsulated hydrographic cable . . . [raising] the possibility of almost revolutionary developments of methods for continuously measuring sub-surface temperatures and other variables from equipment towed beneath the surface at normal cruising speeds.[43]

Over the course of the period ending in mid-1950, the broad categories of research that would be pursued over nearly a decade had become well established. As summarized in a report by Ahlstrom (1950), the SIO vessels gathered data in several major areas of investigation. The first was physical oceanography, including studies of upwelling, transport of water, chemistry of the water, and "the causal mechanisms behind the circulation the ocean." Second was the study of phytoplankton, concerned with evaluating the "crop" of marine plants and particularly "the relation of fluctuations in the productivity of marine plants to physical and chemical processes in the ocean . . . and the effect . . . on the animal populations." Third was the study of zooplankton, especially its effect on survival rates of larval and adult sardines. Fourth were marine vertebrate studies, conducted in close collaboration with the U.S. Fish and Wildlife Service scientists, especially in pursuit of what seemed a promising and dramatic possible breakthrough in understanding the dynamics of relations between sardine stocks and other species, especially the anchovy and possibly the saury. As Frances Clark of the state agency rather dolefully observed at about the time of this summary report: "Scripps is doing the new and spectacular and appears to get a lot of praise and glory."[44] (See also Pan-American Fisherman 1950).

By contrast, it was the state Fish & Game Division scientists who were tied down to what Clark termed "the routine drudgery without much glory," work in which "no one is interested . . . and it is without publicity value."[45] The state vessels and scientists continued to pursue research on the lines that had been pioneered by their agency since the 1930s, studying distribution, harvest statistics, and samples that reflected survival of year classes. Despite the lack of publicity that irritated Clark, the agency was also contributing importantly to the grid station program and the building data base of synoptic oceanographic and biological data (Ahlstrom 1950).

The South Pacific Fishery Investigations scientists continued, through cruises and in their own and SIO laboratories, to explore the environmental relationships manifested in the research on sardine recruitment and survival that had been highlighted in their earlier report. Other, smaller, elements of the sardine research program under MRC included a small-scale bench project at the California Academy of Sciences that involved experiments with sardine schooling behavior, and correlation and analysis by all the cooperating agencies of the commercial catch statistics that were being generated by government resource-management agencies in Oregon, Washington, and British Columbia, as well as by the USFWS and the California state agency (Ahlstrom et al. 1950).

This 1950 report indicates how far the orientation and guiding vision of the program had gone beyond the sardine management issue by its emphasis on more general phenomena of the oceans. Upwelling received full discussion, and there was extensive analysis of food supply and food chains, the relationship of nutrient supply to intraspecific and interspecific competition, and the possibilities of mortality associated with disease-producing organisms as well as predation by competing species in fishery populations. The report declared the emerging character of the sardine research program to be

> studying the sardine in its environment in order to understand how this environment — physical, chemical and biological — affects the survival of the sardines when young and their distribution (availability) when they are of commercial size. . . . *We are studying the sardine 'at home.'* . . . To date, little more than a good beginning has been made on the study of environmental conditions. Yet it is rather certain that before we can hope to predict fluctuations in abundance of the sardine fishery *we must first investigate the environment thoroughly enough to understand the effects of physical and biological processes on the sardine population.*[46]

The ethos that by then pervaded the leadership's conceptions of the broad direction and ramifying significance of the research was expressed in correspondence among the project scientists in 1949 regarding a name for their program. John Marr, who had become chief of the South Pacific Fishery Investigations of the federal agency, proposed "Cooperative Sardine Research Program," a title that fitted nicely into the bureaucratic niche existing in his agency in the form of its Pilchard Research Program budget category. Responding for the state agency, Frances Clark suggested "Cooperative Marine Research Program," which had the advantage that it "does leave the way open for tying the work in with other fisheries" (though she added, "This may or may not be an advantage"). Revelle carried the day, suggesting on behalf of SIO that he would "favor something a little more comprehensive" than the title Marr had put forward; hence he suggested "Cooperative California Fisheries Research Program."[47] Soon afterward, the name that was to become permanent and universally referred to by the acronym CalCOFI began to appear on the project's publications.[48]

Throughout the early years of CalCOFI research, the publicity releases prepared by the University of California and other agencies stressed, as did Ahlstrom and Hubbs in reviewing a 1952 public relations statement for radio use, that "although this is a sardine investigation, the investigation is contributing to a better knowledge of all the fisheries and to a much better understanding of the ocean itself."[49] Similarly, the publicity efforts underlined that the ramifying implications for ocean science *methodology* were also of key significance. Indeed, the radio broadcast release stated that, whatever the fate of California's sardine population and the sardine fishing industry, "perhaps in the long run, the most significant thing about the sardine investigation is that it's demonstrating the feasibility of large-scale fisheries research."[50]

The rapid development of an ecosystemic approach to research issues and to the actual design of the MRC-CalCOFI program did not entirely dominate the project's history in the earliest years. As mentioned already, a major theme — really part of the contextual fabric — was the continuing tension in the larger arena of state politics, centering on whether or not strict management controls — even suspension altogether of commercial sardine fishing — should be imposed on the industry. McEvoy (1986) has argued that the scientists at SIO and USFWS in effect willingly ran interference for the industry, which was heavily aligned against the cause of regulation. An extension and continually ramifying expansion of the research project, he contends, was entirely congenial to scientists who consequently enjoyed unprecedented funding for basic research; and it worked to frustrate the intentions of the California Fish & Game Division marine scientists, who firmly believed that overfishing, whether alone or in conjunction with other forces, was the instrumental factor in the sardine's decline and possible imminent disappearance.

It seems to this writer that a somewhat different scenario was being played out — that the basic oceanographers and federal agency scientists consistently regarded it as their role to generate good data, work out the best possible research design, pursue the leads that scientific judgment suggested (however they might ramify the work), and let the political branches of government decide about regulation. To allow MRC or CalCOFI to be split apart by differences on an explosive policy issue would be to sacrifice the harmony and possibly the survival of a precious and productive scientific enterprise. In other words, the separation of science from tough policy decisions — a luxury the Fish & Game Agency was not afforded — was more a natural concomitant of the type of research CalCOFI was undertaking than a matter of the science fraternity's cynicism or something even more sinister.[51]

A coordinate theme, as the environmental and ecosystemic vision came to dominate CalCOFI design, was the building up of a record of concrete accomplishment in science — the cumulative body of research that within a decade after the MRC founding had made the California Current probably the most intensively studied marine fishery area in the world.[52]

The Research Achievements, a Data Glut, and CalCOFI Reorganization

Summarizing what MRC and CalCOFI had achieved up to 1957, John Isaacs of SIO, John Marr of USFWS, and John Radovich of the California Department of Fish and Game categorized the major research accomplishments as follows. First, sardine spawning grounds had been identified over a much wider area of the California and Baja California offshore region than had previously been recognized.[53] Second, annual estimates had been made since 1950 of the number of fish spawning in each of the four major areas; eggs and larvae had been estimated annually, as had "the abundance, distribution, and age composition of juveniles and adults on the inshore nursery grounds." Third, the numbers of adult sardines had been estimated annually since 1952. Fourth, studies had been made on various aspects of spawning, mortality, north-south migration patterns, and schooling habits of sardine. Fifth, in the studies of nutrients, the project leaders had concluded that the presence of phosphate and other nutrients did not appear to be a factor limiting phytoplankton in the region. In the traditional areas of oceanographic study, the cruises had produced an uninterrupted time series (which, of course, would be continued for five more years on the original grid pattern) on temperature, salinity, currents, and other variants.[54]

To this list of accomplishments should have been added the remarkable breakthroughs in geology that came out of a notable exploratory cruise program in 1952–54. The SIO ships cooperated with the *Charles H. Gilbert* of the POFI project to investigate the waters between Hawaii and the eastern Pacific. In addition to locating rich new areas for tuna fishing, the cruises made important discoveries about the seabed configuration east of Hawaii (Sette 1952, 1955).

The last finding that was summarized in the 1957 report — that "information on the identity, location, and abundance of the eggs and larvae of many species, including the anchovy, jack mackerel, Pacific mackerel, saury, and hake, [had] been obtained annually since 1950" — proved to be of truly determinative significance for future CalCOFI research. As Ahlstrom wrote (1964) concerning these early years of data collection: "It was a fortunate circumstance that the sardine was found to have a wide areal distribution and an extended spawning season." The breadth of distribution was discovered virtually from the outset of the MRC-CalCOFI cruises, and the findings indicated a great extent of range and the length of spawning season. These findings in turn prompted the investigators "to look at large chunks of the California Current system off California and Baja California rather continuously" (Ahlstrom 1964).

Because the investigations had been carried into so vast an area of the deepwater Pacific, and because the nets had brought up massive determinative evidence that the anchovy and sardine populated the same regions (and evidence also that there were other species, especially hake and mackerel, that must interact in some ways with the sardine, their food supply, and their activities in the larger physical environment) two things followed. First, the researchers were led more and more deeply into interspecific dynamics, a direction of study that would within a few years lead to conclusions on anchovy-sardine competition that would dominate CalCOFI science for a long time. And second, the scientists and their agencies were inspired to grapple with the mysteries of the more comprehensive systems of ocean ecology in ways that greatly transcended narrow concerns with the sardine.

These developments were reflected in the formal statements of program objective that the MRC occasionally adopted during the 1950s and early 1960s.

In the 1950 CalCOFI progress report, the program was summarized as one "to seek out the underlying principles that govern the Pacific sardine's behavior, availability, and total abundance" (quoted in Murphy 1960). In 1954 the objectives had been broadened "to include the . . . mackerel, jack mackerel, and anchovy." The program objective was stated still more comprehensively in 1957, as determination of "what controls variations in populations, size and availability off the west coast of North America of sardines and, as their scientific and industrial importance requires, of anchovy, jack mackerel, Pacific mackerel, herring, squid, and others"[55] (See table 1).

Accurately reflecting the move into ramifying, comprehensive collection of ecosystem data across all the ocean science disciplines, CalCOFI adopted an even broader definition of its objectives in 1961 (Murphy 1963):

> To acquire knowledge and understanding of the factors governing the abundance, distribution, and variation of the pelagic marine fishes. The oceanographic and biological factors affecting the sardine and its ecological associates in the California Current System will be given emphasis. It is the ultimate aim of the investigations to obtain an understanding sufficient to predict, thus permitting efficient utilization of the species, and perhaps manipulation of the population.

After nearly a full decade of CalCOFI research, however, it had become painfully evident that *ramification* of the research, the expansion of studies into comprehensive investigations of the ecosystem, was something very different from *integration*. One very troubling issue was the continuing uncertainty as to the causes of the sardine decline; in 1957, even after this once-great fishery had nearly disappeared, the MRC was still declaring formally that explanation of this "catastrophic decline" must remain a top research priority. Whatever the brilliant achievements of MRC and CalCOFI research up to that time and afterward (including the pioneering studies of anchovy-sardine interspecific dynamics that would be published in the early 1960s), the record was made against the background of unchecked disaster for the California sardine resource.

TABLE 1

Marine Research Committee (CalCOFI) Revenues and Expenditures by Agency, 1947–64

1. Total revenues:	$1,738,718 adjusted
2. Expenditures, by agency:	
MRC committee operating expenses	$104,439
MRC program coordination	11,141
Grants to:	
U.S. Fish and Wildlife Service	598,430
Scripps Institution of Oceanography	75,737
California Division of Fish & Game	198,062
Hopkins Marine Station (Stanford Univ.)	85,838
California Academy of Sciences	125,890
3. Total expenditures by type of investigation:	$1,790,261 adjusted
Sardines	$915,564
Mackerel	582,971
Anchovies	219,310
Herring	11,381
Squid	61,035

Source: Financial Record of Marine Research Committee, document dated Aug. 7, 1964, in MRC Minutes, SIO Archives.

The second major area of unresolved work that was identified by MRC scientists in 1957 was in descriptive oceanographic studies. Many thousands of days had been spent at sea; the shore laboratories were staffed at levels which, however inadequate for the data that was coming in, were unprecedented in West Coast biological and oceanographic study; and the lack of very significant year-to-year variation in weather and oceanographic conditions during the entire period 1947–56 had persuaded the CalCOFI leadership to maintain the intensive level of repetitive studies at the grid stations, in lieu of shifting to a more selective sampling approach (Murphy 1963; Ahlstrom 1964). Yet the volume of chemical, physical, and biological sampling was far outstripping the capacity of the shore labs to process the data.[58]

By 1957–60 the CalCOFI program therefore was in serious danger of sinking of its own weight. The scientific vision that had pushed the project into ramifying aspects of Pacific Ocean–wide meteorology and geology, and that had also generated the vast volume of accumulating planktonic, physical, and chemical samples at the La Jolla laboratories was now recognized as militating against effective analysis of the relationships in the marine ecosystem. The best minds on the project were agreed on what must be done: Sette, for example, declared flatly that the project must pause and shift from collection to analysis, with priority to explaining the sardine collapse. The primary task, Sette declared, must be that of "connecting up what has happened in the realm of physics, chemistry and planktonic life in the sea with what has happened to the abundance, distribution, reproduction and mortality of the sardine." Donald McKernan, director of the federal Bureau of Commercial Fisheries, also pressed the MRC to shift from comprehensive collection of descriptive oceanographic data to an emphasis on analysis. Future research should be "intensified," not ramified, he argued, and should be "guided toward a study of the inner workings of the ocean-atmosphere 'engine'."[60]

Concern for better focus and emphasis on developing new hypotheses — and effective explanatory interpretations of the data — translated, predictably enough, into a call for organizational reform. The Isaacs-Marr-Radovich report urged such a course, recommending a reduction of the MRC's continuing oversight, with CalCOFI "leadership, direction, responsibility and authority" to be placed in a four-person committee of representatives from SIO, the state agency, the USFWS, and MRC. Even more important, however, the proposed MRC "representative" should be a "broad and practical senior scientist" who would actively coordinate all the CalCOFI research and serve "as an integrative force."[61]

The idea of having a senior scientist play the key role of proactive coordinator — an effectively supervisory role, representing the MRC, but with a professional commitment above all to the integrity of the scientific enterprise — was an old one in CalCOFI. Indeed, at the very beginning of the project effort, Chapman had wanted such a scientist-coordinator position to be integral, but the few industry supporters of the research plan would not support the appointment of a coordinator, fearing they would entirely lose their influence on the direction (and perhaps the content as well) of the research.[62] With the crisis that CalCOFI faced from an awesome data backlog, and with the sardine problem still unresolved even as a matter of theory-after-the-depletion, it was decided that a coordinator must be hired "at whatever cost" to facilitate more effective coordination and move the project forward on the lines Sette, McKernan, and the project scientists themselves now wanted.[63]

Thus after much political jockeying and further pressure from the senior agency representatives, the MRC moved in November 1958 to appoint Garth Murphy as the first CalCOFI coordinator. Having himself authored important sardine studies under the auspices of CalCOFI, Murphy was equally attuned to the applied management mission of the project and to the larger ecosystemic vision that had moved the project since its outset. Under his direction CalCOFI budget, administration, and allocation of scientific priorities were put in tighter shape, and he apparently enjoyed the confident backing of the key MRC members to whom he (and CalCOFI) reported.[64]

Maintaining Momentum and Providing a New Focus: 1958–64

If giving new impetus to solving the sardine "mystery" and more effectively integrating the approach to ecosystem analysis was the coordinator's dual mandate, Murphy could point to a large measure of success within five years of his appointment. By the mid-1960s, CalCOFI research had come to a strong, if highly controversial, focus upon the anchovy-sardine relationship and its implications for fisheries management in the California Current.

Meanwhile, however, both through continuing MRC financial support and the larger influence of the now-traditional CalCOFI ecosystem research agenda, SIO and marine fisheries studies generally in the Pacific continued to examine the fishery stocks in relation to the relevant ocean environment. The legacy of CalCOFI, in this respect, carried over into the important studies of fishery dynamics and management conducted under Schaefer's direction by the Inter-American Tropical Tuna Commission in the 1950s and 1960s (Barrett 1980). The legacy also carried over into the era's larger, truly international web of complementary and coordinated research projects on ocean fisheries and environment — projects that included the NORPAC, EPOC, and International North Pacific Fisheries Convention studies (see Miles et al. 1982).

A shift back to the more focused applied-management issue had become evident in CalCOFI discussions even before Murphy was named coordinator. Industry representatives on the MRC, most notably Chapman, had of course long pushed for such an emphasis. But the chief proponent in the working science group became John Isaacs of SIO, who as early as 1959 authored a "Proposed Program in Fisheries Research" for CalCOFI consideration. In this document, Isaacs proposed applying knowledge from data already gathered on the sardine to analyze more universal dynamics, focusing on hake, anchovy, saury, squid, jack mackerel, and Pacific mackerel. The resulting theories should be used for what Isaacs termed "sophisticated experiments (quite unlike the conventional management)" involving interventions through commercial fisheries to reduce target stocks that had preyed on other species or competed for their food. Such interventions would amount to an outright "alteration of the fish population toward the composition of preferred sport and commercial species." In this view, the commercial fishery was a tool to be used for the elaborate and comprehensive bioengineering of the California Current (Isaacs 1959).

If the vision reached far ahead of both the data and the available theory in 1959, it was not long before the idea of interventionist management in such a mode resurfaced in CalCOFI discussion. This time the new coordinator, Murphy, along with Isaacs and Ahlstrom, took the lead. Ahlstrom's egg and larval surveys had revealed a dramatic increase in the anchovy population, occurring synchronously with the sardine's critical decline in the waters they shared. The anchovy-sardine ratio in the larval collections, Ahlstrom reported in 1964, had risen from 3.9:1 in 1952 to 16:1 in 1957, 23.6:1 in 1958, and 46.8:1 in 1959. (See also Ahlstrom 1963.) Pointing at the obvious policy conclusion — that purposeful reduction of the anchovy might relieve stress on the sardine — Ahlstrom (1964) made a rather imprecise yet telling suggestion: "Until now," he wrote,

> we have been in the role of observers. We have been watching what has been happening in the ocean. Whether we can successfully be participants, shaping the course of the events, remains to be seen. Certainly the latter has been one of the prime objectives of ocean research.

The full policy implications were left to be spelled out by Murphy and Isaacs (1964), who explicitly stated to the MRC what had been left unspoken in Ahlstrom's presentation: that, since the oceanic regime had come to favor the anchovy (in some relationship to the selective, intensive fishery for sardine), a new regime of unselective "trophic level harvesting rather than selective harvest within a trophic level" could serve to redress the situation. Research findings suggested, they went on,

> that the process is reversible, either by a protracted period of years in which the environment clearly favors the sardines and/or by re-deploying man's effect on the community in such a way as to favor the sardine. The practicality of this depends on more definite knowledge of the exact ways in which the two species interact.

On this foundation a proposal that a managed fishery for anchovy should be initiated was quickly built and adopted by the MRC. This became the "great experiment" idea, one that roiled the political waters within the MRC despite the CalCOFI scientists' apparent consensus that their data and judgments warranted it. The idea also proved controversial in fishery policy circles, even in some highly respectable quarters in fishery science, and certainly in the political arena at Sacramento. The sudden rise of the fabled, if short-lived, anchovy fishery in Peru cast serious doubt over the economic feasibility of the proposal, undermining whatever slender political chances may have remained for it.[65]

The anchovy harvest proposal provided a short-term focus, causing endless trouble in the MRC and perhaps exposing CalCOFI to the kind of treacherous political crosscurrents that the project's scientific leaders had long feared would result from excessively detailed concern with applied management issues. But all the while, the participating agencies carried on the broader mission of ecosystemic research, in some respects continuing the tradition of collecting and ramifying data while in other respects pioneering new scientific techniques and developing new theory for the Pacific Ocean system and marine systems more generally.

In the ensuing period of CalCOFI history, which is beyond the scope of this paper, these multiple lines of study, and efforts to integrate them into modern marine science theory, have constituted one element of the MRC-CalCOFI legacy. In the last forty years interdisciplinary studies, taking the entire marine system as their ultimate subject, have become the standard in marine research; and the new "fisheries oceanography" has come to dominate the analysis of ocean fauna and their environments. These two developments in scientific method and research constitute the most enduring legacy of CalCOFI's founding vision and four decades of California Current science.

ACKNOWLEDGMENTS

The author is indebted to Berta Schweinberger, J.D., and Barbara Leibhardt, M.A., Ph.D., former Sea Grant trainees at UC Berkeley, for their assistance; and to Arthur McEvoy, associate professor of history, Northwestern University, whose doctoral dissertation the author directed and whose scholarship, both his own and that done in collaboration with the author, on California marine science and policy, is indispensable background to this work and is cited in the references. The author also extends warm thanks to Deborah Day and others of the Scripps Institution of Oceanography Archives, Janet Ness and others of the University of Washington Library Manuscript Division, and Gary Lundell of the University of Washington Archives for their generous advice on research problems and their indefatigable assistance in use of archival materials. Archivists of the National Archives in Washington, and of the California Academy of Sciences, San Francisco, also extended very cordial assistance.

My colleagues William T. Burke, University of Washington; David Caron, John Dwyer, and Roger Hahn, University of California, Berkeley; and James Sullivan, California Sea Grant College Program, all are owed thanks for their sustained (and sustaining) interest and suggestions. John Marr, USFWS (ret.) graciously provided a careful critical reading. Responsibility for any errors is, however, entirely my own.

This paper is based largely upon research supported by grants from the California Sea Grant College Program (project no. R/MA-30, grant no. NA85AA-D-SG140), with funding from the U.S.

Department of Commerce (NOAA), and from the State of California, to the Program in Ocean Law and Policy, Center for the Study of Law and Society, UC Berkeley. This funding was augmented by the California Policy Seminar, University of California, and Boalt Hall School of Law, UC Berkeley.

NOTES

1. O. E. Sette, director of the USFWS South Pacific Fishery Investigations project based at Stanford University, was also important in developing the project design, but he was much more in the background than the others mentioned in the text, so far as the political effort in California was concerned. Prof. Revelle has said that Sverdrup, a physical scientist, himself "didn't think much about biology," but Sverdrup was certainly positive toward the idea of working closely with fisheries scientists (Revelle 1986). Moreover, Sverdrup had himself engaged in studies of upwelling that bore directly on the issue of nutriment levels and fish spawning and survival (Sverdrup 1948). He later revisited the basic problems he had explored in the late 1930s and had advanced through his entrepreneurial role in MRC's formation in 1946–48 (Sverdrup 1952).

   For detail on the history of the project during 1946–48, and especially its political context, with brief analysis of the long-term achievements, see McEvoy and Scheiber 1984 and Shor 1978. A range of significant policy issues and fisheries science of both the 1930s and the later period, especially the 1960s, are covered well in McEvoy 1986.

   Sette and Ahlstrom (1948) recount the results of prewar research involving U.S. Fish and Wildlife Service and University of California scientists on the relationship of environmental conditions in the California Current to sardine spawning.
2. Letter of M. Phister to Chapman, Dec. 29, 1950, Wilbert M. Chapman Papers, University of Washington Library (hereafter cited as UW).
3. The phrase proto-MRC was coined by Garth Murphy, in "Summation of Calcofi," manuscript report (presented before the California Marine Research Committee meeting, Balboa, Calif., April 11, 1963) in Minutes of the Marine Research Committee, Scripps Institution of Oceanography Archives, La Jolla; hereinafter cited as SIO Archives. On the factionalism of pure scientists versus managers, I have relied on Revelle 1986.
4. Letter of Chapman to Phister, Sept. 19, 1947, Chapman Papers, UW.

   Dr. Frances Clark of the California Fish & Game Division characterized the history of her agency's relations with the federal scientists as follows:

   > In our relations with Fish and Wildlife we went at it backward. Clashed, fought, and finally cooperated so that in general things are now running smoothly but we all have to be continually on our guard to avoid new clashes. Man is jealous by nature and scientists or pseudoscientists are no exception, perhaps among the worst.

   (Letter of Clark to Carl Hubbs, April 6, 1948, Carl Hubbs Papers, SIO Archives.)

   Tensions between the state and federal agencies did not disappear after the sardine project's founding. Indeed, some of the top USFWS leadership believed it was the wisest course for them to keep clear of the project and the political crosscurrents of debate over proposals for placing strict limits on the harvest of sardine—proposals that were being put forward regularly by the Fish & Game Division scientists in public forums. In early 1953, the state scientists openly conveyed their suspicion that USFWS personnel were not turning over all of their data. Bristling at the charge of "secretiveness" in hoarding data from cruises engaged in the sardine research, L. A. Walford, chief of the Branch of Fishery Biology in the federal agency, wrote to USFWS Assistant Director Kask: "I agree with you that the Service should plan an orderly retirement from sardine research, beginning immediately with the preparation and publication of findings." (Memorandum, Walford to Kask, March 2, 1953, File 80, ser. 121, USFWS Records, Record Group 22, National Archives, Washington.) The next day, however, he backed off and reported to Kask that in speaking with Don Saxby, a prominent packing industry executive in California, he "got the impression that it would be extremely difficult and probably impolitic for us to withdraw." (Memorandum, Walford to Kask, March 3, 1953, File 80, ser. 121, USFWS Records, Record Group 22, National Archives, Washington.)
5. See Shor 1978. The University of California, thanks to Revelle's ingenuity, had—as President Sproul ironically put it—acquired its very own navy overnight. Sproul did say, not without some anger, that he would have preferred to have been consulted in advance about the negotiations. But after a timely visit from a sardine industry delegation, Sproul came around and gave his retroactive blessing to the new fleet. Sverdrup-Sproul correspondence, Feb.–March 1947, Director's Files, SIO Archives.
6. Chapman to Vern Knudsen, Oct. 22, 1947, Chapman Papers, UW. For a biographical study of Chapman's long and influential career, see Scheiber 1986.
7. Chapman to Montgomery Phister, Sept. 19, 1947, Chapman Papers, UW; Francis Clark to Carl Hubbs, April 6, 1948, Subject Files: Marine Life Research, SIO Archives.
8. This project was the Pacific Oceanic Fishery Investigations (POFI); it was based in Honolulu upon its establishment in 1947–48, and it focused on tropical tuna resources of the Pacific. There were three divisions for research, the biological division (under Schaefer) being the most important; the others were technology, with a focus on preservation and processing, and fishing. The significance of POFI is discussed in Scheiber, in press, b.
9. The state of the art was exemplified in the extraordinary book by Sverdrup et al. (1942) synthesizing research to the time in oceanography, and summarizing, in the course of argument, much of the Pacific research Sverdrup and his associates had accomplished in Pacific waters. See also Scheiber 1986. For an example of thinking on agendas, see esp. O. E. Sette (1943). A classic statement of research method in the early years of modern fisheries oceanography is in W. F. Thompson 1919.
10. An early proponent of the theory that tuna were abundant in the tropical Pacific was Albert Herre, author of influential papers on fisheries of that area (see, e.g., Herre 1940). Herre's influence on American scientists' concern to explore tuna resources was a powerful one, as testified by Wilbert Chapman, who in 1944 termed himself something of a "disciple" of Herre on that issue. (Chapman to William F. Thompson, Oct. 28, 1944, William F. Thompson Papers, UW Archives, Seattle.) On the institutional background and shortcomings of fisheries research specifically within California, see McEvoy 1986.
11. On Japan's knowledge of salmon, in contrast to the almost-nil understanding of deepwater movements of North Pacific salmon, see Herrington 1989 and Scheiber 1989. A summary of salmon management problems is in Larkin 1970.
12. This will be developed later in the text. Evidence on point is a manuscript article by D. Huntsman (1949), in which Huntsman discussed the difficulties encountered over many years of his and others' research—especially in research until 1934 on herring, and since 1934 on salmon—in achieving a useful set of theories concerning the relationships of oceanographic and biological research. He contended that "the factors determining concentration of [marine] fish . . . is an oceanographic matter." Huntsman continued:

    > It is really an ecological problem, involving the relations between organisms and their environments, between the ocean and the life therein. Twenty-five years ago I visualized it [the problem of why fish concentrate as they do] as the problem of limiting factors, of the factors limiting the distribution and abundance of marine organisms. I studied such obvious factors as temperature, salinity and light. . . . (but) made no particular impression. The field of study was still too vast and inchoate for easy comprehension or for solution of the problem in foreseeable time. Ecology, as being study of marine organisms and their environment, had been immeasurably large, and even study of the *relations* between organisms and their environment that determine

> their distribution and abundance was proving too large. How could the problem be effectively narrowed? Obvious narrowing was to take one or a few organisms and one or a few local environments. Study of an organism throughout its range in distribution seemed advisable in order to see the picture through contrasts. . . .

13. There was also a remarkable sense of shared excitement. For as Roger Revelle once recalled, in that era of ocean science, since *everything* needed to be studied, virtually every expedition was certain to turn up important new data, every plankton-net haul brought up surprises (Sharp 1988; Revelle 1986).

14. In May 1945, for example, Frances Clark of the California fisheries laboratory set forth her reflections on what could be learned from the catch analyses that her agency had been doing since the 1920s—and what problems remained, apparently beyond what catch data and tagging could illuminate. "Boat catch studies," she observed, "will tell us if the sardine population is holding its own, gaining or loosing [sic] as the result of fishing. It will not explain changes which occur." Proposing closer studies of age groupings in a sampling program to complement the boat catch data, Clark observed that "the weakest link in our whole investigation is our lack of knowledge of recruitment." She proposed that the sardine investigations should thus be expanded significantly, to include both surveys of young fish and larval fish surveys, and "general oceanographic investigations." (Letter from Frances Clark to Richard Van Cleve, May 1945, Van Cleve Papers, UW Archives.)

    Another illuminating exchange between these two sardine experts, four years later, dealt with the importance of juvenile survival and what was needed technically to do the necessary kind of research (letter of Dec. 29, 1949, from Van Cleve to Clark, in the Van Cleve Papers, UW Archives).

    The relationship of upwelling to nutriment levels, and the latter in relation to spawning, had been opened up for the sardine in the California Current by research conducted by Sverdrup on upwelling and then specifically by Ahlstrom on salinity patterns and spawning, in 1946–47. This research and its implication are discussed in a memorandum by Harald Sverdrup (1948).

    In 1949, Frances Clark was excitedly engaged in preparing a paper on the management of pelagic fisheries, hoping to "develop the need for sound biological, statistical, and oceanographical information, and thorough cooperation between fisheries investigators in the entire Pacific area." (Manuscript letter, 1949, in the Van Cleve Papers.) For Sverdrup and associates on upwelling, see also Sverdrup et al. 1942.

15. Biographical data on these figures is scattered throughout Shor 1978, Scheiber 1986, McEvoy 1986, and Sharp 1988.

16. "Memorandum on the Need for Oceanographic Studies for Pacific Coast Fisheries," Manuscript, marked 9 Oct. 1946 (signed by Joseph Craig, Frances N. Clark, Donald McKernan, and Oscar E. Sette), copy in Director's Files, SIO Archives.

17. R. O. Clover (Navy Hydrographic Office) to Albert M. Day (Fish and Wildlife Service), Feb. 11, 1947, copy in SIO Director's Files, SIO Archives.

18. Iselin, remarks to the conference "The Position of SIO in the University, the State, and the Nation" (La Jolla, March 1951), transcript (copy in SIO Archives).

    On the same lines, Chapman constantly reiterated the theme that the sardine project was only one strand in a "web of research" that embraced the entire Pacific Ocean (see Chapman 1947).

19. Revelle, remarks to the conference "The Position of SIO in the University, the State, and the Nation" (La Jolla, March 1951), transcript (copy in SIO Archives).

20. Letter of Revelle (Office of Naval Research, Washington) to Col. I. M. Isaacs (California Sardine Products Institute), Nov. 29, 1947, copy in SIO Director's Files, SIO Archives. Revelle incorporated verbatim some of these same passages in the early proceedings of CalCOFI, in a presentation of the projected SIO role in the cooperative project Memorandum: Marine Life Research Program, May 3, 1948, manuscript in Subject Files: Marine Life Research Program, SIO Archives.

21. Revelle to Isaacs, Nov. 29, 1947, SIO Director's Files.

22. It should be noted that whereas Revelle stressed the anomalous situation that prevailed, as a cause of sardine depletion, other scientists stressed that it was the "normal conditions" or "average conditions" which had to be identified—that is, "normal" relationships in the ecological system within which the sardines existed (Walford 1948).

    The rhetoric makes it seem, at first blush, that the conceptions in question were at odds. But I think that however they phrased the problem rhetorically, the principal designers of the New Oceanography's approach to ecological systems—both for the tuna, in the POFI project, and for the sardine, in the MRC project—recognized that "normal" relationships had to be defined in order to understand what deviations from those norms, or anomalies, affected reproduction, survival, abundance, and availability of the species. The problem was dealt with in a revealing letter by Sette, discussed in text at note 24 below.

23. Revelle to Isaacs, Nov. 29, 1947, SIO Director's Files, SIO Archives.

24. Sette to J. G. Burnette, Nov. 15, 1946, Fish and Wildlife Service Records, File 829.1, Record Group 22, U.S. National Archives.

    Although not expressing the vision of ecosystem research so explicitly as did Revelle or Sette, R. E. Foerster, director of the Pacific Biological Station of the Fisheries Research Board of Canada, similarly anticipated a research design for marine fisheries studies that would seek to isolate the relevant variables in physical environment: "It has seemed to me," he wrote in 1948,

    > that in tackling the biological phase of oceanography—and it is an important phase in developing the general picture of the relation of variation in oceanographic conditions to variations in abundance and/or availability of fish populations—we should, for the first few years at least, explore the importance of many factors, such as variations in nitrates, phosphates, carbonates, oxygen, phytoplankton, zooplankton[;] determine the relationships, if any, with a view to subsequently eliminating as many as possible and retaining for general survey only those that seem to have a real bearing or influence on abundance or availability of fish and can be used for prediction purposes, if such is ever feasible. There are obviously limits to how much field work and collection of samples, etc., can be done by a vessel and its technical and scientific personnel. . . .

    Foerster to J. L. McHugh, Aug. 16, 1948, in Subject Files: Marine Life Research Program, SIO Archives. (Foerster at this time was preparing plans for the Nanaimo-based oceanographic project SARDINE, and was in correspondence with the California group concerning possible coordination. See Dale Leipper [SIO] to John P. Tully, Aug. 10, 1948, Subject Files: Marine Life Research Program, SIO Archives.)

25. Indeed Chapman in particular, playing the parlous role of middleman between the industry and the scientists, repeatedly warned that "research on the high seas is expensive and time consuming," and that industry needs the "damned biologists," like it or not. (Letter to Phister, May 2, 1947, copy in William F. Thompson Papers, UW Archives, Seattle.)

26. Letter of Chapman to Miller Freeman, Aug. 11, 1947, Miller Freeman Papers, UW Library.

27. The Technical Committee was composed of Robert Miller of the California Academy, Sette of the federal service, Sverdrup, and Richard S. Croker of the California Division of Fish and Game (head of the Marine Fisheries Research Laboratory and its studies at sea). (Minutes of the MRC, April 28, 1948, in SIO Archives.)

28. Sverdrup and Walford had collaborated in studies of upwelling in relation to sardine spawning, and Walford had continued his larvae and egg studies in 1946–47 in waters off Point Conception and Baja California. The work is described in a letter by Sverdrup (1948).

29. Minutes of the MRC, April 28 and May 19, 1948, SIO Archives; Sette to Revelle, May 11, 1948, Marine Life Research Files, SIO Archives.

30. Miller report, in April 1948 MRC Minutes, SIO Archives. The original manuscript has notations (in John Isaacs' hand?) indicating

the agency to which each function was primarily assigned (shown in bracketed comments in extract quoted in text, above).

31. John Marr (acting chief, Southern Pacific Investigations, US Fish and Wildlife Service) to Carl Eckart (director, SIO), Sept. 1, 1948, SIO Director's Files, SIO Archives; Report of a conference between Walford, Silliman, Marr, Eckart, and Revelle, Washington, D.C. 9-17-48 (manuscript), in SIO Subject Files: Marine Research Committee, SIO Archives; MRC Minutes, Sept. 27, 1948 (includes budget items), Subject Files: MRC, SIO Archives.
32. Memorandum of Marine Life Research Conference, Dec. 30, 1948 (manuscript dated Jan. 10, 1949), SIO Subject Files: Marine Life Research, SIO Archives.
33. Eckart to Walford, June 28, 1948, SIO Director's Files: Marine Life Research, SIO Archives.
34. San Diego *Union*, Nov. 28, 1948, clipping in Carl Hubbs Papers, SIO Archives (quoting testimony of Richard Croker before the state assembly's interim committee on fish and game, citing the drop in the catch from 403,700 tons in 1945–46 to 124,200 tons in 1948; Croker recommended a 100,000-ton limit).
35. Revelle to John Marr, Dec. 14, 1948, copy in SIO Subject Files: Marine Life Research, SIO Archives. (Marr had suggested that the cruise of the federal ship *Black Douglas* be postponed until the April–July period, to permit full reoutfitting. Marr to Eckart, Nov. 22, 1948, ibid.)
36. Marine Life Research Conference, memorandum of Dec. 30, 1948, meeting with participating agencies (dated Jan. 10, 1949), copy in SIO Subject Files: Marine Life Research, SIO Archives.
37. Standard analyses of federal-state relations, in the literature of federalism and governance, of course treat at length the various types of federal grants-in-aid to states but entirely neglect even the possibility that the flow might ever run in the opposite direction. See, e.g., Wright 1982; cf. Scheiber 1980.
38. Plans of the Bureau of Marine Fisheries, California Division of Fish and Game, for Expanded Sardine Research and Budget Requests of the Marine Research Committee (manuscript marked "July 20, 1949"), copy in SIO Subject Files: Marine Life Research, SIO Archives.
39. Ibid. (On the continuing political travails of the state agency's scientists and their efforts to bring the sardine fishing under control, see McEvoy 1986.)
40. Progress report of the South Pacific Fishery Investigations, U.S. Fish and Wildlife Service, in the Expanded Pilchard Research Program, 1 May–31 July 1949 (manuscript report), copy in SIO Subject Files: Marine Life Research Program, SIO Archives.
41. Ibid.
42. Roger Revelle to Robert C. Miller, Sept. 13, 1949, enclosing copy of the May 1–July 31, 1949 SIO report, copy in SIO Subject Files: Marine Life Research Program, SIO Archives.
43. Ibid.
44. Letter of Clark to Hubbs, June 1, 1950, Subject Files: Marine Life Research, SIO Archives.
45. Ibid. See also McEvoy 1986, pp. 200–201, for more substantive controversy between the state agency and the federal and SIO scientists, concerning the proper way in which the issue of sardine depletion vis-à-vis commercial fishing ought to be presented to the public.
46. Ahlstrom 1950, italics added.
47. Letters from Marr to Revelle, Sept. 9, 1949; Clark to Marr, Sept. 13, 1949; and Revelle to Marr, Sept. 12, 1949, copies in SIO Subject Files: Marine Life Research Program, SIO Archives.
48. Annual reporting and publication of the scientific projects (augmenting the quarterly agency reports) were ordered beginning in 1950, after discussion at the June 7–8, 1950, meetings of the Technical Advisory Committee and the MRC. Such a report, the committees declared, would serve as "a summary of progress and results to date. . . . [and] should be widely distributed . . . as a basis for consideration by the fishing industry and the legislature of the desirability of continuing the program of the Marine Research Committee." (Revelle Memorandum, July 13, 1950, to Thomas Manar, copy in Marine Life Research: Publicity file, Hubbs Papers, SIO Archives.) Here, then, was the formal origin of the annual *CalCOFI Reports* that in 1990 recognize the project's fortieth anniversary.
49. Hubbs, discussing Ahlstrom's views, in letter of Hubbs to Chandler Harris (UCLA Public Information Office), 3 March 1952, in Marine Life Research: Publicity file, Hubbs Papers, SIO Archives.
50. University of California, Public Information—Radio, "The Missing Sardine," Broadcast #3061, U.E. 1260, Sunday, April 6, 1952, Columbia Broadcasting System, Los Angeles (manuscript radio text, copy in Hubbs Papers, SIO Archives).
51. That is to say, I still adhere to the view taken in McEvoy and Scheiber 1984 (page 406), but which my coauthor in that study has largely abandoned (see McEvoy 1986), that "the very complexity of ecology research—rendered progressively more complex by the emerging interdisciplinary approach that MRC funds fostered—made delay and indecision on policy a more likely result, at least for several years." It was probably the scientists' concern "not to hurry or be popular, but to be right. . . . That the resultant stalemate played into the hands of an industry that wished to avoid regulation was in that respect incidental—though it had tragic consequences for the fishery" (McEvoy and Scheiber 1984).

    See also the views of Radovich (1981), stressing "agency-based perspectives" that he feels led the state scientists (committed to regulation) in a direction divergent from that which the entire corporate history of their agency suggested was the best, or at least the prudent, course for the federal scientists.

    Years later, some of the leading scientists who had been associated with CalCOFI since its beginnings explicitly voiced this view of the need for neutrality. Thus Revelle and John Isaacs, responding to pressures for the CalCOFI scientists and the MRC to take a position on a key matter of policy regarding anchovy reduction plant permits, warned "that the MRC and CalCOFI should remain non-political and should not enter into the existing [policy] controversies." The chairman of MRC since its founding, Robert Miller of the California Academy, then "read from section 729 of the Fish and Game Code which . . . essentially [read] that MRC cannot make recommendations, it can only point out facts and make estimates of the situation." (Minutes of MRC meeting of Aug. 13, 1963, copy in Subject Files: Marine Research Committee, SIO Archives.) Later on, Miller wrote of the "superb job of getting previously warring agencies to work peaceably and even enthusiastically together" as an important achievement of CalCOFI and basis for its research accomplishments. (Robert Miller to Wilbert Chapman, Feb. 3, 1964, Robert Miller Papers, California Academy of Sciences Archives.)

    Also relevant in coming to a judgment of scientists' behavior in this era is the commitment of some, such as Ahlstrom, that the "extremely important function" of MRC as "one of the best coordinating mechanisms he knew of in fisheries research . . . has kept people working amicably in the same ocean on the same problem;" and that any split caused by dealing with explosive political issues that could be resolved in other arenas would work against this coordination, which "he submitted . . . (was) the greatest value of MRC and . . . should be preserved." (Minutes of MRC meeting of Jan. 19, 1965, SIO Archives.)

    Resolution of the difference in interpreting the scientists' and MRC roles must turn, at least in part, on whether one judges that the evidence of harm to stocks from overfishing was so compelling by even 1947, let alone 1952, that scientists who failed to register opinions on the side of suspension or tighter regulation were in effect irresponsible. See also text, *infra*, at note 57.
52. Especially so, of course, by dint of the intensive (monthly) data collection at all stations of the enormous grid that was established in 1948–49. (See CalCOFI 1989; Revelle 1986.) A few years later, an MRC member wrote that "Our [California] offshore seas and their inhabitants are better known and understood than any in the world with the possible exception of the Norwegian Sea" (Bruce 1963). In 1959 John Isaacs asserted, "It is safe to say that there has never been another study that resulted in so thorough an understanding of a pelagic species of fish as that [which] CalCOFI and earlier studies

have obtained on the sardine." (Isaacs, in Appendix to Minutes of the Marine Research Committee, July 30, 1959 meeting, SIO Archives.)

53. Four or more spawning areas were early identified, one in the Gulf of California, others off southern Baja California, central Baja California, and an area off the southern California and northern Baja California coast. (Technical Committee report, in MRC Minutes for Dec. 19, 1957, SIO Archives.)
54. Ibid.
55. Report of the Special Technical Committee, MRC Minutes, Dec. 19, 1957, SIO Archives, also quoted in Murphy 1960. It is noteworthy, also, that indicating the legislature's (and the fishing industry's) recognition and approval of this expanding agenda, new taxes were levied on mackerel and anchovy, to augment the revenues (which were steadily declining because of the continued fall in sardine landings) from the original sardine tax authorized in 1947. State and federal general appropriations for SIO, USFWS, and California state agency research continued to support the larger program that the MRC funds augmented, so that in 1959 it was estimated that the total spent for programs directly linked to CalCOFI research represented a level of $130,000 of MRC funds from the landings taxes, $600,000 of SIO funds, $250,000 of USFWS funds, and $200,000 of the California Department of Fish and Game funds. (MRC Minutes of Dec. 1, 1959 meeting, SIO Archives.)
56. Report of the Special Technical Committee, Minutes of MRC meeting, Dec. 19, 1957, SIO Archives.
57. Reference here is to the work of Ahlstrom, Isaacs, Murphy, and Paul Smith in the post-1960 period as well as to that of Ahlstrom, Walford, Marr, and Clark in the years from 1937 to 1960. On their respective contributions, see, inter alia, McEvoy 1986, Ahlstrom and Radovich 1970. Throughout the entire period of CalCOFI research to the mid-1960s, the California Fish and Game scientists unsuccessfully sought urgently to obtain full regulatory powers over the sardine fleet, but even the definitive collapse that occurred in 1952–53 (when the catch went from 145,000 tons to 15,000) failed to win them the authority they sought (Ahlstrom and Radovich 1970).
58. The great weather shift that occurred in 1957 and 1958 did finally give the SIO and other CalCOFI scientists new insight into variations from normal conditions and their impact on the fisheries. A major symposium was held in 1958—"1957 and 1958, the Years of Change"—and reported on by John Isaacs in Minutes of MRC meeting, June 10, 1958, SIO Archives.
59. Letter of Sette to J. G. Burnette, Chairman, MRC, Dec. 4, 1957, in Minutes of MRC meeting, Dec. 19, 1957, SIO Archives.
60. McKernan to Burnette, Dec. 12, 1957, ibid; O. E. Sette to Burnette, Dec. 4, 1957, ibid.
61. Report of the Special Technical Committee, in Minutes of MRC Meeting, Dec. 19, 1957, SIO Archives.
62. Letter of Chapman to Phister, Sept. 19, 1947, Chapman Papers, UW. Chapman had wanted John Kask, then of the California Academy of Sciences, to be named coordinator, partially because of Kask's personal qualities but partially too because the Academy was seen as a neutral player in the politics of fisheries science in California. As it worked out, the Technical Advisory Committee that was appointed in 1948 served the coordinating function, and Director Robert C. Miller of the Academy was its chairman for more than 17 years. (Robert C. Miller to Chapman, Feb. 3, 1964, Robert C. Miller Papers, California Academy of Sciences Archives.)
63. The quotation is from Miller to Chapman, Feb. 3, 1964, Miller Papers, California Academy of Sciences Archives.
64. See Baxter 1982. Later, in the mid-1960s, when MRC had been expanded to include sportfishing and labor representatives, and when the frustration of the commercial fishing and cannery interests with a standoff—in MRC and in state policy bodies—on proposals to open and expand the anchovy fishery caused new and deeper rifts within MRC, the coordinator did come under some heavy criticism for what one industry representative (Chapman) regarded as his failure to exercise sufficient control over the agencies in the project. (Chapman, comments reported in Minutes of MRC meeting of Jan. 1965, SIO Archives. See also Scheiber 1986 on Chapman's efforts in this period to liberalize more generally the regulation of California commercial fisheries and to reduce the influence of the sports interests.) Murphy's scholarly contributions are considered in Ahlstrom and Radovich 1970 and in McEvoy 1986.
65. The political story is recounted in McEvoy 1986, pp. 215–20; on Chapman and the industry's role in the controversy, see Scheiber 1986. Beyond the purview of the present study is the further research done by Isaacs on sedimentary evidence of the historic sardine "is an unusual event" in what he termed the normal "hake-anchovy complex" of the California Current biomass. (Paper presented to the MRC meeting of May 11, 1965, Minutes, SIO Archives.) On the various studies by Lenarz, Smith, and McCall on the interpretation of the anchovy-sardine data and the implications for management that this important work suggested, see the discussion in McEvoy 1986, pp. 232–235.

REFERENCES

Ahlstrom, E. H. 1950. A report on the problems and objectives of the cooperative sardine research program [marked "May 1950," written with assistance from persons working in the program], copy in SIO Subject Files: Marine Life Research: General, SIO Archives.

———. 1963. Larval fish studies (manuscript report, prepared for Marine Research Committee), in Minutes of MRC meeting of 11 April 1963, copy in Subject Files: MRC, in SIO Archives.

———. 1964. Egg and larval surveys (manuscript report, prepared for Marine Research Committee), in Minutes of MRC meeting of 6 March 1964, Appendix, copy in Subject Files: MRC, in SIO Archives.

Ahlstrom, E. H., and J. Radovich. 1970. Management of the Pacific Sardine. *In* A century of fisheries, N. Benson, ed. American Fisheries Society, Special Publication No. 7, Washington: American Fisheries Society, pp. 183–193.

Barrett, I. 1980. Development of a management regime for the Eastern Pacific tuna fishery. Ph.D. dissertation, College of Fisheries, University of Washington, Seattle.

Baxter, J. L. 1982. The role of the Marine Research Committee and CalCOFI. Calif. Coop. Oceanic Fish. Invest. Report 23:35–38.

Bruce, E. F. 1963. The impact of new technology on ocean fish conservation and management as seen by the Marine Research Committee (transcript of talk before the Wildlife Society, no date), copy in Minutes of MRC meeting of March 1963, Subject Files: MRC, SIO Archives.

CalCOFI. [1989] California Cooperative Oceanic Fisheries Investigations. (CalCOFI, no date, but 1989), not paginated.

Chapman, W. M. 1947. Statement in testimony, in 80th Cong., 1st Sess., House of Rep. Comm. on Merchant Marine and Fisheries, Development of the high seas fishing industry: Hearings, May 12, 1947. Washington: GPO, 1949, pp. 3–11.

Deacon, M. 1990. The establishment of marine stations and early commercial fisheries research in Scotland. Unpublished paper for California Sea Grant College Program/UC Intercampus Economic History program conference, Berkeley, May 1990. Copy on file, Center for the Study of Law and Society, UC Berkeley.

Dymond, J. 1948. European studies of the populations of marine fisheries, Bulletin of the Bingham Oceanographical Collections 11:55.

Fleming, D. 1971. Roots of the new conservation movement. Perspect. Am. Hist. 6:7–91.

Herre, A. 1940. Distribution of fish in the tropical Pacific. *In* Sixth Pacific Science Congress, Proceedings, Pacific Science Academy 3:587.

Herrington, W. C. 1988. Interview by the author, Staffordville, Connecticut, June 1988. (Notes in author's files.)

———. 1989. In the realm of diplomacy and fish: some reflections on International Convention on High Seas Fisheries in the North Pacific Ocean and the Law of the Sea negotiations. Ecology Law Quar. 16:1087.

Huntsman, D. 1949. Fisheries oceanography (manuscript received in May 1949 by the SIO Director's Office and circulated at SIO), in SIO Subject Files: Marine Life Research, SIO Archives.

Idyll, C. P. 1969. The science of the sea. *In* Exploring the ocean world: a history of oceanography. New York: Crowell, pp. 2–21.

Isaacs, J. 1959. Proposed program in fisheries research (manuscript report to Marine Research Committee), in Minutes of MRC meeting of 30 July, 1959, copy in SIO Subject Files: MRC, SIO Archives.

Knauss, J. 1990. Remarks at Symposium of CalCOFI Conference, October 27, 1989. Calif. Coop. Oceanic Fish. Invest. Rep. 31: (this volume).

Larkin, P. A. 1970. Management of Pacific salmon of North America. *In* A century of fisheries, N. Benson, ed. American Fisheries Society, Special Publication No. 7, Washington: American Fisheries Society, pp. 223–235.

Los Angeles Times. 1949. Fleet of six ships launches biggest fish hunt in history. Los Angeles Times, June 22, 1949, clipping in Publicity: MLR file, Carl Hubbs Papers, SIO Archives.

McEvoy, A. 1986. The fisherman's problem: ecology and law in the California fisheries, 1850–1980. New York: Cambridge University Press.

McEvoy, A., and H. N. Scheiber. 1984. Scientists, entrepreneurs, and the policy process: a study of the post-1945 California sardine depletion. J. Econ. Hist. 44:393–413.

McHugh, J. L. 1970. Trends in fishery research. *In* A century of fisheries, N. Benson, ed. American Fisheries Society, Special Publication No. 7, Washington: American Fisheries Society, pp. 25–56.

Miles, E. A., S. Gibbs, D. Huhart, C. Dawson, and D. Teeter. 1982. The management of marine regions: the North Pacific: an analysis of issues relating to fisheries, marine transportation, marine scientific research, and multiple use conditions and conflicts. Berkeley: University of California Press.

Murphy, G. 1960. Operational analysis of CalCOFI research (Sept. 7, 1960). Report to Marine Research Committee, in Minutes of MRC meeting, 4 Oct. 1960, Exhibit #1, copy in SIO Subject Files: MRC, SIO Archives.

———. 1963. Summation of Calcofi. (Manuscript report to Marine Research Committee), in Minutes of MRC meeting, 11 April 1963, copy in Subject Files: MRC, SIO Archives.

Murphy, G., and J. Isaacs. 1964. Species replacement in marine ecosystems with reference to the California Current. (Manuscript report), in Minutes of meeting of MRC, March 6, 1964, Appendix, copy in Subject Files: MRC, SIO Archives.

Pacific Fisherman. 1947. August 1947 symposium issue (special symposium under the title, "Who Will Harvest the Pacific," introduced by the editors as an "outline of the resources latent in the Pacific Fisheries Frontier").

Pan-American Fisherman. August 1950. (Summary of SIO role in sardine research and related oceanographic studies) Clipping in Subject Files: Marine Life Research, SIO Archives, p. 4.

Radovich, J. 1981. The collapse of the California sardine fishery. *In* Resource management and environmental uncertainty, M. H. Glantz and J. D. Thompson, eds. New York: Wiley, 110–111.

Revelle, R. R. 1986. Interview by the author, La Jolla, May 1986. Notes in author's files.

Russell, E. S. 1942. The overfishing problem. The De Lamar Lectures at the Johns Hopkins University. Cambridge: Cambridge Univ. Press.

Scheiber, H. N. 1980. Federalism and legal process: historical and contemporary analysis of the federal system. Law and Society Rev. 14:663–722.

———. 1986. Pacific Ocean resources, science, and Law of the Sea: Wilbert M. Chapman and the Pacific Fisheries, 1945–70, Ecology Law Quar. 13:381–534.

———. 1988. Wilbert Chapman and the revolution in U.S. Pacific Ocean science and policy, 1945–1951. *In* Nature in its greatest extent: western science in the Pacific, P. Rehbock and R. MacLeod, eds. Honolulu: University of Hawaii Press.

———. 1989. Origins of the Abstention Doctrine in ocean law: Japanese-U.S. relations and the Pacific fisheries, 1937–1958. Ecology Law Quar. 16:23–99.

———. In press, a. U.S. ocean fishery research in the Pacific, 1945–1970, Deutsche Hydrograph. Zeit.

———. In press, b. Science, commercial interests, and the policy process in U.S. oceans research: the POFI tuna program, 1948–64. Paper to the International History of Science Congress, Hamburg and Munich, to be distributed as an Ocean Law and Policy Working Paper, Center for the Study of Law and Society, School of Law, University of California, Berkeley.

Sette, O. E. 1943. Studies on the Pacific pilchard or sardine (*Sardinops caerulea*): I. Structure of a research program to determine how fishing affects the resource. U.S. Fish and Wildlife Service, Spec. Sci. Report, No. 19.

———. 1952. Letter to POFI industry advisory committee, June 20, 1952, Pacific Oceanic Fishery Investigations Records, U.S. Fish and Wildlife Service Papers, National Archives.

———. 1955. Considerations of mid-ocean fish production as related to oceanic circulatory systems. J. Mar. Res., 14:398.

Sette, O. E., and E. H. Ahlstrom. 1948. Estimations of the abundance of the eggs of the Pacific pilchard (*Sardinops caerulea*) off southern California during 1940 and 1941. J. Mar. Res. 7:511–541.

Sharp, S. 1988. Observations on the Office of Naval Research and international science, 1945–1960: oral history interview of Roger Revelle. La Jolla: Scripps Institution of Oceanography.

Shor, E. N. 1978. Scripps Institution of Oceanography: probing the oceans, 1936 to 1976. San Diego: Tofua Press.

Sverdrup, H. U. 1948. The sardine problem. Manuscript memorandum prepared for a journal editor as a background document and enclosed with a letter by Sverdrup to Charles M. Hatcher (editor, Tuna Fisherman), Feb. 23, 1948, SIO Subject Files: MRC, SIO Archives.

———. 1952. Some aspects of the primary productivity of the sea. FAO Fish. Bull. 5(6):215–223.

Sverdrup, H. U., M. W. Johnson, and R. H. Fleming. 1942. The oceans: their physics, chemistry, and general biology. New York: Prentice-Hall.

Thompson, W. F. 1919. The scientific investigation of marine fisheries, as related to the work of the Fish and Game Commission in southern California. California Fish and Game Commission Fish. Bull. No. 2.

Walford, L. A. 1948. The case for studying normal patterns in fishery biology, J. Mar. Res. 7:506–510.

Wooster, W. 1949. Memorandum, Warren Wooster to Roger Revelle, 3 May 1949, Subj: Personnel assigned to MLR chemistry division, Marine Life Research files, SIO Archives.

Wright, D. S. 1982. Understanding intergovernmental relations, 2nd edition. Montery, Calif.: Brooks Cole.

SETTLEMENT OF JUVENILE CALIFORNIA HALIBUT, *PARALICHTHYS CALIFORNICUS*, ALONG THE COASTS OF LOS ANGELES, ORANGE, AND SAN DIEGO COUNTIES IN 1989

M. JAMES ALLEN
MBC Applied Environmental Sciences
947 Newhall Street
Costa Mesa, California 92627

KEVIN T. HERBINSON
Southern California Edison Company
P.O. Box 800
Rosemead, California 91770

ABSTRACT

Juvenile California halibut (*Paralichthys californicus*) typically occur in bay nursery grounds along the coast of California and the west coast of Baja California. Recent surveys using small-meshed beam trawls indicate that some settlement to the bottom also occurs in shallow coastal areas of southern California. This survey of halibut settlement in bay and coastal areas was designed to determine (1) the relative settlement rates of juvenile halibut in bay and coastal environments and (2) the survival of small juveniles in the shallow coastal zone. Areas off Los Angeles, Orange, and San Diego counties were surveyed from April to September 1989. Three stations were sampled at each of four coastal sites (Hermosa Beach, Long Beach, San Onofre, and Carlsbad) and two bay sites (Anaheim Bay and Agua Hedionda Lagoon). A 1.0-m beam trawl was used in bays and a 1.6-m beam trawl along the coast. A total of 288 samples was collected at depths of 0–3 m in bays and 6–13 m along the coast. These samples indicated that juvenile California halibut did settle into shallow coastal waters of the areas surveyed during 1989. Settlement was greatest at Anaheim Bay, Hermosa Beach, and Long Beach. The settling halibut remained in bays throughout the study period. Although transforming fish were also found at coastal sites throughout the study period, successful settlement occurred only from July to September and then only in the semiprotected sites (Hermosa Beach and Long Beach).

RESUMEN

Los juveniles del lenguado de California (*Paralichthys californicus*) se encuentran en general en las bahías criaderas a lo largo de la costa de California y de la costa occidental de la península de Baja California. Estudios recientes con red de arrastre de bao de malla fina indican que el establecimiento en el fondo se da en ciertas áreas costeras de poca profundidad del sur de California. Este estudio del establecimiento del lenguado en bahías y áreas costeras fue diseñado con el propósito de: (1) determinar el reclutamiento relativo de los juveniles, y (2) determinar la supervivencia de los juveniles pequeños en las zonas poco profundas. Se estudiaron algunas áreas costeras de los condados de Los Angeles, Orange y San Diego, de abril a septiembre de 1989. Se tomaron muestras en cuatro localidades costeras (Hermosa Beach, Long Beach, San Onofre y Carlsbad) y en dos lagunas costeras (Anaheim Bay y Agua Hedionda Lagoon). Se muestrearon tres estaciones en cada localidad utilizando una red de arrastre de bao de 1.0 m en las lagunas y una red de 1.6 m en las áreas costeras. Se colectaron 288 muestras en total, a profundidades de 0–3 m en las lagunas y de 6–13 m a lo largo de la costa. El estudio de estas muestras indicó que en 1989 los juveniles del lenguado de California se reclutaron en las aguas costeras poco profundas de los condados investigados. El mayor reclutamiento se presentó en Anaheim Bay, Hermosa Beach y Long Beach. Los juveniles permanecieron en las bahías costeras durante el período de estudio. Si bien también se encontraron algunos individuos en otros sitios costeros durante el estudio, el reclutamiento exitoso occurrió solamente de julio a septiembre y únicamente en los sitios semiprotegidos (Hermosa Beach y Long Beach).

INTRODUCTION

California halibut (*Paralichthys californicus*) is an important species to the ecology and fisheries of coastal southern California. As a juvenile it is a characteristic component of the bay (i.e., coastal lagoon) fish community. As an adult it is an important ambushing predator of nearshore fishes (M.J. Allen 1982) and an important species in the marine sport and commercial fisheries of California (NMFS 1985; CDFG 1989).

Crucial portions of habitat are being threatened by human encroachment. California halibut are generally thought to require bays for nursery grounds, and thus these areas may be crucial to their survival (Haaker 1975; Kramer and Hunter 1987; L.G. Allen 1988a; Kramer, in press). Most small juveniles are found in bays; it is only at a larger size

---

[Manuscript received February 1, 1990.]

that halibut move to coastal waters, where they recruit to the fisheries (Haaker 1975).

Until recently, little was known about the settlement of California halibut from the plankton to the bottom. The abundance of juveniles in bays has been known for some time (Haaker 1975), as has the paucity of small juveniles (<150 mm) in open coastal areas (M.J. Allen 1982; Plummer et al. 1983). However, the distribution and biology of newly settled juveniles has only recently been studied (Kramer and Hunter 1987; L.G. Allen 1988a, b; Kramer and Hunter 1988; L.G. Allen et al., in press; Kramer, in press).

Surveys in the vicinity of Alamitos Bay in 1983–85 indicated that recently settled California halibut were most abundant in the protected habitat of Alamitos Bay, less abundant in the semiprotected habitat of Long Beach Harbor, and least abundant on the open coast at Sunset Beach (L.G. Allen 1988a). Recent surveys in bays and open coast areas of San Diego County revealed little coastal settlement in 1987, but substantial coastal settlement in 1988 (Kramer and Hunter 1987, 1988; Kramer, in press). Another survey in 1988 along the open, nearshore coast of southern California from Point Conception, Santa Barbara County, to San Mateo Point, Orange County, found that the greatest numbers of settling individuals occurred in southern Santa Monica Bay and in Long Beach Harbor (L.G. Allen et al., in press).

Thus, although settlement of California halibut to the open coast of southern California has been described, little is known of halibut's interannual settlement success there. Because of the importance of California halibut to the fisheries, and the continuing encroachment of human activity in the bay nursery grounds, it is important to continue studies of settlement patterns.

The objective of this study was to determine the relative settlement and survival of juvenile California halibut in selected bay and coastal environments of Los Angeles, Orange, and San Diego counties during the spring and summer of 1989.

METHODS

Study Area

The study area extended from Hermosa Beach to Carlsbad, California. Within this area six sites were surveyed: Hermosa Beach, Long Beach, Anaheim Bay, San Onofre, Carlsbad, and Agua Hedionda Lagoon (figures 1 and 2). These included two bay sites (Anaheim Bay and Agua Hedionda Lagoon), two semiprotected coastal sites (Hermosa Beach and Long Beach), and two exposed coastal sites (San Onofre and Carlsbad). The Agua Hedionda Lagoon, Carlsbad, and San Onofre sites are located in San Diego County and have been sampled in previous surveys (Kramer and Hunter 1987, 1988). Anaheim Bay is in Orange County; Long Beach and Hermosa Beach are in Los Angeles County.

The bay sites are fully protected from offshore swells; the semiprotected coastal areas are partially protected; and the exposed coastal sites are more fully exposed to offshore swells. Depending on wind and swell direction, the Hermosa Beach site may be variably exposed or protected during the year. It is protected from swells from the south or southwest during the summer by the Palos Verdes Peninsula and offshore islands but is exposed to western swells, which occur primarily during the winter (Maloney and Chan 1974). The Long Beach site is protected from swells from the northwest, west, and southwest by the Palos Verdes Peninsula, breakwaters, and offshore islands. The San Onofre site is fully exposed to swells from the south and the southwest, and the Carlsbad site to swells from the west and south.

At all sites, stations were located randomly within blocks stratified by depth. The water depth of the blocks ranged from 0.0 to 0.8 m, 1.0 to 1.5 m, and 3.0 to 3.5 m in bays, and from 6 to 8 m, 8 to 11 m, and 11 to 15 m along the coast. For analyses, these blocks are represented as stations with depths of 0.5, 1.0, and 3.0 m in the bays and 6.0, 10.0, and 13.0 m

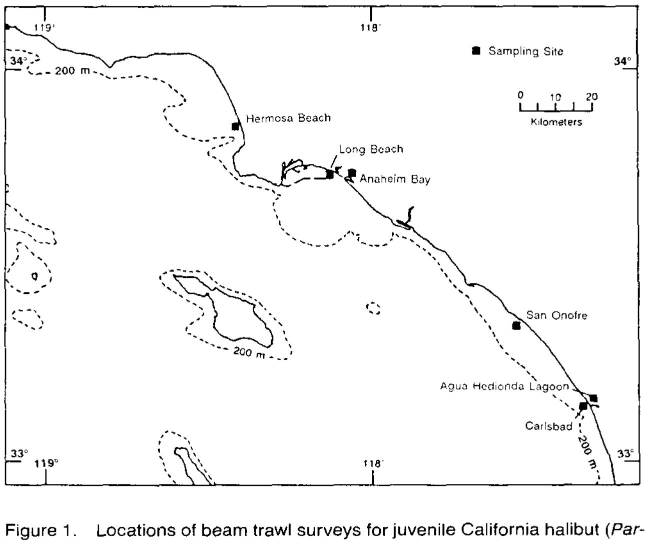


Figure 1. Locations of beam trawl surveys for juvenile California halibut (*Paralichthys californicus*), April–September 1989.

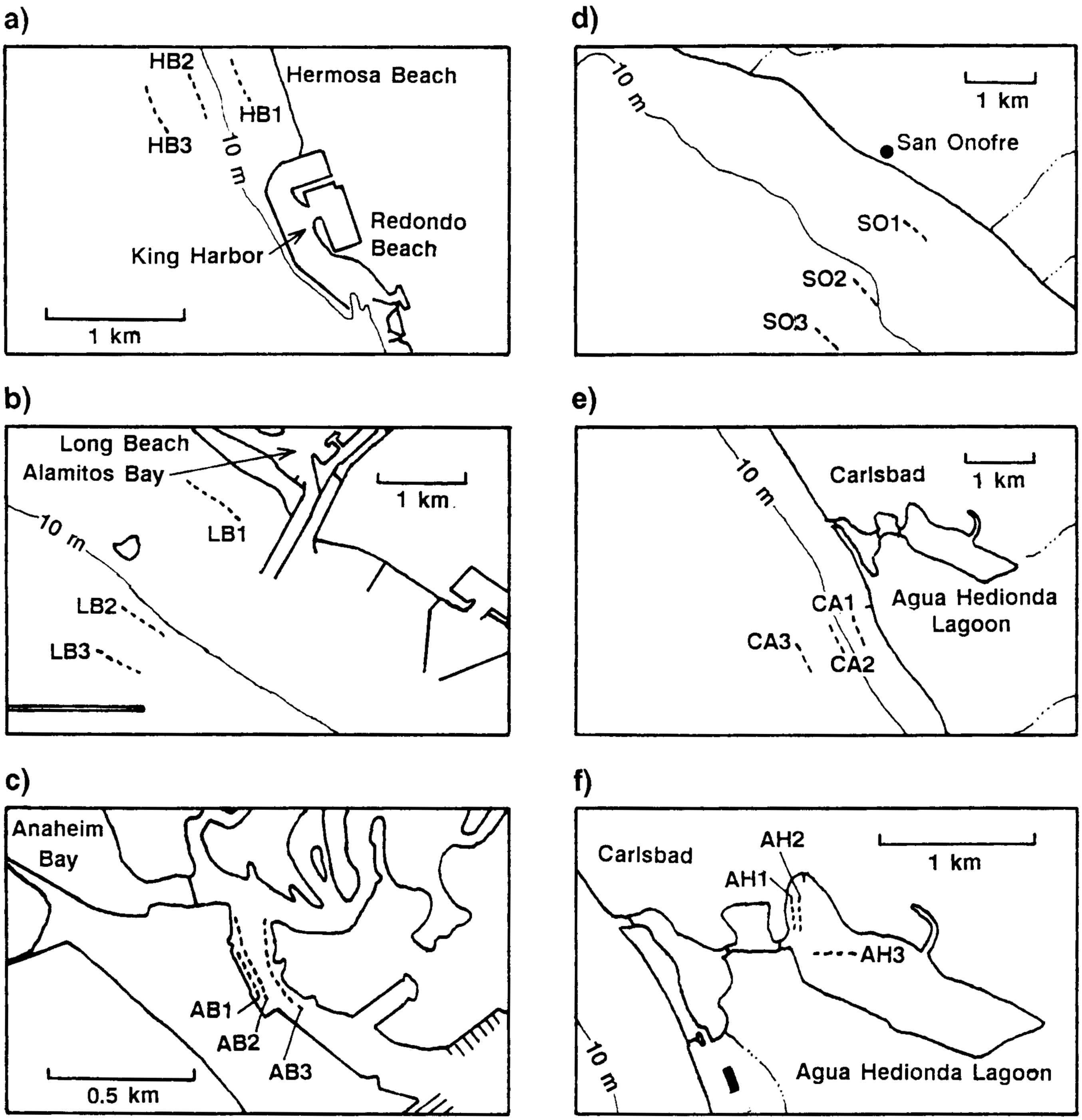


Figure 2. Stations sampled by beam trawl along the southern California coast, April–September 1989: *a*, Hermosa Beach; *b*, Long Beach; *c*, Anaheim Bay; *d*, San Onofre; *e*, Carlsbad; and *f*, Agua Hedionda Lagoon.

along the coast, approximately the same depths as sampled by Kramer and Hunter (1987, 1988). Station coordinates are given in MBC 1990.

Sampling Methods

Fish were collected with the same nets used by Kramer and Hunter (1987, 1988). Coastal samples were collected with a 1.6-m beam trawl and bay samples with a 1.0-m beam trawl. All nets had 2.5-mm stretch-mesh netting. The beam trawls were equipped with a wheel and revolution counter (meter wheel) which recorded the distance that the trawl traveled on the bottom, although the meter wheel occasionally clogged with plant debris. At coastal stations loran C coordinates (longitude and latitude) were recorded at the beginning and end of each haul. In the bays, 200-m trawl paths were measured and marked with buoys to provide a separate indication of towing distance, in case the meter wheel fouled. Depth was measured with sonar at coastal sites and with a sounding line at bay sites.

Coastal trawling was conducted from the R/V *Westwind*, a 14.6-m research vessel. Bay trawling at 1.0-m and 3.0-m stations was conducted from a 5.2-m Boston whaler. At the 0.5-m stations the beam trawl was pulled by two field workers on foot.

Three 10-min (coast) or 200-m (bay) trawls were attempted during daylight hours at each site's three stations, for a total of nine replicates per site. This should have resulted in 54 samples per survey, but lost or broken nets and heavy loads of algae often reduced the available sampling time. Thus fewer replicates were completed at some stations. Physical characteristics of each tow are given in MBC 1990.

Each of the six sites was sampled in April, May, June, July, August, and September of 1989. These were the months of major settlement of California halibut into the coastal environment off San Diego County during 1988 (Kramer and Hunter 1988), although settlement into bays can begin as early as November.

Although this study emphasized California halibut, all fish captured were retained for identification and measurement. Most were returned to the laboratory for processing; however, large specimens were identified to species, measured, weighed, and released in the field. Because transforming halibut and other juvenile fishes are small, most debris (and invertebrates) was returned to the lab for closer examination; only large debris was discarded in the field. Specimens and debris were fixed in buffered 10% Formalin-seawater.

In the laboratory the samples were rinsed of Formalin after about a week and transferred to 70% isopropyl alcohol. Samples were then sorted to separate fish from invertebrates and debris. Fish were identified to species, measured, and weighed. The standard length (SL) of each bony fish or total length of each cartilaginous fish was measured to the nearest millimeter. For abundant species, subsamples of up to 200 individuals were measured separately. The total weight of each species in a sample was weighed on a Mettler balance to the nearest 0.01 g.

Near-bottom water samples were collected at each station with Van Dorn bottles, generally after the last haul at the station. Temperature and pH of these samples were measured in the field with a Horiba analyzer. Station values of these oceanographic parameters are given in MBC 1990.

Data Analysis

The bottom area actually sampled in each tow was calculated using meter-wheel readings or distances measured in the field. When fouling had occurred or the meter-wheel reading was obviously too low, the distance traveled was estimated. This estimate was 200 m in bays and 315 m along the coast. The coastal estimate was based on the average distance attained by all "good" tows. The area sampled was computed as the product of the distance towed and the width of the trawl — 1.0 m for bays and 1.6 m for the coast. Estimated areas for replicates with extremely low readings were 200 m^2 for bays and 504 m^2 for the coast.

Length-frequency histograms of California halibut were generated for each survey and were plotted by area and month. Densities of halibut size groups at various sites were also determined. The relationship between settlement and temperature was determined by linear regression analysis.

RESULTS

Sampling Effort

From April to September, 288 trawl samples were collected, 36 to 52 each month. All stations were sampled, but fewer than three replicates were obtained at some stations because of fouling by algae or sand. Totals of 190 samples were collected along the coast and 98 in the bays; of the coastal samples, 95 were collected at semiprotected and 95 at exposed stations. From 47 to 50 samples were collected at each site, 29 to 35 at each depth in the bays, and 60 to 64 at each depth along the coast.

The total area sampled was 11.5 ha, 1.6–2.1 ha per month. A total of 9.7 ha was sampled along the coast and 1.8 ha in bays; along the coast 4.8 ha were sampled in the semiprotected habitat and 4.9 in the exposed habitat. At each coastal site, 2.4–2.5 ha were sampled; at each bay site 0.9 ha were sampled. At each depth zone within the bays, 0.6–0.7 ha were sampled; at each depth zone along the coast, 3.1–3.5 ha were sampled.

Physical Oceanography

Monthly near-bottom water temperatures at stations in this study ranged from 13.8° to 24.7°C (table 1). Monthly temperatures at stations along the coast ranged from 13.8° to 22.0°C and in bays from 18.3°

TABLE 1
Water Temperature and pH in Coastal and Bay Habitats, 1989

	Coastal				Bay			
Month	Min.	Max.	Mean	SD	Min.	Max.	Mean	SD
Temperature (°C)								
April	15.0	18.2	16.4	0.9	18.3	21.7	19.7	1.1
May	13.9	18.3	16.6	1.5	18.3	18.9	18.6	0.3
June	14.4	18.1	16.2	1.3	18.9	22.8	20.9	1.3
July	17.3	22.0	20.1	1.2	21.3	24.7	23.1	1.1
August	13.8	21.1	17.9	2.7	21.0	23.0	22.2	0.9
September	16.1	20.4	18.0	1.2	20.3	22.8	21.2	0.8
pH								
April	7.2	7.7	7.5	0.1	7.3	7.9	7.7	0.2
May	7.4	7.8	7.7	0.1	7.7	8.2	8.0	0.2
June	6.6	7.6	7.3	0.3	6.6	6.9	6.8	0.1
July	7.3	7.9	7.7	0.1	7.9	8.0	8.0	0.1
August	7.4	10.2	8.1	0.1	7.9	8.2	8.1	0.1
September	7.9	8.3	8.1	0.1	7.8	8.1	7.9	0.1

to 24.7°C. Means ranged from 18.6° to 23.1°C in the bays and from 16.2° to 20.1°C along the coast. Monthly mean temperatures at sampling sites were greater in the bays than along the coast (table 1; figure 3). In the bays the highest mean temperature occured in July and the lowest in May. Along the coast the highest mean temperature also occurred in July, but the lowest occured in June. The highest temperatures in both bay and coastal areas were during the last three months of the survey period.

Hydrogen ion concentration (pH) in this study ranged from 6.6 to 10.2 (table 1). pH values had a greater range along the coast than in the bays; however, monthly means had a greater range in the bays than along the coast. The monthly mean pH values at bay sites were generally equal to or higher than those found at coastal sites, except in June and September, when values were higher along the coast. Along the coast the lowest mean pH occurred in June and the highest in August and September. In the bays the lowest pH also occurred in June and the highest in August.

Distribution and Settlement

Catch parameters. California halibut occured in 55% of the samples collected. The fish were almost equally common along the coast and in the bays, being found in 56% of the coastal samples and 53% of the bay samples. We collected 762 halibut in the survey, about 2% of all the fish captured (48,994). More halibut were taken along the coast (457) than in the bays (305). Along the coast, more were taken in the semiprotected habitat (348) than in the exposed habitat (109). However, these absolute numbers are a result of variation in sampling effort due to differences in net sizes used, tow lengths, and number of hauls. Standardization of the catch by area trawled indicated that the mean halibut density was highest (158 fish/ha; SD = 275) in the bays, intermediate (94 fish/ha; SD = 184) at semiprotected coastal sites, and lowest (25 fish/ha; SD = 44) at exposed coastal sites.

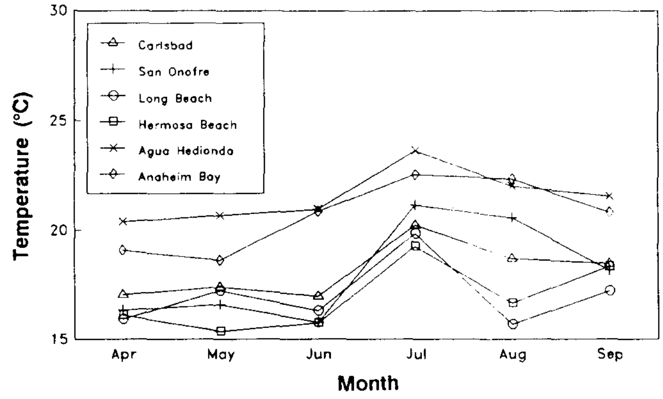


Figure 3. Mean monthly temperatures at sites surveyed for juvenile California halibut (*Paralichthys californicus*), 1989.

The total weight of California halibut taken in the survey was 37.7 kg, about 15% of the total fish biomass collected (243.3 kg). Total halibut biomass was much greater (36.7 kg) at coastal sites than in bays (0.9 kg). Along the coast the biomass was greater (20.8 kg) in the semiprotected habitat than in the exposed habitat (15.9 kg). Again, these absolute values result from differences in sampling effort at bay and coastal sites. Standardization by area indicated that the mean biomass density was highest (4.8 kg/

ha; SD = 9.0) in the semiprotected coastal habitat, intermediate (3.3 kg/ha; SD = 6.1) in the exposed coastal habitat, and lowest (0.5 kg/ha; SD = 1.3) in bays.

Size and population structure. California halibut ranged in size from 6 to 503 mm SL, with a mean length of 74 mm (SD = 88 mm). The population sampled by the survey was strongly skewed to the right, with a modal size of 10 mm (figure 4). Few fish were collected in the 100 to 140-mm size classes or in size classes above 350 mm.

The population structure in the bays was similar to that along the coast, being dominated by fish less than 100 mm SL (figure 5). However, the modal size was 10 mm along the coast and 20 mm in the bays. In addition, fish greater than 140 mm constituted only a small portion of the population in the bays, and no individuals of 240 mm or longer were taken. However, fish longer than 140 mm formed a significant portion of the coastal population, which included many individuals longer than 240 mm. There were few fish between 100 and 140 mm SL in the coastal population.

Distinct differences in the size-frequency distribution of California halibut were apparent when coastal sites were subdivided into semiprotected and exposed habitats (figure 6). In the exposed coastal habitat a strong mode was apparent at 10 mm; only three individuals were taken in the range of 20 to 140 mm SL. In contrast, the size-frequency distribution in the semiprotected areas was similar to that for both coastal habitats combined, with many individuals between 20 and 140 mm SL; thus 10-mm and 150- to 300-mm fish constituted a smaller percentage of the population.

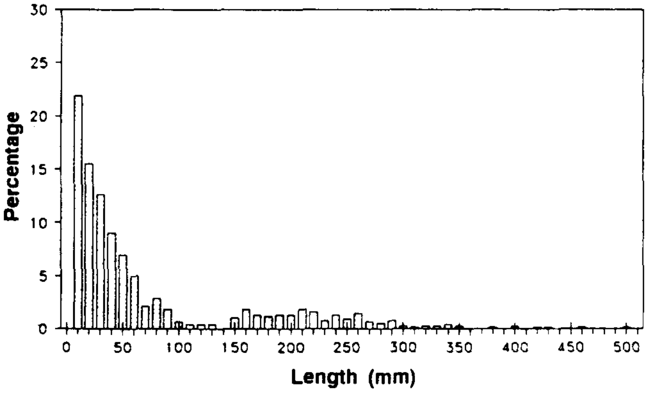
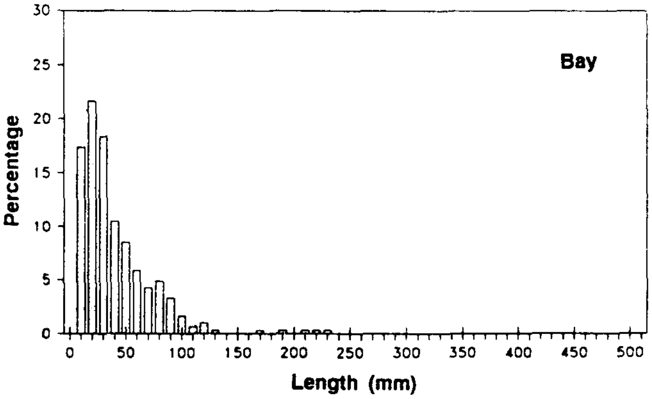


Figure 4. Overall size distribution of California halibut (*Paralichthys californicus*) in beam trawl surveys along the southern California coast, April–September 1989.



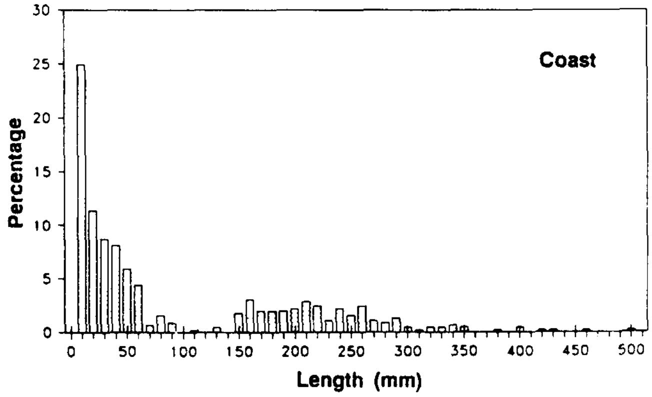


Figure 5. Size distribution of California halibut (*Paralichthys californicus*) in bay and coastal habitats along the southern California coast, April–September 1989.

The densities of size groups varied from site to site (figure 7). Settling and recently settled individuals (<21 mm) were most dense in Anaheim Bay, followed by Hermosa Beach and Carlsbad. The densities of this size group were low at Long Beach and San Onofre, and the group was absent at Agua Hedionda Lagoon. Larger age 0 fish (21–100 mm) were more than twice as dense in Anaheim Bay as at Hermosa Beach, the area with the next greatest density. Densities of this size group were low at Long Beach and Agua Hedionda Lagoon, and the group was virtually absent at Carlsbad and San Onofre. Older fish (>100 mm) were most dense at Hermosa Beach, followed by Carlsbad and Long Beach. The density of this size group was low in Agua Hedionda Lagoon and Anaheim Bay, and at San Onofre. In areas where it was present, the 21 to 100-mm group dominated.

Changes in the population structure of California halibut with time differed between bay and coastal

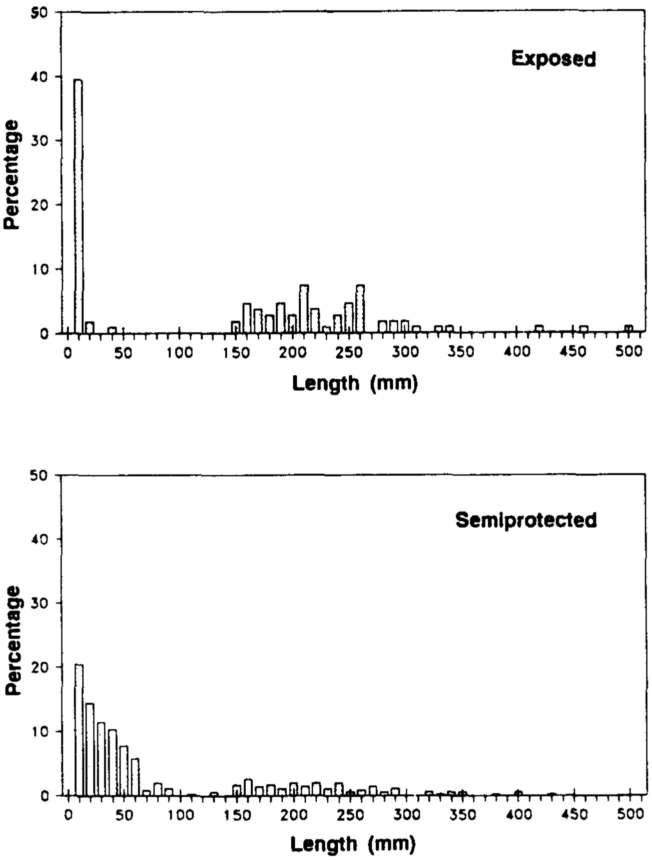


Figure 6. Size distribution of California halibut (*Paralichthys californicus*) in exposed and semiprotected coastal habitats along the southern California coast, April–September 1989.

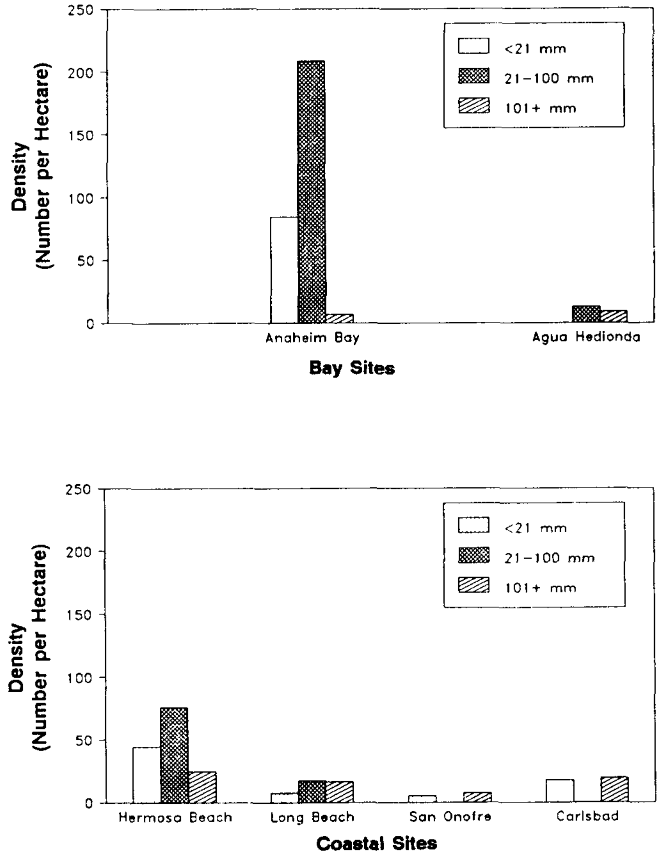


Figure 7. Mean density (number of fish/ha) of juvenile California halibut (*Paralichthys californicus*) at sampling sites off southern California, April–September 1989.

habitats (figures 8 and 9). In bays the primary mode consisted of 10-mm fish from April to June, and 20- to 40-mm fish from July to September (figure 8). Fish of 50 mm SL or less were relatively abundant in all months, and some larger fish were occasionally captured. In August and September the percentage of 50- to 100-mm fish increased.

In the coastal habitat, the 10-mm size class predominated from April to July, the 20-mm class in August, and the 50-mm class in September (figure 9). Although fish greater than 150 mm were present in all months, individuals of 20 to 100 mm SL were rare until August and September, when they became abundant. However, this size group was found only at semiprotected coastal sites (Hermosa Beach and Long Beach).

The settlement of California halibut, as indicated by the monthly density of fish <21 mm, varied by site and time (figure 10). From April to June and in September, settlement was highest at Anaheim Bay; from July to August it was highest at Hermosa Beach. In Anaheim Bay settlement was already high in April and increased greatly from May to June. Settlement dropped dramatically in July, then returned to relatively high levels again in August and September. At Hermosa Beach settlement showed a gradual increase from April to August and fell to very low levels in September. Settlement was greatest in May at Long Beach, and in July at Carlsbad and San Onofre.

California halibut settled into different locations at different temperatures (figure 11). In Anaheim Bay, settlement was high from 18.5° to 22.3°C, with a peak at 21°C. Settlement dropped dramatically at 22.5°C (in July). However, at Hermosa Beach, settlement occurred between 15.5° and 19.5°C, with a peak at 16.8°C. At Carlsbad settling halibut were found from 17° to 20.5°C, at Long Beach primarily

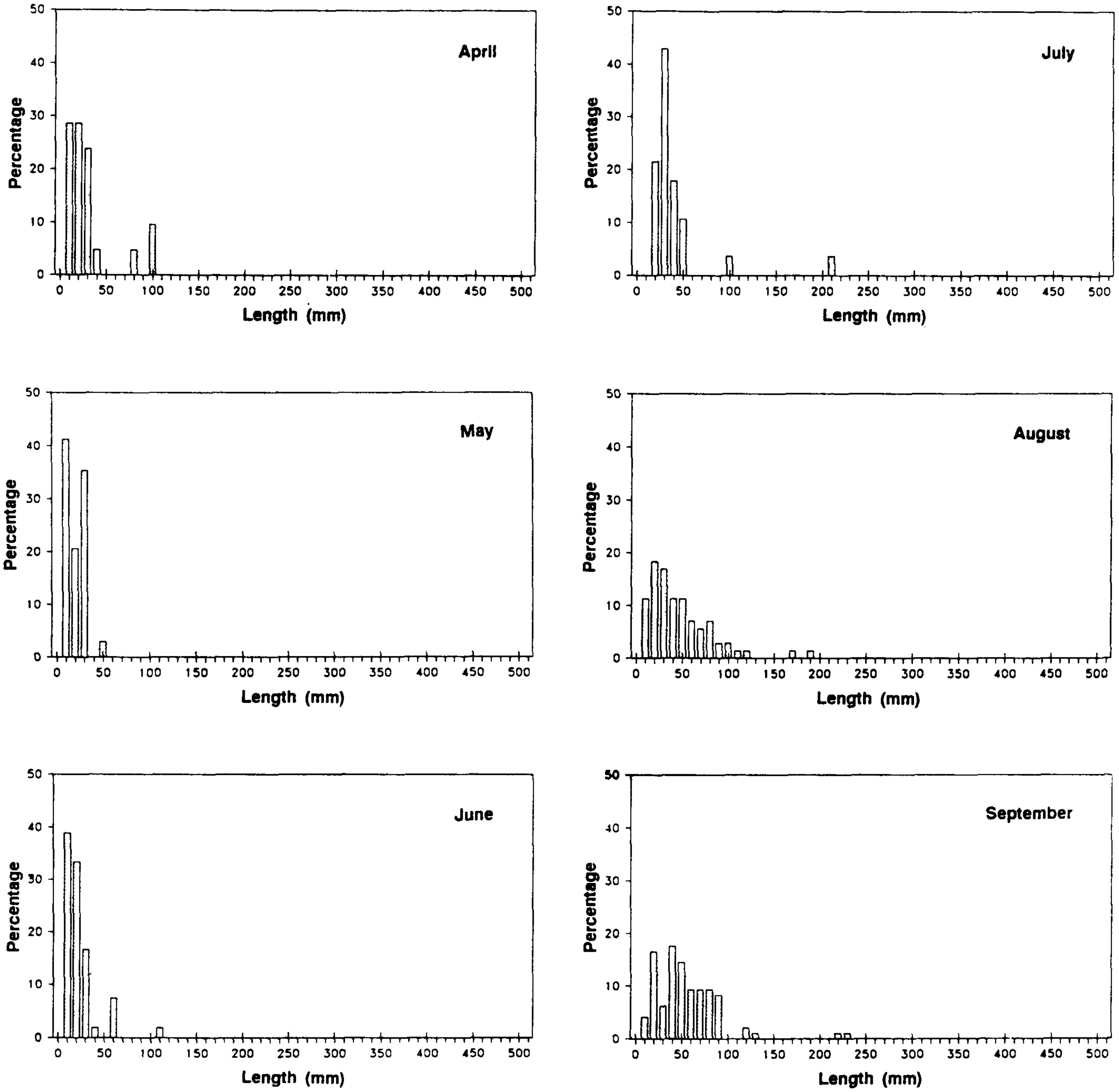


Figure 8. Monthly size distribution of California halibut (*Paralichthys californicus*) in bay habitats of southern California, 1989.

at 17°C, and at San Onofre at 21.2°C. The regression of density of settling individuals versus temperature ($y = 1.680x - 5.3$; $r = 0.0096$; $r^2 = 0.009$; d.f. = 34; figure 12) was not significant at $p = 0.05$. The regression of the log ($x + 1$) density versus temperature ($y = -0.02x + 1.436$; $r = 0.088$; $r^2 = 0.007$; d.f. = 34) was also not significant.

DISCUSSION

Juvenile California halibut have been known for some time to occur in bays of southern California. Haaker (1975) found large numbers of juveniles in Anaheim Bay and suggested that they remain there until they reach about 200 mm, at which time they emigrate to the coast. Nets used in Haaker's study

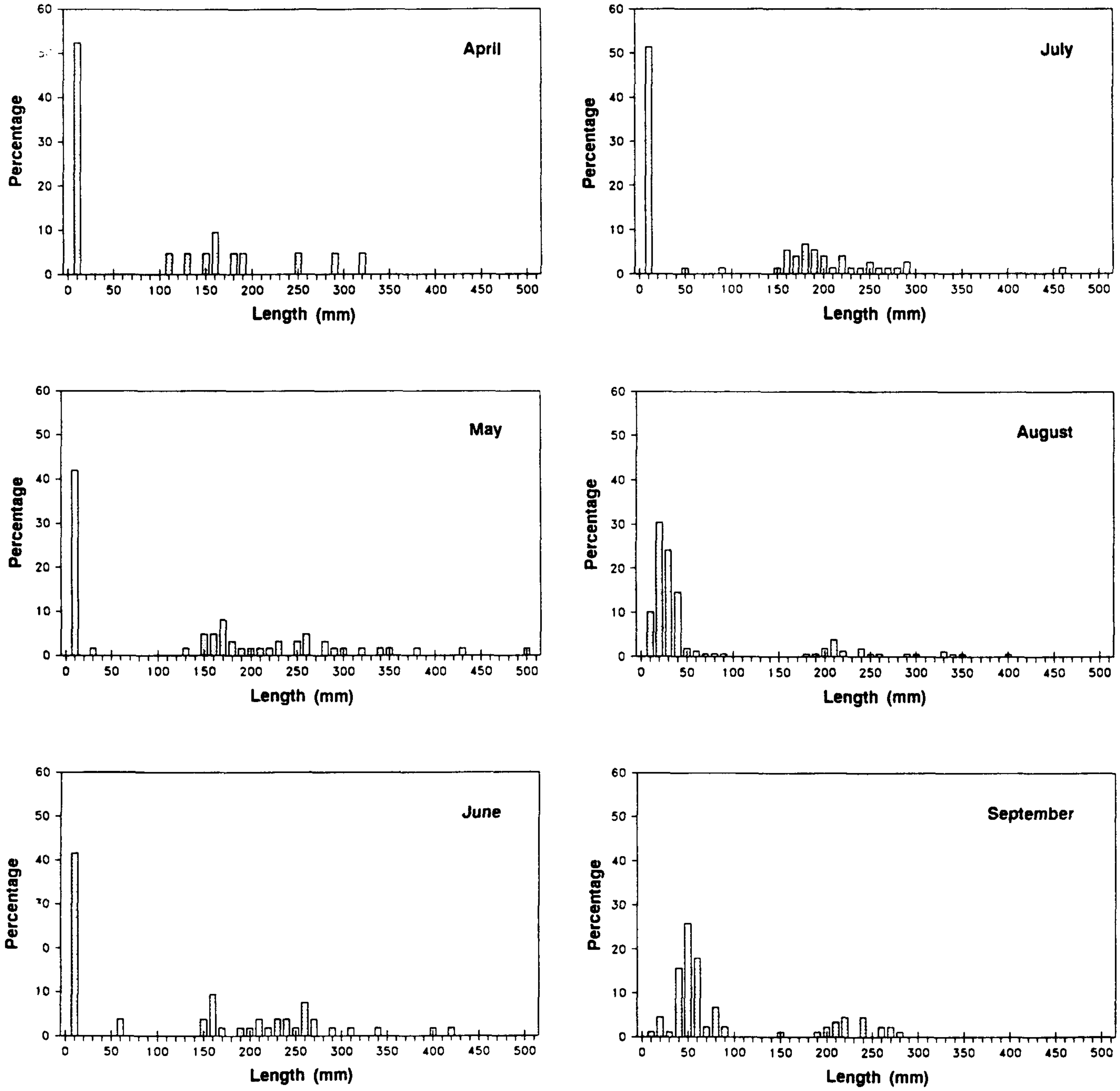


Figure 9. Monthly size distribution of California halibut (*Paralichthys californicus*) in coastal habitats off southern California, 1989.

included bag seines, commonsense seines, and small otter trawls with minimum mesh sizes of 3–6 mm (Klingbeil et al. 1975).

No California halibut smaller than 150 mm were found along the coast in 1972–73 in an extensive otter trawl survey (342 samples) (M.J. Allen 1982). However, that study sampled depths from 10 to 200 m using small otter trawls with a minimum stretch-mesh size of 12.5 mm. Using similar gear, Plummer et al. (1983) surveyed the nearshore zone off San Onofre and Oceanside at depths of 6–30 m. They captured 1,580 California halibut, but fewer than 2% were shorter than 100 mm SL. Fish smaller than 100 mm are seasonally abundant in Elkhorn Slough, Mugu Lagoon, Alamitos Bay, Anaheim Bay, Newport Bay, Agua Hedionda Lagoon, and Mission Bay

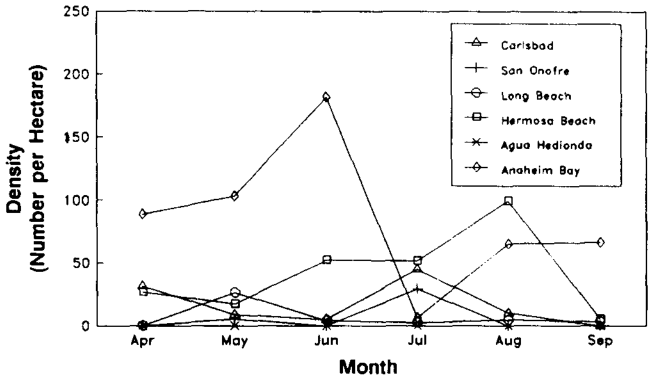


Figure 10. Monthly density (number of fish/ha) of recently settled (SL <21 mm) juvenile California halibut (*Paralichthys californicus*) at sampling sites off southern California, 1989.

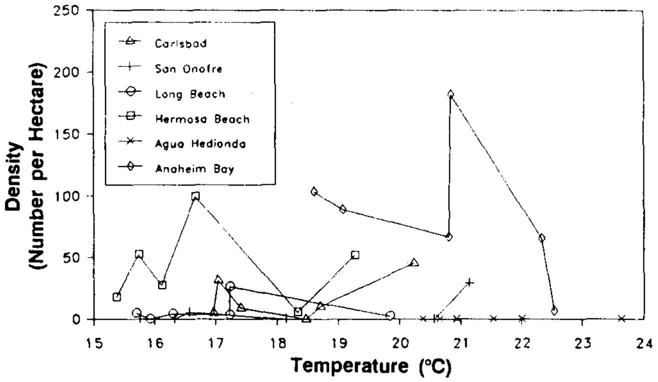


Figure 11. Relationship of density (number of fish/ha) of recently settled (SL <21 mm) juvenile California halibut (*Paralichthys californicus*) to temperature at sampling sites off southern California, April–September 1989.

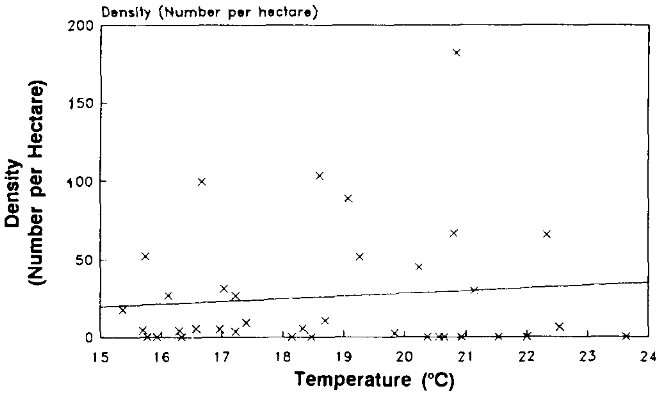


Figure 12. Relationship of density (number of fish/ha) of recently settled juvenile (SL <21 mm) California halibut (*Paralichthys californicus*) to temperature in surveys off southern California, April–September 1989.

(Haaker 1975; Plummer et al. 1983; Kramer and Hunter 1987; L.G. Allen 1988a; Kramer and Hunter 1988; Kramer, in press). Plummer et al. (1983) found no California halibut of <205 mm SL at depths less than 5 m (the surf zone) off northern San Diego County, although individuals have occasionally been noted there. The junior author of this study has observed small California halibut near the surf zone at Torrance.

Using seines, beam trawls, and otter trawls with a minimum mesh size of 2 mm, L.G. Allen (1988a) found recently settled California halibut in protected (Alamitos Bay) and semiprotected (Long Beach Harbor) habitats. With the same gear as that used in the present study, Kramer and Hunter (1987) found that California halibut <50 mm SL were abundant in Agua Hedionda Lagoon and Mission Bay in 1987; however, individuals of this size were rare along the open coast at depths of 6–14 m. In 1988 densities were again relatively high in Agua Hedionda Lagoon and Mission Bay, and had increased in coastal areas, although they were low in San Diego Bay (Kramer and Hunter 1988). Coastal settlement was highest at La Jolla and lowest at San Onofre (Kramer, in press). L.G. Allen et al. (in press) also found coastal settlement of California halibut in 1988. Among nearshore areas at depths of 5–10 m from Point Conception to San Mateo Point, age 0 California halibut (i.e., <80 mm) were most abundant in southeastern Santa Monica Bay (El Segundo and Malaga Cove) and Long Beach Harbor (Belmont Shore); settlement was highest in July. That study used the same type of beam trawl we used along the coast for this survey, with four 5-min tows at a station.

Kramer (in press) compared the selectivity of various gears used in studies of small California halibut. Beach seines were found to be less effective than the 1.0-m beam trawl in capturing juvenile halibut in bays. The 1.0- and 1.6-m beam trawls were equally effective at capturing halibut <80 mm SL; however, the 1.6-m beam trawl was more effective than the 1.0-m beam trawl for collecting fish >80 mm. In the study reported here, the lack of large fish in bay catches is probably at least partly due to the decreased effectiveness of the 1.0-m trawl used in bays, compared to the 1.6-m trawl used along the coast. Kramer (in press) also found that the 1.6-m beam trawl captured a greater proportion of halibut <200 mm SL, whereas the standard 7.6-m otter trawl used in southern California coastal surveys captured more halibut >200 mm. This may partly account for the low catches of halibut of this size in otter trawl surveys (e.g., M.J. Allen 1982; Plummer et al. 1983).

The present study confirms the findings of L.G. Allen (1988a) that juvenile California halibut are more abundant in bays and semiprotected coastal locations than in exposed coastal locations. The present study also substantiates findings of other studies (Kramer and Hunter 1987, 1988; L.G. Allen et al., in press; Kramer, in press) indicating that halibut do settle in some places along the open coast in some years. It also supports L.G. Allen et al. (in press), who found the greatest coastal settlement in southeastern Santa Monica Bay and Long Beach Harbor. It also confirms studies (Kramer and Hunter 1987; L.G. Allen 1988a; Kramer and Hunter 1988; Kramer, in press) indicating that successful settlement along the coast is low in the San Onofre–Carlsbad region.

L.G. Allen (1988a) found that the Los Angeles–Long Beach Harbor provides a nursery for California halibut. This finding is not entirely unexpected, because breakwaters protect this area from swells and large waves from the west. However, the importance of southeastern Santa Monica Bay as a nursery is less obvious. Southeastern Santa Monica Bay is an open coast that is directly exposed to western swells (Maloney and Chan 1974). However, in mid to late summer the area is protected from the predominant south and southwestern swells by the Palos Verdes Peninsula (Maloney and Chan 1974).

Juvenile California halibut have previously been observed in this area, but were not documented until recently. As noted above, small California halibut have been observed at Torrance in the past, and in some years juveniles have been found in the seawater cooling system of the Redondo Generating Station (M.D. Curtis, MBC Applied Environmental Sciences, Costa Mesa, Calif., pers. comm.). In 1988 settlement was particularly high at Malaga Cove and El Segundo (L.G. Allen et al., in press); these sites were north and south, respectively, of the Hermosa Beach site used in this study. We found settling halibut at Hermosa Beach from April to September. However, survival (or residency) of these fish only occurred there from August to September, when the Palos Verdes Peninsula provided the greatest protection from southern swells.

At least two semiprotected coastal areas of southern California — southeastern Santa Monica Bay and Los Angeles–Long Beach Harbors — seem to provide suitable nursery grounds for California halibut in some years, particularly late in the summer. The coast north of the La Jolla Peninsula may be another suitable area that is somewhat protected from swells from the south. Kramer (in press) found greater halibut settlement there (at Torrey Pines) than at San Onofre.

The site with the greatest density of recently settled California halibut in this study was Anaheim Bay. The density there in 1989 was about 25% of that found in Alamitos Bay, which had the greatest density from 1983 to 1985 (L.G. Allen 1988a). Compared with sites sampled in 1987 (Kramer and Hunter 1987), the density of halibut at Anaheim Bay in 1989 was about 50% of that found in Agua Hedionda Lagoon (the area of greatest settlement) and was similar to that of Mission Bay (Kramer and Hunter 1987). Settlement in Mission Bay and Agua Hedionda Lagoon dropped in 1988. In 1989 no recently settled halibut were taken at Agua Hedionda Lagoon, although larger age 0 fish (21–100 mm SL) were present at low densities. Thus settlement of California halibut to bays varies internannually, and the importance of a given bay as a nursery changes from year to year.

In this study the density of recently settled California halibut along the coast at Hermosa Beach was about 50% of that found at Anaheim Bay. The density at Hermosa Beach in 1989 was about 100 times that found along the open coast off San Diego County in 1987 (Kramer and Hunter 1987) and 2–3 times greater than that found there in 1988 (Kramer and Hunter 1988). It was slightly less than that found at Malaga Cove in 1988 (L.G. Allen et al., in press).

Kramer (in press) noted that although settlement might be high along coastal San Diego County early in the year, few halibut of 40–60 mm were taken later. Possible reasons for their absence include mortality, dispersal to deep water, and dispersal to bays. Kramer surmised that dispersal to bays was most likely because (1) there is no evidence of dispersal to deep water, and (2) more fish of 21–100 mm were found in bays than fish <21 mm, indicating that settlement must be occurring elsewhere. However, we believe that mortality may be an important factor in exposed coastal areas where there are no suitable bay habitats.

The relatively high survival (or residency) of age 0 fish near Hermosa Beach in 1989 may be related to the proximity of King Harbor, which may provide needed protection, especially in times of greater exposure.

L.G. Allen (in press) found that log-transformed abundance of settling halibut along the coast was significantly and positively correlated with temperature. That study concluded that temperature has a significant influence on the settlement and subsequent distribution of age 0 halibut. But the present survey did not show any significant correlation with temperature. This lack of correlation may partly reflect the more heterogeneous study area that includes both bay and coastal sites. However, in the present study settlement was often greatest at intermediate temperatures at a given site (e.g., Anaheim Bay, Hermosa Beach; figure 11). Other physical or biological factors are apparently more important than temperature in determining settlement at some sites.

Variations in pH are generally directly related to dissolved oxygen (DO) levels (Parsons and Takahashi 1973). When DO levels are low, pH values are generally low; when DO values are high, pH levels are high. Both DO levels and pH values are high when photosynthetic rates are high, and both are low when respiration predominates. The high pH observed along the coast in August and September and in the bays in May, July, and August may reflect high photosynthesitic activities. There does not appear to be any obvious relationship of settlement to pH. Settlement was highest at Anaheim Bay during the period of lowest pH but was highest at Hermosa Beach during a period of high pH (table 1; figure 10).

CONCLUSIONS

California halibut settled from the plankton to the bottom in shallow coastal areas off southern California in 1989 in varying densities depending on habitat type.

Transforming larvae were found throughout the study area, but the abundance (survival and residency) of small juveniles was highest in the bays, intermediate in semiprotected coastal areas, and lowest in exposed coastal areas.

Benthic settlement diminished in August and September, when larger young-of-the-year became more abundant; these larger fish did not appear at exposed coastal locations.

The results of this study substantiate the results of other recent studies indicating that southeastern Santa Monica Bay and Los Angeles–Long Beach Harbors are semiprotected coastal areas that are used as nursery areas by California halibut. This study does not, however, substantiate a correlation between temperature and settlement.

ACKNOWLEDGMENTS

This project was funded by Southern California Edison Company as part of its Natural Resources Management Research Program. We would like to thank Chuck Mitchell of MBC Applied Environmental Sciences for providing the personnel, facilities, and technical assistance to conduct this study. The following members of the MBC staff helped in a variety of technical areas: Bob Moore (field chief, lab, data analysis); Tina Moskowitz (lab, field); Mark Burke (field, lab, data analysis); Don Johnston (lab, field, graphics); Janalyn Sanders (data analysis, graphics); and Bill Stockton (data analysis, graphics). The following members of the MBC staff assisted in the lab: Pat Frost, Scott Harris, Jenny Higuera, Diana Budris, Julie Smith, Steve Burke, Lisa Kasper, Leslie Keyt, and Tiffany Woodworth. Other MBC staff members helped in the following areas: graphics (Larry Jones); word processing (Gini Whitt, Phyllis Barton, and Madine Johnson); report editing (Tom Kauwling); and Spanish editing (Kathy Mitchell and Joan Compton).

We would also like to thank Sharon Kramer (now a member of the MBC staff) and John Hunter of the Southwest Fisheries Center, National Marine Fisheries Service, NOAA, La Jolla, California, for lending their beam trawls and meter wheels for use in this project. We appreciate the help of Larry Nufer of Conshelf Services Inc., who was skipper of the R/V *Westwind*. We also appreciate the technical assistance of Marty Golden, who volunteered to help with the project on his own time.

LITERATURE CITED

Allen, L. G. 1988a. Recruitment, distribution, and feeding habits of young-of-the-year California halibut (*Paralichthys californicus*) in the vicinity of Alamitos Bay–Long Beach Harbor, California, 1983–1985. S. Calif. Acad. Sci., Bull. 87(1):19–30.

———. 1988b. OREHP YOY white seabass trawl survey, 1988, Contract #FG7500, quarterly report for April-June 1988. Prepared for Calif. Dep. Fish and Game, Long Beach. Calif. State Univ., Northridge, Calif., 4 pp.

Allen, L. G., R. E. Jensen, and J. R. Sears. In press. Open coast recruitment and distribution of young-of-the-year California halibut (*Paralichthys californicus*) along the southern California coast between Pt. Conception and San Mateo Pt., June–October, 1988. Calif. Dep. Fish Game, Fish. Bull.

Allen, M. J. 1982. Functional structure of soft-bottom fish communities of the southern California shelf. Ph.D. dissertation, Univ. Calif., San Diego, La Jolla, Calif., 577 pp. (available from Univ. Microfilms Internatl., Ann Arbor, MI. Ref. 8300991).

CDFG (California Department of Fish and Game). 1989. Review of some California fisheries for 1988. Calif. Coop. Oceanic Fish, Invest. Rep. 30:7–17.

Haaker, P. L. 1975. The biology of the California halibut, *Paralichthys californicus* (Ayres) in Anaheim Bay. *In* The marine resources of Anaheim Bay, E. D. Lane and C. W. Hill, eds. Calif. Dep. Fish Game, Fish Bull. 165:137–151.

Klingbeil, R. A., R. D. Sandell, and A. W. Wells. 1975. An annotated checklist of the elasmobranchs and teleosts of Anaheim Bay. *In* The marine resources of Anaheim Bay, E. D. Lane and C. W. Hill, eds. Calif. Dep. Fish Game, Fish Bull. 165:79–115.

Kramer. S. H. In press. Distribution and abundance of juvenile California halibut (*Paralichthys californicus*) off San Diego County, California. Calif. Dep. Fish Game, Fish Bull.

Kramer, S. H., and J. R. Hunter. 1987. Southern California wetland/shallow water habitat investigation, annual report for the fiscal year 1987. U.S. Dep. Commerce, NOAA, NMFS, SWFC, La Jolla, Calif., 12 pp.

———. 1988. Southern California wetland/shallow water habitat investigation, annual report for the fiscal year 1988. U.S. Dep. Commerce, NOAA, NMFS, SWFC, La Jolla, Calif., 15 pp.

Maloney, N. J., and K.-M. Chan. 1974. Physical oceanography. *In* A summary of knowledge of the southern California coastal zone and offshore areas. Vol. I, Physical environment. M. D. Dailey, B. Hill, and N. Lansing, eds. Prepared for U.S. Dep. Interior, Bur. Land Mgmt. by So. Calif. Ocean Studies Consortium, Calif. State Univ. & Coll., Dominguez Hills, Calif., Contract no. 08550-CT4-1, pp. 3-1 to 3–65.

MBC Applied Environmental Sciences. 1990. Distribution of juvenile California halibut (*Paralichthys californicus*) and other fishes in bay and coastal habitats of Los Angeles, Orange, and San Diego counties, in 1989. Prepared for S. Calif. Edison Co., Rosemead, Calif. MBC Applied Environ. Sci., Costa Mesa, Calif. 90-RD-09, 74 pp.

NMFS (National Marine Fisheries Service). 1985. Marine recreational fishery statistics survey, Pacific Coast, 1983–1984. U.S. Dep. Commerce, NOAA, NMFS, Washington, D.C., 189 pp.

Parsons, T. R., and M. Takahashi. 1973. Biological oceanographic processes. New York; Pergamon Press, 186 pp.

Plummer, K. M., E. E. DeMartini, and D. A. Roberts. 1983. The feeding habits and distribution of juvenile-small adult California halibut (*Paralichthys californicus*) in coastal waters off northern San Diego County. Calif. Coop. Oceanic Fish. Invest. Rep. 24:194–201.

GENETIC STRUCTURE OF WHITE SEABASS POPULATIONS FROM THE SOUTHERN CALIFORNIA BIGHT REGION: APPLICATIONS TO HATCHERY ENHANCEMENT

DEVIN M. BARTLEY
Department of Animal Science
University of California
Davis, California 95616

DONALD B. KENT
Sea World Research Institute
1700 South Shores Drive
San Diego, California 92109

ABSTRACT

Although some fisheries biologists have expressed concern over the practicality of hatchery enhancement of marine fisheries, hatchery technology has advanced significantly during the 1980s. Marine hatchery enhancement programs are now addressing ecology and genetics as well as animal husbandry. To describe the genetic structure of white seabass and apply the information to hatchery enhancement, we used starch-gel electrophoresis to assess the level and distribution of genetic variability of seabass from nine areas in the Southern California Bight. Average heterozygosity per sample estimates ranged from 0.024 to 0.064. Indices of genetic identity between samples were greater than 0.99. Because only 3% of the total gene diversity was due to intersample differences, and because an estimate of number of migrants exchanging genes among samples was approximately 9 per generation, we believe little population subdivision exists within the study area. Genetic diversity in six hatchery samples of white seabass was 15% lower than that found in the wild, possibly because small numbers of brood stock contribute to mass spawns. Although the genetic structure of progeny from a single mass spawn may differ from wild samples, successive mass spawns increased the genetic variability of the entire hatchery product. Therefore, continuous mass spawning of white seabass over the course of the spawning season appears to be an effective means of preserving genetic diversity.

RESUMEN

A pesar de las dudas expresadas por los biólogos pesqueros con respecto a la utilidad de las piscifactorías para el mejoramiento de las pesquerías marinas, la tecnología de estos criaderos ha mostrado avances significativos en la última década. Hoy día los programas de mejoramiento dirigen su atención hacia la ecología y la genética, así como también hacia la cría de los animales. El nivel y la distribución de la variabilidad genética del róbalo blanco fueron evaluadas con "starch gel" electroforesis horizontal en nueve áreas costeras del sur de California (Southern California Bight). El propósito de este estudio es el de describir la estructura genética del róbalo blanco y utilizar esta información en el mejoramiento de los criaderos. La heterocigosidad promedio observada en cada muestra fue de 0.024 a 0.064. Los índices de identidad genética entre muestras fueron mayores de 0.99. Dado que el 3% de la heterocigosidad total se debió a las diferencias entre las muestras y dado que el número estimado de individuos migratorios con intercambio genético entre las muestras fue aproximadamente de 9, creemos que existe muy poca subdivisión en la población del área estudiada. La diversidad genética en seis muestras de róbalo de criadero fue 15% menor que la observada en la población silvestre. Esto se debe probablemente al número reducido de reproductores utilizados en los desoves en masa. A pesar de que la estructura genética de la progenie obtenida en cada desove en masa pueda diferir de las muestras de poblaciones silvestres, desoves consecutivos incrementaron la variabilidad genética cuado se considera la producción total del criadero. Por lo tanto, los desoves consecutivos durante el período del desove del róbalo blanco parece ser un método efectivo para mantener la diversidad genética en los criaderos.

INTRODUCTION

At the 1988 meeting of the California Cooperative Oceanic Fisheries Investigations, some attendees expressed concern about the practicality of hatchery enhancement of marine fisheries. In light of past attempts at enhancement, much of this concern is justified. Many early hatchery enhancement programs had no demonstrable effect on the target population (see references in MacCall 1989). In the case of Pacific salmon, many hatchery programs were developed to mitigate habitat degradation resulting from water development projects (Netboy 1974). Clearly, failing to evaluate a hatchery's contribution to the target population, and using hatcheries to justify habitat destruction are unacceptable.

[Manuscript received February 1, 1990.]

However, hatchery enhancement programs are now addressing ecological and genetic concerns as well as animal husbandry, and evaluation programs are being implemented (Rutledge 1989). Biochemical genetics has become one important tool in the study of fishery biology and aquaculture (Ryman and Utter 1987). Applying biochemical genetic techniques to hatchery enhancement will help the hatcheries produce viable fish by preserving genetic diversity (Kincaid 1983; Allendorf and Ryman 1987). At the same time, the use of genetic markers will increase fishery scientists' ability to evaluate hatchery contribution to wild populations (Murphy et al. 1983; Seeb et al. 1986).

The objective of the research reported here is to apply biochemical and population genetic techniques to hatchery enhancement efforts on white seabass, *Atractoscion nobilis* (Scianidae), in the Southern California Bight region. Numbers of this species in the Southern California Bight have been declining, and the species has almost completely disappeared from the central California waters that historically supported the center of the commercial fishery (Skogsberg 1939; Vojkovich and Reed 1983). We use allozyme data from horizontal starch-gel electrophoresis of soluble proteins to describe the level and distribution of genetic diversity in natural and cultured populations of white seabass. Although these techniques have been used to describe the population genetic structure of Gulf of Mexico Scianidae (Ramsey and Wakeman 1987), genetic studies of marine fish in the Southern California Bight have, until now, neglected white seabass (Waples 1987).

METHODS

Collections and Samples

We collected 13 samples of juvenile and adult white seabass from the Southern California Bight region using gill nets of approximatley 3.8-, 7.6-, and 8.9-cm mesh. Specific locations and dates of collections were: Point Loma 1988 (Sample size [N] = 40); Point Loma 1987 (N = 50); Mission Bay, San Diego 1988 (N = 36); La Jolla 1988 (N = 23); La Jolla 1987 (N = 90); Encinitas 1988 (N = 5); Encinitas 1987 (N = 100); San Onofre 1988 (N = 35); San Onofre 1987 (N = 16); Seal Rock, San Clemente 1988 (N = 51); San Mateo Point, San Clemente 1988 (N = 34); Dana Point 1988 (N = 17); and Laguna Beach 1988 (N = 13). Muscle and liver tissue were dissected from the fish in the field and frozen in liquid nitrogen. Fish from Mission Bay were dissected at Sea World Reseach Institute, and tissue samples were frozen and stored at −70° C.

We also collected juvenile white seabass from six separate mass spawns at Sea World Research Institute (SWRI): one sample in 1985 (N = 21); one sample in 1986 (N = 72); and four samples in 1988 (N's = 30, 26, 27, and 36). These hatchery samples were designated SWRI-1 through SWRI-6. All six hatchery samples of white seabass descended from approximately 20 brood stock maintained in a 3.0 x 6.0 x 1.2-m pool (19,000 liters) with recirculating seawater. Adult white seabass were induced to spawn by photoperiod and temperature manipulation. Eggs were collected by siphon, and larvae hatched after 2–3 days. White seabass progeny were fed a combination of rotifers, *Artemia*, and commercially available trout feed until they were approximately 9 cm long, at which point they were frozen whole and stored at −70°C. Muscle and liver tissues were dissected and stored at −70°C before electrophoresis.

Electrophoresis

Horizontal starch-gel electrophoresis followed standard procedures (Aebersold et al. 1987). Enzyme systems, tissue distribution, and number of loci are presented in table 1. Gels were made with 12% hydrolyzed potato starch (Sigma Chemical Co.) combined with one of the following buffer systems: AC, an amine citrate buffer from Clayton and Tretiak (1972) adjusted to pH 7.0; R, a discontinuous buffer system (pH 8.0) described by Ridgeway et al. (1970); and MF, a boric acid-Tris system (pH 8.4) from Markert and Faulhaber (1965).

For histochemical staining procedures we followed Shaw and Prasad (1970) and Harris and Hopkinson (1976). For nomenclature we followed Allendorf and Utter (1979); in this system, the most common allele at a locus is designated the 100 allele, and variant alleles are assigned numeric values based on their anodal migration distance relative to the 100 allele. Enzyme abbreviations are in uppercase letters; alleles are designated by the italicized enzyme abbreviation followed by locus number with allelic mobility in parentheses.

Data Analysis

We assessed genetic variability of each fish sample by calculating the frequencies of alleles at each locus, the percentage of polymorphic loci (P), and average heterozygosity (H) (Nei 1973). A locus was considered polymorphic if we observed one variant allele. We calculated genetic identities (I) for each pair of

TABLE 1

Enzyme Systems and Isozyme Loci Analyzed from Liver (L) and Muscle (M) Tissue of White Seabass

Enzyme system	Abbreviation	E.C. number	Number of loci	Tissue	Buffer*
Acid phosphatase	*Acp*	3.1.3.2	2	M,L	MF
Aconitate hydratase	*Ah*	4.2.1.3	2	L	AC
Adenylate kinase	*Ak*	2.7.4.3	1	M	AC
Alcohol dehydrogenase	*Adh*	1.1.1.1	1	L	AC,MF
Aspartate amino transferase	*Aat*	2.6.1.1	2	M,L	AC,R
Creatine kinase	*Ck*	2.7.3.2	1	M	R
Esterase	*Est*	3.1.1.1	2	M,L	R
Glyceraldehyde-3-phosphate dehydrogenase	*Gapdh*	1.2.1.12	2	M	AC
Glycerol-3-phosphate dehydrogenase	*Gpd*	1.1.1.8	2	M	AC
Glucose phosphate isomerase	*Gpi*	5.3.1.9	3	M	R
Iditol dehydrogenase	*Iddh*	1.1.1.14	1	L	R
Isocitrate dehydrogenase	*Idh*	1.1.1.42	2	M,L	AC
Lactate dehydrogenase	*Ldh*	1.1.1.27	1	M	R
Malate dehydrogenase	*Mdh*	1.1.1.37	1	M,L	AC
Malic enzyme	*Me*	1.1.1.40	1	M,L	MF
Mannose phosphate isomerase	*Mpi*	5.3.1.8	1	M,L	R,MF
Phosphogluconate dehydrogenase	*6 Pdg*	1.1.1.44	1	M,L	AC
Phosphoglucomutase	*Pgm*	5.4.2.2	1	M,L	AC
Superoxide dismutase	*Sod*	1.15.1.1	1	M,L	MF,R
Triosphosphate isomerase	*Tpi*	5.3.1.1	2	M	MF
Peptidase					
Glycyl leucine	*Dpep*	3.4.13.11	2	M	R
Leucyl glycyl glycine	*Tapep*	3.4.11.4	1	M	R

*Described in text

samples (Nei 1978) and constructed a dendrogram from estimates of I with the unweighted pair-group method (UPGMA; Sneath and Sokal 1973) to examine the relative similarities among populations.

We partitioned total gene diversity (H_T) of wild and hatchery samples to estimate within-sample (H_S) and between-sample (D_{ST}) components and relative gene diversity (G_{ST}) (Nei 1973; Chakraborty and Leimar 1987). To ascertain significant subpopulation structure within the study area, we used a chi-square test to determine if Wright's F_{ST} (1943) was significantly different from zero, as described by Waples (1987). We used G_{ST} to approximate F_{ST} (Nei 1977; Slatkin and Barton 1989). Tests of independence between allelic frequency and location were performed by log likelihood G test (Sokal and Rohlf 1981). G tests were also performed on allelic frequencies and mass spawning samples to determine if significant genetic differences existed among the six hatchery samples. Quantitative estimates of gene flow were calculated from Wright's (1943) fixation index

$$F_{ST} = 1/(4Nm + 1) \quad (1)$$

where Nm is the average number of migrants per generation. Equation 1 was solved for Nm by setting F_{ST} equal to the relative gene diversity (G_{ST}) estimate (Nei 1977). Equation 1 will provide an estimate of the number of migrant fish exchanging genes among samples per generation under the assumptions of selective neutrality of alleles and Wright's (1943) island model of migration. Slatkin and Barton (1989) discussed the sensitivity of equation 1 to various methods of estimating F_{ST}, some selection, and population structure, and found it to be fairly robust.

RESULTS

Allozyme Variation of Wild Samples

We detected allozyme variation in 19 of 33 loci (table 2). The distribution of alleles in wild samples of white seabass was generally homogeneous throughout the study area, except for rare alleles in specific locations. For example, we observed the *Gpd-2 (−133)* allele only in the 1987 sample from San Onofre. In addition, IDH, ME, and MPI polymorphisms were observed in 1988 and not in 1987 samples. The distribution of rare alleles did not follow an obvious pattern. Heterozygosity estimates ranged from 0.033 in the Encinitas 1988 sample to

TABLE 2

Allelic Frequencies at 19 Polymorphic Gene Loci from 13 Wild and 6 Hatchery Samples of White Seabass

		Pt. Loma 1988	Pt.Loma 1987	Mission Bay 1988	La Jolla 1988	La Jolla 1987	Encinitas 1988	Encinitas 1987	San Onofre 1988	San Onofre 1987
Aat-1	(100)	0.975	0.906	0.930	1.000	0.909	0.900	0.870	0.886	0.860
	(115)	0.025	0.021	0.028		0.025	0.100	0.019	0.071	0.031
	(50)		0.073	0.042		0.066		0.111	0.043	0.109
	*N**	40	48	36	23	90	5	79	35	16
Aat-2	(100)	0.872	0.995	0.944	0.935	0.945	1.000	0.992	0.914	1.000
	(125)	0.103	0.005	0.028	0.043	0.049		0.004	0.086	
	(65)	0.026		0.028	0.022	0.006		0.004		
	N	39	50	36	23	86	5	100	35	16
Adh	(−100)	0.462	0.443	0.486	0.304	0.543	0.500	0.438	0.443	0.385
	(−200)	0.487	0.534	0.514	0.652	0.429	0.500	0.531	0.557	0.615
	(−300)	0.051	0.023		0.043	0.028		0.031		
	N	39	44	36	23	35	5	81	35	13
Est-4	(100)	0.936	0.896	0.861	0.978	0.927	1.000	0.950	0.914	0.962
	(89)		0.010	0.056		0.028		0.025		0.038
	(115)	0.064	0.094	0.083	0.022	0.045		0.025	0.086	
	N	39	48	36	23	89	5	40	35	13
Est-5	(100)	0.938	1.000	0.875	0.957	0.925	1.000	1.000	0.900	0.937
	(103)	0.063		0.083	0.043	0.050			0.071	
	(95)			0.042		0.025			0.029	0.063
	N	40	20	36	23	40	5	40	35	16
Gpd-2	(−100)	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	0.967
	(−133)									0.033
	N	40	50	36	23	90	5	100	35	16
Gpi-1	(100)	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(−140)									
	N	40	50	36	23	90	5	80	35	16
Gpi-2	(100)	0.975	1.000	0.958	1.000	0.994	1.000	1.000	1.000	1.000
	(85)	0.025				0.006				
	(122)			0.042						
	N	40	50	36	23	90	5	80	35	16
Gpi-3	(100)	1.000	1.000	1.000	1.000	0.994	1.000	0.987	1.000	1.000
	(78)					0.006		0.013		
	(112)									
	N	40	50	36	23	90	5	80	35	16
Iddh-1	(100)	0.988	0.970	1.00	0.957	0.970	1.000	0.990	0.943	1.000
	(−400)				0.043	0.030		0.005	0.043	
	(700)	0.012	0.030					0.005	0.014	
	N	40	50	36	23	50	5	100	35	16
Idh-1	(100)	1.000	1.000	0.986	0.978	1.000	1.000	1.000	0.986	1.000
	(82)			0.014	0.022				0.014	
	N	40	50	36	23	90	5	100	35	16
Idh-2	(100)	0.975	1.000	0.986	1.000	1.000	1.000	1.000	0.986	1.000
	(119)	0.025		0.014					0.014	
	(62)									
	N	40	50	36	23	90	5	100	35	16
Ldh-3	(100)	0.950	1.000	0.972	0.978	0.983	1.000	0.980	0.986	0.934
	(40)	0.025		0.014		0.017		0.015	0.014	0.033
	(150)	0.025		0.014	0.022			0.005		0.033
	N	40	50	36	23	88	5	100	35	16
Me-1	(100)	0.975	1.000	1.000	0.935	1.000	1.000	1.000	1.000	1.000
	(116)	0.025			0.065					
	N	40	50	36	23	90	5	100	35	16
Mpi	(100)	0.949	1.000	0.917	0.978	1.000	1.000	1.000	0.971	1.000
	(110)	0.051		0.083	0.022				0.029	
	N	39	50	36	23	90	5	100	35	16
6Pgd	(100)	1.000	0.980	1.000	1.000	1.000	1.000	0.995	1.000	1.000
	(95)		0.020					0.005		
	N	40	50	36	23	90	5	90	35	16
Pgm-1	(−100)	0.224	0.410	0.403	0.239	0.250	0.300	0.237	0.371	0.447
	(−167)	0.750	0.590	0.555	0.761	0.750	0.700	0.750	0.586	0.553
	(−200)	0.026		0.042				0.013	0.043	
	N	38	50	36	23	10	5	40	35	16
Sod	(100)	0.988	1.000	0.986	1.000	0.989	1.000	1.000	1.000	1.000
	(109)	0.012		0.014		0.011				
	N	40	48	36	23	88	5	100	35	16
Ah-2	(100)	1.000	1.000	1.000	1.000	1.000	1.000	0.990	1.000	1.000
	(98)							0.010		
	N	40	50	36	5	90	5	99	35	16
H		0.056	0.045	0.064	0.043	0.048	0.033	0.041	0.060	0.048
P		0.39	0.21	0.36	0.30	0.33	0.09	0.30	0.33	0.21

*Sample size

H = average heterozygosity; *P* = proportion polymorphic loci

(*continued on next page*)

TABLE 2 (*continued*)

		Seal Rock 1988	San Mateo Pt. 1988	Dana Pt. 1988	Laguna Beach 1988	SWRI-1 1985	SWRI-2 1986	SWRI-3 1988	SWRI-4 1988	SWRI-5 1988	SWRI-6 1988
Aat-1	(100)	0.800	0.783	0.941	1.000	0.658	1.000	0.917	0.923	0.833	0.853
	(115)	0.030	0.152	0.059		0.250		0.033		0.019	0.015
	(50)	0.170	0.065			0.092		0.050	0.077	0.148	0.132
	*N**	50	23	17	13	21	72	30	26	27	34
Aat-2	(100)	0.908	0.912	0.969	0.923	1.000	1.000	0.967	1.000	0.963	0.686
	(125)	0.071	0.059	0.031	0.077					0.037	0.300
	(65)	0.020	0.029					0.033			0.014
	N	49	34	16	13	21	35	30	26	27	35
Adh	(−100)	0.357	0.411	0.375	0.538	0.833	0.910	0.517	0.923	1.000	0.588
	(−200)	0.633	0.574	0.594	0.462	0.167	0.090	0.483	0.077		0.412
	(−300)	0.010	0.015	0.031							
	N	51	34	16	13	21	72	30	26	27	34
Est-4	(100)	0.930	0.941	0.938	0.962	1.000	1.000	1.000	1.000	1.000	0.958
	(89)	0.020	0.029								
	(115)	0.050	0.029	0.062	0.038						0.042
	N	50	34	16	13	21	72	30	26	27	36
Est-5	(100)	0.940	0.958	0.912	1.000	1.000	1.000	1.000	1.000	1.000	0.771
	(103)	0.040	0.021	0.088							
	(95)	0.020	0.021								0.229
	N	49	24	17	13	21	72	30	26	27	35
Gpd-2	(−100)	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(−133)										
	N	51	34	17	13	21	72	30	26	27	35
Gpi-1	(100)	1.000	1.000	1.000	1.000	1.000	0.708	1.000	1.000	1.000	1.000
	(−140)						0.292				
	N	51	34	17	13	21	72	30	26	27	36
Gpi-2	(100)	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(85)										
	(122)										
	N	51	34	17	13	21	72	30	26	27	35
Gpi-3	(100)	1.000	0.971	0.971	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(78)		0.029								
	(112)			0.029							
	N	51	34	17	13	21	72	30	26	27	35
Iddh-1	(100)	0.979	0.985	1.000	0.924	1.000	1.000	1.000	1.000	1.000	1.000
	(−400)	0.021	0.015		0.038						
	(700)				0.038						
	N	48	34	16	13	21	72	30	26	27	35
Idh-1	(100)	1.000	0.957	1.000	1.000	1.000	1.000	0.983	1.000	0.870	1.000
	(82)		0.043					0.017		0.130	
	N	51	23	17	13	21	72	30	26	27	36
Idh-2	(100)	1.000	0.979	1.000	1.000	1.000	1.000	1.000	1.000	0.944	1.000
	(119)										
	(62)		0.021							0.056	
	N	51	24	17	13	21	72	30	26	27	36
Ldh-3	(100)	0.990	1.000	0.941	1.000	1.000	0.972	1.000	1.000	1.000	1.000
	(40)	0.010		0.029			0.028				
	(150)			0.029							
	N	51	34	17	13	21	72	30	26	27	36
Me-1	(100)	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(116)										
	N	51	34	17	13	21	72	30	26	27	36
Mpi	(100)	0.957	0.850	0.969	1.000	1.000	0.965	1.000	1.000	1.000	1.000
	(110)	0.043	0.150	0.031			0.035				
	N	47	30	16	13	21	72	30	26	27	36
6Pgd	(100)	0.990	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(95)	0.010									
	N	50	34	17	13	21	72	30	26	27	36
Pgm-1	(−100)	0.352	0.221	0.406	0.192	0.857	0.743	0.283	0.462	0.442	0.750
	(−167)	0.637	0.735	0.594	0.769	0.143	0.257	0.667	0.538	0.558	0.250
	(−200)	0.011	0.044		0.038			0.050			
	N	44	34	16	13	21	72	30	26	26	32
Sod	(100)	1.000	0.985	0.971	1.000	1.000	1.000	1.000	1.000	1.000	1.000
	(109)		0.015	0.029							
	N	38	34	17	13	21	72	30	26	27	36
Ah-2	(100)	1.000	1.000	1.000	0.923	1.000	1.000	1.000	1.000	1.000	1.000
	(98)				0.077						
	N	49	29	11	13	21	72	30	26	27	36
H		0.056	0.064	0.052	0.042	0.033	0.03	0.037	0.024	0.036	0.060
P		0.30	0.36	0.30	0.18	0.09	0.15	0.15	0.09	0.15	0.18

0.064 in the San Mateo Point and the Mission Bay samples. The proportion of polymorphic loci was lowest in the Encinitas 1988 sample and highest in the Point Loma 1988 sample. However, because only five fish were collected from Encinitas in 1988, the estimates of genetic variability may be inaccurate.

Cluster analysis based on Nei's genetic identity estimates failed to reveal any geographic structure to the genetic variation in white seabass (figure 1). Genetic identity values between wild samples were all greater than 0.99.

To determine if population substructuring was occuring in natural populations of white seabass, we examined gene diversity analysis quantifying the amount of genetic variation between (D_{ST}) and within (H_S) samples. For natural populations of white seabass sampled in 1987 and 1988, approximately 97% of the variation was derived from within-sample variability (D_{ST} = 0.0017; H_S = 0.0535; H_T = 0.0552 for 1987 samples, and D_{ST} = 0.0013; H_S = 0.0487; H_T = 0.0500 for 1988 samples) and 3% was due to between-sample differences (G_{ST} = 0.304 for 1987 and 0.0259 for 1988).

In spite of this low level of intersample variability, we did find statistical evidence of differences in genetic variability. Wright's (1943) F_{ST} statistic measures the reduction in heterozygosity observed in a population due to population subdivision and is one method of assessing genetic differences between subpopulations. We used G_{ST} to approximate F_{ST} (Nei 1977) and found highly significant F_{ST} values in both the 1987 (F_{ST} = 0.0304; d.f. = 72; X^2 = 247.953) and 1988 (F_{ST} = 0.0259; d.f. = 240; X^2 = 399) samples. However, tests of independence of allele frequency and location on the 1987 and 1988 data sets revealed significant association of alleles and location only for *Aat-1* in 1988 (G = 29.391; d.f. = 16).

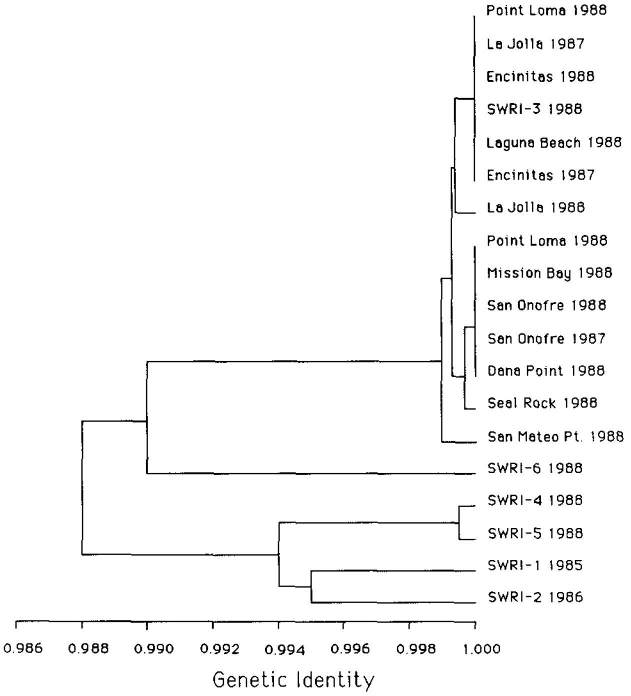


Figure 1. Cluster analysis (UPGMA) of genetic identity values among 19 samples of white seabass.

To determine if significant allele frequency differences existed between samples taken at different years from the same location, tests of independence of allele frequencies and sample year were performed on collections from Point Loma, La Jolla, Encinitas, and San Onofre. Although the summary indices of genetic variation changed between years (H and P, table 2), only the *Aat-3* locus from the Point Loma samples displayed significant temporal heterogeneity (G = 8.518; d.f. = 1).

Estimates of gene exchange were high among wild samples. We estimated the level of gene flow among wild samples to be 8.0 individuals per generation in 1987, and 9.4 in 1988 (equation 1).

Allozyme Variability in Hatchery Samples

We observed eleven polymorphic loci in six hatchery samples of white seabass. However, individual samples had as few as three, and no more than six polymorphic loci (table 2). Heterozygosity levels ranged from 0.024 to 0.060 and were comparable to levels in natural samples.

Although white seabass progeny all came from a common brood stock, differences in allelic variability were apparent. For example, SWRI-2 was the only hatchery sample in which we observed the *Ldh-3(40)*, *Gpi-1(−140)*, and *Mpi-(110)* alleles. However, no *Aat-1* or *Aat-2* polymorphisms were seen in this sample. SWRI-5 was fixed for the common *Adh* allele, whereas other hatchery samples were variable at this locus. The *Est-5* locus was polymorphic only in SWRI-6 sample.

Gene diversity analysis of hatchery samples indicated 84% of the total heterozygosity was due to differences within samples, whereas 16% (G_{ST} = 0.157) originated from between-sample differences, i.e., differences in mass spawnings (D_{ST} = 0.0069; H_S = 0.0370; H_T = 0.0440). Furthermore, significant heterogeneity in allelic frequencies existed among the hatchery groups at six loci: *Aat-1* (G = 20.545; 5 d.f.); *Aat-3* (G = 43.801; 5 d.f.); *Adh* (G = 41.125; 5 d.f.); *Idh-1* (G = 15.934; 5 d.f.); *Pgm-1* (G = 40.031; 10 d.f.); and *Gpi -1* (G = 50.031; 5 d.f.).

Genetic identities between hatchery samples were slightly less than those between wild samples (data not shown). However, all but one of the hatchery samples clustered together in figure 1. The genetic similarity between the cluster of wild samples and a single composite hatchery sample (produced by averaging allelic frequencies from all hatchery samples) was 0.994.

DISCUSSION

The electrophoretic analysis of wild samples of white seabass revealed more genetic variability than was previously reported for this species. Soulé and Senner (unpublished report to California Department of Fish and Game) detected polymorphisms only in alcohol dehydrogenase and phosphoglucomutase enzyme systems. Levels of heterozygosity reported here were slighly higher than the range of values (0.009–0.043) reported for other members of the Scianidae (Ramsey and Wakeman 1987). Waples (1987) reported a range of heterozygosity values of 0.009 to 0.087 (mean = 0.031) for ten species of Southern California Bight marine shorefishes that display diverse life-history characters. Average heterozygosity values of white seabass were similar to two of the species studied by Waples: the wooly sculpin, *Clinocottus analis* (H = 0.046), and the ocean whitefish, *Caulolatilus princeps* (H = 0.049).

Subpopulations of white seabass appear to be genetically similar in the Southern California Bight region. However, significant subpopulation differentiation was discovered among the samples in 1988, and significant allelic frequency heterogeneity existed between sampling years at Point Loma. We do not infer that discrete subpopulations of white seabass exist in the Southern California Bight area. The reasons for these differences are unclear at present, but may be an effect of the rare alleles, nonrandom sampling of the populations, temporal instability of allele frequencies, or selection, as well as discrete subpopulation structure. Hedgecock and Bartley (1988) found significant allelic frequency differences between juvenile California halibut from Mission Bay, San Diego, and adults from Marina del Rey, and discussed several testable hypotheses to account for genetic differences within a theoretically panmictic population. Unfortunately, we do not have size or age data on the present collections of white seabass; therefore a more detailed analysis of the causes of genetic heterogeneity, as was done in studies of northern anchovy (Hedgecock et al. 1989) is not possible at present.

We could detect no consistent geographic, clinal, or temporal component to the observed genetic variation in wild populations of white seabass from the Southern California Bight region. This result was not unexpected, given the past history of genetic studies on pelagic marine fishes (Gyllensten 1985; Waples 1987; Hedgecock et al., 1989). In highly mobile species such as white seabass (Vojkovich and Reed 1983), gene flow among localities is apparently sufficient to homogenize the genetic structure.

The significant F_{ST} values and the occurrence of variant alleles in specific samples suggested that the population of white seabass in the study area may not be panmictic, but rather a dynamic mosaic of very similar subpopulations. Hedgecock et al. (1989 and unpublished data) observed an extremely complex population genetic structure in northern anchovy; allelic heterogeneity existed between sexes, locations, and age classes. Campton and Utter (1987) stated that gene frequencies in subpopulations may randomly fluctuate around a global mean for the population. The random fluctuations may be statistically significant even with substantial levels of gene flow (Allendorf and Phelps 1981). Waples (1987) observed isozyme loci with significant F_{ST} values (i.e., subdivision) in two marine shorefishes with limited dispersal capabilities — the viviparous black perch, *Embiotica jacksoni* (average F_{ST} = 0.444), and the wooly sculpin (average F_{ST} = 0.042). Marine species that Waples judged to have high dispersal capabilities showed a lower range of F_{ST} values (0–0.028) than we observed in white seabass.

The results of this study have favorable implications for hatchery enhancement programs. Levels of genetic variability detected here are similar to levels in other species to which genetic marking techniques have been applied (Murphy et al. 1983; Seeb et al. 1986). Furthermore, we detected several alleles that could be used as genetic markers to differentiate hatchery stocks from wild stocks. Frequencies of alleles such as *Ldh-3(40* or *150)*, *Mpi-(110)*, or *Aat-1(115* or *50)* could be increased in hatchery populations through selective breeding, and used to quantify hatchery contribution to a target population (Pella and Milner, 1987).

Hydrologic and zoogeographic data suggest that the Southern California Bight represents a zoogeographic province bounded by Magdalena Bay, Baja California, to the south, and by Point Conception to the north (Briggs 1974). The province is characterized by fish fauna that show little regional genetic differentiation (Waples 1987). Although our initial description of the population genetic structure of white seabass suggests that brood stock could be collected and progeny released at convenient locations within the study area, one should proceed carefully before implementing such a hatchery program. Life-history data on spawning patterns, migration, age of maturity, and growth rate must be collected throughout the Southern California Bight region to determine if the allozyme similarity in the area is reflected by phenotypic similarity. We do not know if mixing the populations of white seabass in the Southern California Bight through transfers of hatchery-produced fish would cause problems of hybridization of locally adapted stocks and result in outbreeding depression (Altukhov and Salmenkova 1987). In addition, we know nothing of the contribution of subpopulations from the offshore islands. Genetic analyses of samples from offshore islands and samples outside the Southern California Bight may reveal more genetic differences that may be a consideration in white seabass enhancement efforts in lower Baja California or northern California.

The progeny groups produced from a single brood stock at SWRI were more different from each other than were the wild populations from separate locations. Furthermore, the hatchery populations were slightly differentiated from natural populations. The differences in genetic variability among hatchery samples are most likely due to different adults contributing to successive mass spawns. The relative contribution of adult brood stock to mass spawnings may also change over time. For example, the fish contributing to the 1986 mass spawn possessed the *Gpi-1(−140)* allele. Because this allele was not seen subsequently, we presume the adult (or adults) made little or no further contribution to the progeny samples. Sampling error and small sample sizes may also account for some differences in allelic frequency or presence of rare alleles in hatchery samples.

It is important to note that progeny from a single mass spawn have a different genetic profile and may have less variability than natural populations; when additional progeny samples are analyzed, the genetic variability of the entire hatchery product is increased. However, there is a limit to the genetic variability in progeny from a limited number of adults. We observed a 15% decrease in total gene diversity in hatchery samples compared to wild samples (0.0440/0.0516). A founding population of $N = 15$ individuals (such as in a mass spawning tank) should preserve $1\text{-}1/2N$ or 97% of the variation of the original population. This relation will only hold if N is the effective population size (N_e). N_e of the white seabass brood stock may be much less than N because of different reproductive output and unequal sex ratio of adults (Allendorf and Ryman 1987). We should point out that the progeny groups we analyzed represent a small proportion of the progeny produced at SWRI.

The precise number of adults participating in the spawns could not be determined, because the genotypes of the brood stock are unknown and because spawning is difficult to see. If all adults contributed equally to each spawn, we would expect to see the same alleles in each hatchery sample. Because we observed different alleles in successive progeny groups, we believe that only a few adults are involved. Mass spawns result when one or two females ovulate, and each female is fertilized by one to four males (personal observation). Therefore, although a single mass spawn may represent a genetic contribution of only a few spawners, a series of mass spawns, each with different brood stock contributing gametes, may represent a genetic contribution of a large number of brood stock. Furthermore, a production hatchery may be able to have several tanks of brood stock spawning, thereby increasing the effective population size of adults and the genetic variability of the progeny. In a hatchery enhancement program for white seabass in the Southern California Bight region, the genetic profile of progeny from mass spawnings should be monitored over extended periods of time to insure that levels of genetic variability are maintained. Although white seabass brood stock are adapted to current hatchery conditions, dominance hierarchy or reproductive senescence in certain individuals may reduce the number of adults spawning and thus decrease the genetic diversity of progeny.

Technology now exists for producing large numbers of white seabass in a hatchery environment. But the feasibility of enhancing a depressed natural population of white seabass is still controversial. The allozymic data presented in this paper address genetic considerations associated with hatchery enhancement. Recently developed molecular genetic techniques can also be applied to fishery analyses. DNA-level polymorphisms from mitochondrial DNA, and DNA fingerprints may reveal additional markers for hatchery fish and may provide a means to follow the contribution of individual brood stock (Hallerman and Beckman 1988). We recommend the continued collection of allozyme data from offshore islands and areas outside the Southern California Bight, as well as the incorporation of DNA analyses to better understand the population genetic structure of natural and hatchery populations of white seabass.

ACKNOWLEDGMENTS

This research was supported by the Ocean Resources Enhancement and Hatchery Program within the California Department of Fish and Game. We gratefully acknowledge the efforts of R. F. Ford, C. Donohoe, P. Dutton, R. Orhun, and commercial gillnetters R. Miller and R. Phillips in collecting white seabass. We thank D. Campton and L. McClenaghan for computer programs and discussions, D. Trusedale for laboratory assistance, A. Ibarra for the Spanish translation of the abstract, S. Fox for preparing the tables, and D. Hedgecock and an anonymous referee for helpful reviews.

LITERATURE CITED

Aebersold, P. B., G. A. Winans, D. J. Teel, G. B. Milner, and F. M. Utter. 1987. Manual for starch gel electrophoresis: a method for the detection of genetic variation. Springfield, Va.: NOAA Technical Report NMFS 61, 19 pp.

Allendorf, F. W., and S. R. Phelps. 1981. Use of allelic frequencies to describe population structures. Can. J. Fish and Aquat. Sci. 38:1507–1514.

Allendorf, F., and N. Ryman. 1987. Genetic management of hatchery stocks. *In* Population genetics and fishery management, N. Ryman and F. Utter, eds. Seattle: Univ. Wash. Press, pp. 141–160.

Allendorf, F. W., and F. M. Utter. 1979. Population genetics. *In* Fish physiology, vol. 8, W. J. Hoar, D. J. Randall, and J. R. Brett, eds. pp. 407–454.

Altukhov, Y. P., and E. A. Salmenkova. 1987. Stock transfer relative to natural organization, management, and conservation of fish populations. *In* Population genetics and fishery management, N. Ryman and F. Utter eds. Seattle: Univ. Wash. Press, pp. 333–343.

Briggs, J. C. 1974. Marine zoogeography. N.Y.: McGraw-Hill, 475 pp.

Campton, D. E., and F. M. Utter. 1987. Genetic structure of anadromous cutthroat trout *(Salmo clarki clarki)* populations in the Puget Sound area: evidence of restricted gene flow. Can. J. Fish. Aquat. Sci. 44:573–582.

Chakraborty, R., and O. Leimar. 1987. Genetic variation within a subdivided population. *In* Population genetics and fishery management, N. Ryman and F. Utter, eds. Seattle: Univ. Wash. Press, pp. 89–120.

Clayton, J. W., and D. N. Tretiak. 1972. Amine-citrate buffers for pH control in starch gel electrophoresis. J. Fish. Res. Board Can. 29:1169–1172.

Gyllensten, U. 1985. The genetic structure of fish: differences in the intraspecific distribution of biochemical genetic variation between marine, anadromous and freshwater species. J. Fish. Biol. 26:691–699.

Hallerman, E. M., and J. S. Beckman. 1988. DNA-level polymorphisms as a tool in fisheries science. Can. J. Fish. Aquat. Sci. 45:1075–1087.

Harris, H., and D. A. Hopkinson. 1976. Handbook of enzyme electrophoresis in human genetics. Amsterdam: North Holland Publishing Co. 281 pp.

Hedgecock, D., and D. M. Bartley. 1988. Allozyme variation in the California halibut, *Paralichthys californicus*. Calif. Fish Game 74:119–127.

Hedgecock, D., E. S. Hutchinson, G. Li, F. L. Stone, and K. Nelson. 1989. Genetic and morphometric variation in the Pacific sardine, *Sardinops sagax caerulea:* comparisons and contrasts with historical data and with variability the northern anchovy, *Engraulis mordax*. Fish. Bull. U.S. 87:653–671.

Kincaid, H. L. 1983. Inbreeding in fish populations. Aquaculture 33:215–227.

MacCall, A. D. 1989. Against marine fish hatcheries: ironies of fishery politics in the technological era. Calif. Coop. Oceanic Fish. Invest. Rep. 30:46–48.

Markert, C. L., and I. Faulhaber. 1965. Lactate dehydrogenase isozyme patterns of fish. J. Exp. Zool. 159:319–332.

Murphy, B. R., L. A. Nielsen, and B. J. Turner. 1983. Use of genetic tags to evaluate stocking success for reservoir walleyes. Trans. Am. Fish. Soc. 112:457–463.

Nei, M. 1973. Analysis of gene diversity in subdivided populations. Proc. Nat. Acad. Sci. USA 70:3321–3323.

———. 1977. F-statistics and analysis of gene diversity in subdivided populations. Ann. Hum. Genet. 441:225–233.

———. 1978. Estimation of average heterozygosity and genetic distance from a small number of individuals. Genetics 89:583–590.

Netboy, A. 1974. The salmon: their fight for survival. Boston: Houghton Mifflin, 613 pp.

Pella, J. J., and G. B. Milner. 1987. Use of genetic marks in stock composition analysis. *In* Population genetics and fishery management, N. Ryman and F. Utter, eds. Seattle: Univ. Wash. Press, pp. 247–276.

Ramsey, P. R., and J. M. Wakeman. 1987. Population structure of *Sciaenops ocellatus* and *Cynoscion nebulosus* (Pisces). Copeia 1987:682–695.

Ridgeway, G. J., S. W. Sherburne, and R. D. Lewis. 1970. Polymorphisms in the esterase of Atlantic herring. Trans. Am. Fish. Soc. 99:147–151.

Rutledge, W. P. 1989. The Texas marine hatchery program — it works! Calif. Coop. Oceanic Fish. Invest. Rep. 30:49–52.

Ryman, N., and F. Utter. 1987. Population genetics and fishery management. Seattle: Univ. Wash. Press. 420 pp.

Seeb, J. E., L. W. Seeb, and F. M. Utter. 1986. Use of genetic marks to assess stock dynamics and management programs for chum salmon. Trans. Am. Fish. Soc. 115:448–454.

Shaw, C. R., and R. Prasad. 1970. Starch gel electrophoresis of enzymes — a compilation of recipes. Biochem. Genet. 4:297–320.

Skogsberg, T. 1939. The fishes of the family Scianidae (croakers) of California. Calif. Fish Game Bull. 54:1–62.

Slatkin, M., and N. H. Barton. 1989. A comparison of three indirect methods for estimating average levels of gene flow. Evolution 43:1349–1368.

Sneath, P. H. A., and R. R. Sokal. 1973. Numerical taxonomy. San Francisco: W. H. Freeman, 573 pp.

Sokal, R. R., and F. J. Rohlf. 1981. Biometry. New York: W. F. Freeman, 859 pp.

Vojkovich, M., and R. J. Reed. 1983. White seabass, *Atractoscion nobilis*, in California-Mexican waters: status of the fishery. Calif. Coop. Oceanic Fish. Invest. Rep. 24:79–83.

Waples, R. S. 1987. Multispecies approach to the analysis of gene flow in marine shore fishes. Evolution 41:385–400.

Wright, S. 1943. Isolation by distance. Genetics 28:114–138.

THE VERTICAL DISTRIBUTION AND FEEDING HABITS OF TWO COMMON MIDWATER FISHES (*LEUROGLOSSUS STILBIUS* AND *STENOBRACHIUS LEUCOPSARUS*) OFF SANTA BARBARA

GREGOR M. CAILLIET
Moss Landing Marine Laboratories
P.O. Box 450
Moss Landing, California 95039

ALFRED W. EBELING
Department of Biological Sciences
and Marine Science Institute
University of California
Santa Barbara, California 93106

ABSTRACT

Leuroglossus stilbius (Bathylagidae) was abundant in the nearshore Santa Barbara Basin (SBB), but less so in the more offshore Santa Cruz Basin (SCB). *Stenobrachius leucopsarus* (Myctophidae) was abundant in both basins. *L. stilbius* is adapted morphologically to feed by suction and to eat smaller, less active organisms. *S. leucopsarus* is better adapted to feed by grasping, and to eat larger, faster, and more elusive prey. In the SBB, *L. stilbius* fed, mostly at night in surface waters, on larvaceans and salps all year, reflecting the seasonally consistent abundance of these prey items. In the SCB, it fed less intensely, mostly at night in surface waters, and its diet varied with the seasonal abundance of its gelatinous prey. *S. leucopsarus* fed mainly on crustaceans, and it did not exhibit a distinct feeding chronology. It ate similar prey all year in both basins, but euphausiids dominated the diet when they were most abundant. Thus *L. stilbius* is well adapted to inshore, eutrophic midwater habitats, where it can easily eat abundant larvaceans and salps, and may act as a trophic link between the shallow gelatinous zooplankton and the deep sea through its diffuse vertical migrations. Offshore, its primary food is less dense, only seasonally available, and restricted to surface waters. *S. leucopsárus* is better adapted to eat the more varied food resources offshore. Its cohesive pattern of vertical migrations takes it into the food-rich surface waters at night, where it consumes crustacean prey and trophically transports their calories to the deep sea.

RESUMEN

La abundancia de *Leuroglossus stilbius* (Bathylagidae) fue más alta en la cuenca de Santa Barbara (SBB) que en la cuenca de Santa Cruz (SCB), ubicada a mayor distancia desde la costa. La abundancia de *Stenobrachius leucopsarus* (Myctophidae) fue similar en ambas cuencas. La especie *L. stilbius* presenta adaptaciones morfológicas que le permiten alimentarse por succión y predar sobre organismos pequeños y poco activos. A diferencia, la especie *S. leucopsarus* está mejor adaptada a una alimentación activa, predando sobre presas más grandes y más escapadizas. *L. stilbius* se alimentó principalmente de noche en aguas superficiales de la SBB. Durante todo el año el alimento consistió de apendicularias y salpas, reflejando la persistente disponibilidad de estos organismos a lo largo del ciclo anual. El régimen alimenticio fue similar en la SCB, si bien la dieta varió de acuerdo con la variación estacional en la abundancia de las presas. No se observaron diferencias cronológicas marcadas en los hábitos alimenticios de *S. leucopsarus*. La dieta estuvo compuesta principalmente por crustáceos, en especial de eufaúsidos cuando éstos eran más abundantes. El alimento fue similar en ambas cuencas. Por consiguiente, *L. stilbius* es una especie adaptada al ambiente costero eutrófico donde puede alimentarse de apendicularias y salpas en abundancia, y que debido a sus migraciones verticales difusivas es probable que represente el eslabón trófico entre el zooplancton gelatinosos de aguas poco profundas y las aguas oceánicas profundas. Lejos de la costa, su alimento principal está menos densamente distribuído, y solamente disponible estacionalmente y restringido a las aguas superficiales. La especie *L. leucopsarus* está mejor adaptada a un régimen alimenticio oceánico más variado. Su patrón de migración vertical cohesivo le permite predar en aguas superficiales ricas en alimento (donde consume crustáceos) y transportar las calorías hacia las aguas profundas.

INTRODUCTION

According to Lavenberg and Ebeling (1967) "the diversity of the mesopelagic and bathypelagic faunas increases with vertical expansion of their habitats offshore." This trend is evident when one compares the fish faunas of two deep-sea basins off Santa Barbara, California (Ebeling et al. 1970a; Brown 1974). The Santa Barbara Basin (SBB), located inshore of the Channel Islands, is relatively shallow (600 m,

---

[Manuscript received February 2, 1990.]

with a 425-m sill), generally isolated from other basins, and enriched by coastal runoff; it has a relatively simple but abundant epipelagic and upper mesopelagic fish fauna. In contrast, the offshore Santa Cruz Basin (SCB) is much deeper (>2000 m) and is outside the immediate coastal influence, in closer contact with the deeper oceanic environment; it contains a more diverse but less abundant fish fauna. Here, allochthonous species increase in relative abundance with large-scale seasonal changes in water-mass types, and bathypelagic species occur below 500 m.

Off California, evidence suggests that phytoplankton production and standing crop are highest inshore (Malone 1971), and the zooplankton diversity increases as zooplankton density decreases offshore (Longhurst 1967). The inshore SBB is regarded as highly productive (Emery 1960; Soutar and Isaacs 1969; Sholkovitz and Gieskes 1971). Ebeling et al. (1970a) reported that catch volumes of fishes were higher there, whereas volumes of invertebrate micronekton did not differ significantly between basins. The variability in the offshore catch was greatly influenced by seasonal invasions of bulky organisms like salps. If salps are eliminated from the analysis, invertebrate standing crop was also greatest in the SBB.

The California smoothtongue, *Leuroglossus stilbius*, (Bathylagidae) and northern lampfish, *Stenobrachius leucopsarus*, (Myctophidae) are the dominant fishes in the midwater community of animals that is especially abundant off southern California (Ahlstrom 1969; Ebeling et al. 1970a, 1970b; Brown 1974). *L. stilbius* was ranked first, and made up 58% of all fishes sampled. Its catch rate averaged 6.1 adults per kilometer flow through an 1.8-m Isaacs-Kidd midwater trawl (IKMT) in the SBB. It ranked only third, 16%, and 0.4 km^{-1} in the SCB. Likewise, *S. leucopsarus* ranked second, and had abundances and catch rates of 33% and 5.4 km^{-1} inshore, and ranked fifth, and had values of 11% and 0.9 km^{-1} offshore. Farther offshore, the numbers of both species dwindle, but *S. leucopsarus* is a bit more abundant than *L. stilbius* (Pearcy 1976; Willis and Pearcy 1980). Although the distributional centers of both fishes occur off California, *L. stilbius* ranges from Alaska to the Gulf of California, while *S. leucopsarus* occurs all the way from the Bering Sea to the tip of Baja California (Miller and Lea 1972; Hart 1973; Eschmeyer et al. 1983).

Ebeling et al. (1970b) theorized that "among mesopelagic fishes *L. stilbius* and to a lesser extent *S. leucopsarus* may best exploit the rich inshore basins of the borderland." What, then, differentially regulates the sizes of the inshore and offshore populations of these species? Of the four key factors listed by MacArthur and Connell (1966) that regulate population sizes (reproduction, migration, mortality, and food resources), we decided to evaluate the fourth. We investigated the morphological adaptations, feeding habits, and vertical migration patterns of the two fishes to determine what kinds of prey they took, how they migrated vertically relative to prey availability, how their use of available food might influence their relative success in the inshore and offshore areas, and how they might affect the vertical flux of organic material in the water column.

MATERIALS AND METHODS

Fishes were collected from 1964 through 1968 during 33 regular cruises of the R/V *Swan* off Santa Barbara, California (cf. Ebeling et al. 1970a; Brown 1974). All collections were made with a 1.8-m IKMT, which had a lining of 1-cm stretch mesh netting, followed by a standard zooplankton net and a cod-end sampler, divided into four chambers by electronically closed gates (Aron et al. 1964; Bourbeau et al. 1966). Samples of animals from particular depth intervals were thus separated by the sequentially closed gates. The trawl's spreader bar contained electronic sensors to monitor depth and water temperature. A flowmeter measured sampling effort in meters trawled. The signals from all sensors and flowmeter were transmitted simultaneously through the towing cable to shipboard recorders.

Collections were regularly made in the generally recognized major depth zones: (1) epipelagic, in the surface wind-mixed layer about 0–200 m; (2) upper mesopelagic, within the permanent thermocline, about 200–400 m; and (3) lower mesopelagic, in the dysphotic depths, below 400 m. Maximum trawl depth varied between 500 and 1000 m, depending on the area sampled. Only shallow and mid-depth samples could be taken in SBB, which lacks a bathypelagic zone. The more typically oceanic and deeper SCB has a bathypelagic zone that extends well below the depth of the lower mesopelagic. Samples were taken throughout the year at night and during the day. A total of 205 stations yielded 631 collections, 363 of which were from discrete-depth hauls whose vertical excursions were at least 50% within the 200-m depth intervals. An additional 40 samples through broader depth intervals were used to estimate seasonal abundance of potential prey, such as salps and euphausiids.

All fishes were preserved on board in 10% buffered Formalin and seawater, and subsequently changed to 45% isopropyl alcohol ashore. Specimens of *L. stilbius* and *S. leucopsarus* were routinely identified, counted, and measured, along with other fishes and invertebrates of each trawl catch within a few months after collection (Ebeling et al. 1970a). Small fishes (less than 50 mm standard length, SL) were arbitrarily distinguished from larger fishes. Very small individuals of both species were caught but were not adequately sampled because many probably escaped through the mesh. Abundances of both species were standardized among collections by sampling effort, measured in meters but defined in units of kilometers towed.

The depth distributions of the two species, pooled for all months, were analyzed separately for four time intervals—late night (0001–0600 hrs); morning (0601–1200 hrs); afternoon (1201–1800 hrs); and early night (1801–2400 hrs)—and for five depth intervals—0–200 m, 201–400 m, and >400 m in the SBB and SCB, plus the deeper zones 600–800 m and >800 m in the SCB. Histograms of bathymetric distribution of abundances were constructed for these four categories of day and night captures in the "at-depth" hauls. The same procedure was followed for analyzing the depth distributions of two species of prey caught in the trawls. Their seasonal abundances, pooled for all years by month and season, were also analyzed for both basins.

Measurements and counts of the following alimentary structures were taken to compare the feeding abilities of the two species: jaw length; width of gape; number and size of teeth; number, size, and structure of gill rakers; and the length and general structure of the stomach, intestine, and pyloric caeca. In all, 647 *L. stilbius* and 677 *S. leucopsarus* were measured, weighed, and dissected. The lengths and weights were used to calculate a "condition factor" (Cailliet et al. 1986). The gut was exposed by opening the coelom. The entire alimentary tract was removed by cutting at the esophagus and pulling it out. The stomach was then split longitudinally from the pyloric sphincter to the esophagus.

For *L. stilbius*, which has two stomachs, the cut was made from the posterior end of the pyloric stomach, through the cardiac stomach to the esophagus. Only the contents of the pyloric stomach were analyzed for this study, since the cardiac stomach contents were more likely to reflect net feeding (Anderson 1967; Lancraft and Robison 1980).

For *S. leucopsarus*, however, there was only one stomach to analyze. Collard (1970) felt that net feeding was minimal in his study of *S. leucopsarus* feeding habits. Likewise, Hopkins and Baird (1975) reported that feeding in midwater trawls did not significantly alter feeding habit results. However, Lancraft and Robison (1980) found simulated prey items in 23.3% of the guts of *S. leucopsarus*. Therefore, we used stomach contents with scales to estimate possible bias in this species. We compared rank orders of diets of fish with scales in their stomachs with those without scales for both basins, using Kendall's nonparameteric rank correlation (Sokal and Rohlf 1969).

With contents intact, the fullness of the gut was subjectively scored as: 0 = empty; 1 = 25% full; 2 = 50% full; 3 = 75% full; and 4 = 100% full. The contents were then removed, identified to the lowest possible taxon, measured with an ocular micrometer, and counted. The percent volume contribution of each prey item was subjectively estimated. Any intestinal parasites were identified, counted, and measured.

The index of relative importance (IRI) of each prey item was estimated for food-containing fish by each time and depth category as a linear combination of its numerical importance (N), volumetric importance (V), and frequency of occurrence (FO) (Pinkas et al. 1971). The numerical importance of a particular item was the percentage ratio of its abundance to the total abundance of all items in the contents. Its volumetric importance was its average percent volume. Its percent frequency of occurrence was the percentage of fish containing at least one individual. The combination resulted in: $\mathrm{IRI} = (N + V) \times F$, which is represented by the area of a rectangle resolved by plotting the three importance measures on a three-way graph (Cailliet et al. 1986). The value of IRI ranges from zero, when all three values are zero, to 20,000, when all three indices are 100% (a monodiet). The IRI ranks the relative importance of dietary items, and the three-way graph indicates which measures of importance were most meaningful.

Stomach contents of the two fishes were compared between basins and among oceanographic periods within basins. These periods were defined as: (I) a surface mixing period from January through April, when cold weather and stormy turbulence causes surface cooling; (II) an upwelling period of surface enrichment from May through July, when the California Current is strongest and intensifies the counterclockwise current gyre over the southern California continental borderland; and (III) a period of thermal stratification, from August through December (Brown 1974; Jones 1971; Sholkovitz and Gieskes 1971).

Possible correlations of rank hierarchies of stomach contents were tested between species, basins, and among oceanographic periods via Kendall's tau rank correlation (Sokal and Rohlf 1969). We used the numerical importance of prey items eaten (lowest possible taxon, but not necessarily species) to calculate several diversity indices. Because all indices produced similar results, we will present only the *NM* (number of moves) indices (Fager 1972).

The differences in abundance of two of the principal food items among oceanographic periods were also estimated. We calcualted the mean numbers of *Salpa fusiformis* and *Euphausia pacifica* per km trawled and compared them with the seasonal occurrence of these prey in the stomachs of *L. stilbius* and *S. leucopsarus*. This analysis could not be done for smaller or delicate items, such as larvaceans, copepods, and ostracods, because of destruction, avoidance, escape, or extrusion.

To determine recency and amount of feeding for different time and depth categories, we combined estimates of fullness and state of digestion. A 4 × 4 matrix of fullness by digestion for each time and depth category resolved major feeding states of (A) not recently eaten or full, including empty stomachs (i.e., fullness states 0, 1, and 2 vs. digestion states 1 and 2); (B) recent but not full (fullness states 0, 1, and 2 vs. digestion states 3 and 4); (C) recent and full (fullness states 3 and 4 vs. digestion states 3 and 4); and (D) full but not recent (fullness states 3 and 4 vs. digestion states 1 and 2). Fullness-recency histograms measured frequencies of the major feeding states for both species between basins among the four time intervals (late night, morning, afternoon, and early night) and the three depth intervals (0–200, 201–400, and >400 m).

RESULTS

Alimentary Morphology

L. stilbius has a smaller mouth than *S. leucopsarus* (figure 1). The mean ratio of upper jaw length to standard length was 6.2 (n = 170) for adult *L. stilbius*, but 17.3 (n = 41) for adult *S. leucopsarus*. The mean ratio of gape width to standard length was 5.9 for *L. Stilbius* and 8.9 for *S. leucopsarus* (figure 2). Assuming that these fishes can open their mouths to a 45° angle, the effective mouth area of *S. leucopsarus* is about four times that of *L. stilbius*.

L. stilbius has very few teeth in its mouth (figure 1). It has none on its premaxilla or tongue and few on its palatine, vomer, and dentary bones (Chapman 1943; Borodulina 1968). *S. leucopsarus*, on the other hand, has well-developed premaxillary, palatine, pterygoid, and dentary teeth (Bolin 1939; Jollie

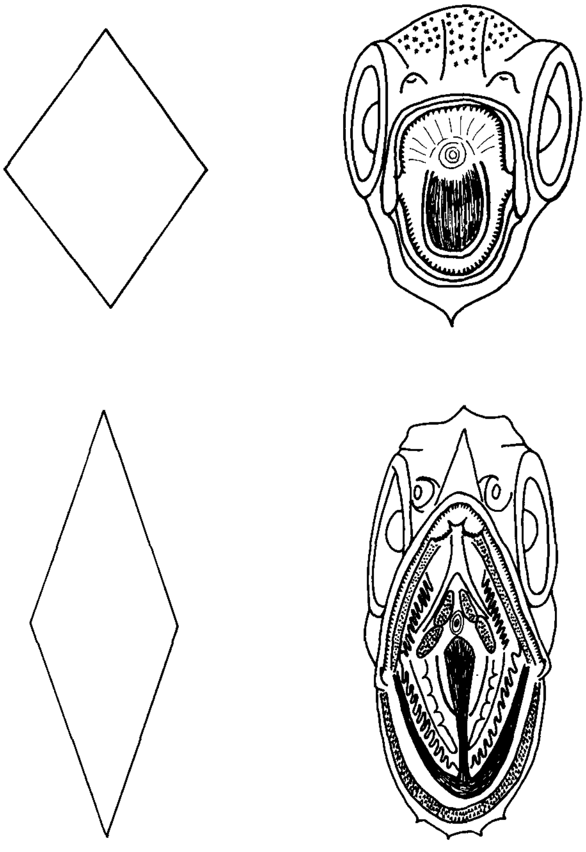


Figure 1. Mouth shape and front view of the head of *L. stilbius* (*above*) and *S. leucopsarus* (*below*).

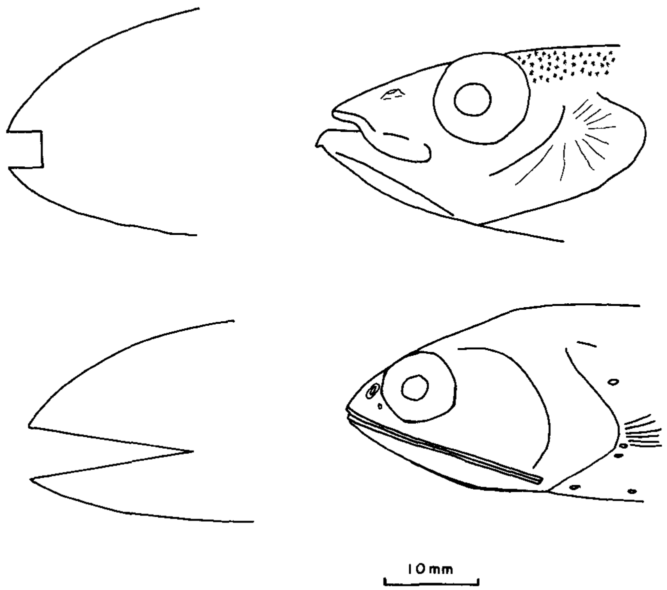


Figure 2. Side views of heads and mouths of a fish with a small mouth and *L. stilbius* (*above*), and a fish with a large mouth and *S. leucopsarus* (*below*).

1954; Berry 1964). Unlike *L. stilbius*, it has well-developed pharyngeal teeth to help move food into the gut (Jollie 1954). With more teeth, *S. leucopsarus* may better grasp and hold larger and more active prey.

L. stilbius has significantly more gill rakers on its first arch (26–29, cf. Borodulina 1968) than *S. leucopsarus* (17–19, cf. Jollie 1954; figure 3). The front edges of its rakers are smooth, not toothed like those of *S. leucopsarus*. The average distance between rakers of an individual measuring 60 mm SL was only 0.3 mm, compared with 0.7 mm for *S. leucopsarus*. Also, the gill rakers of *L. stilbius* are more broadly flattened (Borodulina 1968) and tend to close together when water is forced over them.

The stomachs of the two fishes also differ markedly. *L. stilbius* has a double stomach, with a cardiac portion preceding the pyloric portion (figures 4 and 5). The cardiac stomach is covered with a black pigment, an adaptation that may prevent light from bioluminescent prey from showing through (McAllister 1961). This stomach has a very thick wall, and its inner mucosa is made up of many posteriorly oriented rugae (figure 5). The pyloric stomach is thin-walled and flexible. In contrast, *S. leucopsarus* has only one thick-walled stomach, also covered with a black pigment (McAllister 1961). The internal mucosa is made up of typhlosole ridges that run longitudinally (figure 5).

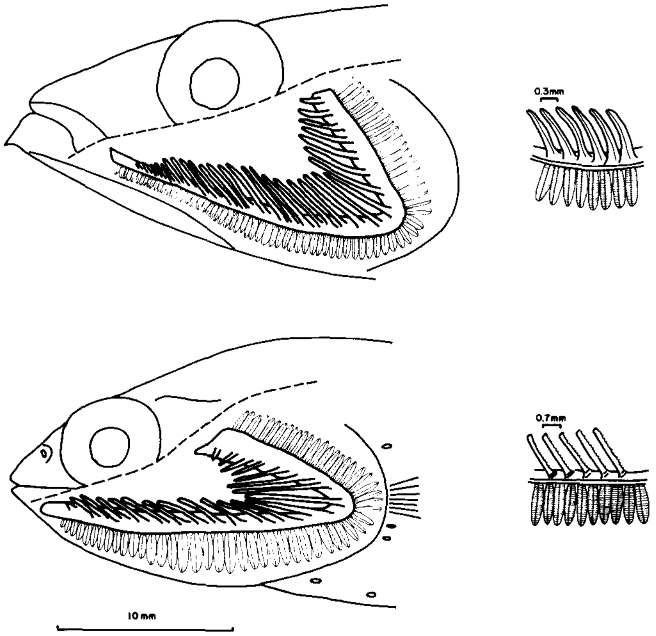


Figure 3. Lateral cutaway view of the gill arches and rakers of *L. stilbius* (*above*) and *S. leucopsarus* (*below*).

The number of pyloric caeca and relative intestinal lengths also differ considerably (figure 5). *L. stilbius* has more caeca (8–11, cf. Borodulina 1968) than *S. leucopsarus* (4–6, cf. Jollie 1954). Its intestine length averaged 50.5% of its SL ($n = 22$), compared with only 28.0% for *S. leucopsarus* ($n = 35$).

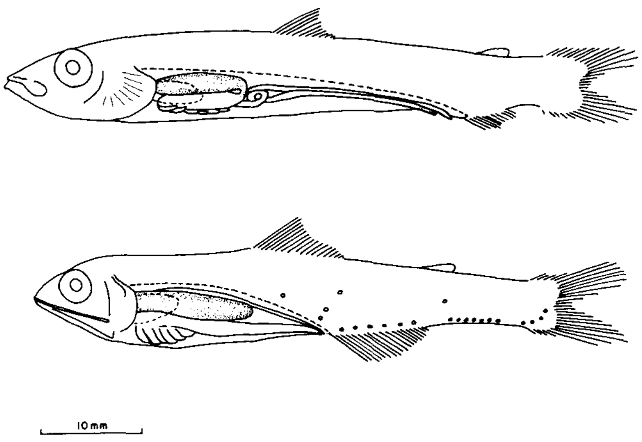


Figure 4. Lateral cutaway view of the body and alimentary tract of *L. stilbius* (*above*) and *S. leucopsarus* (*below*).

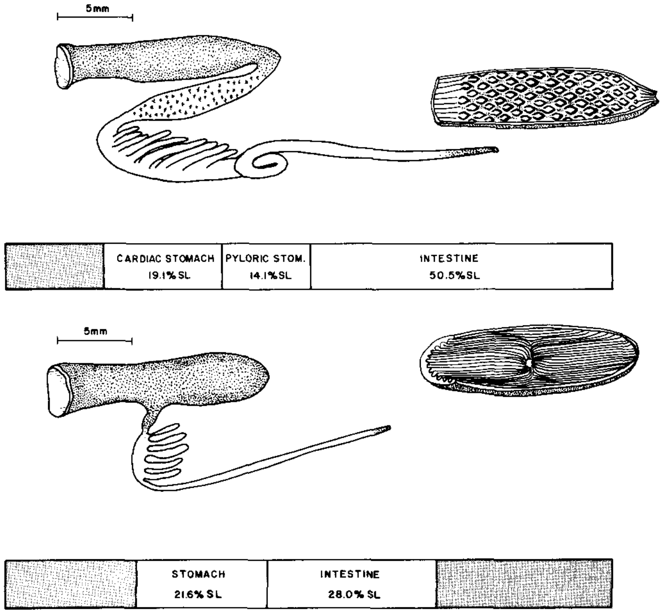


Figure 5. Internal alimentary structures of *L. stilbius* (*above*) and *S. leucopsarus* (*below*). The upper left drawing for each species is the alimentary canal, showing the stomach(s) (stippled and starred), intestinal tract, and pyloric caeca. The upper right drawing for each species shows a longitudinal cutaway of the stomach, either lined with posteriorly oriented rugae (*L. stilbius*) or typhlosole ridges (*S. leucopsarus*). Bar diagrams indicate the average lengths of stomach(s) and intestine relative to standard length for both species.

Feeding Habits

In the SBB, *L. stilbius* ate primarily larvaceans (genus *Oikopleura*) and salps (probably *Thalia democratica* and *S. fusiformis*: Berner 1957, 1967; Hubbard and Pearcy 1971; M. Silver, pers. comm.), followed, in order, by ostracods, small copepods (1–2 mm), zoea, and *E. pacifica* (figure 6). Salps, which were larger than the more numerous larvaceans, made up the greater dietary bulk. Less-important items included copepods (2–3 mm and <1 mm), chaetognaths, fish eggs, siphonophores, nauplii, and larger copepods (>3 mm).

The rank order of prey in *L. stilbius* stomachs was significantly correlated between basins (Kendall's tau = 0.51, $P << 0.01$). Even so, the SCB fish had a slightly more varied diet, which differed in minor ways. They ate relatively fewer ostracods, more nauplii, and more large copepods, with such things as amphipods (*Hyperia galba*) and shrimp (mysids and sergestids) in lesser amounts. Also, the larvaceans were both numerically and volumetrically important, while the salps were important only volumetrically. In both basins the prey diversity indices (Fager 1972) were low (SBB, $NM = 0.20$ and SCB, $NM = 0.11$), indicating that *L. stilbius* concentrated on only a few prey items, hence the truncated shape of the IRI diagrams (figure 6). Net feeding could not have biased these results, which were based on pyloric stomach contents only. Indeed, fish scales were never found in these stomachs.

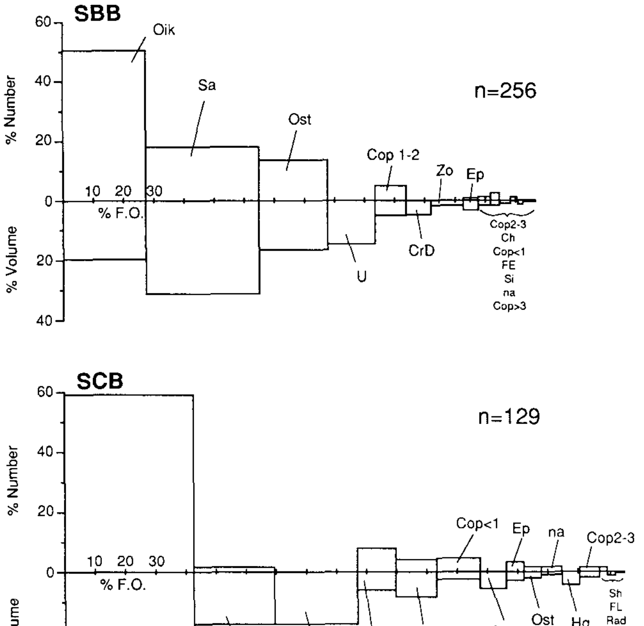


Figure 6. Percent composition of major prey items in number, volume, and frequency of occurrence (% F.O.) and ranked by the index of relative importance (IRI) from left to right for *L. stilbius* in the Santa Barbara Basin (SBB) and Santa Cruz Basin (SCB). n = the number of guts analyzed. Code of abbreviations: Ch = chaetognaths; Cop <1 = copepods smaller than 1 mm; Cop 1–2 = copepods 1–2 mm; Cop 2–3 = copepods 2–3 mm; Cop >3 = copepods larger than 3 mm; CrD = crustacean debris; Ep = *Euphausia pacifica* (euphausiid); FE = fish eggs; FL = fish larvae; FS = fish scales; Hg = *Hyperia galba* (amphipod); na = nauplii; Nd = *Nematoscelis difficilis* (euphausiid); Oik = *Oikopleura* spp. (larvacean); Ost = ostracods; Pcr = *Paraphronima crassipes* (amphipod); Rad = radiolarian; Sa = salps; Sh = shrimp (mysids and sergestids); Si = siphonophores; Zo = zoea; U = unidentifiable.

The diet of *S. leucopsarus* was also similar between basins (tau = 0.63; $P << 0.01$) but was uncorrelated with that of *L. stilbius* (SBB: tau = 0.16, $P \sim 0.35$; SCB: tau = 0.08, $P \sim 0.64$). In the SBB, *S. leucopsarus* ate ostracods, *E. pacifica*, and a variety of "large" copepods, with no item predominating unless all size classes of copepods are pooled (figure 7). Less-important items were fish eggs, the euphausiid *Nematoscelis difficilis*, zoea, the amphipod *H. galba*, shrimp (mysids and sergestids), chaetognaths, fish larvae, siphonophores, salps, and "small" copepods (<1 mm).

The SCB *S. leucopsarus* ate relatively more euphausiids (figure 7). Like *L. stilbius*, they ate relatively fewer ostracods, and more large copepods. Amphipods were mainly *Paraphronima crassipes*. The prey diversity inshore ($NM = 0.28$) was lower than in fish offshore ($NM = 0.34$), but in both cases was higher than for *L. stilbius*, implying that *S. leucopsarus* generally ate more types of food, hence the elongated appearance of the *S. leucopsarus* IRI diagrams (figure 7).

Fish scales occurred in relatively high frequencies in *S. leucopsarus* stomachs (26.5% of SBB and 13.1%

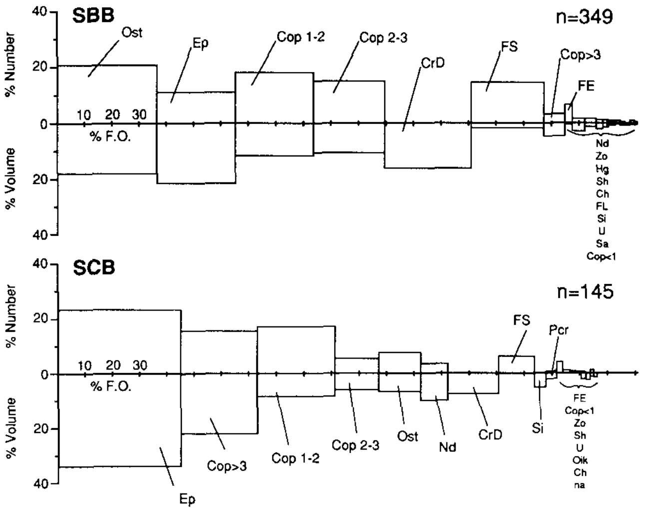


Figure 7. Percent composition of major prey items for *S. leucopsarus*. Abbreviations as for figure 6.

of SCB fish). These scales, an unlikely food, were probably ingested in the trawl net as "net feeding" (Collard 1970; Hopkins and Baird 1975; Lancraft and Robison 1980). However, "scaled" and "unscaled" diets were significantly correlated (SBB: tau = 0.83, $P << 0.001$; SCB: tau = 0.92, $P << 0.001$). Therefore, net feeding apparently did not systematically bias the observed dietary composition, and fish with scales in their stomachs were not eliminated from the analysis.

Seasonal Variation in Feeding Habits

In the SBB, the prey of *L. stilbius* reflected the relatively even yearly distribution of the food supply. Fish ate about the same kinds of prey all year, mostly larvaceans and salps (figure 8), and the ranks of food items were significantly correlated among seasons ($W = 0.85$, $P << 0.001$). Salps were equally abundant in the cold mixing (I) and upwelling (III) periods (figure 9), but were lower during the spring upwelling season (II), which may account for a slight coincident change in the fish's diet. Although larvaceans ranked first in dietary importance during periods I and II, salps ranked first during the warmer stratification season (III), when they were slightly more common in the inshore plankton. During period III, ostracods ranked second, and larvaceans third in the fish's diet. We have no way of assessing the availability of larvaceans like *Oikopleura*. Euphausiids were eaten in noticeable numbers only during period I, followed in rank by large copepods, crab zoea, and small copepods. During periods II and III, ostracods, small copepods, and large copepods completed the diet.

In the SCB, however, significant changes in the diet of *L. stilbius* seemed to reflect concomitant seasonal changes in the food supply. The fish ate mostly larvaceans and salps during periods I and II, but mostly copepods during period III (figure 8). Ranks of dietary items were not significantly correlated between periods I and II (tau = 0.18, $P \sim 0.4$), mainly because larvaceans did not dominate in period II and salps did not even rank in the top six during period I. Periods II and III were also not correlated (tau = 0.45, $P \sim 0.02$) because copepods of all sizes were commonly eaten during period III, but larvaceans and salps were not. Of secondary rank during period I were large and small copepods, crab zoea, and chaetognaths. During period II, salps, small copepods, euphausiids, and zoea were secondary. During period III, a diversity of large and small copepods outranked zoea, larvaceans, and salps. SCB catches of *S. fusiformis* (unlike those from the SBB) decreased abruptly from period I to periods II and III (figure 9).

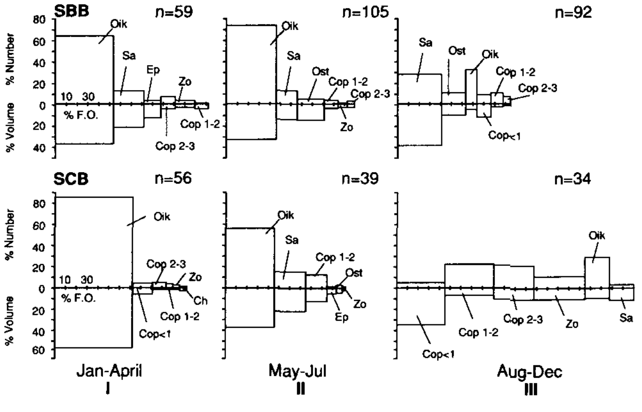


Figure 8. Percent composition of the six top-ranking prey items for L. stilbius for three oceanographic periods. Abbreviations as for figure 6.

In the SBB, the relatively varied diet of *S. leucopsarus* did not reflect the seasonal changes in food supply. *S. leucopsarus* ate mostly small copepods and

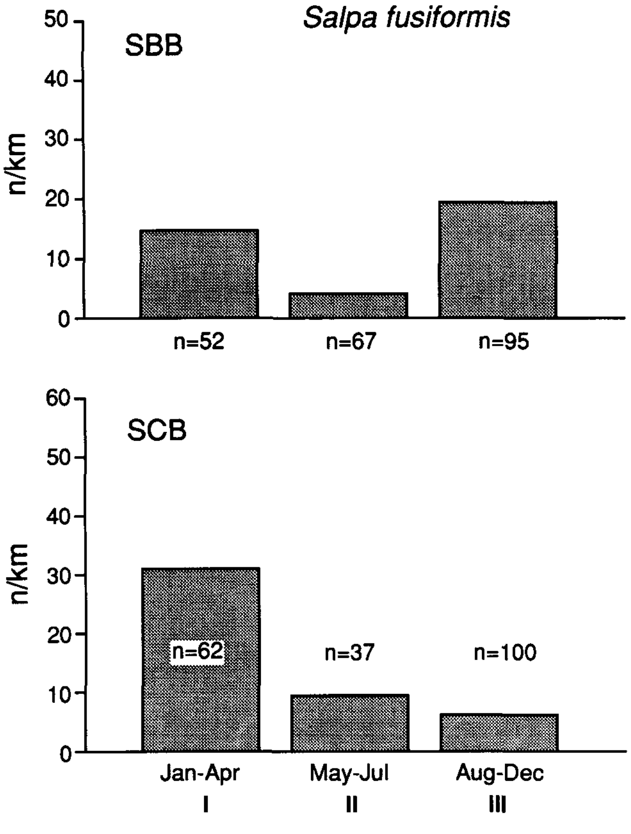


Figure 9. Changes in abundance (number per km sampled) of *Salpa fusiformis*, a major prey of *L. stilbius*, from IKMT discrete depth tows (n = sample size) in the upper 500 m of the Santa Barbara (*above*) and Santa Cruz (*below*) basins for three oceanographic periods.

ostracods during period I, but more euphausiids (*E. pacifica*) during periods II and III (figure 10). Ranks of food items differed significantly between periods I and II (tau = 0.37, $P \sim 0.045$), but were correlated between periods II and III (tau = 0.45, $P < 0.01$). *E. pacifica* catches were low during period I (figure 11), when the fish ate mostly copepods and ostracods. *E. pacifica* ranked first in the diet during periods II and III, but was much more abundant in catches during period II than III. The remainder of the fish's diet comprised large copepods, euphausiids, and chaetognaths during period I; large and small copepods, ostracods, and fish eggs during period II; and ostracods, small copepods, other euphausiids (*N. difficilis*), and large copepods during period III.

In the SCB, however, the dominant prey of *S. leucopsarus* were also the most abundant in midwater trawl catches. *S. leucopsarus* ate both large and small copepods during period I, when *E. pacifica* was not abundant, and *E. pacifica* during periods II and III (figure 10), when they were abundant in the plankton (figure 11). Ranks of food items differed markedly between periods I and II (tau = 0.20, $P \sim 0.3$), and between periods II and III (tau = 0.49, $P \sim 0.04$), even through *E. pacifica* dominated the diet in these two periods. The remainder of the diet comprised the same kinds of items eaten in the SBB.

Diel Vertical Migrations

L. stilbius migrated vertically in a diel pattern that differed somewhat between basins. In the SBB, the fish were abundant in the surface waters during the afternoon and early night, in deep waters during late night, and at mid-depth during the morning (figure 12). A significant portion of the population was

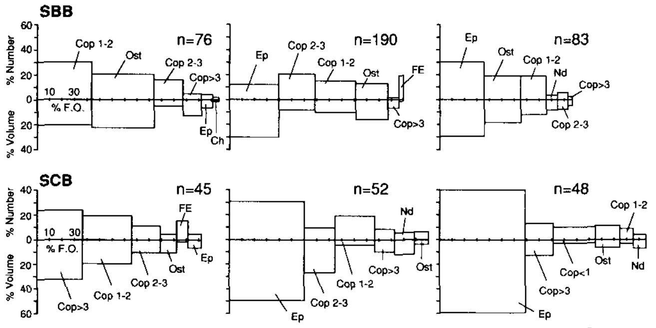


Figure 10. Percent composition of the six top-ranking prey items for *S. leucopsarus* for three oceanographic periods. Abbreviations as for figure 6.

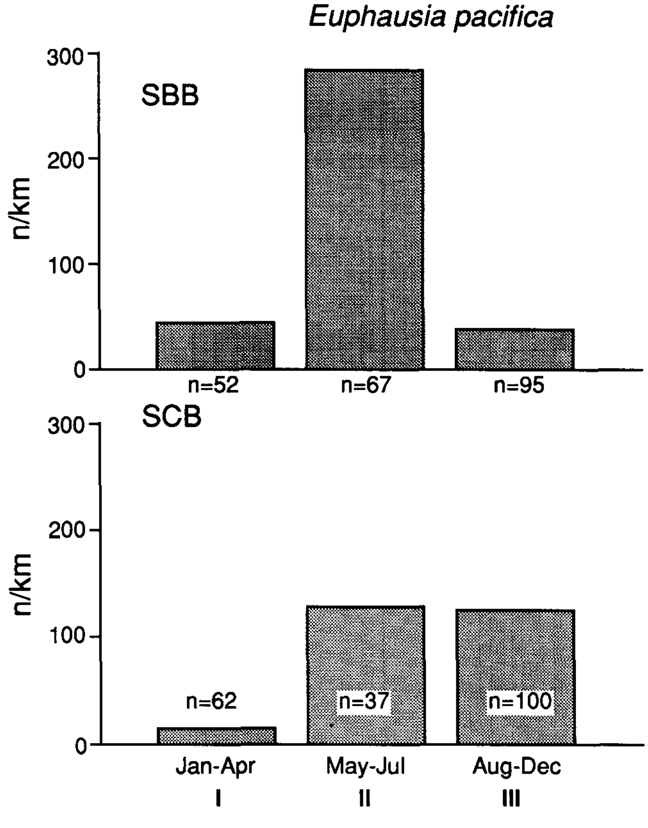


Figure 11. Changes in abundance of *Euphausia pacifica*, a major prey of *S. leucopsarus*, from IKMT discrete depth tows (n = sample size) in the upper 500 m of the Santa Barbara (*above*) and Santa Cruz (*below*) basins for three periods.

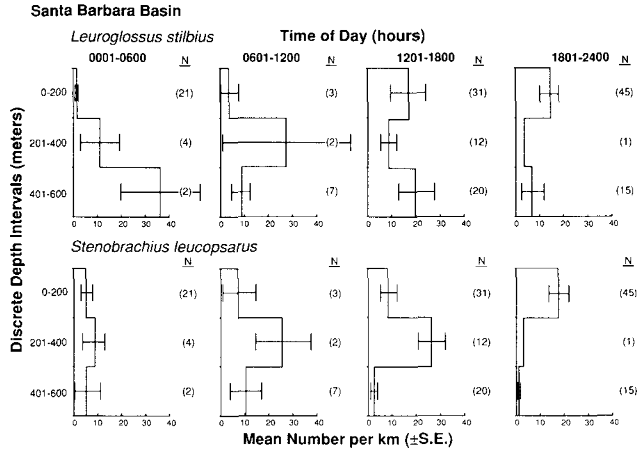


Figure 12. Diel vertical distribution patterns for *L. stilbius* and *S. leucopsarus* in the Santa Barbara Basin. Data were pooled for all months among four 6-hour time intervals and three 200-m depth intervals. The horizontal axis measures abundances, ± standard errors, standardized by trawling effort as numbers per km flow. The numbers in parentheses represent the sample size (number of trawls) for each time-depth category.

found near the bottom of this shallow basin during all periods, and the fish did not stay near the surface all night. In the deeper SCB, they had a broader vertical distribution: some of the fish were found in the surface waters in the evening, especially during the late night and early morning, and more were found in deeper water (401–600 m), especially during late night and daytime (figure 13). A significant portion of the population occurred at mid-depths (<400 m) during all periods.

The migratory pattern of *S. leucopsarus*, on the other hand, was quite predictable and similar between basins. In both the SBB and SCB, most of the population was found in the surface waters at night and at mid-depth during the day (figures 12 and 13). Consequently, the shallow SBB did not seem to compress the vertical range of *S. leucopsarus* like it did that of *L. stilbius*, nor did a significant portion of the *S. leucopsarus* population occur below 400 m in either basin.

Fullness and Recency of Feeding Relative to Vertical Migration

In the SBB, *L. stilbius* apparently fed most intensely during the night in the surface waters; it fed some during both day and night at mid-depth (figure 14). Fish caught at night in the surface waters and at mid-depth had the highest percentages of "recently full" stomachs. Fish caught in the surface during the daytime, and below 400 m at all times had very high percentages of "not recent or full" stomachs, and therefore had not been actively feeding. Fish from the mid-depth during the day tended to have contents equally distributed over the fullness-digestion categories, indicating that their stomachs contained previously ingested items mixed with newly ingested ones.

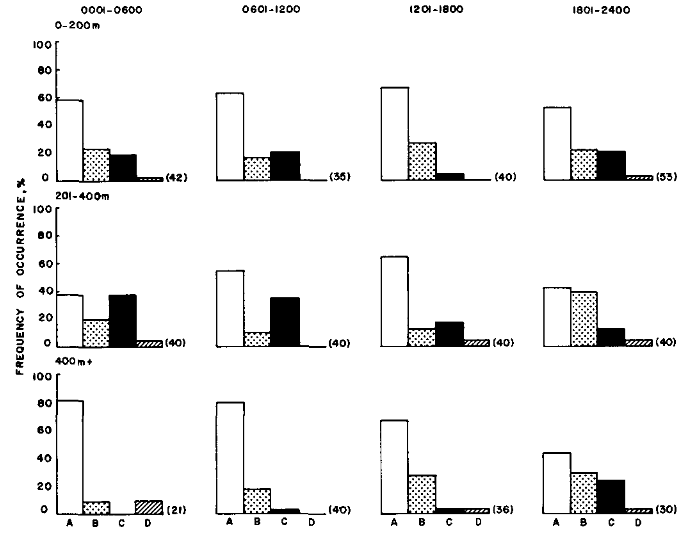


Figure 14. Fullness and recency-of-feeding histograms for *L. stilbius* in the Santa Barbara Basin pooled over all months among four 6-hour time and three depth intervals. The vertical axis measures the percent frequency of occurrence by time-depth category for fullness-recency states: *A* (*open*), not recently eaten or full; *B* (*stippled*), recent but not full; *C* (*shaded*), recent and full; and *D* (*hatched*), full but not recent. The numbers in parentheses equal the number of fish in each time-depth category.

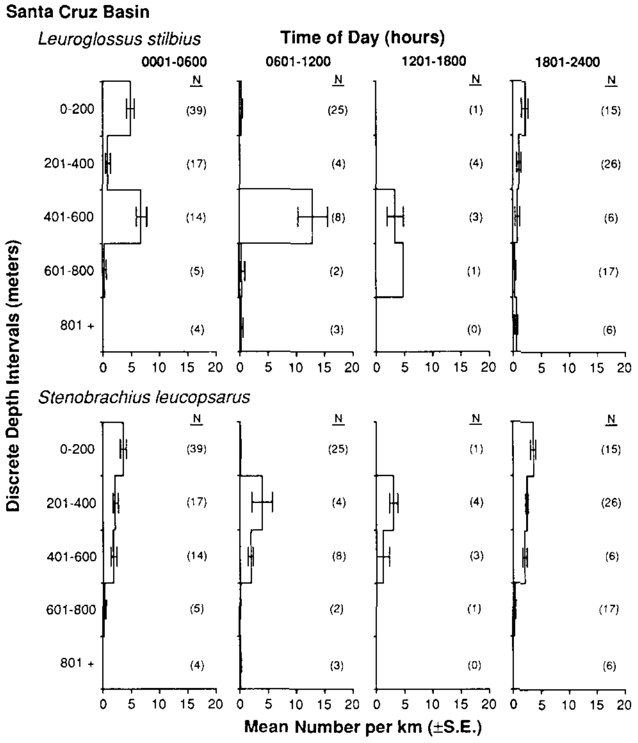


Figure 13. Diel vertical distribution patterns of the two species in the Santa Cruz Basin. Note the difference in the horizontal axis dimensions. All other details as in figure 12.

In the SCB, however, *L. stilbius* generally fed less intensely, and mostly at night. Fish had relatively high percentages of "recent but not full" stomachs only during the night near the surface, and of "full and recent" stomachs only during late night and at all depths (figure 15). Few fish fed to fullness, and fish had high proportions of "not full or recent" stomachs in all time-depth categories for which there were sufficient samples.

S. fusiformis, a common prey of *L. stilbius*, was mainly limited to the upper 200 m, whenever it was collected (figure 16). In the SBB, this salp was relatively abundant in these surface waters at all times of the day. Only during the late night was it available in deeper (401–600 m) waters. In the SCB, however, these salps were caught mainly at night in the upper 200 m, and they were seldom caught during daylight hours.

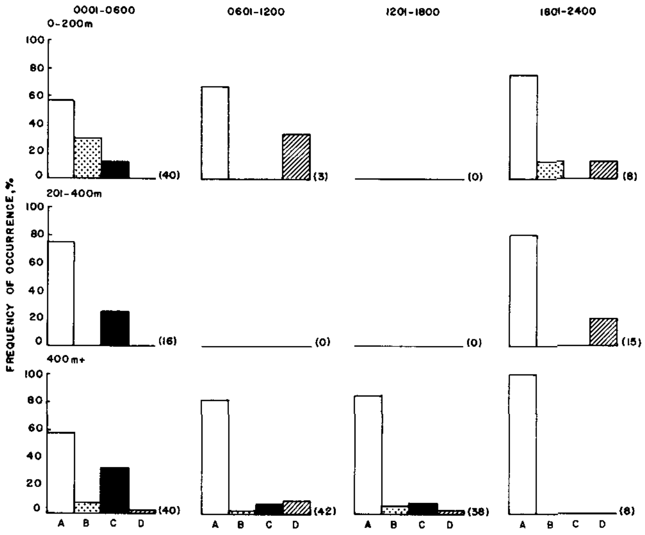


Figure 15. Fullness and recency of feeding histograms for *L. stilbius* in the Santa Cruz Basin. All details as in figure 14.

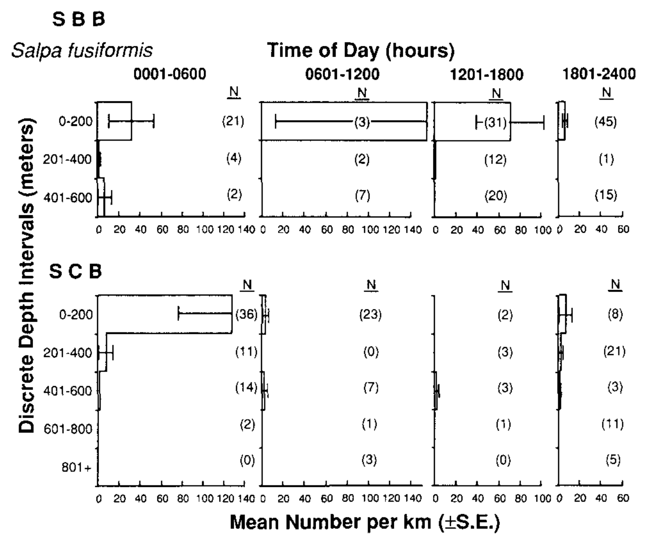


Figure 16. Diel vertical distribution of patterns of *Salpa fusiformis*, a major prey of *L. stilbius*, in the Santa Barbara (*above*) and Santa Cruz (*below*) basins. The horizontal axis measures the abundance standardized for trawling effort as numbers per km flow, ± standard errors. Data were pooled for all months for four 6-hour time intervals and three or five 200-m depth intervals. The numbers in parentheses represent the number of IKMT tows in each time-depth category.

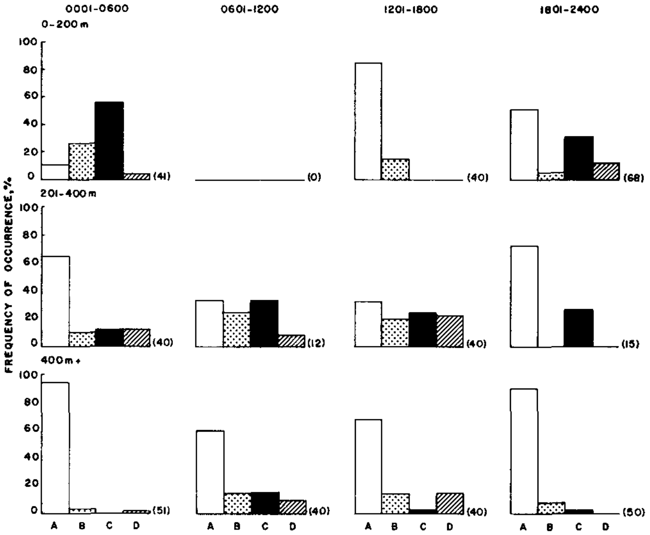


Figure 17. Fullness and recency of feeding histograms for *S. leucopsarus* in the Santa Barbara Basin. All details as in figure 14.

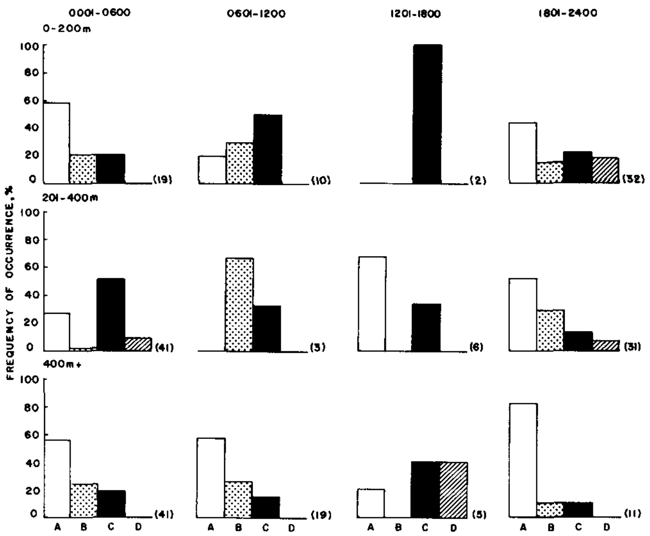


Figure 18. Fullness and recency of feeding histograms for *S. leucopsarus* in the Santa Cruz Basin. All details as in figure 14.

In both basins, *S. leucopsarus* appeared to have fed at all times and depths as the opportunity arose. It did not exhibit a distinct feeding cycle. High percentages of "recent but not full" and "full and recent" stomachs occurred in most time-depth categories (figures 17 and 18). However, the highest percentages of recent feedings were observed in fish from the upper 400 m in both basins. Fish from waters deeper than 400 m generally had high percentages of "not recent or full" stomachs, indicating that they had not eaten much at depth.

In both basins *E. pacifica*, a common prey of *S. leucopsarus*, exhibited a typical migration pattern of occupying the upper 200 m during the night, and dwelling mainly between 200 and 400 m during the day (figure 19). This species was abundant in both basins but did not appear in significant numbers in water deeper than 400 m. Therefore, *S. leucopsarus* must have consumed these euphausiids in the upper 400 m, no matter what time of day.

L. stilbius was more heavily parasitized inshore, whereas *S. leucopsarus* appeared equally parasitized

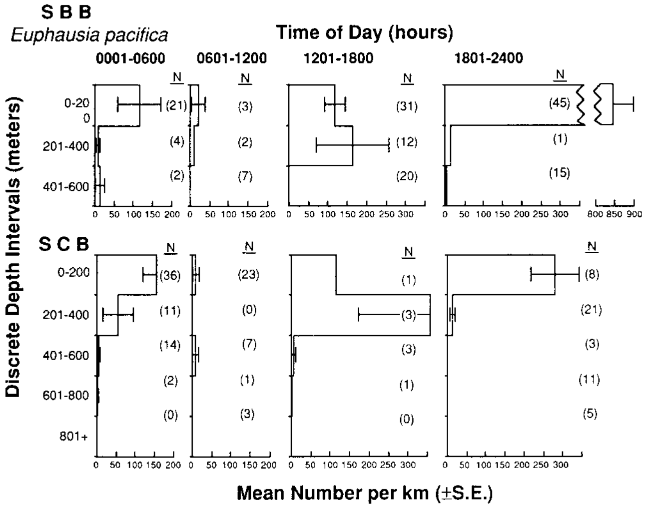


Figure 19. Diel vertical distribution patterns of *Euphausia pacifica*, a major prey of *S. leucopsarus*, in the Santa Barbara (*above*) and Santa Cruz (*below*) basins. All other details as in figure 16.

both inshore and offshore. In the SBB, 29.5% of the *L. stilbius* were parasitized by *Aponurus californicus*, but only 5.7% from the SCB were infected; 4.4% of the *S. leucopsarus* specimens from the SBB were infected by anisakine nematodes, whereas 7.8% from the SCB were infected. The coefficients of conditions (Cailliet et al. 1986) did not differ between species or basin.

DISCUSSION

Alimentary Morphology

Alexander (1967) stated that fishes with smaller mouths can better suck in their prey, whereas fishes with larger mouths can better grasp prey from the side. Therefore, *L. stilbius* would be better at sucking in abundant soft items (soft-bodied salps and larvaceans), and *S. leucopsarus* would be better at grasping a greater diversity of larger or more elusive prey (copepods and euphausiids).

The structure and behavior of soft-bodied, gelatinous prey must be considered when interpreting how *L. stilbius* captures them. Larvaceans secrete houses around themselves, which Alldredge (1976c) proposed to be a protective mechanism. Because *L. stilbius* appeared to have only the larvacean itself in its gut, either it ingested little of the house, or the house is difficult to detect in stomach contents. Underwater observations of *L. stilbius* indicate that they are relatively passive (Barham 1970). It is possible that they can slowly approach larvaceans in their houses and locate the animal either by the beating of its tail or from bioluminescence created by organisms living on or in contact with its house. They can then suck the larvacean out, ingest the whole complex, or scare the animal away from its house and then catch it and suck it in.

Salps are patchy and seasonally common, and can exist solitarily or in strings (Berner 1967; Hubbard and Pearcy 1971; Silver 1975). They are probably encountered by individual fish, presumably in the surface waters and sometimes in the daytime when *L. stilbius* can see them and suck them in. Both gelatinous prey were often found in quantity in an individual gut. Thus, *L. stilbius* must feed often on patches of prey.

Yasuda (1960a,b) reasoned that a fish's gape width determines its ability to trap its prey, while its jaw length determines the size of its prey. Because the two species have similar gape widths, they should be equally adept at trapping. But *S. leucopsarus* has the longer jaw and should therefore eat larger prey, as substantiated by the studies of feeding habits.

In general, the gill rakers of most types of fishes constitute a sieve for filtering and catching food (Martin and Sandercock 1967; Yasuda 1960c; Yasuda and Hiyama 1957). Plankton feeders generally have especially well-developed gill sieves comprising many rakers and accessory processes. Obviously the rakers of *L. stilbius* make the more effective barrier for retaining smaller prey. The more widely spaced and toothed rakers of *S. leucopsarus* are probably better at retaining larger food.

Gut length and pyloric caeca may influence size and quantity of food eaten. Groot (1969) and Darnell (1970) found that fishes with relatively longer guts and many caeca tended to eat smaller prey items and more frequently. In this study *L. stilbius*, with a double stomach, a long intestine, and more caeca, usually ate large quantities of small, soft foods like salps, larvaceans, and copepods, whereas *S. leucopsarus*, with its single stomach, short intestine, and fewer caeca, more often ate single euphausiids and larger copepods. The rugae and typhlosole ridges presumably help these fishes process food through the cardiac stomachs (Kapoor et al. 1975).

All aspects of alimentary morphology, therefore, indicated that *L. stilbius* is better adapted for continuously gorging itself with abundant smaller and often gelatinous prey. In contrast, *S. leucopsarus* should feed more sporadically on smaller quantities of larger prey. Indeed, *L. stilbius* tended to have "recently full" stomachs (rather than "recent but not full"), implying that it feeds mostly to fullness. *S.*

leucopsarus tended to have more "recent but not full" stomachs, implying that it feeds more sporadically and not usually to fullness.

Feeding Habits

Previous studies of food habits of bathylagids are few and sketchy. Hopkins and Torres (1989) found that *Bathylagus antarcticus* ate, among other things, gelatinous coelenterates. Anderson (1967) found that 70% of the cardiac stomachs of *L. stilbius* examined from the San Pedro Basin contained fish eggs, 60% had copepods, and 44% had fish scales, while 40% of the pyloric stomachs contained salps, 35% had copepods, 25% had euphausiids, 20% had eggs, and 20% had larvaceans. He concluded that the prey of *L. stilbius* are less mobile than those of *Triphoturus mexicanus*, a common lanternfish off southern California. Noble (1968) found similar prey but noted the dearth of fast, active chaetognaths in *L. stilbius* stomachs. All studies indicate that *L. stilbius* eats relatively small items, although our results showed that larvaceans and ostracods are more abundant food than euphausiids, at least off Santa Barbara.

Myctophids in general have been reported to eat copepods, euphausiids, ostracods, mollusks, fish eggs and larvae, chaetognaths, larval and adult decapod shrimp, insects, siphonophores, tunicates, annelids, sipunculid and nemertine larvae, pycnogonids, and foraminifera (Beebe and Vander Pyl 1944; Aughtry 1953; Paxton 1967b; Anderson 1967; Holton 1969; Legand and Rivaton 1969; Bradbury and Abbott 1970; Nakamura 1970; Raymont 1970; Baird et al. 1975a; Gorelova 1975; Hopkins and Baird 1975; Clarke 1978; Frost and McCrone 1979; Kinzer and Schultz 1985; Young and Blaber 1986; Dalpadado and Gjosaeter 1988).

Our results generally correspond with previous studies, which found that euphausiids and calanoid copepods constitute most of the *S. leucopsarus* diet (Bary et al. 1962; Osterberg et al. 1964; Paxton 1967b; Tyler and Pearcy 1975; Collard 1970). However, fish from the SBB ate the shrimp *Sergestes similis* much less frequently than did fish from Monterey Bay (Barham 1957), and contained fewer amphipods than fish from Saanich Inlet, British Columbia (Bary et al. 1962). No other studies reported as high a frequency of ostracods as we found in SBB fish.

L. stilbius and *S. leucopsarus* have very different feeding habits. *L. stilbius* ingests a relatively narrow variety of prey. It eats large amounts of small, sluggish, herbivorous jellies, which are 90%–95% water (Berner 1957) and presumably not very nutritious. Optimally, it must eat continuously and digest quickly to meet its energy requirements. *L. stilbius* eats small copepods, which may be more difficult forage, only when the larvaceans and salps dwindle in numbers. *S. leucopsarus* eats a greater size range of more nutritious prey, including large predatory crustaceans.

Comparing the feeding habits of these two fishes with their growth characteristics produces an apparent paradox. Childress et al. (1980) reported a higher growth rate for *L. stilbius* than for *S. leucopsarus*, yet *L. stilbius* consumes prey of relatively lower energy content. There are three possible explanations of this paradox. One would be that *L. stilbius* grows large faster, but has tissues that are not as densely constructed (Childress and Nygaard 1973; Childress et al. 1980). A second would be that it expends less energy than *S. leucopsarus* by not regularly migrating, and by foraging more efficiently on larvaceans and salps than *S. leucopsarus* does on the larger, more elusive and vertically migrating crustaceans. A third possibility is that *L. stilbius* is more efficient at assimilating the few calories available in its prey.

Seasonal Variations in Feeding Habits

The lack of seasonal changes in eating habits and available prey in the relatively eutrophic SBB indicates that food was not limiting there. In contrast, the seasonal decrease in food (*S. fusiformis*) available to *L. stilbius* in the SCB during the late summer thermal stratification period may have forced it to seek out copepods, which may be harder to catch. The assumed decrease in primary production during this period may have caused the coincident decline in salp (and presumably larvacean) catches in the trawls, as seen by Hubbard and Pearcy (1971) off Oregon. These filter-feeding organisms require high concentrations of phytoplankton, and thus flourish in replenished surface waters enriched by nutrients brought up from unstratified depths (Silver 1975).

Also, in the SBB *L. stilbius* had few potential competitors, and offshore in the SCB there were only a few more. Its more oceanic relative *Bathylagus wesethi* eats larvaceans and salps (M. Kelley, pers. comm.), but almost never enters inshore waters, and invades the SCB in noticeable numbers only during the fall thermal stratification period. Even then it is far less abundant than *L. stilbius* (cf. Brown 1974). Farther offshore, however, *Bathylagus* spp. far outnumber *L. stilbius* (A. Ebeling, unpublished data). In the SCB, *Bathylagus* spp. may compete only during the warming season, when, coincidentally, *L. stilbius* ate more copepods and fewer jellies.

Offshore, *S. leucopsarus* is probably more abundant than *L. stilbius* because it is a generalized predator and may broaden or narrow its diet as the situation demands. Its feeding habits are much more similar between basins than those of *L. stilbius*, which may have to broaden its diet beyond optimal limits in deeper waters offshore, where its preferred salps and larvaceans are not so concentrated and evenly distributed among seasons. Although *S. leucopsarus* can eat many different items both inshore and offshore, it can also feed on either euphausiids or copepods, depending on how the food supply changes. Because *S. leucopsarus* can eat just about anything it encounters, competition for items like copepods and ostracods may be less in the SBB.

The tendency, during seasons II and III, for offshore *S. leucopsarus* to eat mostly euphausiids, also observed by Collard (1970), could be explained by competition or prey availability. During these periods, myctophid competitors belonging to an "offshore fish group" (cf. Ebeling et al. 1970a) become seasonally abundant and may force *S. leucopsarus* to restrict its diet. An alternate explanation is that euphausiids may become more abundant. *S. leucopsarus* may broaden its diet to include more copepods during the cold winter season when the offshore fishes dwindle in numbers.

Diel Vertical Migrations

Even though fish abundances were standardized by trawling effort, abundances varied considerably among collections. For either species, this variability could be a function of disjunct distributions among depth zones, areas, or seasons; differences in ability to avoid the net, which is size-specific for fishes (Aron and Collard 1968); or a tendency to occur in clumps (Pearcy 1964; McGowan and Fraundorf 1966; Harrisson 1967; Alldredge et al. 1984). Avoidance or escape may be more important during the day than at night (Pearcy and Laurs 1966). However, any daytime avoidance could be negated if either species is lethargic at diurnal depths, as indicated by Barham (1970), or it may be enhanced if they are hanging there but are quite ready to flee at the approach of a predator or midwater trawl (cf. Robison 1972).

Since surface waters contain more food than deep waters (Vinogradov 1974; Marshall 1954, 1980), *L. stilbius* and *S. leucopsarus*, like many other mesopelagic fishes, should benefit from regular feeding migrations toward the surface at night. They may retreat to deeper waters during the daytime to rest, digest, and avoid predation (cf. McLaren 1963; Paxton 1967a; Nafpaktitis 1968; Marshall 1954, 1980). Indeed, the common prey of both fishes tend to inhabit the upper 400 m, and many of them migrate vertically in a diel pattern.

Several authors have observed that *L. stilbius* concentrate at mid-depth during daytime but broaden their vertical distribution by dispersing upward at night, usually not in a distinct layer (Anderson 1967; Tucker 1951; Clarke 1970; Ebeling et al. 1970b). Other authors contend, from direct observations made off San Diego from deep submersibles, that *L. stilbius* seldom ascend above 500 m and therefore do not exhibit a daily migratory pattern, but at times they do come to the surface in large numbers (Barham 1970; Pickwell et al. 1970). This somewhat unpredictable behavior helps explain the high variability in the vertical distribution data, especially in the SBB (figure 12).

To optimize its feeding strategy, *L. stilbius* must sometimes visit the surface waters where larvaceans and salps occur. Our samples indicated that most of these fish descended before daylight, although possible laggards may avoid the trawl in sunlit waters during the daytime. Our medium-speed trawls may have caught them effectively in the dark but not during the daytime. But occasionally our trawl did catch many individuals near the surface during the day. Unfortunately, few shallow hauls were made during the day in the SCB.

Many authors have noted the diel vertical migration of *S. leucopsarus*; the fish is one component of the sonic scattering layer and tends to respond to a specific isolume (Tucker 1951; Barham 1957; Fast 1960; Bary et al. 1962; Pearcy and Laurs 1966; Paxton 1967a; Taylor 1968; Bary and Pieper 1970; Barham 1970; Clarke 1970; Pearcy and Mesecar 1970; Ebeling et al. 1970a,b). Others have also noted that not all of the population ascend toward the surface waters every night (Paxton 1967b; Barham 1970; Clarke 1970; Zahuranec and Pugh 1970).

Fullness and Recency of Feeding Relative to Vertical Migration

Prey of different species are most likely digested at varying rates under different conditions (Windell 1967). In general, the stomachs of small fishes probably empty in about 12 hours (Anderson 1967; Tyler 1970). However, since all four categories of fullness and recency of feeding occurred in both species, it should not matter how long digestion takes because the recency and fullness indices will be relative. Estimates of how recently a particular gut was filled, however, are not possible without data on digestion rates (Hopkins and Baird 1977).

The digestibility of the common prey of *L. stilbius* may shed some light on the fullness/recency data and the feeding cycle of this species. Shelbourne (1962) observed that soft tissues of *Oikopleura* were quickly digested after capture by larval plaice. This implies that the mostly intact larvaceans in recently full stomachs of *L. stilbius* were newly ingested. In the only other study of the feeding cycle of this species, Anderson (1967) found that the guts of *L. stilbius* in the Catalina Basin were fuller with only partially digested material at night than during the day. He suggested that *L. stilbius* feeds readily at the surface at night, but he could not determine if there was much feeding at greater depths. Our relatively high percentages of "recently full" stomachs, as compared with "recent but not full" stomachs among fish caught in the surface waters indicate that *L. stilbius* feeds to fullness whenever possible.

Existing studies of *S. leucopsarus* compare favorably with ours in that the fish were found to feed mostly at night near the surface (Anderson 1967; Holton 1969). But they were also found to feed in the morning and afternoon (Paxton 1967b; Tyler and Pearcy 1975). However, the digestibility of prey consumed by *S. leucopsarus* must be interpreted differently. Since this fish eats mostly crustaceans, digestion may take several hours. Therefore, relatively undigested items may persist in the stomachs of deep fish that had fed earlier in shallower waters. Like *L. stilbius*, *S. leucopsarus* never had a high percentage of "full but not recent" stomachs, implying that the fish clear their stomachs rapidly. Because many of their stomachs were empty, and because their proportion of "recent but not full" stomachs often exceeded their proportion of "full and recent" stomachs for most depths, these fish may feed whenever they can, mostly on larger, less digestible items. Thus they seldom completely fill their stomachs. Also, the primary prey of *S. leucopsarus* are found between 0 and 200 m at night and between 200 and 400 m during the day (Vinogradov 1968, 1974; Youngbluth 1976; figure 19). The ultimate resolution of this question awaits an evaluation of digestion rates of mesopelagic fishes at different temperatures (Gorelova 1975; Hopkins and Baird 1977; Young and Blaber 1986; Dalpadado and Gjosaeter 1988; Kinser and Schultz 1985).

These two midwater fishes appear to benefit from vertical migration in different ways. *L. stilbius* can occupy surface waters, either in the afternoon or at night, where it can use its large eyes to find salps and larvaceans, and be protected from predation by its silvery coloration. The rest of the time it can find refuge from surface predators in deeper waters. *S. leucopsarus*, on the other hand, with its photophores and large mouth, most likely migrates to deeper water to seek refuge from predation, but can feed at all times and depths on copepods and co-migrating euphausiids. Thus its regular migration to the surface it not obligatory, because the fish can consume prey at any depth. Yet vertical migrations may place these fishes in surface currents that might help them find concentrations of prey (Isaacs et al. 1974).

Factors Controlling Abundance of **L. stilbius** *and* **S. leucopsarus**

Several possible factors may explain why *L. stilbius* is less successful offshore than *S. leucopsarus*. Both species appear to breed successfully in both places. Analysis of egg sizes versus size of females (Childress et al. 1980) indicates that both species attain sexual maturity in the two localities. Also, larvae of the two fishes occur abundantly in both basins (Ahlstrom 1965). In the SBB, 50% of all *L. stilbius* captures and 45% of all *S. leucopsarus* captures were of young (<50 mm SL) or larvae; in the SCB, the figures were 70% for *L. stilbius* and 50% for *S. leucopsarus* (Brown 1974). Ebeling et al. (1970b) also concluded that all growth stages of these two common fishes were abundant in both places.

Even though the two species differ in seasonal abundances, there is no evidence that they enter or leave the two basins at different rates. Brown (1974) reported that the more physiographically and hydrographically isolated inshore SBB restricted faunal intrusions from the offshore oceanic environment. Ebeling et al. (1970a) defined an offshore fish group consisting of "tropical" species, which increase in numbers in the SCB and farther offshore during the summer and fall when the California Current weakens. *L. stilbius* and *S. leucopsarus* co-occur with these "offshore fishes," but both species also belong to a resident "inshore" community of midwater animals, which abound in both basins throughout the year. Their otoliths, found in bottom cores (Soutar and Isaacs 1969), indicate that both species have occupied the SBB for at least 2,000 years.

Despite differences in their parasite infection rates, both species seem equally healthy and robust in the two basins. *L. stilbius* is more heavily parasitized by trematodes in the SBB than the SCB (Noble

1968; Noble and Orias 1970). *S. leucopsarus*, on the other hand, is more heavily parasitized by cestodes in the SBB but is equally infected with nematodes in both basins (Noble and Collard 1970). Our observations of parasitism concur with the above studies, and condition factors did not significantly differ within species between basins.

Predation does not seem likely to be a key factor. The same kinds of predators, with the possible exception of relatively large, deep-sea fishes such as *Chauliodus* (Borodulina 1973), occur in both places and could eat both species. It is possible that the more bathymetrically compressed SBB habitat could concentrate more predators, but there is no evidence of this.

Deepsea smelts are reportedly eaten by rockfish (Lambert 1960), albacore (McHugh 1952), and cetaceans (Fitch and Brownell 1969). In particular, *L. stilbius* is eaten by albacore and bluefin tuna (Pinkas et al. 1971). One *Chiasmodon niger*, a predatory bathypelagic fish from the San Clemente Basin, had a large adult *L. stilbius* in its distended stomach (Borodulina 1973).

Likewise, myctophids are reportedly eaten by a variety of predators, including cephalopods; large pelagic fishes such as sharks, tunas, rockfishes, and swordfish; other deep-sea fishes; sea birds; and marine mammals (e.g., Marshall 1954; Paxton 1967b; Tyler and Pearcy 1975; Ainley et al. 1986). In particular, *S. leucopsarus* has been eaten by sharks (Hubbs 1917), salmon (Shimada 1948), albacore and bluefin tuna (Pinkas et al. 1971), rockfishes (Eigenmann and Eigenmann 1890; Starks and Morris 1907; Pereyra et al. 1969), and cetaceans (Fitch and Brownell 1969).

It therefore seems most probable that feeding habits best account for the differential success of these two mesopelagic fishes offshore. The way in which they use the available food resources may have a great deal to do with their relative success in different habitats.

Potential Role of These Fishes in Energy Transport to the Deep Sea

In spite of their apparent lack of nutritional value, numerous midwater fishes have been reported to feed on salps and larvaceans (e.g., Gorelova 1974, 1975; Baird et al. 1975b; Kashkina 1986; Longhurst and Harrison 1988; Hopkins and Torres 1989). How these fishes utilize the gelatinous zooplankters is still uncertain. Kashkina (1986) proposed that the tunica is only partially assimiliated, if at all, and it must take considerable energy for a fish to consume sufficient material to constitute a meal. On the other hand, because salps and larvaceans filter out small particles in the water column, including phytoplankton (Silver 1975; Alldredge 1976a), and are in turn consumed by other micronekton (Alldredge 1976b; Michaels and Silver 1988), they must provide energy throughout the open water column (Morris et al. 1988). Indeed, *L. stilbius* and *S. leucopsarus*, through their consumption of salps, larvaceans, and crustaceans, and through their vertical migrations, must play an active role in transporting energy sources from the surface to deeper water (cf. Pearcy et al. 1977), at least in the form of fecal matter, which sinks at several cm sec^{-1} (Robison and Bailey 1981). Research is needed to determine if these fishes influence the rates of vertical flux of organic matter in the open ocean.

ACKNOWLEDGMENTS

We thank R. W. Holmes, J. H. Connell, E. R. Noble, B. Robison, and B. Nafpaktitis for critically reviewing the original manuscript. G. S. Arita, D. W. Brown, S. B. Collard, F. A. DeWitt, Jr., H. Genthe, L. Hendrian, R. M. Ibara, M. S. Love, P. Setzer, and H. Tyler helped with the field work and laboratory data analyses. R. M. Ibara and M. Palmgren wrote computer programs that helped with the statistical analyses. The University of California, Santa Barbara Computer Center provided additional computer time. This study was supported by NSF grants GB 2867 and 4698 for ship time and GB 4669 and 7973 for the shore-based analyses. A University Fellowship, University of California, Santa Barbara, provided the senior author with one year of support.

LITERATURE CITED

Ahlstrom, E. H. 1965. Evaluation of fishes resources of the Pt. Arguello area. Part I. Fish larvae of the Pt. Arguello area. Report AT (49-7)-2428, Division of Biology and Medicine, U.S. Atomic Energy Commission.

———. 1969. Mesopelagic and bathypelagic fishes in the California Current Region. Calif. Coop. Oceanic Fish. Invest. Rep. 13: 39–44.

Ainley, D. G., W. R. Fraser, C. W. Sullivan, J. J. Torres, T. L. Hopkins, and W. O. Smith. 1986. Antarctic mesopelagic micronekton: evidence from sea birds that pack ice affects community structure. Science 232: 847–849.

Alexander, R. M. 1967. Functional design in fishes. London: Hutchinson and Co., 164 pp.

Alldredge, A. L. 1967a. Appendicularians. Sci. Am. 235(1):94–102.

———. 1976b. Discarded appendicularian houses as sources of food, surface habitats, and particulate organic matter in planktonic environments. Limnol. Oceanogr. 21(1):14–23.

———. 1976c. Field behavior and adaptive strategies of appendicularians (Chordata: tunicata). Mar. Biol. 38:29–39.

Alldredge, A. L., B. H. Robison, A. Fleminger, J. J. Torres, J. M. King, and W. M. Hamner. 1984. Direct sampling and in situ observation of a persistent copepod aggregation in the mesopelagic zone of the Santa Barbara Basin. Mar. Biol. 80:75–81.

Anderson, R. 1967. Feeding chronology in two deep-sea fishes off California. M. S. thesis, University of Southern California.

Aron, W. I., and S. B. Collard. 1968. A study of the influence of net speed on catch. Limnol. Oceanogr. 14:242–249.

Aron, W. I., N. Raxter, R. Noel, and W. Andrews. 1964. A description of a discrete depth plankton sampler with some notes on the towing behavior of a 6-foot Isaacs-Kidd midwater trawl and a 1-m ring net. Limnol. Oceanogr. 9:324–333.

Aughtry, R. H. 1953. A note on the mass mortality of the myctophid fish *Tarletonbeania crenularis*. Copeia 1953(3):190–192.

Baird, R. C., T. L. Hopkins, and D. F. Wilson. 1975a. Diet and feeding chronology of *Diaphus taaningi* (Mytophidae) in the Cariaco Trench. Copeia 1975(2):356–365.

Baird, R. C., N. P. Thompson, T. L. Hopkins, and W. R. Weiss. 1975b. Chlorinated hydrocarbons in mesopelagic fishes of the eastern Gulf of Mexico. Bull. Mar. Sci. 25(4):473–481.

Barham, E. G. 1957. The ecology of sonic scattering layers in the Monterey Bay area. Stanford University, Hopkins Mar. Sta. Tech. Rep. 1:1–182.

———. 1970. Deep-sea fishes: lethargy and vertical orientation. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 101–119.

Bary, B. M., and R. E. Pieper. 1970. Sonic-scattering studies in Saanich Inlet, British Columbia: a preliminary report. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 601–611.

Bary, B. M., W. E. Barraclough, and R. Herlinveaux. 1962. Scattering of underwater sound in Saanich Inlet, British Columbia. Nature 194:36–37.

Beebe, W., and M. Vander Pyl. 1944. Eastern Pacific Expeditions of the New York Zoological Society. XXXIII. Pacific Myctophidae (fishes). Zoologica 29:59–95.

Berner, L. D., Jr. 1957. Studies on the Thaliacea of the temperate northeast Pacific Ocean. Ph.D. thesis, University of California, Los Angeles.

———. 1967. Distributional atlas of Thaliacea in the California Current region. Calif. Coop. Oceanic Fish. Invest. Atlas (8):1–322.

Berry, F. H. 1964. Aspects of the development of the upper jaw bones in teleosts. Copeia 1964(3):375–384.

Bolin, R. 1939. A review of the myctophid fishes of the Pacific coast of the United States and of lower California. Stanford Ichthyol. Bull. 1:89–156.

Borodulina, O. D. 1968. Taxonomy and distribution of the genus *Leuroglossus* (Bathylagidae, Pisces.) Prob. Ichthyol. 8:1–10.

———. 1973. The feeding of mesopelagic predatory fish in the open ocean. J. Ichthyology, Acad. Sci. USSR 12(4):692–702.

Bourbeau, F., W. D. Clarke, and W. Aron. 1966. Improvements in the discrete depth plankton sampler system. Limnol. Oceanogr. 11:422–426.

Bradbury, M. G., and D. P. Abbott. 1970. Studies on the fauna associated with the deep scattering layers in the equatorial Indian Ocean, conducted on R/V Te Vega during October and November 1964, *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 409–452.

Brown, D. W. 1974. Hydrography and midwater fishes of three contiguous oceanic areas off Santa Barbara, California. Nat. Hist. Mus., Los Angeles Co., Contrib. Sci. (261):1–30.

Cailliet, G. M., M. S. Love, and A. W. Ebeling. 1986. Fishes: a field and laboratory manual on their structure, identification, and natural history. Belmont, California: Wadsworth Publishing Co., 194 pp.

Chapman, W. M. 1943. The osteology and relationships of the bathypelagic fishes of the genus *Bathylagus* Gunther with notes on the systematic position of *L. stilbius* Gilbert and *Therobromus callorhinus* Lucas. J. Wash. Acad. Sci. 33:147–160.

Childress, J. J., and M. H. Nygaard. 1973. The chemical composition of midwater fishes as a function of depth of occurrence off southern California. Deep-Sea Res. 20:1093–1109.

Childress, J. J., S. M. Taylor, G. M. Cailliet, and M. H. Price. 1980. Patterns of growth, energy utilization and reproduction in some meso- and bathypelagic fishes off southern California. Mar. Biol. 61:27–40.

Clarke, T. A. 1978. Diel feeding patterns of 16 species of mesopelagic fishes from Hawaiian waters. Fish. Bull. 76(3):495–513.

Clarke, W. D. 1970. Comparison of different investigative techniques for studying the deep scattering layers. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 551–562.

Collard, S. B. 1970. Forage of some eastern Pacific midwater fishes. Copeia 1970(2):348–354.

Dalpadado, P., and J. Gjosaeter. 1988. Feeding ecology of the lanternfish *Benthosema pterotum* from the Indian Ocean. Mar. Biol. 99:555–567.

Darnell, R. M. 1970. Evolution and the ecosystem. Am. Zool. 10:9–17.

Ebeling, A. W., G. M. Cailliet, R. M. Ibara, F. A. DeWitt, Jr., and D. W. Brown. 1970a. Pelagic communities and sound scattering off Santa Barbara, California. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 1–19.

Ebeling, A. W., R. M. Ibara, R. J. Lavenberg, and F. J. Rohlf. 1970b. Ecological groups of deep-sea animals off southern California. Bull. Los Angeles Co. Mus. Nat. Hist., Science (6):1–43.

Eigenmann, C. H., and R. S. Eigenmann. 1890. Additions to the fauna of San Diego. Proc. Calif. Acad. Sci. 2(3):1–24.

Emery, K. O. 1960. The sea off southern California. New York: John Wiley, 336 pp.

Eschmeyer, W. N., E. S. Herald, and H. Hammann. 1983. A field guide to Pacific coast fishes of North America. Boston: Houghton Mifflin Company, 336 pp.

Fager, E. W. 1972. Diversity: a sampling study. Am. Nat. 106:293–310.

Fast, T. N. 1960. Some aspects of the natural history of *Stenobrachius leucopsarus* Eigenmann and Eigenmann. Ph.D. thesis, Stanford University.

Fitch, J., and R. L. Brownell, Jr. 1969. Fish otoliths in cetacean stomachs and their importance interpreting feeding habits. J. Fish. Res. Bd. Can. 25:2561–2574.

Frost, B. W., and L. E. McCrone. 1979. Vertical distribution, diel vertical migration, and abundance of some mesopelagic fishes in the eastern subarctic Pacific Ocean in summer. Fish. Bull. 76(4):717–749.

Gorelova, T. A. 1974. Zooplankton organisms from the stomachs of lantern-fish juveniles of the family Myctophidae (in Russian; English abstract). Okeanologiia 14(4):713–718.

———. 1975. The feeding of fishes of the family Myctophidae. J. Ichthyol. 15(2):208–219.

Groot, S. J. de. 1969. Digestive system and sensorial factors in relation to the feeding behaviour of flatfish (Pleuronectiformes). J. Conseil 32:385–395.

Harrisson, C. M. H. 1967. On methods for sampling mesopelagic fishes. *In* Symp. Zool. Soc. London, pp. 71–126.

Hart, J. L. 1973. Pacific fishes of Canada. Fish. Res. Bd. Can. Bull. (180) 740 pp.

Holton, A. A. 1969. Feeding behavior of a vertically migrating lanternfish. Pac. Sci. 23:325–331.

Hopkins, T. L., and R. C. Baird. 1975. Net feeding in mesopelagic fishes. Fish. Bull. 73(4):908–914.

———. 1977. Aspects of the feeding ecology of oceanic midwater fishes. *In* Oceanic sound scattering prediction, N. R. Andersen and B. J. Zahuranec, eds. New York: Plenum Press, pp. 325–360.

Hopkins, T. L., and J. J. Torres. 1989. Midwater food web in the vicinity of a marginal ice zone in the western Weddell Sea. Deep-Sea Res. 36(4):543–560.

Hubbard, L. T., and W. G. Pearcy. 1971. Geographic distribution and relative abundance of Salpidae off the Oregon coast. J. Fish. Res. Bd. Can. 28:1831–1836.

Hubbs, C. L. 1917. A note on the food of *Squalus sucklii*, the Californian dogfish. Copeia 1917(3):37–38.

Isaacs, J. D., S. A. Tont, and G. L. Wick. 1974. Deep scattering layers: vertical migration as a tactic for finding food. Deep-Sea Res. 21:651–656.

Jollie, M. T. 1954. General anatomy of *Lampanyctus leucopsarus* (Eigenmann and Eigenmann). Ph.D. thesis, Stanford University.

Jones, J. H. 1971. General circulation and water characteristics in the Southern California Bight. Rep. Calif. Coastal Water Res. Project, Los Angeles, California.

Kapoor, B. G., H. Smit, and I. A. Verighina. 1975. The alimentary canal and digestion in teleosts. Adv. Mar. Biol. 13:109–239.

Kashkina, A. A. 1986. Feeding of fishes on salps (Tunicata, Thaliacea). Voprosy Ikhtiologii (3):440–447. Translated in 1987 by Scripta Technica, Inc. ISSN0032-9452/86/0004-0057.

Kinzer, J., and K. Schulz. 1985. Vertical distribution and feeding patterns of midwater fish in the central equatorial Atlantic. I. Myctophidae. Mar. Biol. 85:313–322.

Lambert. 1960. The food of the redfish *Sebastes marinus* (L.) in the Newfoundland area. J. Fish. Res. Bd. Can. 17:235–243.

Lancraft, T. M., and B. H. Robison. 1980. Evidence of post-capture ingestion by midwater fishes in trawl nets. Fish. Bull. 77(3):713–715.

Lavenberg, R. J., and A. W. Ebeling. 1967. Distribution of midwater fishes among deep-water basins of the southern California shelf. *In* Proc. Symp. Biol. Calif. Islands, Santa Barbara Botanic Garden, R. M. Philbrick, ed. Pp. 185–201.

Legand, M., and J. Rivaton. 1969. Cycles biologiques des poissons mesopelagiques de l'est de l'Ocean Indien. Troisieme note: action predatrice des poissons micronectoniques. Cah. ORSTOM (ser. Oceanogr.) 7:29–45.

Longhurst, A. R. 1967. Diversity and trophic structure of zooplankton communities in the California Current. Deep-Sea Res. 14:393–408.

Longhurst, A. R., and W. G. Harrison. 1988. Vertical nitrogen flux from the oceanic photic zone by diel migrant zooplankton and nekton. Deep-Sea Res. 35(6):881–889.

MacArthur, R. H., and J. H., Connell. 1966. The biology of populations. New York: John Wiley, 200 pp.

Malone, T. C. 1971. The relative importance of nannoplankton and netplankton as primary producers in the California Current system. Fish. Bull. 69:799–820.

Marshall, N. B. 1954. Aspects of deep-sea biology. London: Hutchinson, 379 pp.

———. 1980. Deep-sea biology: developments and perspectives. New York: Garland STPM Press, 566 pp.

Martin, N. V., and F. K. Sandercock. 1967. Pyloric caeca and gill raker development in lake trout, *Salvelinus namaycush*, in Algonquin Park, Ontario. J. Fish. Res. Bd. Can. 24:965–974.

McAllister, D. E. 1961. A collection of oceanic fishes from off British Columbia with a discussion of the evolution of black peritoneum. Bull. Nat. Mus. Can. 172:39–43.

McGowan, J. A., and V. J. Fraundorf. 1966. The relationship between size of net used and estimates of zooplankton diversity. Limnol. Oceanogr. 11:456–469.

McHugh, J. L. 1952. The food of albacore (*Germo alalunga*) off California and Baja California. Bull. Univ. Calif. Scripps Inst. Oceanogr. 6:161–172.

McLaren, I. A. 1963. Effects of temperature on growth of zooplankton, and the adaptive value of vertical migration. J. Fish. Res. Bd. Can. 20:685–727.

Michaels, A. F., and M. W. Silver. 1988. Primary production, sinking fluxes and the microbial food web. Deep-Sea Res. 35(4):473–490.

Miller, D. J., and R. N. Lea. 1972. Guide to the coastal marine fishes of California. Calif. Fish Game, Fish Bull. 157:1–235.

Morris, R. J., Q. Bone, R. Head, J. C. Braconnot, and P. Nival. 1988. Role of salps in the flux of organic matter to the bottom of the Ligurian Sea. Mar. Biol. 97(2):237–242.

Nafpaktitis, B. G. 1968. Taxonomy and distribution of the lanternfishes, genera *Lobianchia* and *Diaphus*, in the North Atlantic. Dana-Rept. 73:1–131.

Nakamura, E. L. 1970. Observations on the biology of the myctophid *Diaphus garmani*. Copeia 1970(2):374–377.

Noble, E. R. 1968. The flagellate *Cryptobia* in two species of deep sea fishes from the eastern Pacific. J. Parasit. 54:720–724.

Noble, E. R., and S. B. Collard. 1970. The parasites of midwater fishes. *In* A symposium on diseases of fishes and shellfishes, S. Sniesko, ed. Spec. Publ. Am. Fish. Soc. No. 5, p. 57068.

Noble, E. R., and J. D. Orias. 1970. The trematode *Aponurus californicus* n. sp. and its host, the deep-sea smelt, *Leuroglossus stilbius*. Trans. Am. Microsc. Soc. 89:413–417.

Osterberg, C., W. G. Pearcy, and H. Curl, Jr. 1964. Radioactivity and its relationship to oceanic food chains. J. Mar. Res. 22:2–12.

Paxton, J. R. 1967a. A distributional analysis for the lanternfishes (Family Myctophidae) of the San Pedro Basin, California. Copeia 1967(2):422–440.

———. 1967b. Biological notes on southern California lanternfishes (Family Myctophidae). Calif. Fish Game 53:214–217.

Pearcy, W. G. 1964. Some distributional features of mesopelagic fishes off Oregon. J. Mar. Res. 22:83–102.

———. 1976. Seasonal and inshore-offshore variations in the standing stocks of micronekton and macrozooplankton off Oregon. Fish. Bull. 74(1):70–80.

Pearcy, W. G., and R. M. Laurs. 1966. Vertical migration and distribution of mesopelagic fishes off Oregon. Deep-Sea Res. 13:153–165.

Pearcy, W. G., and R. S. Mesecar. 1970. Scattering layers and vertical distribution of oceanic animals off Oregon. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 381–394.

Pearcy, W. G., E. E. Krygier, and N. H. Cutshall. 1977. Biological transport of zinc-65 into the deep sea. Limnol. Oceanogr. 22(5):846–855.

Pereyra, W. T., W. G. Pearcy, and F. E. Carvey, Jr. 1969. *Sebastodes flavidus*, a shelf rockfish feeding on mesopelagic fauna, with consideration of the ecological implications. J. Fish. Res. Bd. Can. 25:2211–2215.

Pickwell, G. V., R. J. Vent, E. G. Barham, W. E. Batzler, and I. E. Davies. 1970. Biological acoustical scattering off southern California, Baja California, and Guadalupe Island. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 490–507.

Pinkas, L., M. S. Oliphant, and I. L. K. Iverson. 1971. Food habits of albacore, bluefin tuna, and bonito in California waters. Calif. Fish Game, Fish Bull. 152:1–105.

Raymont, J. E. G. 1970. Problems of the feeding of zooplankton in the deep sea. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean. Sci., Washington, D.C. No. 5, pp. 136–148.

Robison, B. H. 1972. Distribution of the midwater fishes of the Gulf of California. Copeia 1972(3):448–461.

Robison, B. H., and T. G. Bailey. 1981. Sinking rates and dissolution of midwater fish fecal matter. Mar. Biol. 65:135–142.

Shelbourne, J. E. 1962. A predator-prey size relationship for plaice larvae feeding on *Oikopleura*. J. Mar. Biol. Assn. U.K. 42:243–252.

Shimada, B. M. 1948. Records of lanternfish in Puget Sound. Copeia 1948(3):227.

Sholkovitz, E. R., and J. M. Gieskes. 1971. A physical-chemical study of the flushing of the Santa Barbara Basin. Limnol. Oceanogr. 16:479–489.

Silver, M. 1975. The habitat of *Salpa fusiformis* in the California Current as defined by indicator assemblages. Limnol. Oceanogr. 20(2):230–237.

Sokal, R. R., and F. J. Rohlf. 1969. Biometry. San Francisco: W. H. Freeman and Co. 776 pp.

Soutar, A., and J. D. Isaacs. 1969. History of fish populations inferred from fish scales in anaerobic sediments off California. Calif. Coop. Oceanic Fish. Invest. Rep. 13:63–70.

Starks, E. C., and E. L. Morris. 1907. The marine fishes of southern California. Univ. Calif. Publ. Zool. 3:159–251.

Taylor, F. H. C. 1968. The relationship of midwater trawl catches to sound scattering layers off the coast of northern British Columbia. J. Fish. Res. Bd. Can. 25:457–472.

Tucker, G. 1951. Relations of fishes and other organisms to the scattering of underwater sound. J. Mar. Res. 10:215–238.

Tyler, A. V. 1970. Rates of gastric emptying in young cod. J. Fish. Res. Bd. Can. 27:1177–1189.

Tyler, H. R., and W. G. Pearcy. 1975. The feeding habits of three species of lanternfishes (Family myctophidae) off Oregon, USA. Mar. Biol. 32:7–11.

Vinogradov, M. E. 1968. Vertical distribution of the oceanic zooplankton. Israel Program for Sci. Transl., Jerusalem (1970), 330 pp.

———. 1974. Depth of the nighttime rise of deep scattering layers in the central Pacific. Oceanology 14(6):891–895 (Translated by Scripta Technica, Inc. for the Am. Geophys. Union).

Willis, J. M., and W. G. Pearcy. 1980. Spatial and temporal variations in the population size structure of three lanternfishes (Myctophidae) off Oregon, USA. Mar. Biol. 57:181–191.

Windell, J. T. 1967. Rates of digestion in fishes. *In* The biological basis of freshwater fish production, S. Gerking, ed. New York: John Wiley, pp. 151–173.

Yasuda, F. 1960a. The feeding mechanism in young fishes. Rec. Oceanogr. Works Japan 5:132–138.

———. 1960b. The feeding mechanisms in some carnivorous fishes. Rec. Oceanogr. Works Japan 5:153–160.

———. 1960c. The relationship of the gill structure and food habits of some coastal fishes in Japan. Rec. Oceanogr. Works Japan 5:139–152.

Yasuda, F., and Y. Hiyama. 1957. Mechanism of utilization of plankton by some fishes. Rec. Oceanogr. Works Japan 3:85–91.

Young, J. W., and S. J. M. Blaber. 1986. Feeding ecology of three species of midwater fishes associated with the continental slope of eastern Tasmania, Australia. Mar. Biol. 93:147–156.

Youngbluth, M. J. 1976. Vertical distribution and diel migration of euphausiids in the central region of the California Current. Fish. Bull. 74(4):925–936.

Zahuranec, B. J., and W. L. Pugh. 1970. Biological results from scattering layer investigations in the Norwegian Sea. *In* Proc. Internat. Symp. Biol. Sound Scattering in the Ocean, G. B. Farquhar, ed. Maury Center Ocean Sci., Washington, D.C. No. 5, pp. 360–380.

THE MARKET FOR FISH MEAL AND OIL IN THE UNITED STATES: 1960–1988 AND FUTURE PROSPECTS

CYNTHIA J. THOMSON
National Marine Fisheries Service
Southwest Fisheries Center
P.O. Box 271
La Jolla, California 92038

ABSTRACT

Fish meal is used in the United States largely as a high-protein ingredient in poultry feed. Prices of domestic fish meal are determined by world market conditions for fish meal as well as other oil meals. Faced with limited fish meal supplies and little control over prices, the U.S. poultry industry has substituted other ingredients and made use of technological advances to satisfy the nation's growing demand for table birds.

A number of factors have been identified that may significantly affect future demand, supply, and prices of fish meal and oil, both in the United States and abroad. These include (1) increases in world aquaculture production, (2) possible development of a domestic market for hydrogenated fish oil, (3) recent changes in the Alaska pollock fishery, (4) efforts to develop marketable products for direct human consumption from reduction species, and (5) the status of the Japanese and South American sardine fisheries.

RESUMEN

La harina de pescado es un ingrediente de alto contenido proteico utilizado corrientemente en la alimentación de aves de corral en los Estados Unidos de Norteamérica. El mercado mundial determina el precio de la harina, como así también el de otras harinas de orujo. Ante el problema de un limitado abastecimiento de harina de pescado y un reducido control en los precios, la industria de la pollería se vió obligada a sustituír otros ingredientes y hacer uso de los avances tecnológicos con el fin de satisfacer la creciente demanda de estas aves de corral.

Se han identificado un número de factores que pueden afectar significativamente la futura demanda, el abastecimiento y el precio de la harina de pescado y otros aceites, tanto en los Estados Unidos como en el exterior, a saber: (1) el aumento en la producción de las piscifactorías, (2) el posible desarrollo de un mercado doméstico de aceite de pescado hidrogenado, (3) los cambios recientes en la pesquería del bacalao de Alaska, (4) los esfuerzos para desarrollar productos de consumo directo por el ser humano, y por último (5) el estado de las pesquerías en el Japón y en Sudamérica.

INTRODUCTION

The fish meal and oil industry began in the nineteenth century in northern Europe and North America. The oil was manufactured from surplus fish caught in the herring fishery, and was used to tan leather and as an ingredient in products such as soap. The residue was originally used as fertilizer. However, since the turn of this century, the use of fish for fertilizer has diminished considerably because increasing amounts have been diverted to the production of fish meal (FAO 1986).

The fish meal production process, which is known as reduction, involves cooking the fish, removing the water and oil from it, drying the solid material left behind, and grinding it into a meal. Fish meal is used in the United States largely as a high-protein ingredient in poultry feed. It is also used in feeds for pigs, farmed fish, fur-producing animals, laboratory animals, and household pets.

The oil that is removed in the reduction process is marketed as an ingredient for industrial products (such as paints and lubricants) and foods (such as margarines and shortenings). Most of the fish oil produced in the United States has historically been exported to Europe for use in margarines. Until very recently, the U.S. Food and Drug Administration did not allow the use of fish oil in products for domestic human consumption.

Approximately one metric ton (MT) of fish meal is produced from each 4–5 MT of fish harvested. The oil yield is more variable and depends on the species and the time of year when the fish are caught (Vondruska 1980). For example, the oil content of northern anchovy is low during the winter and spring spawning period, and highest in late summer (Lasker and Smith 1977). The wholesale value of U.S. fish meal production has historically exceeded the value of oil production by a factor of two to four (table 1).

[Manuscript received February 3, 1990.]

TABLE 1
Quantity and Value of Fish Meal and Oil Produced in the United States

	Production quantity (1000's of metric tons)		Value (millions of dollars)	
Year	Fish meal	Fish oil	Fish meal	Fish oil
1960	263.2	94.8	25.3	13.0
1961	282.4	117.1	31.9	14.3
1962	283.3	113.4	35.6	11.0
1963	232.2	84.3	30.2	10.9
1964	213.5	81.7	28.0	13.3
1965	230.5	88.7	35.7	14.9
1966	203.4	74.7	32.3	12.5
1967	191.6	55.5	26.0	6.1
1968	213.3	79.0	30.3	7.3
1969	229.2	77.0	39.8	9.3
1970	244.2	93.5	46.4	18.2
1971	265.6	120.4	44.5	20.8
1972	259.0	85.5	48.3	13.1
1973	253.2	101.9	119.1	25.6
1974	264.6	107.9	83.5	49.2
1975	253.4	111.4	64.6	32.6
1976	271.3	92.8	95.7	31.2
1977	248.4	60.7	96.5	28.4
1978	320.9	134.4	120.2	60.7
1979	329.2	121.4	133.3	54.1
1980	322.3	141.8	132.9	57.9
1981	281.3	83.6	117.6	33.1
1982	330.4	157.6	121.2	53.6
1983	339.0	181.1	129.1	66.8
1984	334.7	169.1	112.6	61.0
1985	319.6	129.3	83.1	41.9
1986	308.1	152.8	82.4	43.7
1987	349.6	135.4	120.9	35.5
1988	283.5	101.9	129.2	43.6

References: U.S. Department of the Interior 1960–1970; U.S. Department of Commerce 1971–1988.

Fish and shellfish landed commercially in the United States are used for human consumption and industrial products. Over 85% of industrial use is attributable to reduction; the remainder consists of bait and animal food. Since 1960, industrial uses have accounted for 36%–53% of total landings but only 4%–11% of total ex-vessel revenues on an annual basis (table 2). Reduction landings tend to be large in quantity but low in value relative to landings used for direct human consumption.

SUPPLY

Production

The species used for reduction are small, oily, pelagic fishes that are not marketable in large quantities for human consumption. In the United States, these include northern anchovy (*Engraulis mordax*) on the Pacific Coast and menhaden (*Brevoortia tyrannus* and *B. patronus*) on the Atlantic and Gulf coasts. A substantial reduction fishery once existed in California for Pacific sardine (*Sardinops sagax*), but the fishery collapsed in the early 1950s (Radovich 1981). The state of California lifted its moratorium on sardine landings in 1986, but so far has allowed modest harvests to be taken for nonreduction uses only.

Although most fish meal is produced from whole fish, about 10% is produced from the by-catches and byproducts of other fisheries. Examples are tuna-mackerel and pollock meals, which are produced from the scraps remaining after these species are processed into other market products.

As indicated in table 3, U.S. fish meal production ranged from 200 to 350 thousand MT per year from 1960 through 1988. Before 1982, menhaden meal constituted 55%–80% of annual production. Since 1982, menhaden's share has been even higher, averaging 85% of total production. Tuna-mackerel meal contributes 20,000–45,000 MT per year.

Anchovy meal production peaked in 1975 at 25,100 MT, when its share of total production was 10%. However, meal production from this species has been much lower in most other years and has been negligible since 1983, largely because of economic factors rather than low abundance. From 1983 through 1988 the fish meal price ranged from $240 to $440 per MT, and the ex-vessel price received by the menhaden fleet ranged from $80 to $115 per MT. However, for reasons that are not clear, the ex-vessel anchovy price offered by California processors has remained at record low levels (below $35 per MT). As a result, the California reduction fleet has not found it profitable to target on anchovy and has directed increasing amounts of effort to more lucrative species such as mackerel, tuna, and squid (Thomson et al. 1989).

Imports and Exports

Figure 1 describes the contributions of Peru and Chile to world exports of fish meal. From 1960 to 1972 Peruvian anchoveta (*Engraulis ringens*) accounted for 50%–63% of all the fish meal traded in international markets. A combination of overfishing and poor recruitment led to the collapse of the fishery during the 1972–73 El Niño (Glantz 1979). The recovery of the fishery has enabled Peru to significantly increase its exports in the 1980s, though not to the high levels of the 1960s and early 1970s. Chile's fish meal exports began to increase in the early 1970s, as a result of its developing sardine (*Sardinops sagax*) and jack mackerel (*Trachurus murphyi*) fisheries. Since 1980 Chile's exports have exceeded Peru's.

TABLE 2
U.S. Commercial Finfish and Shellfish Landings and Ex-Vessel Value (Millions of Dollars) by Disposition of Catch

	Human food		Industrial use[b]		Total	
Year	Landings[a]	Value	Landings	Value	Landings	Value
1960	1133.1	—	1108.6	—	2241.7	354
1961	1129.5	—	1223.4	—	2352.8	362
1962	1152.1	—	1276.4	—	2428.6	396
1963	1159.4	—	1039.2	—	2198.6	377
1964	1132.6	—	927.2	—	2059.8	389
1965	1173.6	408	993.2	29	2166.8	446
1966	1166.9	437	813.5	24	1980.4	472
1967	1073.9	414	765.3	17	1839.1	440
1968	1064.1	468	822.8	29	1887.0	497
1969	1052.8	492	914.5	35	1967.3	527
1970	1150.8	565	1079.6	48	2230.3	613
1971	1107.2	604	1168.9	47	2276.1	651
1972	1104.5	702	1075.3	46	2180.0	748
1973	1087.7	836	1115.8	101	2203.6	937
1974	1132.2	844	1120.8	88	2253.0	932
1975	1118.1	904	1094.1	73	2212.2	977
1976	1258.7	1257	1185.2	92	2444.0	1349
1977	1339.0	1440	1051.9	114	2390.9	1554
1978	1441.1	1733	1293.2	121	2734.3	1854
1979	1505.0	2093	1337.7	141	2842.7	2234
1980	1657.4	2092	1282.8	145	2940.2	2237
1981	1608.9	2277	1102.2	111	2711.1	2388
1982	1490.1	2247	1398.0	143	2888.1	2390
1983	1468.7	2203	1452.0	152	2920.7	2355
1984	1505.9	2206	1414.3	144	2920.3	2350
1985	1494.1	2198	1344.5	128	2838.6	2326
1986	1539.1	2641	1196.6	122	2735.6	2763
1987	1789.9	2979	1338.1	136	3128.0	3115
1988	2081.1	3362	1181.2	158	3262.3	3520

[a] Thousands of metric tons, round weight; excludes weight of mollusk shells.

[b] Over 85% processed into meal, oil, and solubles. The remainder is used for shell products, bait, and animal food.

References: U.S. Department of the Interior 1960–1970; U.S. Department of Commerce 1971–1988.

The two countries together account for 40%–50% of world fish meal exports in the 1980s.

U.S. imports of fish meal have tended to follow the worldwide pattern of availability. As indicated in table 4, Peru provided us with 52%–90% of our imported supplies from 1960 through 1972. Peruvian imports declined significantly after the collapse of the anchoveta fishery, and the resulting void was not substantially filled by anyone else until the mid-1980s. In recent years, Chile has emerged as our major foreign supplier. We also import a modest but fairly steady amount of meal (averaging about 30,000 MT annually) from Canada, and smaller and more variable amounts from miscellaneous other countries.

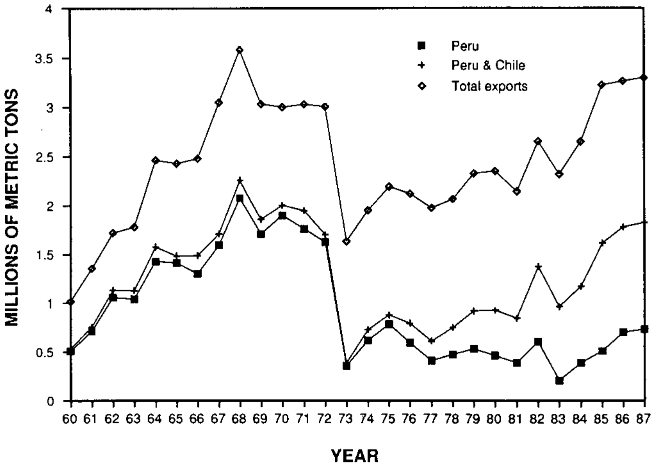


Figure 1. Fish meal exports by Peru, Chile, and all countries, 1960–87.

Up until 1970, U.S. fish meal exports were negligible and tended to be disregarded in published statistics. Exports have fluctuated widely from 4,300 to 77,400 MT during 1970–88. Exports exceeded imports in 1978, 1980, and 1983 (table 4).

Total fish meal supply (i.e., production plus imports minus exports) has declined somewhat in the post-1972 period relative to earlier years (figure 2). The variability in supply closely parallels the variability in net imports (i.e., imports minus exports). Domestic production (depicted by the difference between supply and net imports) has been much more stable by comparison.

TABLE 3
Fish Meal Production in the United States by Species (Thousands of Metric Tons)

Year	Menhaden	Tuna-Mack	Anchovy	Other	Total
1960	198.1	24.0	0.0	41.0	263.2
1961	224.6	19.2	0.0	38.6	282.4
1962	217.5	24.1	0.0	41.7	283.3
1963	167.1	24.5	0.0	40.6	232.2
1964	145.4	19.1	0.0	48.9	213.5
1965	159.7	23.0	0.0	47.8	230.5
1966	122.5	23.0	4.1	53.9	203.4
1967	108.0	23.1	5.1	55.3	191.6
1968	129.9	26.1	2.5	54.7	213.3
1969	144.7	24.4	10.3	49.8	229.2
1970	171.1	24.2	14.7	34.2	244.2
1971	200.5	26.6	7.0	31.6	265.6
1972	175.6	39.2	10.1	34.1	259.0
1973	171.3	39.6	20.0	22.4	253.2
1974	185.0	43.7	12.8	23.1	264.6
1975	173.6	33.7	25.1	20.9	253.4
1976	192.9	36.4	20.1	21.9	271.3
1977	175.4	36.1	17.3	19.6	248.4
1978	250.8	45.9	1.9	22.2	320.9
1979	254.7	43.0	9.0	22.5	329.2
1980	246.0	42.6	7.1	26.6	322.3
1981	209.4	42.8	9.3	19.9	281.3
1982	273.9	32.1	7.3	17.1	330.4
1983	286.6	37.8	0.5	14.2	339.0
1984	285.7	33.7	0.0	15.3	334.7
1985	279.0	31.3	0.0	9.3	319.6
1986	268.8	33.7	0.0	5.6	308.1
1987	303.4	38.3	0.0	8.0	349.6
1988	228.9	34.5	0.0	20.1	283.5

References: U.S. Department of the Interior 1960–1970; U.S. Department of Commerce 1971–1988.

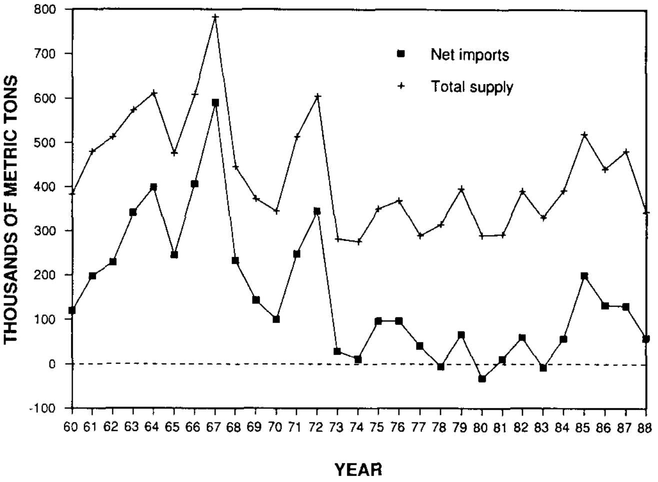


Figure 2. U.S. supply and net imports of fish meal, 1960–88.

PRICES

High-protein oil meals, like fish meal, are traded in a very competitive international market. The soybean meal price is generally considered to be a leading indicator for fish meal and other oil meal prices. Figure 3 is a graph of U.S. soybean meal and menhaden meal prices, both corrected for inflation to 1988 dollars. The prices are highly correlated, with the price differential largely attributable to the difference in protein content.

TABLE 4
U.S. Fish Meal Imports, Exports, and Net Imports (Thousands of Metric Tons)

	Imports by country of origin					Exports	Net imports
Year	Peru	Chile	Canada	Other	Total		
1960	61.7	19.1	28.1	10.8	119.7	—	119.7
1961	137.3	11.0	34.7	14.6	197.6	—	197.6
1962	168.9	8.3	38.8	12.9	228.9	—	228.9
1963	258.9	21.4	46.3	14.8	341.4	—	341.4
1964	315.7	11.7	49.7	21.2	398.3	—	398.3
1965	190.3	5.2	39.7	10.4	245.6	—	245.6
1966	250.9	81.1	39.6	34.6	406.2	—	406.2
1967	401.4	37.1	42.3	110.2	591.0	—	591.0
1968	182.6	18.3	13.0	19.2	233.1	—	233.1
1969	99.8	19.6	19.2	4.7	143.3	—	143.3
1970	73.9	6.4	22.4	2.4	105.1	4.3	100.8
1971	181.1	0.0	52.3	23.5	256.9	9.2	247.7
1972	319.5	0.0	25.0	11.1	355.6	9.4	346.2
1973	37.9	0.0	22.3	1.9	62.1	33.3	28.8
1974	26.7	0.0	27.5	7.8	62.0	50.3	11.7
1975	68.5	7.0	30.8	1.1	107.4	10.7	96.7
1976	72.0	0.0	30.8	24.6	127.4	30.0	97.4
1977	14.2	2.0	22.5	35.2	73.9	32.7	41.2
1978	6.0	0.0	29.7	4.1	39.8	46.0	−6.2
1979	25.6	7.5	24.7	23.5	81.3	14.2	67.1
1980	6.0	0.0	22.0	16.9	44.9	77.4	−32.5
1981	0.0	24.3	22.0	7.6	53.9	42.6	11.3
1982	4.7	42.8	22.4	6.6	76.5	16.2	60.3
1983	6.5	23.4	20.9	10.8	61.6	70.2	−8.6
1984	0.0	43.5	21.4	10.8	75.7	18.3	57.4
1985	0.0	131.6	23.0	77.0	231.6	31.4	200.2
1986	12.4	105.6	16.3	33.8	168.1	34.9	133.2
1987	27.9	94.4	29.2	27.1	178.6	46.9	131.7
1988	46.7	25.4	32.0	16.3	120.4	68.0[a]	59.4

[a] Error in published statistics corrected per Steve Koplin, NMFS, Washington, D.C., pers. comm.
References: U.S. Department of the Interior 1960–1970; U.S. Department of Commerce 1971–1988.

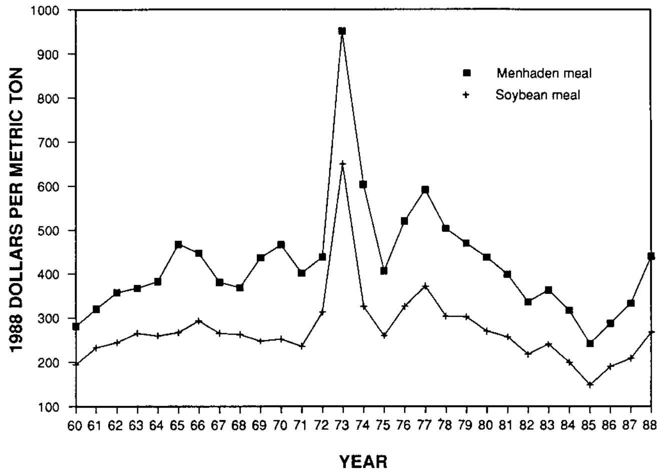


Figure 3. U.S. menhaden meal price (60% protein, bulk, f.o.b., East Coast/Gulf plants) and soybean meal price (50% protein, bulk, Decatur, Illinois) in 1988 dollars, 1960–88.

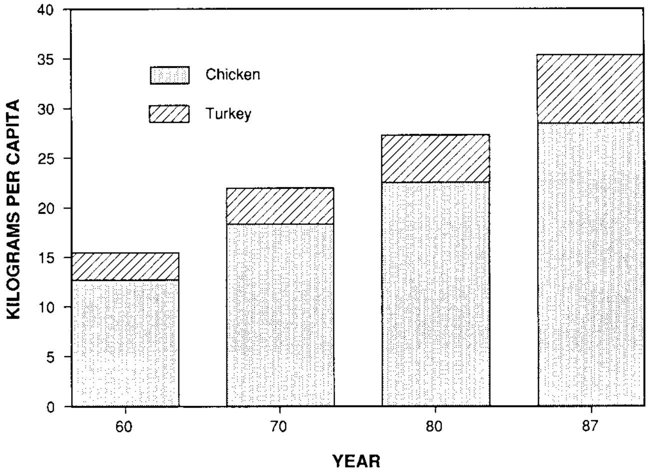


Figure 4. U.S. per capita consumption of chickens and turkeys in 1960, 1970, 1980, and 1987.

A notable feature of the graph is the 1973 price increase, which resulted from a serious worldwide shortage of oil meals. Several factors contributed to this shortage, including (1) major failures of oil meal crops around the world, (2) increases in fuel-related production costs due to the Arab oil embargo, and (3) the collapse of the Peruvian anchoveta fishery.

DEMAND

Demand for Poultry Products

In the years since World War II, the average American diet has shifted away from grain products in favor of more animal protein. This increased demand for protein is partially reflected in the shift from farm production of chickens to the factory-style mass production of commercial broilers that we see today. Approximately 80% of the fish meal consumed in the United States is used as an ingredient in poultry feed (Vondruska 1980). The final demand for poultry products is an indicator of poultry feed usage and the demand for fish meal.

U.S. egg production increased steadily from 61 billion eggs in 1960 to 70 billion eggs in 1967. In the twenty years since 1967, annual egg production has not exceeded the 1967 production level. This leveling of production is due to two offsetting factors: (1) a decline in per capita egg consumption, and (2) an increase in population. Because of increased productivity per layer, the stock of laying hens has declined slightly from 295 million hens in 1960 to 280 millions hens in 1987 (U.S. Department of Agriculture 1960–1988; Rogers 1978). These trends suggest that total feed usage by laying hens has not changed significantly since 1960.

The situation with regard to table birds is quite different (figure 4). Poultry consumption, measured in ready-to-cook weight, has more than doubled from 15.5 kilograms (kg) per person in 1960 to 35.3 kg in 1987. This change, compounded by the increase in population over this same period, has resulted in a dramatic increase in poultry production. Table birds slaughtered under federal inspection increased almost fourfold from 3.1 million MT (live weight) in 1960 to 12.2 million MT in 1987 (U.S. Department of Agriculture 1960–1988).

Accommodation of Poultry Industry to Limited Fish Meal Supplies

The large increase in poultry production has not been accompanied by a commensurate increase in fish meal usage. The poultry industry has accommodated itself to limited fish meal supplies by substituting other ingredients in poultry feed mixes, and by making technological changes to promote rapid growth of chicks.

Over 70% of the cost of producing chickens and turkeys, excluding processing and marketing costs, consists of feed (Vondruska 1980). As a result, small changes in feed prices can have a major effect on total costs. U.S. poultry feed mixers are very sophisticated in their use of linear programming techniques to determine least-cost combinations of ingredients (Hansen 1981/1982). They are also very quick to change feed composition in response to changes in prices (Huppert 1980; Thomson 1984).

The role of fish meal in these linear programming models is best understood by examining its nutritional contribution to poultry feed. All fish meal contains lysine and methionine, which are essential for the development and rapid growth of chicks. These amino acids are not found in grain meals, except for soybean meal, which contains high levels of lysine. Lysine and methionine are also available in synthetic form. The synthetic versions can be used to obtain a proper amino acid balance in feed mixes that do not contain fish meal (Titus and Fritz 1971; Vondruska 1980; Hansen 1981/1982).

Including fish meal in the diet of laying hens reduces mortality by retarding the accumulation of fat in their livers (Ralph Ernst, USDA/UC Cooperative Extension, Oakland, Calif., pers. comm.). Fish meal also produces a significant growth response in table birds. Nutritional requirements for table birds depend upon a bird's stage of growth, so feed composition varies accordingly. For chickens and turkeys, the maximum inclusion rate for fish meal is about 8%–9% for starter rations and 7% for grower rations. Higher rates than this tend to give a "fishy" flavor to the final product. Desirable minimum inclusion rates are 1%–2% for starter rations and 0%–1% for grower rations (Vondruska 1980).

Thus, one way that feed mixers have been able to satisfy the increased demand for poultry feed in spite of having smaller amounts of fish meal has been by substituting other ingredients. For table birds, they have reduced fish meal from maximum to minimum recommended levels in starter rations and eliminated fish meal entirely from grower rations. They have also largely eliminated fish meal from layer rations (Ralph Ernst, USDA/UC Cooperative Extension, Oakland, Calif., pers. comm.). These changes are consistent with Kolhonen's (1974) prediction that, "In the long run fish meal will be used as a unique small-quantity ingredient in high-quality feeds rather than as a high-amount protein source."

Fish meal use has also been reduced by technological improvements resulting in shorter time to market for chickens and turkeys. In 1960 it took approximately nine weeks to bring a three-pound broiler to market; today it takes six weeks (Ralph Ernst, USDA/UC Cooperative Extension, Oakland, Calif., pers. comm.). As a result, feed requirements (including fish meal requirements) per bird have declined.

FUTURE TRENDS

Aquaculture Demand for Fish Meal

Aquaculture production in the United States almost tripled from 1980 to 1985 (table 5). World production has also increased dramatically, from 4.6 million MT in 1975 to 8.9 million MT in 1985 (table 6). Currently about 10% of the world's fish meal production is used to feed farm-raised finfish and shellfish (FAO 1989). By one estimate (Rhodes 1988), world aquaculture production will reach 22 million MT by the year 2000 and account for about 25% of the world's aquatic harvest. This and other similar projections suggest a long-term increase in demand for fish meal in aquaculture.

TABLE 5

U.S. Aquaculture Production, 1980 and 1985, by Species Group (Metric Tons)

Species group	1980	1985
Catfish	34,855	123,344
Salmon	3,455	38,320
Crawfish	10.849	29,545
Trout	21,836	23,000
Baitfish	10,000	11,276
Oysters	10,755	10,215
Other finfish	—	6,364
Other shellfish	391	1,411
Total	92,141	243,675

Reference: Rhodes 1988.

TABLE 6

World Aquaculture Production in 1975, 1980, and 1985, by Species Group (Thousands of Metric Tons)

Species group	1975	1980	1985
Finfishes	2628.8	3206.8	5697.2
Crustaceans	29.7	75.0	281.6
Mollusks	1961.2	3299.7	2885.7
Subtotal	4619.7	6581.5	8864.5
Seaweeds & other	NA	NA	3565.1
Total	NA	NA	12429.6

Reference: Rhodes 1989.

Alternative Uses for Fish Oil

The National Fish Meal and Oil Association submitted a petition to the U.S. Food and Drug Administration in 1986 requesting approval to use hydrogenated and refined fish oils in products for human consumption. The FDA recently granted approval for the hydrogenated oil, paving the way for its use in products such as shortenings and pastries. However, because of U.S. Department of Agriculture standards for margarine, fish oil still cannot be used in margarines.

The portion of the petition pertaining to refined fish oil is still pending. Unlike hydrogenated oil, refined oil contains omega-3 fatty acids, which have been shown to provide a wide variety of health benefits (Pique 1986). Adding refined fish oil to products such as salad dressings could enhance their nutritional value and marketability. However, the long-term prospects for this are uncertain, since (1) FDA approval may or may not be forthcoming, and (2) the technology necessary to address the problem of rancidity in refined oil is not well developed (Paul Bauersfeld, NMFS, Charleston, S.C., pers. comm.).

Japanese and South American Sardine Fisheries

Japan has historically been a major world producer of fish meal. From 1960 through 1971 Japan produced 9%–15% of the world's fish meal; its share of production increased to 15%–21% from 1972 through 1987. Until the mid-1980s Japan was also a net importer of meal (table 7).

Japan derives most of its fish meal from its sardine *(Sardinops melanosticta)* fishery, which has produced two periods of high yield in this century. Japan's sardine landings increased through the early 1900s to a peak of 1.75 million MT in 1935, then gradually declined to 9,200 MT by 1965 (Lluch-Belda et al., in press). Since 1965, landings have again increased dramatically (figure 5). Although Japan continues to take most of the harvest, the Soviet Union and South Korea have also participated in the fishery since the late 1970s.

TABLE 7

World Production and Japanese Production, Imports, and Exports of Meals and Solubles from Animals of Aquatic Origin (Thousands of Metric Tons)

Year	World production	Japan Production	Japan Imports	Japan Exports
1960	2,076.0	312.7	19.4	6.3
1961	2,580.0	362.2	23.3	4.9
1962	2,900.0	390.0	38.5	18.1
1963	2,902.0	328.4	84.3	3.6
1964	3,666.0	353.1	102.3	6.2
1965	3,615.0	344.5	112.6	13.1
1966	4,170.0	423.5	95.6	15.8
1967	4,660.0	420.3	86.8	11.3
1968	5,060.0	500.5	150.2	6.8
1969	4,750.0	594.2	108.0	183.0
1970	5,450.0	671.0	94.7	24.5
1971	5,400.0	692.0	21.7	37.7
1972	4,320.0	735.9	56.8	28.6
1973	4,020.0	791.0	87.3	17.8
1974	4,570.0	773.7	74.5	31.3
1975	4,510.0	839.6	70.6	49.3
1976	4,890.0	745.9	59.5	49.0
1977	4,575.4	857.2	181.1	37.5
1978	4,916.1	890.2	84.9	64.3
1979	5,089.9	895.6	101.6	57.7
1980	4,971.9	879.9	141.0	43.3
1981	5,056.2	898.9	84.1	73.7
1982	5,394.1	1,004.1	44.3	135.7
1983	5,282.6	1,133.5	95.1	79.6
1984	6,097.7	1,262.7	61.6	135.3
1985	6,275.2	1,166.6	80.3	157.4
1986	6,661.3	1,179.1	161.5	167.2
1987	6,394.6	1,112.7	187.3	216.6

Reference: Food and Agriculture Organization 1960–1987.

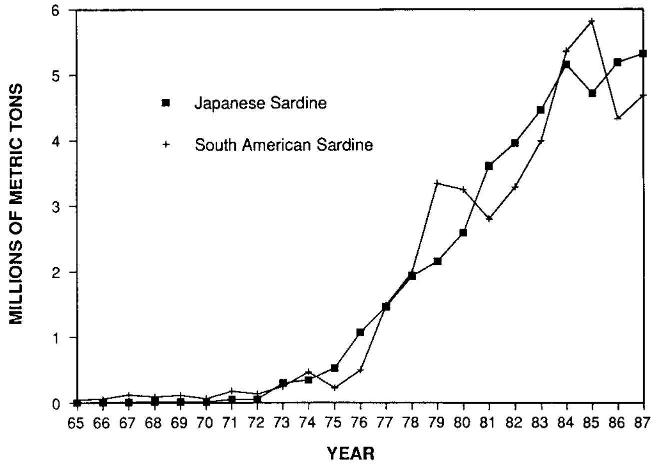


Figure 5. Landings of Japanese and South American sardine, 1965–87.

The South American sardine *(Sardinops sagax)* fishery has experienced similar rapid growth in the past two decades. Annual sardine landings by Chile and Peru have increased from negligible amounts in the mid-1960s to approximately 5 million MT (figure 5).

Given the record harvests experienced in recent years, the sardine fisheries of Japan and South America warrant close attention. A change in the status of these stocks could significantly affect the availability and price of fish meal.

New Products from Reduction Species

Efforts are ongoing to develop marketable products for direct human consumption from species traditionally used for reduction. For instance, a processor in Virginia has been exploring the economic feasibility of making a marketable surimi from menhaden (Malcolm Hale, NMFS, Charleston, S.C., pers. comm.). The Fishermen's Cooperative in San Pedro, California, is test-marketing canned sardines for human consumption. Although the ultimate outcome of these enterprises is uncertain, the expectation is that alternative uses will be found for reduction species over the long term.

Americanization of the Alaska Pollock Fishery

Significant increases in U.S. harvesting of and processing capacity for the Alaska pollock *(Theragra chalcogramma)* have resulted in a drastic curtailment of foreign landings and joint venture operations in recent years. As indicated in table 8, landings of Alaska pollock by foreign vessels declined from an

TABLE 8
Landings of Alaska Pollock (Metric Tons)

Year	Domestic	Joint venture	Foreign	Total
1977	323	0	1,009,826	1,010,149
1978	1,765	0	1,074,077	1,075,842
1979	2,551	0	1,047,150	1,049,701
1980	1,409	11,800	1,119,126	1,132,335
1981	1,741	58,950	1,117,455	1,178,146
1982	1,479	128,886	1,051,949	1,182,314
1983	1,382	283,104	973,050	1,257,536
1984	10,894	444,256	1,032,249	1,487,399
1985	42,109	614,337	851,870	1,508,316
1986	59,160	904,111	352,682	1,315,953
1987	250,407	1,057,315	3,596	1,311,318
1988	570,285	826,564	0	1,396,849

Reference: U.S. Department of Commerce 1977–1988.

approximate annual average of one million MT during 1977–85 to zero in 1988. Joint venture landings, which peaked at one million MT in 1987, are also expected to decline to zero in 1990, and domestic landings are expected to increase commensurately.

With the Americanization of the fishery and recent increases in fish meal processing capacity in shoreside plants and aboard U.S. factory trawlers, pollock meal is expected to become an increasingly large component of U.S. fish meal production. Additional impetus may be provided by the North Pacific Fishery Management Council, which is currently considering a change in regulations to require full use of the resource. Should such an amendment be adopted, it could lead to similar requirements for other Alaska groundfish species.

In 1988, the United States produced approximately 15,000 MT of pollock meal from the offal generated in the preparation of surimi and fillets/blocks (Vondruska et al. 1989). Assuming a fish meal yield of 10% from round weight (Steve Koplin, NMFS, Washington, D.C., pers. comm.) and an average annual harvest of 1.2 million MT, U.S. pollock meal production could reach 120,000 MT annually. This would be a significant addition to the 200,000 to 350,000 MT of fish meal that we currently produce each year.

Much of the pollock meal produced in recent years has been exported to Taiwan's eel farms (Jerry Babbitt, NMFS, Kodiak, Alaska, pers. comm.). Future increases in pollock meal production may also be exported abroad rather than absorbed into the U.S. market. Depending on the magnitude of this trade, the United States could reverse its long-standing status as a net importer of fish meal and become a net exporter.

LITERATURE CITED

FAO (Food and Agriculture Organization of the United Nations). 1960–1987. Yearbook of fishery statistics: fishery commodities.

———. 1986. The production of fish meal and oil. FAO Fisheries Technical Paper 142, 63 pp.

———. 1989. GLOBEFISH Highlights, no. 1/89:28.

Glantz, M.H. 1979. Science, politics and economics of the Peruvian anchoveta fishery. Mar. Policy 3(1):201–210.

Hansen, T. 1981/1982. A model of the world fish meal market. Fish. Res. 1:271–288.

Huppert, D. D. 1980. An analysis of the United States demand for fish meal. Fish. Bull. 78(2):267–276.

Kolhonen, J. 1974. Fish meal: international market situation and future. Mar. Fish. Rev. 36(3):36–40.

Lasker, R., and P. Smith. 1977. Estimation of the effects of environmental variation on the eggs and larvae of the northern anchovy. Calif. Coop. Oceanic Fish. Invest. Rep. 19:128–137.

Lluch-Belda, D., R. J. M. Crawford, T. Kawasaki, A. D. MacCall, R. H. Parrish, R. A. Schwartzlose, and P. E. Smith. In press. Worldwide fluctuations of sardine and anchovy stocks: the regime problem. S. Afr. J. Mar. Sci.

Pique, G. G. 1986. Omega-3: the fish oil factors. San Diego: Omega-3 Project, Inc., 163 pp.

Radovich, J. 1981. The collapse of the California sardine fishery: what have we learned? *In* Resource management and environmental uncertainty, M. H. Glantz, ed. New York: John Wiley & Sons, pp. 107–136.

Rhodes, R. J. 1988. Status of world aquaculture: 1987. Aquaculture magazine buyer's guide, pp. 4–18.

———. 1989. Status of world aquaculture: 1988. Aquaculture magazine buyer's guide, pp. 6–20.

Rogers, G. B. 1978. Costs, prices and productivity in the poultry and egg industries. Poultry and egg situation. Washington, D.C., U.S. Dept. of Agriculture, pp. 26–29.

Thomson, C. J. 1984. A model of fish meal supply and demand in the United States, 1966–1981. NMFS, SWFC Admin. Rep. LJ-84-04, 27 pp.

Thomson, C., C. Cooney, and J. Morgan. 1989. Status of the California coastal pelagic fisheries in 1988. NMFS, SWFC Admin. Rep. LJ-89-14, 23 pp.

Titus, H. W., and J. C. Fritz. 1971. The scientific feeding of chickens. Fifth edition. Danville, Ill. The Interstate Printers & Publishers Inc. 312 pp.

U.S. Department of Agriculture. 1960–1988. Agricultural statistics. Washington, D.C.

U.S. Department of Commerce, National Oceanic and Atmospheric Administration, National Marine Fisheries Service. 1971–1988. Fisheries of the United States. Washington, D.C.

U.S. Department of the Interior, Fish and Wildlife Service, Bureau of Commercial Fisheries. 1960–1970. Fishery statistics of the United States. Washington, D.C.

Vondruska, J. 1980. Postwar production, consumption, and prices of fish meal. *In* Resource management and environmental uncertainty. M. H. Glantz, ed. New York: John Wiley & Sons, pp. 285–316.

Vondruska, J., R. Kinoshita, and M. Milazzo. 1989. Situation and outlook for surimi and surimi-based food. NOAA Technical Memorandum NMFS-SEFC-233, 16 pp.

BATHYMETRIC PATTERNS IN SIZE, AGE, SEXUAL MATURITY, WATER CONTENT, AND CALORIC DENSITY OF DOVER SOLE, *MICROSTOMUS PACIFICUS*

JOHN R. HUNTER, JOHN L. BUTLER, CAROL KIMBRELL, AND ERIC A. LYNN
National Marine Fisheries Service
Southwest Fisheries Center
P.O. Box 271
La Jolla, California 92038

ABSTRACT

Ninety-eight percent of the spawning biomass of Dover sole, *Microstomus pacificus*, in central California waters live in a region of the continental slope between 640 and 1006 m (350–550 fath.) characterized by low oxygen concentrations (0.27–0.36 ml/l) and cold temperatures (5.9°C–3.2°C). Juvenile Dover sole settle on the continental shelf and gradually move down the slope over their lifetime, reaching the oxygen minimum zone as they become sexually mature. Fifty percent of Dover sole in central California reach sexual maturity when about 31 cm long and about seven years of age. The ontogenetic movement down the slope continues after sexual maturity and is accompanied by a marked increase in water content of the body and a consequent decrease in caloric density per gram wet weight. For example, caloric density decreases from about 86 kcal per gram wet weight (83% water) for a 275-mm fish living at 200–400 m, to 60.3 kcal per gram wet weight (90% water) for a fish 440 mm long living at about 900 m. Female Dover sole may live as long as 53 years, and males 58 years. Water content appears to be a function of age as well as length and depth.

RESUMEN

El 98% de la biomasa del desove del lenguado de las aguas de California central, *Microstomus pacificus*, vive en un área del talud continental entre los 640 y los 1006 m caracterizada por bajas concentraciones de oxígeno (0.27–0.36 mL/L) y bajas temperaturas (5.9°C–3.2°C). Los juveniles se establecen en la plataforma continental y gradualmente, durante su desarrollo, se van moviendo hacia el talud y a lo largo de él, llegando a la zona de mínimo oxígeno cuando alcanzan la madurez sexual. El 50% del lenguado alcanza la madurez sexual cuando mide aproximadamente 31 cm de longitud, a los 7 años de edad. El movimiento descendiente a lo largo del talud durante la ontogénesis continúa pasada la madurez sexual, y es acompañado por un incremento marcado en el contenido de agua del cuerpo y consequentemente, por un decrecimiento en la densidad calórica por gramo de peso húmedo. Por ejemplo, la densidad calórica de un pez que mide 275 mm y vive a 200–400 m de profundidad decrece de 86 kcal por gramo de peso húmedo (83% de agua) a 60.3 kcal (90% de agua) para un pez que mide 440 mm de longitud y vive a 900 m de profundidad. Las hembras pueden vivir hasta proximadamente los 53 años de edad y los machos hasta los 58. El contenido de agua parece estar relacionado con la edad y el largo del individuo y con la profundidad a la cual vive.

INTRODUCTION

Dover sole, *Microstomus pacificus*, are found from the Aleutian Islands in the Bering Sea to Baja California (Eschmeyer et al. 1983). The U.S. fishery for Dover sole occurs from Point Conception, California, to the Canadian border. Dover sole inhabit depths ranging from about 55 to 1300 m. Older and larger fish usually occur in the deeper portion of the depth range, and younger and smaller fish in the shallower depths. A seasonal inshore migration has been described: fish move into deep water in the fall before the spawning season and into shallow water in the summer (Hagerman 1952; Alverson 1960; Pearcy et al. 1977). Most individuals apparently remain in the same general locality throughout their lives. Although longshore movements of up to 360 mi (579 km) in seven years have been recorded, 97% of tagged individuals were recaptured within 50 km of where they were tagged (Westrheim and Morgan 1963).

Large Dover sole from deep water are often "jellied" (have flesh with an unusually high water content). This "jellied" consistency limits the market value of fillets from large Dover sole because their desirability is reduced (Hendricksen et al. 1986). Owing to the ontogenetic migration into greater depths, and the extensive depth range, the demographic and physiological characteristics of Dover sole change strikingly with depth, age, and length. Thus neither the dynamics nor the ecology of Dover

---

[Manuscript received April 23, 1990.]

sole populations can be properly analyzed or understood without a careful evaluation of the relation between depth and key physiological and population variables.

The objective of this paper is to describe the relationships between depth, length, age, sexual maturity, water content, caloric density, and biomass of Dover sole. We also provide data on the temperature and oxygen content of the habitat in which fish of different length and age are found. Our analysis does not include data for the summer months, when fish may have a shallower distribution (Alverson 1960).

METHODS

Sea Collections

Research trawl collections were taken at depths between 69 and 1394 m (38–762 fath.) off the central California coast between Point Conception and Half Moon Bay, California, during 1985–88 by NOAA-Southwest Fisheries Center (SWFC) personnel (table 1). Trawl collections were opportunistic before 1987. In 1987, samples were taken at 183-m (100-fath.) intervals along transect lines; in 1988 a random sampling design stratified by depth was used (figure 1).

In all years fish were sexed and measured for total length. Before January 1987, fish were randomly sampled from each collection until 25 females had been identified and their ovaries preserved for later assessment of maturity; the females and some males were frozen for subsequent extraction of otoliths and determination of water content as described below. During January–February 1987, either all of the Dover sole in the trawl collection or 100 fish were randomly sampled, and 25 females were assessed for gonad maturation. Four to six fish of each sex were weighed and frozen for later removal of otoliths and tissue for analysis of water content. The bottom temperature (reversing thermometer on Nansen bottle) and oxygen content (Winkler titration) were measured for 17 Nansen cast stations ranging from 183 to 1,280 m (100–700 fath.; figure 1, left, triangles). The total trawl catch of Dover sole was weighed during 1987 and 1988. In 1988, up to 100 Dover sole were randomly sampled and weighed by sex; the maturation state of all ovaries was determined.

Age Determination

Ages were determined from otoliths removed from 341 females and 64 males captured during 1985–86 and from 154 females and 97 males captured in 1987. Left otoliths were embedded in epoxy resin and cut with a diamond wafering blade in a thin cross section through the nucleus from dorsal to ventral (Chilton and Beamish 1982). Thin sections were mounted on microscope slides with Eukitt mounting medium, polished, and read with a compound microscope. A typical otolith section is shown in figure 2. Counting procedures followed those of Chilton and Beamish (1982). Each otolith was read independently by three observers without knowledge of the length of the fish. Otoliths whose readings differed by more than 10% among the readers were reread using the same protocol until the readings agreed to within 10%. Estimated age was the average of the individual readings. Parameters of the von Bertalanffy equation relating length and age were estimated by the simplex method (O'Neill 1971).

TABLE 1
Sources and Numbers of Dover Sole Used for Analyses

Dates	Sampling type[a]	Number of positive trawl collections			Number of Dover sole used in various analyses					
			Depth							
		N	Min	Max	Percent water	Calories	Age	Size at depth	Sexual maturity	Oxygen and temperature
12/3–12/12/85	OP	11	94	704	108	7	37	195	104	—
1/14–2/24/86	PS	2	330	600	115	—	180	—	—	—
3/5–3/7/86	OP	8	52	500	165	18	14	210	—	—
5/2–5/4/86	OP	3	175	537	60	2	62	74	—	—
1/11–2/15/87	LT	49	99	705	265	—	255	3225	—	17[b]
2/23–4/9/88	SR	51	38	602	—	—	—	2800	—	—
			Total specimens		713	27	548	6504	104	—

[a] OP = opportunistic trawl samples; PS = port samples; LT = line-transect trawl samples; SR = stratified random trawl samples.
[b] Number of Nansen cast stations.

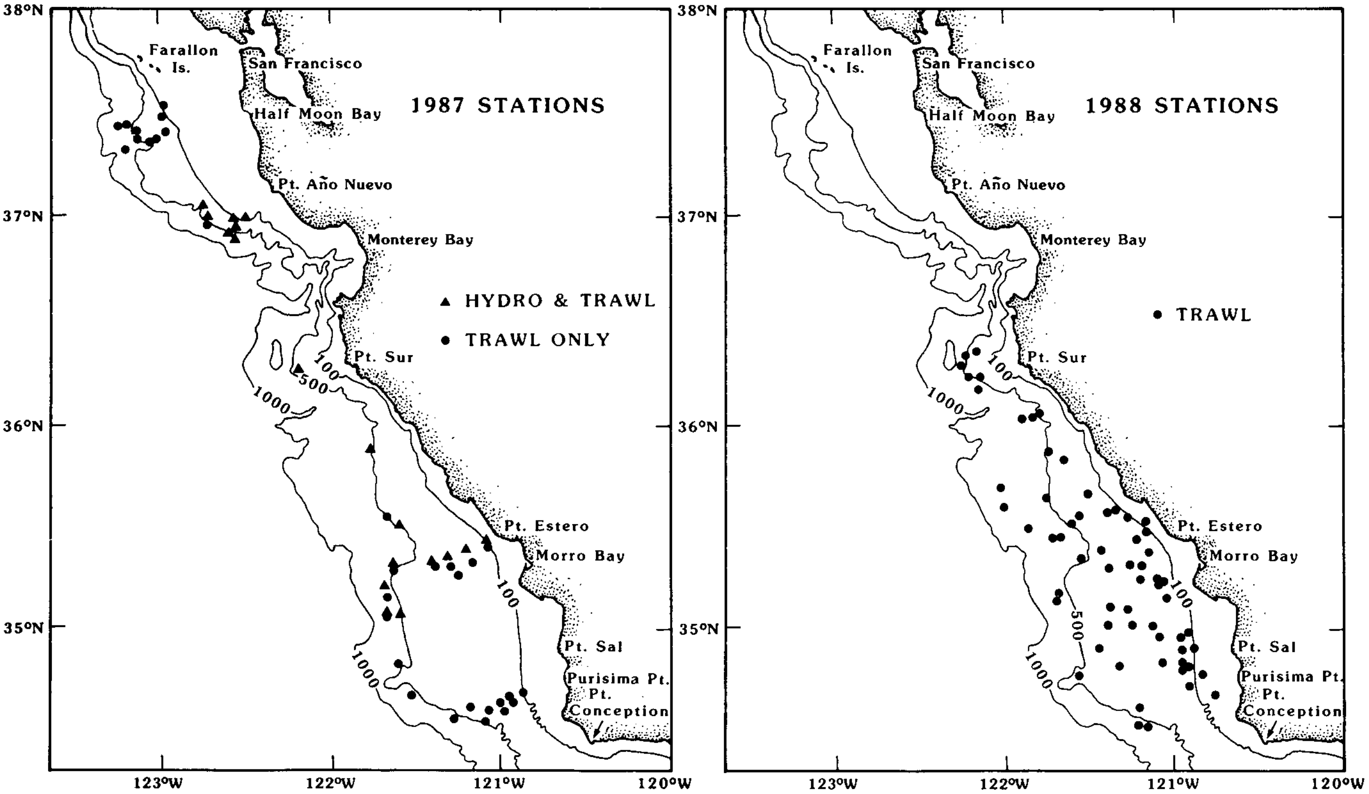


Figure 1. *Left*, trawl stations occupied on the January–February 1987 groundfish cruise of the R/V *David Starr Jordan*. *Right*, trawl stations occupied on the February–March 1988 groundfish cruise of the R/V *David Starr Jordan*.

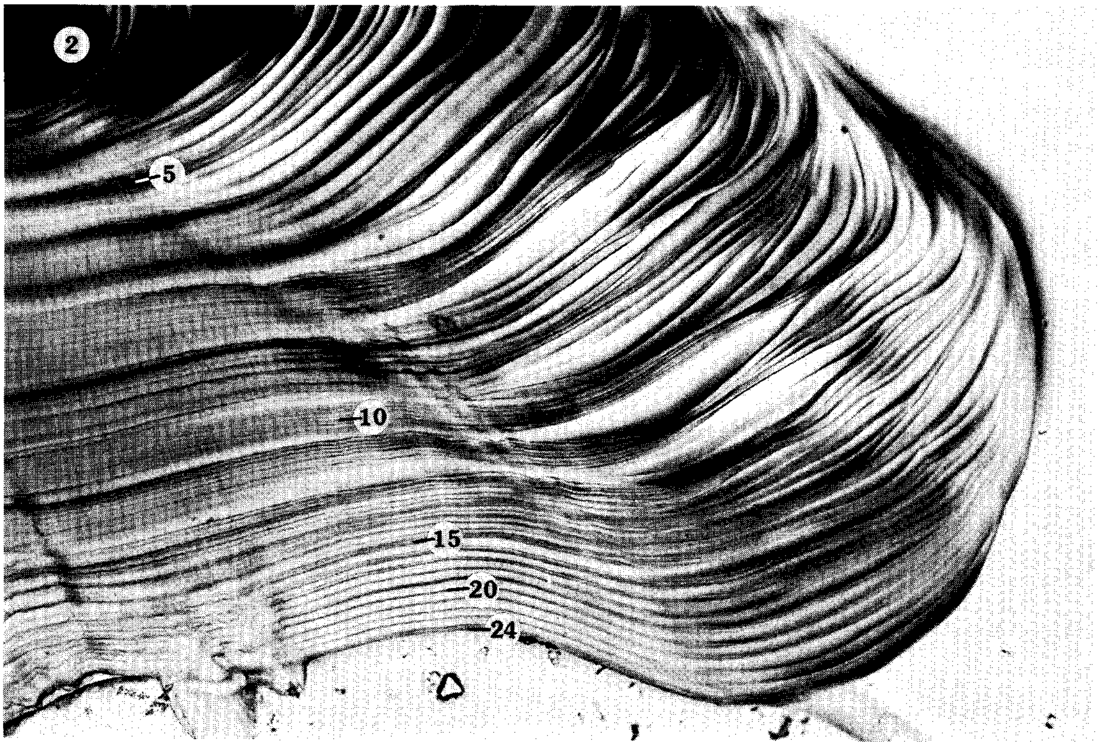


Figure 2. Thin section of the left otolith of a 24-year-old Dover sole. Numbers indicate years.

Biomass

We used the mean biomass in the area swept by trawls, and total area within three depth strata to estimate Dover sole biomass. The survey area extended from 34°30'N, 120°30'W to 36°30'N, 122°30'W (Butler et al. 1989).

Sexual Maturity

The size at first maturity of females taken in December 1985 was estimated by logistic regression analysis (Draper and Smith 1981; Engelman 1988). Data from the other surveys (table 1) were not appropriate for estimating sexual maturity because the spawning season began before the first survey. The possibility of mistaking immature ovaries for postspawning ovaries was minimized because December appears to be early in the spawning season (Hunter, unpublished data). Ovaries with yolked oocytes were considered to be mature, and those without yolked oocytes to be immature.

Water Content

We measured water content by determining the wet weight of a tissue sample (about 3.5 g) and then drying it to constant weight at 60°C. The white muscle samples were taken from the right side of the fish between the lateral line and the dorsal fin at the insertion of dorsal rays 30 to 36, and the red muscle samples were taken from the right side above the lateral line and behind the eye.

To determine whether a single tissue sample represented water content of the whole fish, we measured the entire water content of 20 females, 6 males, and one fish of indeterminate sex by grinding and drying the entire fish after removing red and white muscle tissue samples from the locations described above. We then regressed water content (in percent of wet weight) of the entire fish (*H*) on tissue sample water content (figure 3):

$$H = -5.08 + 1.05\,h_1, \qquad (1)$$

where h_1 is water content of white muscle ($r^2 = 0.906$, $n = 27$; $p < 0.01$). The slope of this equation did not significantly differ from 1.0, nor did the intercept differ from 0. On the basis of these results we assumed that the percentage of water in the white muscle tissue samples was the same as that of the whole fish. Whole fish water content was estimated (equation 1) from white muscle tissue samples collected at sea for 106 fish collected in 1985, 335 fish collected in 1986 (90% females), and 265 fish (males and females) collected in 1987.

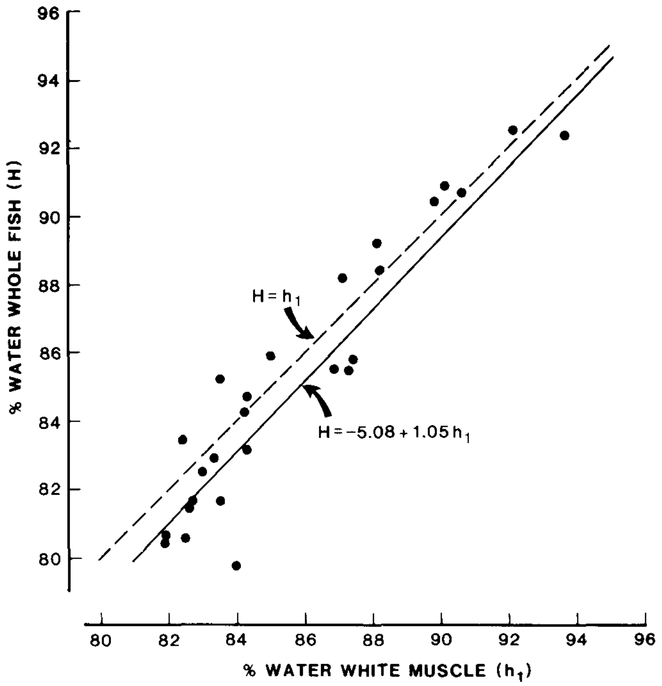


Figure 3. Relation of water content of whole Dover sole to water content of white muscle. *Solid line* is regression line fit to data; *dashed line* is one-to-one relationship.

Water content of the red muscle tissue sample was also linearly related to water content of the entire fish, although the relationship was poorer than that for white muscle:

$$H = -51.1 + 1.62\,h_2, \qquad (2)$$

where h_2 is water content of red muscle ($r^2 = 0.837$, $n = 27$; $p < 0.01$).

Caloric Density and Fat Content

To determine caloric density (kcal/gm) of entire fish for 26 Dover sole, we used a Parr bomb calorimeter and standard techniques (Parr Instrument Co. 1960; Paine 1971). The values reported here are the means of three determinations. Fish were selected by size to obtain caloric measurements over a wide range of water content (the fish sampled ranged from 80% to 94% water).

To determine fat content we used a Soxhlet extraction with 2:1 chloroform-methanol. This technique did not provide an accurate estimate of total fat content because the extraction period (48–72 hrs) was too short for fish with a high fat content, such as Dover sole. Notwithstanding this bias and other problems associated with chloroform-methanol extractions (Dobush et al. 1985), we believe that the data provided useful information about fat concentration.

RESULTS

Length and Depth

The mean length of Dover sole increases rapidly with depth over the first 300 fath. (549 m) and thereafter more slowly. The relationship between depth and size of Dover sole varied little among years (figure 4, top and middle). The combined data for all surveys clearly indicate that males taken at a given depth are smaller than females (figure 4, bottom).

Age and Growth

Male and female Dover sole differ in size at age (figure 5). Estimates from counts of annuli in thin sections of otoliths indicate that the fish live long lives. The maximum estimated age was 56 years for a 460-mm male and 51 years for a 492-mm female.

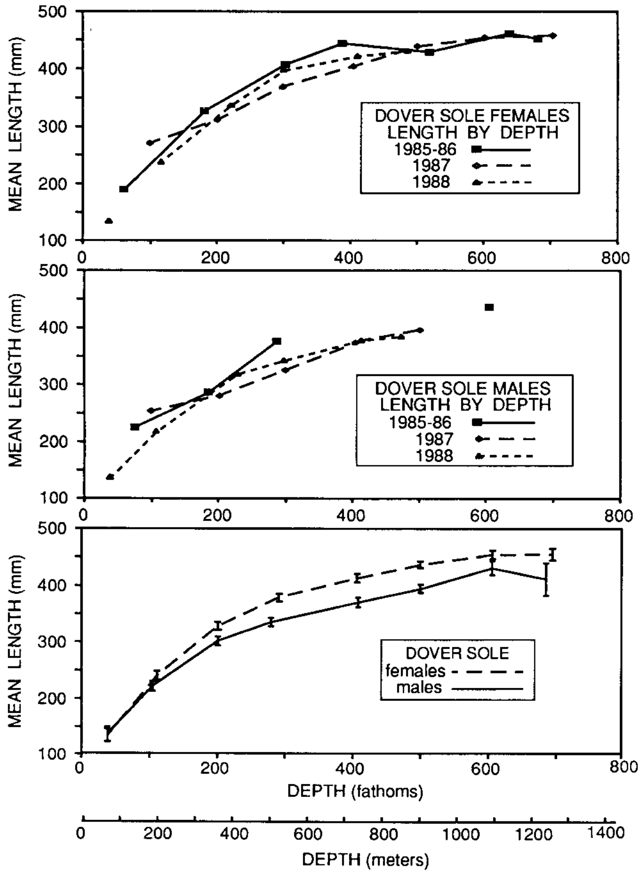


Figure 4. Mean length of Dover sole collected at different depths. *Top*, females; *middle*, males; *bottom*, all years combined with ±2 SE.

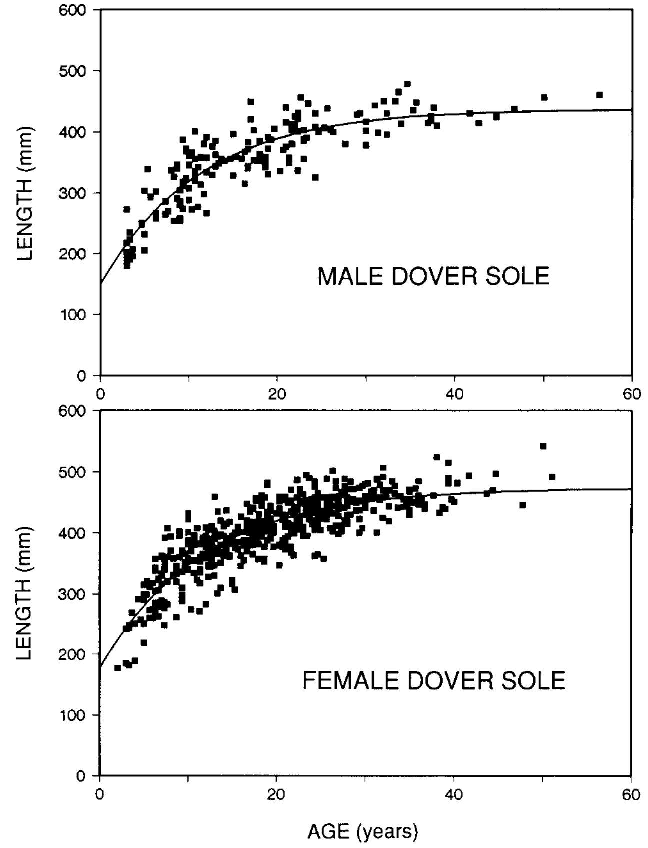


Figure 5. *Top*, length and age (calculated from otolith section) of male Dover sole. *Solid line* is von Bertalanffy curve with L_∞ = 437 mm, growth parameter K = 0.089, and t_0 = −4.7 yr. *Bottom*, length and age (calculated from otolith section) of female Dover sole. *Solid line* is von Bertalanffy curve with L_∞ = 474 mm, growth parameter K = 0.085, and t_0 = −5.5 yr. See text for equation 3.

Parameters of the von Bertalanffy growth model,

$$L_t = L_\infty(1 - e^{-K(t - t_0)}), \tag{3}$$

were L_∞ = 437 mm, K = 0.089, and t_0 = −4.7 years (n = 161, $p < 0.01$) for male Dover sole. Estimates of parameters for females were L_∞ = 474 mm, K = 0.085, and t_0 = −5.5 years (n = 495, $p < 0.01$). Male and female Dover sole longer than 40.0 cm may be any age from 8 years to 40 or 50 years. Neither our methodology nor any other has been validated for Dover sole. Nevertheless, we believe our age estimates approximate the true age, because the otolith section method has been validated for another species in the same habitat (*Anoplopoma fimbria*; Chilton and Beamish 1982).

Our age estimates are much greater than those obtained from scales (Demory 1972; Mearns and Harris 1975). However, tag-and-recapture studies

(Pikitch and Demory 1988) have shown that scales underestimate the age of Dover sole. The maximum age observed in this study (56 years) is in agreement with the longevity that Pikitch and Demory (1988) predicted from tag-and-recapture studies and errors in scale readings. A significant fraction (40%) of females in our samples were older than 20 years. Our results suggest that estimates of the productivity of Dover sole stocks based on ages estimated from scales may be seriously flawed.

Water Content

Data for females taken from December 1985 through May 1986 indicate that the mean water content of white muscle remains constant at about 82% in 15–30-cm females but increases with length in larger Dover sole to about 90% in 50-cm females (figure 6). The concentration of water in the red muscle followed a similar trend, but the increase in water content with length was much less. Average water content of red muscle in 50-cm females was only 85%. These data indicate that red muscle is conserved. The red muscle in the anterior upper trunk region of the eyed side of the body is the largest concentration of red muscle in the body and is probably used to control head movements during feeding. The color of the red muscle changed with fish length. Larger fish had darker red muscle, indicating that the myoglobin content of the muscle may be higher in larger fish.

The mean water content of males taken in January–February 1987 did not differ from that of females from that sample within the same 5-cm length class (ANOVA for five 5-cm length classes, 254–324 to 475–524 mm, per sex where $p = 0.90$; figure 7). The water content of Dover sole (both male and female) as a function of length showed little variation between years; the data for December 1985–May 1986 were essentially the same as those for January–February 1987 (figure 7). Combining data for all years and sexes indicates that the average water content for fish less than 275 mm was constant at 82.9% ($n = 90$, SD = 1.00), and for fish greater than 275 mm it increased with length (L) according to the equation

$$H = 72.7 + 0.036\,L \qquad (4)$$

($r^2 = 0.58$, $n = 506$; $p < 0.01$).

Water Content, Age, Length, and Depth

In the previous sections we showed that water content increases with fish length and that length increases with depth and age. In this section we describe the relationships between water content, length, depth, and age of Dover sole females. A

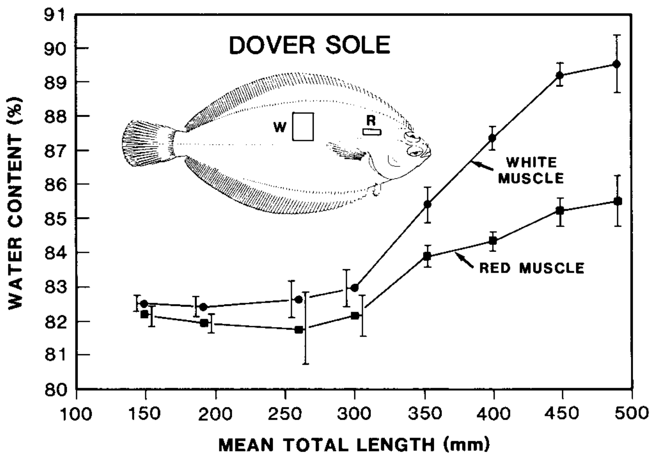


Figure 6. Water content in white and red muscle versus total length of female Dover sole. Insert shows locations of tissue samples—W for white muscle and R for red muscle.

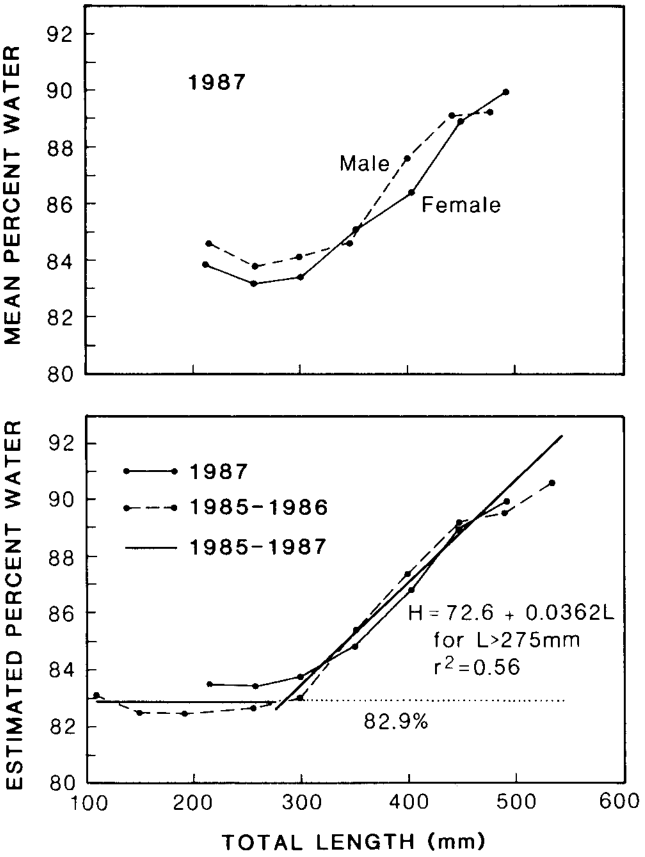


Figure 7. *Top*, water content of white muscle versus total length of female ($N = 163$) and male ($N = 98$) collected in 1987. *Bottom*, water content in white muscle of both sexes for 1985–86 (*dashed line*), 1987 (*thin line*), and 1985–87 (*thick line*).

stepwise multiple regression analysis indicated that all three independent variables (age, length, and depth) accounted for significant variation in the dependent variable, water content: the coefficient for age was the first selected (table 2). The final equation was

$$H = 78.7 + 0.1226\ T + 0.0123\ L + 0.0026\ D \quad (5)$$

where H is water content (percent of wet weight), T is age (years), L is length (mm), and D is depth (fath.) ($R^2 = 0.69$, $n = 519$; $p < 0.01$). Biological interpretation of the significance of the individual regression coefficients in this equation could be misleading (Sokal and Rohlf 1981) because, as is indicated in the correlation matrix (table 2), all the variables are correlated with each other; length is correlated with age and depth as well as water content.

Caloric Density

Caloric density (kcal per g ash-free dry weight) was correlated with fat concentration (F, in percent of dry weight). This relationship was less precise ($r^2 = 0.57$, $n = 26$; $p < 0.01$) than one would expect on physiological grounds because of our failure to extract all the lipids in some of the fish. Nevertheless, figure 8 clearly indicates that caloric density and fat concentration are linked, as would be expected.

TABLE 2

Analysis of the Relation Between Water Content (*H*) of Dover Sole and Their Age (*T*), Length (*L*), and Depth (*D*)

	Stepwise regression		
Step	1	2	3
Constant	82.63	78.31	78.71
Age (*T*)	0.2213	0.1272	0.1226
t-ratio[a]	28.28	10.94	10.69
Length (*L*)		0.0157	0.0123
t-ratio		10.25	7.30
Depth (*D*)			0.00260
t-ratio			4.61
S	1.82	1.66	1.63
R^2	60.73	67.37	68.62

		Analysis of variance			
Source	DF	SS	MS	*F*	*p*
Regression	3	2,981.00	993.67	375.31	<0.0005
Error	515	1,363.52	2.65		
Total	518	4,344.52			

Source	DF	Seq SS
Age (*T*)	1	2,638.47
Length (*L*)	1	288.64
Depth (*D*)	1	53.89

	Matrix of correlation coefficients		
	Water (*H*)	Age (*T*)	Length (*L*)
Age	0.779		
Length	0.773	0.789	
Depth	0.626	0.578	0.682

[a] For $p = 0.05$, the *t*-ratio is 1.96.

No significant relationship existed between caloric density and water content. There was no apparent relationship between fat concentration and water content ($r^2 = 0.02$) when length was not included as a variable. When length (L) was included as an additional third dependent variable in a stepwise multiple regression analysis (table 3), a definite relationship existed between caloric density (C_d) and

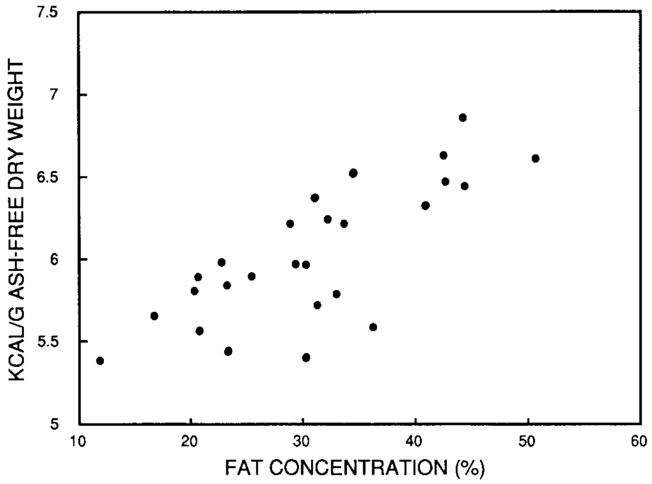


Figure 8. Caloric density versus fat concentration in Dover sole.

TABLE 3

Analysis of Variance Stepwise Regression of the Caloric Density of Dover Sole (C_d, kcal/g ash-free dry weight) on Their Total Length (*L*, in mm) and Water Content (*H*, in Percent)

	Summary	
Step	1	2
Constant	8.755	13.540
Water (*H*)	−0.032	−0.104
t-ratio[a]	−1.65	−5.86
Length (*L*)		0.0042
t-ratio		5.76
S	402	263
R^2	10.21	63.22

		Analysis of variance			
Source	DF	SS	MS	*F*	*p*
Regression	2	2.725	1.363	19.76	<0.0005
Error	23	1.586	0.069		
Total	25	4.311			

Source	DF	Seq SS
L	1	0.358
H	1	2.367

[a] For $p = 0.05$, the *t*-ratio is 2.07.

water content of white muscle (H, in percent wet weight) yielding the equation

$$C_d = 13.540 + 0.0042\,L - 0.1044\,H \tag{6}$$

with $R^2 = 0.60$, $n = 26$; $p < 0.01$ (figure 9). To further evaluate these data we analyzed covariance by arranging the data into three length classes (114–296 mm, 303–396 mm, and 408–506 mm) and regressed water content on calories within length class. This analysis indicated that the relationship between calories and water content differed significantly among the three length classes (233, 350, and 453 mm mean total length; $p < 0.01$). For the overall mean water content (85.6%), the adjusted mean kcal per g ash-free dry weight are 5.634 at 233 mm, 6.078 at 350 mm, and 6.424 at 453 mm. The relationship between caloric density and length was not significant ($r^2 = 0.037$, $n = 27$; $p = 0.169$). Thus, the relationship between caloric density and water content changes with length.

The relationship between length, water content, and caloric density in terms of wet weight (C_w) was also evaluated. The multiple regression equation was

$$C_w = 651 + 0.0742\,L - 7.06\,H \tag{7}$$

where C_w = kcal per 100 g wet weight, L = length, and H = water content of white muscle ($R^2 = 0.98$, $n = 26$; $p < 0.001$). R^2 is high because the water content of the fish largely determines the caloric density when it is expressed on the basis of wet weight.

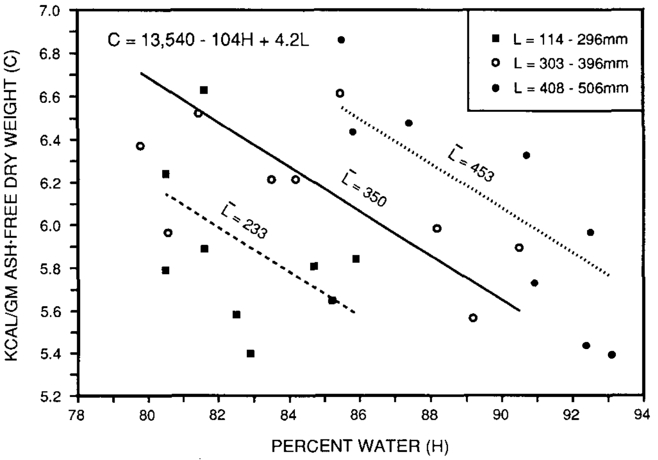


Figure 9. Caloric density and water content of Dover sole in three size classes. (See equation 6 in text for multiple regression relation of caloric density with length and water content.)

We also wished to examine how calorie content varied with depth. We used the multiple regression equation 6 to compute the caloric density (C, in kcal per g ash-free dry weight) of female Dover sole as a function of length and water content. We grouped the estimated caloric densities into seven 100-fath. (183-m) depth classes and calculated a mean and standard deviation (SD) for each class; the SD includes the variance associated with equation 6 (Draper and Smith 1981; figure 10, top).

A significant difference existed between the mean caloric density of females in the 100-fath. depth class and those in all other depth classes combined ($t = -7.34$, d.f. $= 37.7$, $p < 0.001$; d.f. was computed from the formula given by Zar [1984]). The mean caloric density for females taken in the 100-fath. depth class was 5.761 kcal per g ash-free dry weight ($n = 34$, SD $= 292$) and that of females taken at

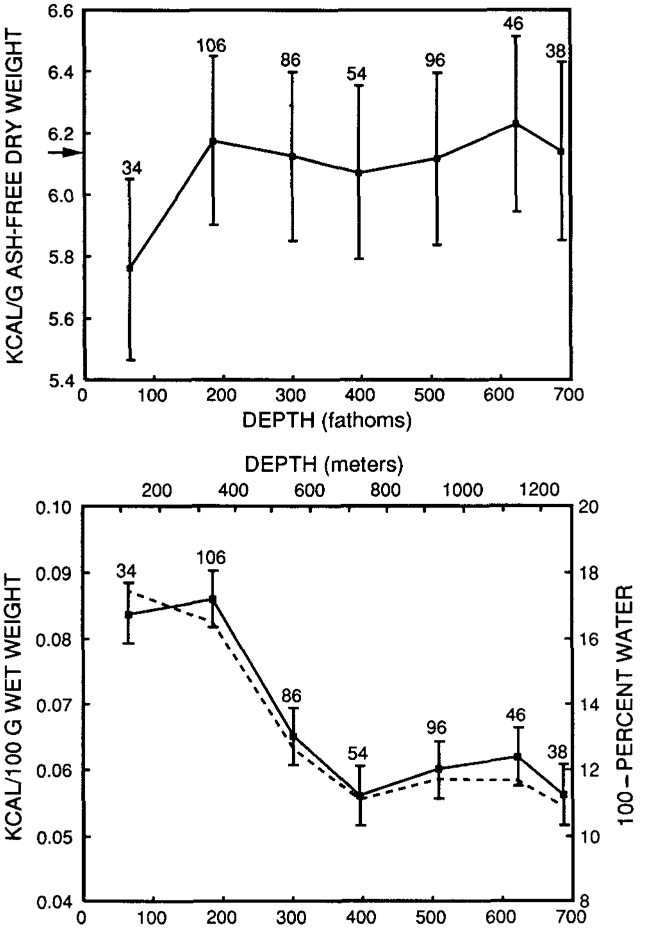


Figure 10. *Top*, kcal per g ash-free dry weight, SD and N within 100-fath. depth classes. Arrow is mean caloric density for depths >150 fath. *Bottom*, kcal per 100 g wet weight (*left axis*) and the complement of water content (*right axis*) within 100-fath. depth classes.

depths greater than 150 fath. was 6.141 kcal (n = 426, SD = 272). Thus, the Dover sole living in shallow water (50–150 fath.; 91–274 m) have a lower caloric density per unit dry weight than Dover sole living at greater depths.

A different pattern in the change in caloric density with depth occurs if caloric density is expressed in terms of wet weight rather than dry. Using the same data and equation 7, we recalculated the average caloric density of females on a wet-weight basis per depth class. On a wet-weight basis, the caloric density (kcal per g wet weight) declines with depth because of the increase in water content (figure 10, bottom). As would be expected, the decrease with depth in caloric density of Dover sole wet weight mimics the decline in the complement of the water content (100 − percent water; figure 10).

These measurements were made during the period in which Dover sole mature sexually and begin to spawn (December–February). Other patterns may exist at other times of the year.

Sexual Maturity

The sexual maturity of Dover sole females was estimated by calculating the fraction mature (M) for each of seven 50-mm length classes (255–526 mm). These data were fit by maximum likelihood estimates of the parameters to a logistic model

$$M = \frac{e^{a + bx}}{1 + e^{a + bx}} \tag{8}$$

where $a = -9.947$, standard error (SE) of a = 0.00688, and t = 4.657 (d.f. = 102, $p < 0.01$); b = 0.0320, SE of b = 2.159, and $t = -4.608$ (d.f. = 102, $p < 0.01$); and n = 104. This equation predicts that 50% of Dover sole females of 311-mm length are mature (figure 11). The smallest Dover sole with advanced yolked oocytes in our collections was 290 mm long. Judging from our age-length relation for female Dover sole (equation 3), a length of 311 mm corresponds to an age of about 7 years, and a length of 290 mm corresponds to about 6 years.

The mean lengths per depth class were used to calculate how sexual maturity varied with depth. Mean female lengths were computed for each of eight 100-fath. (183-m) depth classes (all trawl data, 1985–88; n = 4,412; table 1; figure 4), and the maturity was calculated using equation 8. This analysis indicated that sexual maturity increased with depth from about 60% at 200 fath. to 90% or more at depths of 300 fath. (549 m) or greater.

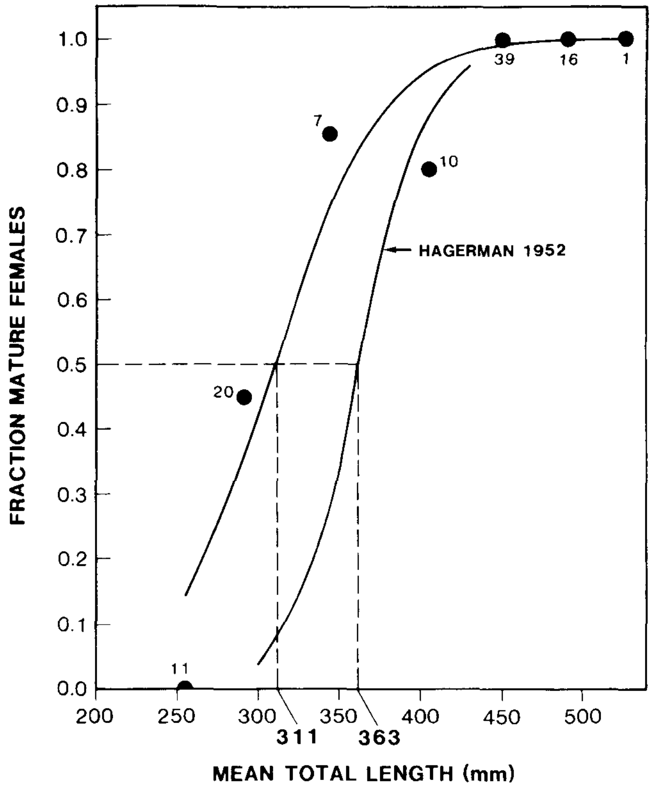


Figure 11. *Circles*, observed fraction of Dover sole that are mature in December within 50-mm length classes with N indicated; *solid line*, calculated fraction mature from logistic regression equation (equation 8); *Hagerman line* calculated similarly; *dashed line*, length at 50% maturity.

Oxygen Minimum Zone

Perhaps the most striking features of the environment occupied by Dover sole are the oxygen minimum and dysaerobic zones. In the 1987 survey the oxygen minimum zone ($O_2 < 0.5$ ml/l) occurred between 640 m (350 fath.) and 1010 m (550 fath.), which is similar to the depth range (280–550 fath.) during 1981–82 reported by Mullins et al. (1985) off Point Sur, California. The dysaerobic zone ($O_2 < 1$ ml/l) began at 457 m (250 fath.). Most sexually mature females were observed in the dysaerobic zone. We estimated that 86% of the spawning biomass existed in the oxygen minimum zone (table 4; figure 12) on the basis of the middle depth stratum (250–549 fath.) used in the 1987 and 1988 surveys (Butler et al. 1989). We also estimated that nearly all (93%) of Dover sole living above the dysaerobic zone were juvenile.

The oxygen minimum zone is also characterized by low temperatures (figure 12). The water in the dysaerobic zone ranged from 5.9°C at 530 m (290 fath.) to 3.2°C at 1262 m (690 fath.), and that in the

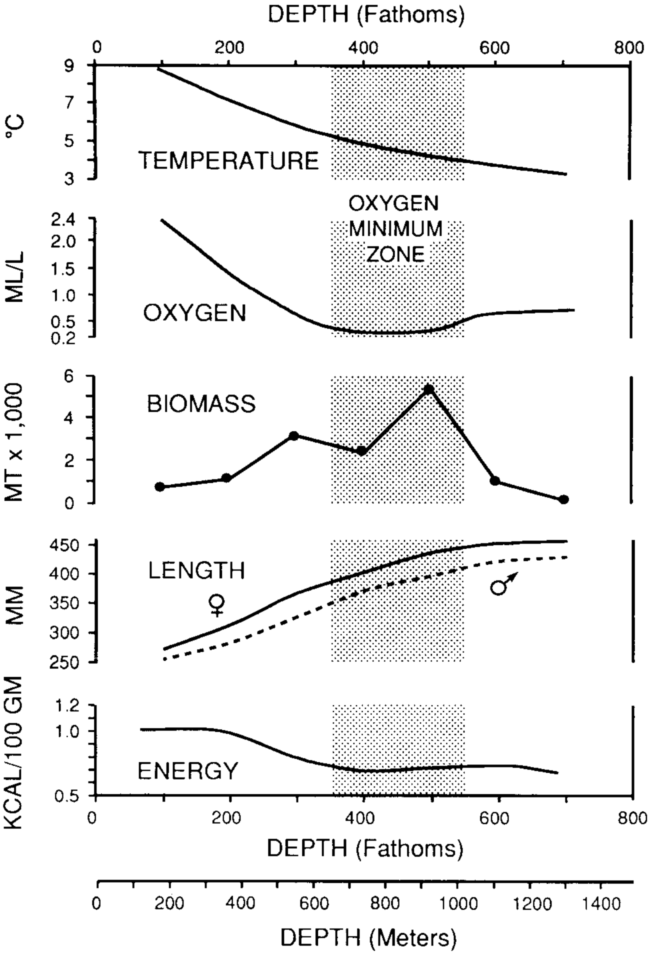


Figure 12. Bottom temperature, dissolved oxygen, biomass (MT) in study area, length (mm), and energy content (kcal/100 g wet weight) of Dover sole as a function of depth for 1987.

oxygen minimum zone from 4.9°C (700 m; 383 fath.) to 4.3°C (902 m; 493 fath.). Reduced metabolism due to temperature may help Dover sole cope with the low oxygen concentrations in the region.

DISCUSSION

Maturity

Our estimate of the length at first maturity differs from that estimated by Hagerman (1952). We fit equation 8 to Hagerman's data (250–630 mm, n = 846) and estimated the length at 50% maturity of female Dover sole landed in Eureka, California, during 1948–49 to be 363 mm ($a = -17.85$, SE of a = 1.975, $t = -9.042$, d.f. = 842, $p < 0.01$; b = 0.0492, SE of b = 0.0050, t = 9.829, d.f. = 842, $p < 0.01$; and n = 844). We compared the coefficients of the logistic regressions for our data to those collected by Hagerman using the Z-test (Zar 1984;

TABLE 4

Computation of the Proportion of Spawning Biomass of Female Dover Sole (O) in the Oxygen Minimum Zone (OMZ[a])

Variables		1987				1988				1987 + 1988			
Symbol	Definition	Above	In	Below	All zones	Above	In	Below	All zones	Above	In	Below	All zones
B	Total biomass[b]	2,677	13,154	824	16,655	3,216	14,054	26	17,297	2,946	13,604	425	16,975
	±2SE (MT)	907	2,899	499	2,161	1,024	2,891	18	2,013				
R	$\frac{\text{Female wt.}^{c}}{\text{Female + male wt.}}$	0.77	0.81	0.92	—	0.77	0.81	0.92	—	0.77	0.81	0.92	—
Q	Female biomass [B x R]	2,061	10,655	758	13,474	2,476	11,384	24	13,884	2,268	11,019	391	13,679
W	Average female weight (g)	275	814	1,122	—	294	840	976	—	278	819	1,118	—
S	Fraction female with active ovaries[d]	0.420	0.736	0.816	—	0.420	0.771	0.816	—	0.420	0.743	0.816	—
O	Spawning female biomass [Q x S] (MT)	866	7,842	618	9,326	1,040	8,777	20	9,836	953	8,187	319	9,459
	(Percent)	9	84	7	100	11	89	<0.1	100	10	86	3	100

[a] Oxygen minimum zone $O_2 < 0.5$ ml/l; depth 250–549 fath. Above OMZ, depth < 250 fath.; below OMZ, depth ≥ 550 fath.

[b] From Butler et al. 1989. The biomass numbers for 1987 + 1988 are an arithmetic mean of the separate years' estimates.

[c] Estimated for 1985, 1987, and 1988 trawl survey data combined (no estimate made for individual years; from C. H. Lo, unpublished data, SWFC).

[d] $F = 0.28e^{(-0.027T)}$, stratum 1; $F = 0.152e^{(0.0018W - 0.0075T)}$, stratum 2; and $F = 0.78e^{(-0.003T)}$, stratum 3; where $T = -15$ and W = average female weight. The same equations were used for 1987 and 1988 estimates because they were computed by combining survey data from 1985–88 (from C. H. Lo, unpublished data, SWFC).

TABLE 5
Comparison of the Coefficients for Logistic Length and Maturity Equations for Female Dover Sole

	a	Variance	Z	b	Variance	Z
Present study	−9.947	4.661	2.702	0.03202	0.000047	−2.025
Hagerman (1952)	−17.854	3.901		0.04921	0.000025	

table 5). Because both sample sizes were large, we used the normal deviate Z with critical value of $> |2|$. The regression coefficients are significantly different at the .05 level, assuming that these distributions are normal. Thus it appears that Dover sole females from central California matured in 1985 at a smaller size than did those from Eureka, California, in the 1940s. For example, 50% of female Dover sole from central California mature when they reach 311 mm, whereas 50% maturity occurred at 363 mm in females from Eureka (1948–49). This 52-mm difference may be equivalent to an average difference in the age of first maturity of four to five years (assuming fish from central California in the 1980s grew at about the same rate as fish from Eureka in the late 1940s).

Similarly, Yoklavich and Pikitch (1989) compared their 1985 maturity estimate for Dover sole from Oregon to one made for the same area 35 years earlier. They found all females greater than 320 mm were mature in 1985, whereas 35 years earlier (Harry 1959) only 50% of Oregon females of 380 mm were mature and only 15% of the Dover sole less than 380 mm were mature. Yoklavich and Pikitch concluded that Dover sole from Oregon presently mature at smaller size and probably younger age than they did 35 years ago.

Because of possible sampling biases, we hesitate to attribute the differences between recent and older estimates to real biological differences. Bias could result from failure to obtain a representative sample over the full bathymetric range of Dover sole. In addition, the two older estimates (Hagerman 1952 and Harry 1959) may be biased because samples were taken from the fishery during spawning season. This process could lead to an overestimate of the average size of fish at first maturity, because females in postspawning condition could have been wrongly classified as immature. On the other hand, the methods employed by ourselves and Yoklavich and Pikitch (1989) were similar; in both studies the samples were taken in December 1985, which is early enough in the spawning season that biases from misclassification of postspawning fish seem unlikely. If our samples and those of Yoklavich and Pikitch (1989) accurately represent their respective regional populations, then females in Oregon matured at a smaller size than those in central California. For example, all Oregon females longer than 320 mm were mature, whereas in central California, only 57% (SD = 8%) of 320-mm females were mature. Yoklavich and Pikitch (1989) suggest that the long-term effects of the size-selective trawl fishery off Oregon was a compensatory decrease in the size of first maturity of Dover sole. This hypothesis may also explain the difference between our estimates for central California and theirs for Oregon, since the central California fishery (Morro Bay) for Dover sole is a new fishery with only minor landings until the last four or five years, whereas the Dover sole fishery in Oregon has been active for over forty years.

High Water Content

High water content (jellied flesh) has been reported for four marine flatfishes: Dover sole (Fisher et al. 1987; Puckett 1989); winter flounder, *Pseudopleuronectes americanus* (Pearcy 1961); yellowfin sole, *Limanda aspera* (Kizevetter et al. 1965); and American plaice, *Hippoglossoides platessoides* (Templeman and Andrews 1956). Roff (1982, 1983) attributed high water content in flatfishes to degradation of muscle tissue during periods of gonad development and maintenance during winter fasting (Roff 1982, 1983). On the other hand, Puckett (1989) concluded after examining the water content of Dover sole that depth, annual reproductive cycles, size, age, and the onset of sexual maturity were all involved, but depth was the most important factor. Most significantly, Puckett's analysis indicated that reproductive condition of mature females living at 650–1020 m was not correlated with their water content. He found that the average water content of males and females from deep water remained high throughout the year; for example, quarterly means for females ranged from 89.6% in the winter to 91.6% in the spring. He concluded that the effect of the reproductive season on water content was minimal, and that movement into a deepwater habitat was the key variable.

Dover sole differ from other flatfishes with high water content in two ways: the adult population lives at considerable depths (800–1500 m) and in the oxygen minimum zone. In deep-sea fishes, the caloric density per g wet weight typically decreases with depth (Somero et al. 1983). The change in caloric density of Dover sole with depth follows a similar pattern (figure 12). The water content of deep-living Dover sole is at the high end of the range for the deep-sea fishes examined by Childress and Nygaard (1973), where 90.5% was the highest value recorded. This indicates that the ontogenetic change in water content in Dover sole may be simply an adaptation to deepwater existence, and the explanations proposed for low caloric density of deep-sea fishes may apply equally well to Dover sole. These include the scarcity of food and development of feeding strategies that permit a great reduction in propulsive systems demanding a high metabolic rate (Somero et al. 1983).

Another plausible explanation for high water content is one linked to life in the oxygen minimum zone. The oxygen and energy required to maintain white muscle in the oxygen minimum zone could be significant. Reduction of white muscle content would reduce basal oxygen demand and increase the scope for activity in an oxygen-limited environment.

In contrast to white muscle, red muscle tissue is conserved in Dover sole. This red muscle is located behind the head and at the base of the pectoral fin. This location may indicate a role in feeding. Feeding behavior has been described for the congeneric lemon sole, *Microstomus kitt*, by Steven (1930). This species feeds on polychaetes and also inhabits muddy bottoms. When feeding, a lemon sole raises its head and tail and sits perched on its side, scanning the substrate. When a polychaete is found, the fish pounces with a forward leap, bringing its head down on the prey and strongly arching the anterior body (Steven 1930). Dover sole also feed on polychaetes (Pearcy and Hancock 1978; Gabriel and Pearcy 1981; and Wakefield 1984), and we have observed similar feeding behavior in captive Dover sole in our laboratory. Dover sole perched on the substrate have also been observed by Allen (1982) and Wakefield (pers. comm., October 1988). Conserving red muscle while sacrificing white muscle as the fish becomes more watery may be a mechanism to preserve levels of feeding performance while reducing basal oxygen demand.

Ontogenetic and Seasonal Movements

Tagging studies (Westrheim and Morgan 1963; Quirollo and Kalvass 1987) and fishery data (Alverson 1960) indicate that Dover sole move inshore in the summer. This raises some interesting questions concerning the ontogenetic movements we have described. Do the fish precisely re-sort themselves by depth each fall as they leave their shallow summer habitat? How do they gradually (over decades) increase the depth of their winter habitat? Does high water content affect the extent of seasonal movements?

No data exist to fully answer these interesting questions, but some inferences can be made on the basis of a tagging study conducted by Quirollo and Kalvass (1987). Their data indicate that all Dover sole may not participate in the summer inshore movement. Mature fish tagged and released in shallow water were usually recovered from deep water in the winter and fall and from shallow water in the spring and summer, indicating annual inshore movement. In contrast, most of the mature fish tagged and released in deep water were recovered in deep water regardless of season. Thus the tagging data indicate that two substocks may exist, one that migrates and one that does not. We suggest that the fish composing the migratory substock may be younger and have a lower water content than those composing the nonmigratory substock.

CONCLUSIONS

The deep, cold, and poorly oxygenated region of the continental slope known as the oxygen minimum zone is the habitat for the mature Dover sole in central California. Ninety-eight percent of the spawning biomass of Dover sole occurs in this region. Dover sole spawn at these depths, and their eggs rise to the surface layers.

Juveniles settle on the continental shelf and, with sexual maturity, gradually move down the continental slope. The onset of sexual maturity and the ontogenetic movement into the cold oxygen minimum zone (350–550 fath.; 640–1010 m) is usually associated with an increase in water content, myoglobin content of red muscle, and fat stores. The movement down the slope corresponds with a consistent pattern of increasing size, age, and water content.

The ontogenetic movement down the shelf is gradual and occurs over decades. The average female Dover sole in central California reaches maturity when about 7 years old and 311 mm long. At this time she lives at a depth of about 329 m (180 fath.), and her water content is about 83.8%. We speculate that over the next nine years the annual inshore and offshore movement gradually ceases. Over the same period the water content of the average female gradually increases as she descends to greater depths and enters the oxygen minimum zone (640 m; 350 fath.). By then she is 16 years old, is almost 400 mm long, and has a water content of 87%. By this time she, like 93% of her cohort, is sexually mature, and growth has slowed from 14 mm per year when she was 7 years old to 6 mm per year. Eleven years later the average female has descended to 1006 m (550 fath.), is 27 years old, has grown 44 mm (about 4 mm per year), and has a water content of 88.7%. Water content continues to increase slowly over the next decades, and the fish continue to spawn and move deeper. The oldest female we aged was 51 years. The highest water content was 94.1%. The greatest depth at which we collected Dover sole was 1269 m.

LITERATURE CITED

Allen, M. J. 1982. Functional structure of soft-bottom communities of the Southern California Shelf. Ph.D. thesis, Univ. Cal. San Diego, 577 pp.

Alverson, D. L. 1960. A study of annual and seasonal bathymetric catch patterns for commercially important groundfishes of the Pacific Northwest coast of North America. Pac. Mar. Fish. Comm. Bull. 4, 66 pp.

Butler, J. L., C. Kimbrell, W. C. Flerx, and R. D. Methot. 1989. The 1987–88 demersal fish surveys off central California (34°30′N to 36°30′N). Southwest Fish Cent., Natl. Mar. Fish. Serv., NOAA, Tech. Memo. TM-NMFS-SWFC-133, 44 pp.

Childress, J. J., and M. H. Nygaard. 1973. The chemical composition of midwater fishes as a function of depth of occurrence off southern California. Deep-Sea Res. 20:1093–1109.

Chilton, D. E., and R. J. Beamish. 1982. Age determination methods for fishes studied by the groundfish program at the Pacific Biological Station. Can. Spec. Publ. Fish. Aquat. Sci. 60, 102 pp.

Demory, R. L. 1972. Scales as a means of aging Dover sole (*Microstomus pacificus*). J. Fish. Res. Board. Can. 29:1647–1650.

Dobush, G. R., C. D. Ankney, and D. G. Krementz. 1985. The effect of apparatus, extraction time, and solvent type on lipid extractions of snow geese. Can. J. Zool. 63:1917–1920.

Draper, N., and H. Smith. 1981. Applied regression analysis. New York: John Wiley & Sons, 709 pp.

Engelman, L. 1988. LR stepwise logistic regression, *In* BMDP statistical software manual, vol. 2, W. J. Dixon, ed. Pp. 941–969.

Eschmeyer, W. N., E. S. Herald, and H. Hammann. 1983. A field guide to Pacific Coast fishes of North America. Boston: Houghton-Mifflin Co., 336 pp.

Fisher, R. A., R. A. Fritzche, and G. L. Hendrickson. 1987. Histology and ultrastructure of the 'jellied' condition in Dover sole, *Microstomus pacificus*. Proc. V Congr. Europ. Ichthyol., Stockholm 1985, pp. 345–350.

Gabriel, W. L., and W. G. Pearcy. 1981. Feeding selectivity of Dover sole, *Microstomus pacificus*, off Oregon. Fish. Bull., U.S. 79(4): 749–763.

Hagerman, F. B. 1952. The biology of the Dover sole (*Microstomus pacificus*) (Lockington). Calif. Dep. Fish Game Fish Bull. 85, 48 pp.

Harry, G. Y. 1959. Time of spawning, length at maturity, and fecundity of the English, petrale and Dover soles (*Parophrys vetulus*, *Eopsetta jordani* and *Microstomus pacificus*, respectively). Fish. Comm. Oregon. Res. Briefs 7(1):5–13.

Hendrickson, G. L., R. A. Fritzsche, and H. M. Puckett. 1986. Ecology and possible causes of the "jellied" condition in Dover sole, *Microstomus pacificus*. California Sea Grant biennial report, 1982–1984, University of California Sea Grant College Program, Rep. No. R-CSGCP-020:129–130.

Kizevetter, I. E., E. F. Kleie, A. A. Kirillova, O. M. Mel'Nikova, V. M. Myasoedova, and L. Ya. Ertel. 1965. Technological characteristics of Bering Sea fishes, *in* Soviet fisheries investigations in the northeast Pacific. Part VI, P. A. Moiseev, ed. (Transl. from Russian by Israel Program for Sci. Trans., Jerusalem, 1968.) Pp. 191–258.

Mearns, A. J., and L. Harris. 1975. Age, length, and weight relationships in southern California populations of Dover sole. S. Cal. Coast. Water Res. Proj. TM 219, 17 pp.

Mullins, H. T., K. McDougall, and T. L. Vercoutere. 1985. Oxygen-minimum zone edge effects: evidence from the central California coastal upwelling system. Geology 13:491–494.

O'Neill, R. 1971. Function minimization using a simplex procedure. Algorithm AS47. Applied Statistics, 338.

Paine, R. T. 1971. The measurement and application of the calorie to ecological problems. Annu. Rev. Ecol. Syst. 2:145–164.

Parr Instrument Co. 1960. Oxygen bomb calorimetry and combustion methods. Technical Manual No. 130, 1–56. Moline, Ill.

Pearcy, W. G. 1961. Seasonal changes in osmotic pressure of flounder sera. Science 134:193–194.

Pearcy, W. G., and D. Hancock. 1978. Feeding habits of Dover sole, *Microstomus pacificus*; rex sole, *Glyptocephalus zachirus*, slender sole, *Lyopsetta exilis*; and Pacific sanddab, *Citharichthys sordidus*, in a region of diverse sediments and bathymetry off Oregon. Fish. Bull., U.S. 76:641–651.

Pearcy, W. G., M. J. Hosie, and S. L. Richardson. 1977. Distribution and duration of pelagic life of larvae of Dover sole, *Microstomus pacificus*; rex sole, *Glyptocephalus zachirus*; and petrale sole, *Eopsetta jordani*, in waters off Oregon. Fish. Bull., U.S. 75:173–183.

Pikitch, E., and R. Demory. 1988. Assessment of scales as a means of aging Dover sole. Trans. Am. Fish. Soc. 117:345–349.

Puckett, H. M. 1989. Ecology and possible causes of the jellied condition in Dover sole (*Microstomus pacificus*). M.S. thesis, Humboldt State Univ., Arcata, Calif., 80 pp.

Quirollo, L. F., and P. Kalvass. 1987. Results of Dover sole tagging in waters off northern California, 1969–1971. Calif. Dep. Fish Game Mar. Res. Admin. Rep. No. 87-4, 73 pp.

Roff, D. A. 1982. Reproductive strategies in flatfish: a first synthesis. Can. J. Fish. Aquat. Sci. 39:1686–1698.

———. 1983. An allocation model of growth and reproduction in fish. Can. J. Fish. Aquat. Sci. 40:1395–1404.

Sokal, R. R., and F. J. Rohlf. 1981. Biometry. 2nd ed. W. H. Freeman and Co., 859 pp.

Somero, G. N., J. F. Siebenaller, and P. W. Hochachka. 1983. Physiological adaptations of deep-sea animals. *In* The sea, vol. 8: deep-sea biology, G. T. Rowe, ed. N.Y.: Wiley Interscience.

Steven, G. A. 1930. Bottom fauna and the food of fishes. J. Mar. Biol. U.K. 16:677–706.

Templeman, W., and G. L. Andrews. 1956. Jellied condition in the American plaice *Hippoglossoides platessoides* (Fabricius). J. Fish. Res. Bd. Can. 13:147–182.

Wakefield, W. W. 1984. Feeding relationships within assemblages of nearshore and mid-continental shelf benthic fishes off Oregon. M.S. thesis, Oregon State Univ., Corvallis, Oregon, 102 pp.

Westrheim, S. J., and A. R. Morgan. 1963. Results from tagging a spawning stock of Dover sole, *Microstomus pacificus*. Pac. Mar. Fish. Comm. Bull. 6:14–21.

Yoklavich, M. M., and E. K. Pikitch. 1989. Fecundity and status of maturity of Dover sole, *Microstomus pacificus*, off northern Oregon, Fish Bull., U.S. 87(4).

Zar, J. H. 1984. Biostatistical analysis. New Jersey: Prentice-Hall, 718 pp.

MONITORING INTERANNUAL CHANGES IN SPAWNING AREA OF PACIFIC SARDINE *(SARDINOPS SAGAX)*

PAUL E. SMITH
National Marine Fisheries Service
Southwest Fisheries Center
P.O. Box 271
La Jolla, California 92038

ABSTRACT

It is easier to monitor the spawning area of the Pacific sardine than to mount a full-scale effort to precisely estimate spawning biomass. Monitoring the spawning area may be particularly economical when the sardine is extremely rare or extremely abundant. Such imprecise estimates will probably not answer the management question of whether or not to set a specific biomass quota. The spawning area estimate is a candidate — with aerial surveys, scale sedimentation rates, and acoustic-trawl surveys — for use in interpolating between years when the more precise SWFC daily egg production method is used.

RESUMEN

Resulta más fácil monitorear el área de desove de la sardina del Pacífico que montar un estudio a gran escala que estime con precisión la biomasa del desove. El monitoreo del área del desove parece ser una salida económica cuando la abundancia de la sardina es extremadamente baja o extremadamente alta. Estas estimaciones imprecisas probablemente no contesten las preguntas administrativas relacionadas con la posibilidad de imponer una cuota a la biomasa específica. La estimación del área de desove junto con estudios aéreos, determinaciones de velocidad de sedimentación de las escamas, y estudios de arrastres acústicos sea probablemente el mejor método para interpolar datos entre aquellos años cuando el método mas preciso de la producción de huevos del SWFC es utilizado.

INTRODUCTION

The sardine fishery has been thoroughly reviewed by Ahlstrom and Radovich (1970). The fishery began in Monterey in the nineteenth century and grew to a maximum seasonal catch of just over 700,000 metric tons (MT) in the 1936–37 season. The catches began to disappear from the Pacific Northwest and northern California in 1945–46 and were inconsequential by 1952–53. There was a minor resurgence to 115,000 MT in the 1958–59 season; thereafter the fishery declined to very low levels.

There appears to be some recovery of the Pacific sardine stock (Parrish et al. 1989) off southern California (Wolf and Smith 1986; Wolf et al. 1987), as evidenced by the tendency for the spawning area to increase. Since 1986, small quotas for commercial catch have been permitted (907 MT for all use, 317 MT for live bait, 227 MT for dead bait). The incidental catch of sardine has also increased, particularly in the Pacific mackerel fishery (Wolf 1989). If the spawning biomass continues to increase, there will soon be a need for a management plan, and decisions will have to be made about monitoring the size of the stock. The moratorium on sardine catch for all uses was managed informally from 1974 to 1984 with an annual statement that the biomass appeared to be below 20,000 short tons. Currently, the spawning biomass of sardine is estimated from a spawning area relationship published by Wolf and Smith (1985). The quota of 907 MT for all uses has been constant since 1986, but may be increased by the California Department of Fish and Game.

The Southwest Fisheries Center Coastal Division has devised an absolute, instantaneous daily egg production method to estimate spawning biomass of northern anchovy (Lasker 1985). It remains for a management plan to determine how often and how precisely the spawning biomass of sardine must be monitored. Annual management advice is currently based on the stock synthesis model (Methot 1989; Lo and Methot 1989; Jacobson and Lo 1989).

Zweifel (1973) noted that the number of sardine eggs per positive station remained stable from 1951 to 1960, even as the population of sardine declined by nearly an order of magnitude. Smith and Richardson (1977: table 3.10) showed that the mean number of eggs per positive station varied little, even with different quantitative net tows and population sizes varying from 4 million to 200,000 tons. Wolf et al. (1987: table 2) demonstrated that the mean number per positive net tow remained stable, with a sardine spawning biomass estimated at 20,000 tons; thus, over a measured range of 20,000 to 4

[Manuscript received February 15, 1990.]

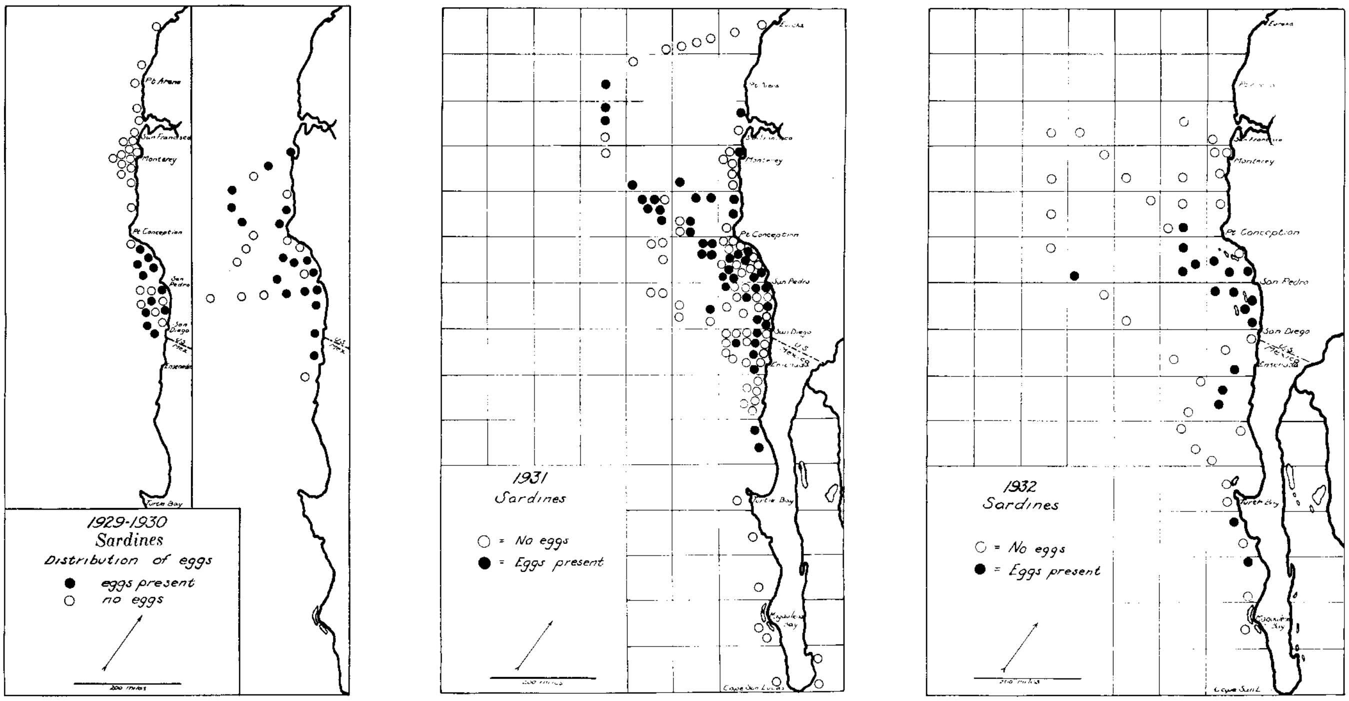


Figure 1. Locations of net tows taken to search for Pacific sardine eggs in April, May, and June 1929; April, May, and June 1930; February–August 1931; and February, April, and May 1932 (Scofield 1934). *Solid circles* indicate that the net tow was positive for eggs; *open circles* indicate that a net tow was taken and examined for eggs but none were found.

million tons of spawning biomass, the distribution of sardine eggs per positive station remains the same. We believe that this circumstance arises from the schooling habit of adults, regardless of the spawning biomass of the population; the further behavioral concentration of sperm and eggs by the fraction of fish involved in spawning at the time of external fertilization in the open sea; and the subsequent dispersal by turbulent diffusion (Smith 1973; Smith and Hewitt 1985; Mangel 1985; Mangel and Smith, in press). Theoretically, the number of eggs per positive station could decline at high biomass concentrations because filter feeding in schooling fishes like the sardine and anchovy may result in incidental cannibalism and predation of the eggs (Gulland 1971; Alheit 1987; Smith et al. 1990).

It is assumed that the management plan for sardine will be similar to that for anchovy, with two thresholds (PFMC 1983). When the spawning biomass is below the lower threshold, no fishery is permitted; when the spawning biomass is between the lower and upper threshold, a fixed fraction of the spawning biomass, based on demographic considerations, is established as a quota for the ensuing year; and when the spawning biomass is above the upper threshold, no further catch is authorized. The lower threshold is established at the point that the costs of the fisher's search for the remaining fish increase, and where further catches would probably delay the recovery of the stock to higher productivity. The upper threshold is established at the point where fleet and processor capitalization and costs of marketing would not be repaid owing to the temporary nature of high biomass. This requires rather precise monitoring of spawning biomass near the lower threshold, and less precise estimates at all other biomass levels. Also, the policy of using indices of abundance rather than annual absolute measures of abundance reduces management costs for assessing stock at higher levels of abundance while providing for a stable fishery.

It is the purpose of this paper to present data to aid in designing programs to estimate and monitor biomass. These programs should contribute to management of the Pacific sardine fishery and to the understanding of interactions among the sardines, other planktivorous fishes, and their environment. The data used in the original paper on this topic by Zweifel (1973) will be extended from 1940 to the present.

METHODS AND RESULTS

Surveys of sardine eggs and larvae were conducted in 1929–32 (Scofield 1934; figure 1), 1939–1941 (Ahlstrom 1948; Ahlstrom 1966; Smith 1972;

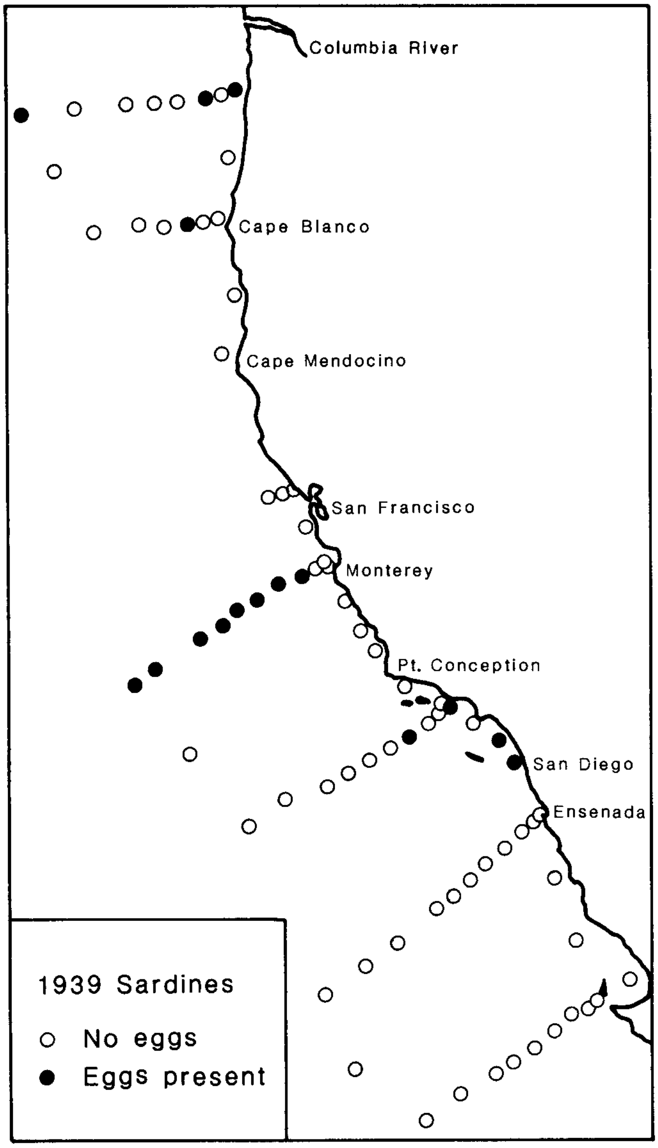


Figure 2. Locations of net tows taken to search for Pacific sardine eggs in May and June 1939 (Ahlstrom 1948). Net tows taken by Scofield (1934) were used to design the cruise.

figure 2); and 1951–89 (CalCOFI on-line data system). Although distributions of eggs and larvae per net tow can be obtained for all these time periods, it is not possible to measure the areal boundaries of the spawning distribution for all sets of years. For this illustration, I have chosen the time series of sardine eggs and larvae for the area surveyed in 1941 (figure 3). At that time, virtual population estimates were based on the assumption that every female over 2 years old was a spawner; maturity and gonadal activity were not checked in each year.

The method used here for estimating biomass is the "index area" method. The quality of this method depends on the assumptions that the area chosen contains all the biomass, is a consistent fraction of the total spawning area, or contains a constant fraction of the spawning biomass. It is likely that all time series used will suffer from violation of the same assumptions, since all values contain measurement error, and the values are drawn from an autocorrelated time series. I do not believe the regression methods used (Williams 1983) are valid for direct estimates of spawning biomass for management purposes. Recognizing these limitations, I will use the well-known procedures for linear regression to evaluate the components of indirect measures of population size.

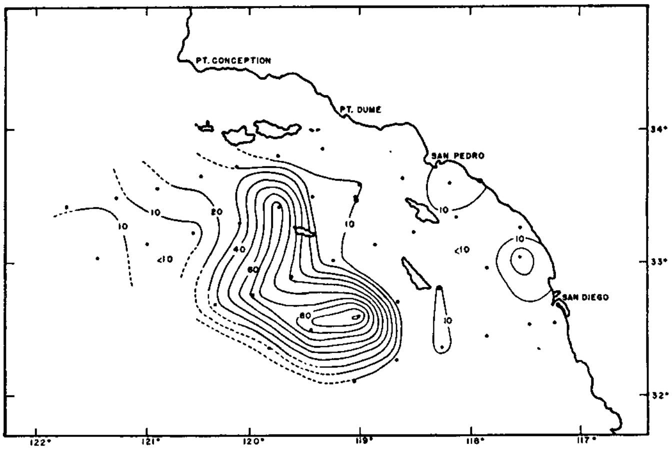


Figure 3. Contoured abundance of sardine eggs during the period of survey in 1941 (Sette and Ahlstrom 1948). These data are included in the analysis.

Regression Analysis

Data on egg and larval abundance are available for selected years for a major region of spawning. In a previous analysis of anchovy and sardine (Smith 1972), only the larval data were used. It was noted that larvae can be sampled over 3 weeks and disperse and cover more area than eggs, which can only be sampled over 3 days. In addition, the eggs retain the distributional characteristics of the schooled adults that spawned them. Although larvae have the statistical advantage of dispersal, over 3 weeks' time there may be considerably more variability in mortality than for eggs. In this study I use both egg and larval data as well as the biomass estimates from virtual population methods used for catch analysis from 1932 through 1965 (Murphy 1966: 1932–44; MacCall 1979: 1945–65; table 1). For the remainder of the text I use the notation as follows:

VPM— the biomass (MT) of sardines 2 years old and older;

E— the egg census estimate of abundance in the entire survey area ($\#/10\text{m}^2$);

TABLE 1

Data for Regression Estimates Using the Census Estimate or the Partitioned Estimates of Eggs or Larvae and the Virtual Population Methods (*VPM*) for Pacific Sardine

Year	*N*	*E*	*L*	*PE*	*PL*	*VPM*
A. Years for which *VPM* is available						
1940	240	699.10	49.13	0.754	0.808	1759.6
1941	210	336.90	36.88	0.629	0.748	2457.1
1951	96	33.33	2.06	0.167	0.146	277.0
1952	152	6.85	3.49	0.099	0.105	136.0
1953	226	0.21	0.07	0.031	0.013	202.0
1954	219	39.67	11.98	0.146	0.110	239.0
1955	142	26.87	7.29	0.169	0.092	170.0
1956	156	47.88	6.90	0.090	0.045	108.0
1957	145	23.00	12.09	0.103	0.097	90.0
1958	171	86.38	8.86	0.298	0.310	177.0
1959	188	182.00	7.88	0.287	0.250	122.0
1960	197	117.14	4.47	0.183	0.168	88.0
1961	73	17.08	1.40	0.164	0.082	54.0
1962	64	1.77	0.95	0.016	0.047	27.0
1963	77	14.22	1.94	0.052	0.039	21.0
1964	183	0.43	0.00	0.022	0.005	11.0
1965	112	4.57	0.79	0.107	0.098	3.0
B. Years for which no *VPM* is available						
1966	169	2.01	0.29	0.012	0.053	
1969	147	0.33	0.26	0.027	0.041	
1972	118	0.00	0.03	0.000	0.008	
1975	267	2.54	0.07	0.026	0.007	
1978	189	0.38	0.18	0.026	0.016	
1981	139	0.99	0.23	0.029	0.007	
1984	141	3.40	6.50	0.064	0.043	
1985	99	10.96	8.80	0.061	0.051	
1986	183	3.45	3.62	0.011	0.044	
1987	81	18.73	23.00	0.062	0.111	
1988	85	40.75	2.25	0.082	0.047	
1989	72	61.00	4.96	0.111	0.167	

Key: *N* = number of net tows included; *E* = egg census estimate of abundance; *L* = larval census estimate; *PE* = proportion of net tows containing sardine eggs; *PL* = proportion of net tows containing sardine larvae; *VPM* = biomass of sardines 2 years old and older.

L — the larval census estimate (#/10m^2; Smith 1972);

PE — the proportion of the net tows containing sardine eggs (positive stations);

PL — the proportion of the net tows containing sardine larvae;

NE — the number of eggs per positive station (#/10m^2); and

NL — the number of larvae per positive station (#/10m^2).

NE and *NL* are not listed in table 1 because they are obtained simply by dividing the census estimate *(E)* by the proportion of positive stations *(PE)*.

Table 2 lists the parameters, the standard errors of estimate of the parameters, the Student's *t* value of the estimate, the probability of the *t* value, the *F* value of analysis of variance, and the probability of the *F* value. No constant is less than .05, thus all the equations were redone forcing the zero-intercept. Omission of the constant when evaluating *PE* did not materially change the *F* value of the analysis of variance.

Possibly the most significant result of this analysis for monitoring the sardine biomass is the weakness of number of eggs or larvae per positive station when coupled with the probability of a positive station; in both cases *p* is greater than .05 and negative in reflecting changes in biomass (*PE* and *NE*, *PL* and *NL* in table 2; see also *NE* in figure 4B). As in the previous analysis (Smith 1972), the constants are not important, and the larval time series, with or without inclusion of number per positive station (*NL*), are marginally better predictors of spawning biomass (actually biomass of age 2+) than the equivalent measures of eggs (table 2). This may simply reflect the longer duration and better mixing with resultant lower variance of the larval estimates. I use the proportion of eggs relationship (*PE*) to extend the biomass time series to the present (table 3; figure 4A).

Another significant result is that the standard errors of the parameter estimates are all relatively large. This implies than none of these estimates would be adequate for year-by-year management advice during close regulation of the fishery. It seems likely that some of this parameter error is due to the fixed maturation at 2 years of age. The largest deviations occur at the times of significant temperature anomalies. It may be that temperature plays an important role in determining the rate of maturation in sardine, as has been found for anchovy (Methot 1989). If the age at first maturity is shown to be influenced by temperature, it is likely that the VPA series could be adjusted from temperature records on hand (see Methot 1989, table 1).

Even with the improvement in accuracy, it does not appear from table 3 that useful estimates can be obtained from the level of sampling effort now being conducted in the quarterly CalCOFI surveys. Although the trend of recovery may be essentially correct, the spawning biomasses indicated may well be overestimates (Wolf and Smith 1986; Wolf et al. 1987). In the absence of an adequate SWFC daily egg production method estimate to provide a recent calibration, it does not seem likely that spawning area estimates alone will be adequate for setting quotas for the Pacific sardine fishery (figure 4A). The spawning area estimate would be useful for monitoring the sardine stock at high levels, and the area and VPM or stock synthesis estimates may be useful for examining this population's impact on the ecosystem. If the stock recovers to more than a million tons it may be necessary to conduct wide-ranging

TABLE 2
Comparison and Evaluation of Regression Parameters for Indirect Estimation of Spawning Biomass of Pacific Sardine

Predictor	Coef	SD	Student's *t*	*p*	*F*-ratio	*p*
A. With constant						
Constant	51.2	113.5	0.45	0.659	29.05	0.000
E	3.0976	0.5747	5.39	0.000		
Constant	−68.40	88.15	−0.78	0.450	68.12	0.000
L	45.489	5.511	8.25	0.000		
Constant	−225.5	108.7	−2.07	0.056	57.00	0.000
PE	2946.9	390.3	7.55	0.000		
Constant	−143.87	83.06	−1.73	0.104	88.96	0.000
PL	2651.7	281.1	9.43	0.000		
Constant	−77.5	214.0	−0.41	0.722	6.98	0.018
NE	1.4407	0.5453	2.64	0.018		
Constant	296.4	252.3	1.17	0.258	0.08	0.780
NL	1.114	3.921	0.28	0.780		
Constant	−85.78	91.08	−0.94	0.362	33.89	0.000
E	−1.034	1.188	−0.87	0.399		
L	58.22	15.64	3.72	0.002		
Constant	−158.8	115.2	−1.38	0.190	31.42	0.000
PE	3559.9	574.2	6.20	0.000		
NE	−0.6284	0.4432	−1.42	0.178		
Constant	−175.0	111.4	−1.57	0.139	42.17	0.000
PL	2647.8	289.2	9.16	0.000		
NL	0.669	1.536	0.44	0.670		
Constant	−67.0	113.2	−0.59	0.565	28.39	0.000
PE	−2425	2019	−1.20	0.253		
PL	5214	1669	3.12	0.009		
NE	−0.6225	0.3882	−1.60	0.135		
NL	2.183	1.519	1.44	0.176		
B. No constant						
E	3.2239	0.4891	6.59	0.000	43.44	0.000
L	43.032	4.455	9.66	0.000	93.30	0.000
PE	2379.4	305.9	7.78	0.000	60.52	0.000
PL	2345.1	231.6	10.12	0.000	102.50	0.000
NE	1.2915	0.3476	3.72	0.002	13.80	0.002
NL	4.527	2.663	1.70	0.109	2.89	0.109
E	−0.789	1.154	−0.68	0.505	45.33	0.000
L	52.27	14.26	3.67	0.002		
PE	3469.8	587.3	5.91	0.000	38.98	0.000
NE	−0.8776	0.4166	−2.11	0.052		
PL	2437.8	268.7	9.07	0.000	49.93	0.000
NL	−0.881	1.234	−0.71	0.486		
PE	−3963	1758	−1.69	0.116	39.12	0.000
PL	5620	1484	3.79	0.002		
NE	−0.6318	0.3781	−1.67	0.119		
NL	1.832	1.364	1.34	0.202		

Key: *E* = egg census estimate of abundance; *L* = larval census estimate; *PE* = proportion of net tows containing sardine eggs; *NE* = number of eggs per positive station; *PL* = proportion of net tows containing sardine larvae; *NL* = number of larvae per positive station.

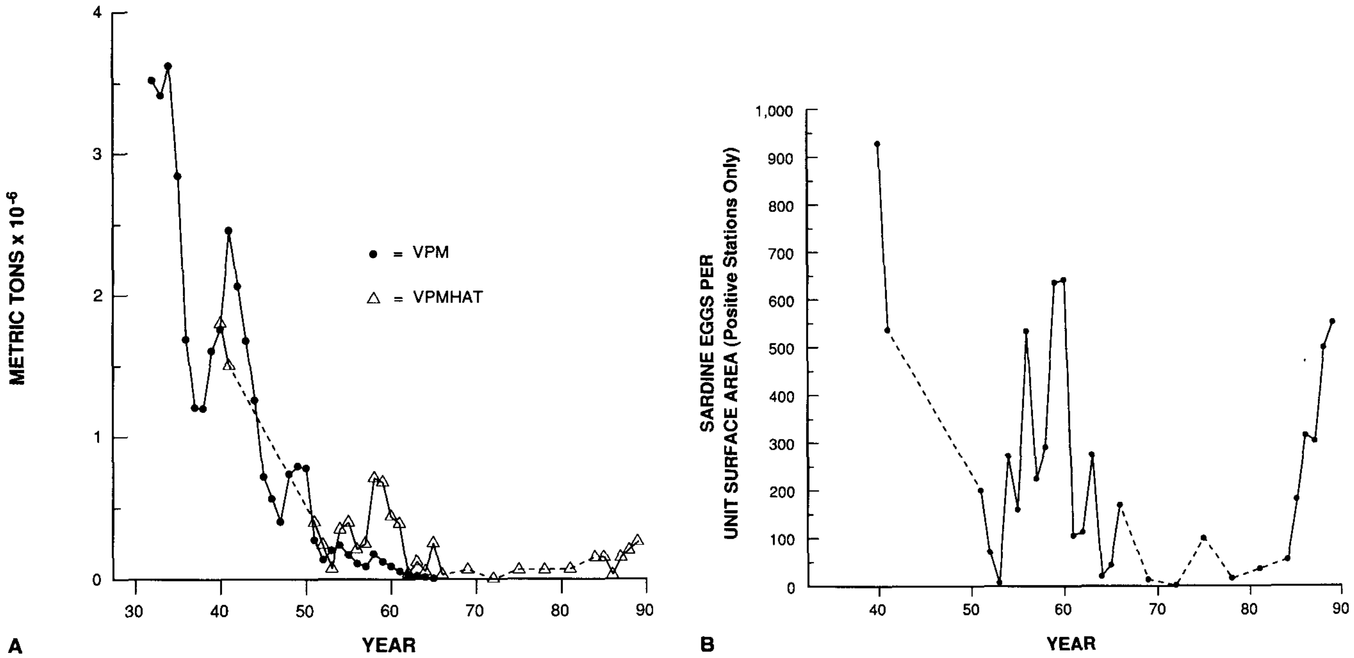


Figure 4. Time series plot of sardine 1932–89 (Murphy 1966: 1932–44; MacCall 1979: 1945–65). *A*, estimates using virtual population methods or egg incidence correlates (*PE*); *B*, estimated egg abundance per unit surface area at positive stations (*NE*).

TABLE 3
Virtual Population Estimates of Sardine Spawning Biomass (2+) Derived from Regression on the Proportion of Positive Egg Stations in 1940–41 and 1951–65

Year	*N*	*PE*	VPMHAT
1940	240	0.754	1800
1941	210	0.629	1500
1951	96	0.167	400
1952	152	0.099	240
1953	226	0.031	74
1954	219	0.146	350
1955	142	0.169	400
1956	156	0.090	210
1957	145	0.103	250
1958	171	0.298	710
1959	188	0.287	680
1960	197	0.183	440
1961	73	0.164	390
1962	64	0.016	38
1963	77	0.052	120
1964	183	0.022	52
1965	112	0.107	250
1966	169	0.012	29
1969	147	0.027	64
1972	118	0.000	0
1975	267	0.026	62
1978	189	0.026	62
1981	139	0.029	69
1984	141	0.064	150
1985	99	0.061	150
1986	183	0.011	26
1987	81	0.062	150
1988	85	0.082	200
1989	72	0.111	260

Key: *N* = number of net tows; *PE* = proportion of net tows containing sardine eggs; VPMHAT = estimate of sardine biomass 2 years old and older based on the regression estimate of table 2, no constant.

surveys (figures 1 and 2) to accomplish a daily egg production estimate based on the extent of spawning in the 1929–32 and 1939 surveys (Scofield 1934; Ahlstrom 1948).

DISCUSSION

Monitoring sardine biomass for fisheries management and ecosystem time series can be done with precise, but elaborate and expensive, egg production methods as used for the baseline studies of northern anchovy (Lasker 1985). Alternatively, indirect methods could be used, such as the deposition rate of scales (Soutar and Isaacs 1974) in anoxic sediments or traps; aerial surveys (Squire 1972); incidence and abundance of eggs or larvae; or trawl-acoustic methods in a mixed technique model (Methot 1989). Sardine stocks in the California Current region and other regions in the world fluctuate by orders of magnitude (Smith and Moser 1988; Lluch-Belda et al. 1989), and rough estimates of biomass may be sufficient for management plans during many periods of the species time sequence.

ACKNOWLEDGMENTS

I thank Patty Wolf, John Hunter, Izadore Barrett, Marc Mangel, Alec MacCall, and an unnamed reviewer for offering suggestions for materially improving the text. I am particularly grateful to Richard Charter and Cindy Meyer for maintaining the CalCOFI data base and writing special programs for extracting the data.

LITERATURE CITED

Ahlstrom, E. H. 1948. A record of pilchard eggs and larvae collected during surveys made in 1939 to 1941. Spec. Sci. Rep. U.S. Fish and Wildl. Serv. Fisheries (54), 82 pp.

———. 1966. Distribution and abundance of sardine and anchovy larvae in the California Current region off California and Baja California, 1951–64, a summary. Spec. Sci. Rep. U.S. Fish and Wildl. Serv. Fisheries (534), 71 pp.

Ahlstrom, E. H., and J. Radovich. 1970. Management of the Pacific sardine. *In* A century of fisheries in North America, N. G. Benson, ed. Special Publication No. 7, American Fisheries Society, Wash. D.C., pp. 183–193.

Alheit, J. 1987. Cannibalism versus egg predation: their significance in anchovies. *In* The Benguela and comparable ecosystems, A. I. L. Payne, J. A. Gulland, and K. H. Brink, eds. S. Afr. J. Mar. Sci. 5:467–470.

Gulland, J. A. 1971. Ecological aspects of fishery research. *In* Advances in ecological research, volume 7, J. B. Cragg, ed. New York: Academic Press, pp. 115–176.

Jacobson, L. D., and N. C. H. Lo. 1989. Spawning biomass of the northern anchovy in 1989. Southwest Fisheries Center Administrative Report LJ-89-17.

Lasker, R., ed. 1985. An egg production method for estimating spawning biomass of pelagic fish: application to the northern anchovy, *Engraulis mordax*. NOAA Tech. Rep. NMFS 36, 99 pp.

Lluch-Belda, D., R. J. M. Crawford, T. Kawasaki, A. D. MacCall, R. H. Parrish, R. A. Schwartzlose, and P. E. Smith. 1989. Worldwide fluctuations of sardine and anchovy stocks: the regime problem. S. Afr. J. Mar. Sci. 8:195–205.

Lo, N. C. H., and R. D. Methot. 1989. Spawning biomass of the northern anchovy in 1988. Calif. Coop. Oceanic Fish. Invest. Rep. 30:18–31.

MacCall, A. D. 1979. Population estimates for the waning years of the Pacific sardine fishery. Calif. Coop. Oceanic Fish. Invest. Rep. 20:72–82.

Mangel, M. 1985. Search models in fisheries and agriculture. Lect. Notes Biomath. 61:105–138.

Mangel, M., and P. E. Smith. In press. Presence-absence sampling for fisheries management. Can. J. Fish. Aquat. Sci. 47.

Methot, R. D. 1989. Synthetic estimates of historical abundance and mortality for northern anchovy. *In* Mathematical analysis of fish stock dynamics, E. F. Edwards and B. A. Megrey, eds. Am. Fish. Soc. Symp. 6:66–82.

Murphy, G. I. 1966. Population biology of the Pacific sardine (*Sardinops caerulea*). Proc. Calif. Acad. Sci. Fourth Series 34:(1)1–84.

Parrish, R. H., R. Serra, and W. S. Grant. 1989. Monotypic sardines, *Sardina* and *Sardinops*: their taxonomy, distribution, stock structure, and zoogeography. Can. J. Fish. Aquat. Sci. 46:2019–2036.

PFMC: Pacific Fishery Management Council. 1983. Northern anchovy fishery management plan. PFMC, 526 S. W. Mill St., Portland, Oregon 97201.

Scofield, E. C. 1934. Early life history of the California sardine (*Sardina caerulea*), with special reference to distribution of eggs and larvae. Calif. Dep. Fish Game Fish Bull. 41:1–48.

Sette, O. E., and E. H. Ahlstrom. 1948. Estimations of abundance of the eggs of Pacific pilchard (*Sardinops caerulea*) off southern California during 1940 and 1941. J. Mar. Res. 7:511–542.

Smith, P. E. 1972. The increase in spawning biomass of northern anchovy, *Engraulis mordax*. Fish. Bull. U.S. 70:849–874.

———. 1973. The mortality and dispersal of sardine eggs and larvae. Rapp. P. -V. Reun. Cons. Int. Explor. Mer 164:282–292.

Smith, P. E., and R. P. Hewitt. 1985. Anchovy egg dispersal and mortality as inferred from close-interval observations. Calif. Coop. Oceanic Fish. Invest. Rep. 26:97–110.

Smith, P. E., and H. G. Moser. 1988. CalCOFI time series: an overview of fishes. Calif. Coop. Oceanic Fish. Invest. Rep. 29:66–78.

Smith, P. E., and S. L. Richardson. 1977. Standard techniques for pelagic fish egg and larva surveys. FAO Fisheries Tech. Pap. 175, FAO Rome, 100 pp.

Smith, P. E., H. Santander, and J. Alheit. 1990. Comparison of mortality rates of Pacific sardine, *Sardinops sagax*, and Peruvian anchovy, *Engraulis ringens*, eggs off Peru. Fish. Bull. U.S. 87:497–508.

Soutar, A., and J. D. Isaacs. 1974. Abundance of pelagic fish during the 19th and 20th centuries as recorded in anaerobic sediment off the Californias. Fish. Bull. U.S. 72:257–273.

Squire, J. L., Jr. 1972. Apparent abundance of some pelagic marine fishes off the southern and central California coast as surveyed by an airborne monitoring program. Fish. Bull. U.S. 70:1005–1019.

Williams, B. K. 1983. Some observations on the use of discriminant analysis in ecology. Ecology 64:1283–1291.

Wolf, P. 1989. Review of some California fisheries for 1988. Calif. Coop. Oceanic Fish. Invest. Rep. 30:7–17.

Wolf, P., and P. E. Smith. 1985. An inverse egg production method for determining the relative magnitude of Pacific sardine spawning biomass off California. Calif. Coop. Oceanic Fish. Invest. Rep. 26:130–138.

———. 1986. The relative magnitude of the 1985 Pacific sardine spawning biomass off southern California. Calif. Coop. Oceanic Fish. Invest. Rep. 27:25–31.

Wolf, P., P. E. Smith, and C. L. Scannell. 1987. The relative magnitude of the 1986 Pacific sardine spawning biomass off California. Calif. Coop. Oceanic Fish. Invest. Rep. 28:21–26.

Zweifel, J. R. 1973. A non-parametric approach to the estimation of relative change in fish population size from egg and larval surveys. Rapp. P. -V. Reun. Cons. Int. Explor. Mer 164:276–281.

CalCOFI REPORTS – INSTRUCTIONS TO AUTHORS

Manuscript should be typed (no dot-matrix printouts, please) **double-spaced** with wide, ragged right margins, and submitted complete with figures, figure captions, and tables, in triplicate to

CalCOFI Coordinator
California Department of Fish and Game
330 Golden Shore, Suite 50
Long Beach, CA 90802

Manuscripts must be received by February 1 of the year in which publication is desired.

Sequence of the material should be TITLE PAGE, ABSTRACT, RESUMEN, TEXT, LITERATURE CITED, APPENDIX (if any), FOOTNOTES (if any), TABLES, LIST OF FIGURES with entire captions, and FIGURES.

Title page should give:
a running head of no more than 60 letters and spaces
title of the article
author(s) name(s) and affiliation(s)
address(es), including Zip Code(s)

Abstract should not exceed one **double-spaced** page and must be submitted both in English and in Spanish (*Resumen*).

Text style will in general follow that of the U.S. Department of Commerce (NOAA) *Fishery Bulletin*. Contributors who are not familiar with this publication will do well to follow *The Chicago Manual of Style* (1982). Authors are strongly urged to compare their typewritten equations with similar expressions in the printed literature, with special attention to ambiguity of the symbols for "one" and for "el," before submitting. Whenever possible, write in the first person, and use active verbs.

Measurements must be given in metric units; other equivalent units may be given in parentheses.

Personal communications and *unpublished data* should not be included in the Literature Cited but may be cited in the text in parentheses. Use *footnotes* only when parentheses will not suffice. List footnotes on a separate sheet.

Literature cited should appear in the text as Smith (1972) or Smith and Jones (1972) or (Smith and Jones 1972; Jones and Smith 1973) or Smith et al. (1972). All literature referred to in the text should be listed (**double-spaced**) alphabetically by the first author on a separate sheet under the heading Literature Cited. Only the authors' surnames and initials will be used. No citation should appear in the list of Literature Cited unless it is cited in the text, tables, or figure captions. Each citation must be complete according to the following:

(article):
Eppley, R. W., E. H. Renger, E. L. Venrick, and M. M. Mullin. 1973. A study of plankton dynamics and nutrient cycling in the central gyre of the North Pacific Ocean. Limnol. Oceanogr. 18(4):543–551.
(book):
Odum, E. P. 1959. Fundamentals of ecology. 2d ed. Philadelphia: Saunders, 546 pp.
(chapter):
Wooster, W. S., and J. L. Reid, Jr. 1963. Eastern boundary currents. *In* The sea, M. N. Hill, ed. New York: Interscience Pub. pp. 253–280.

Tables (with arabic numbers) should be typed (**double-spaced**) separately from the text; each table should start on a separate page and must have a brief title. Please avoid vertical rules and be consistent in format.

Figures, whether drawings or halftones, should be submitted in a format **not larger than 8½ × 11″**. Submit one set of camera-ready figures plus 2 sets of copies. Photographs should be printed on glossy paper. Drawings should be reduced photographically. A composite figure should be submitted as a single photograph or at least as a single careful paste-up. Figures will appear as either single-column (85-mm-width limit), or double-column (178-mm-width limit); or as full page. Special cases should be discussed with the editor before submittal. After reduction, no letter or number should be smaller than 1 mm. Special note should be taken of the disappearance of decimal points during reduction. If commercially prepared shading is used, make a trial reduction to ensure that the patterns do not merge at the required reductions. The determining factor for size should be the complexity of detail to be shown.

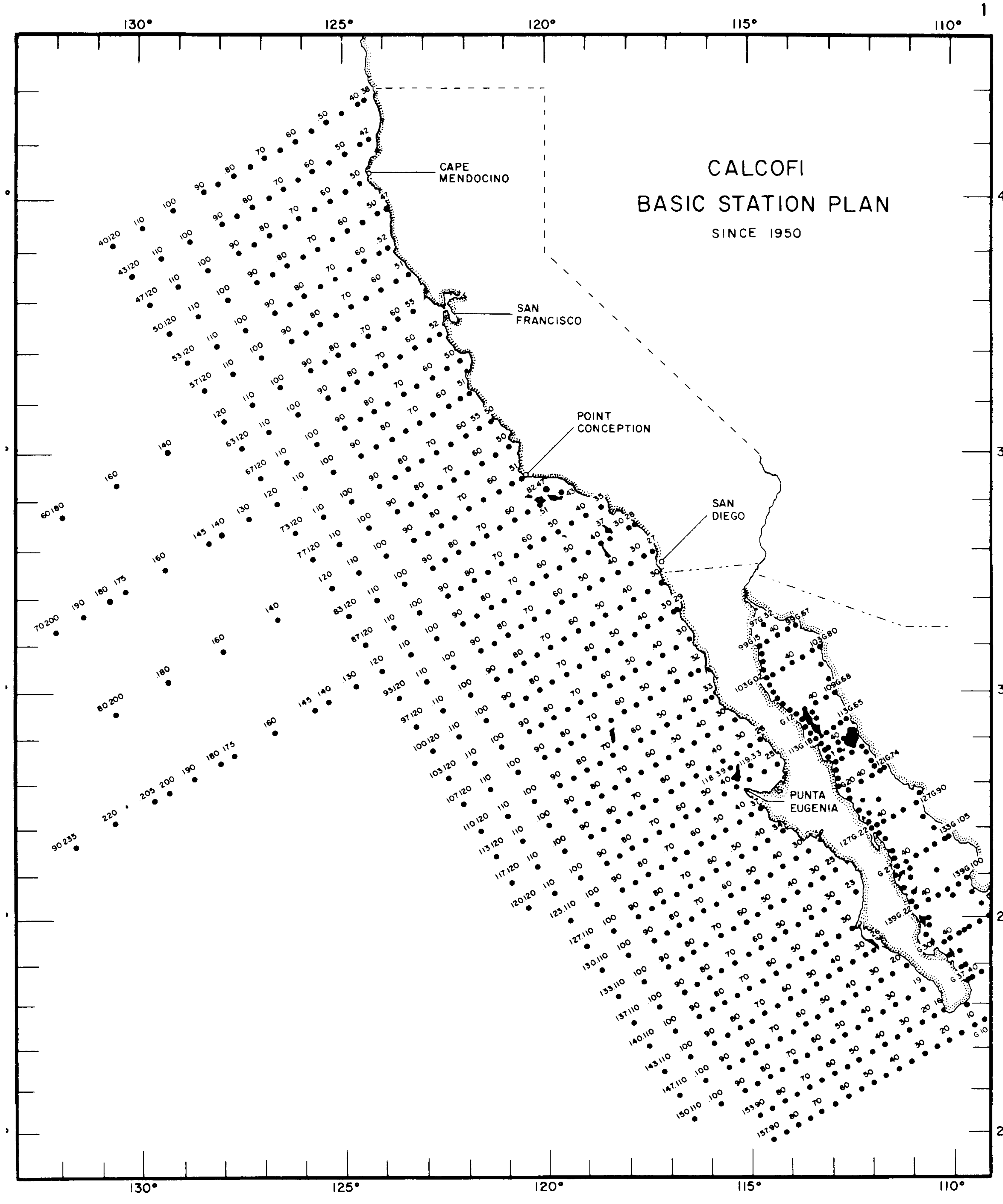
Each figure must have a *caption*; captions should be typed, **double-spaced**, in numbered sequence on a separate sheet. Illustrative materials submitted for publication are often first prepared for oral presentation in slide format. Authors should take special care that slide-format material submitted to *CalCOFI Reports* is appropriate to printed format with respect to economy, redundancy, and style.

Acknowledgments, if included, should be placed at the end of the text and may include funding source.

Reprint orders will be mailed (to senior author only) on publication of the completed book. No covers will be supplied, and there will be no further reproduction.

The *CalCOFI Reports* will use the *CalCOFI Atlas* full-page chart format where the material would be best used overlaid on the *CalCOFI Atlas* charts for purposes of comparison and where the material presented is of insufficient scope and quantity to warrant the publication of an atlas.

The CalCOFI Editorial Board will consider for publication, in the section entitled "Scientific Contributions," manuscripts not previously published elsewhere that bear some relationship to the following with respect to the Californias, the California Current, and the Gulf of California:
marine organisms
marine chemistry, fertility, and food chains
marine fishery modeling, prediction, policy, and management
marine climatology, paleoclimatology, ecology, and paleoecology
marine pollution
physical, chemical, and biological oceanography
new marine instrumentation and methods.



CALCOFI

BASIC STATION PLAN
SINCE 1950

CONTENTS

Indexed in *Current Contents*. Abstracted in *Aquatic Sciences and Fisheries Abstracts* and *Oceanic Abstracts*.