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

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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.]

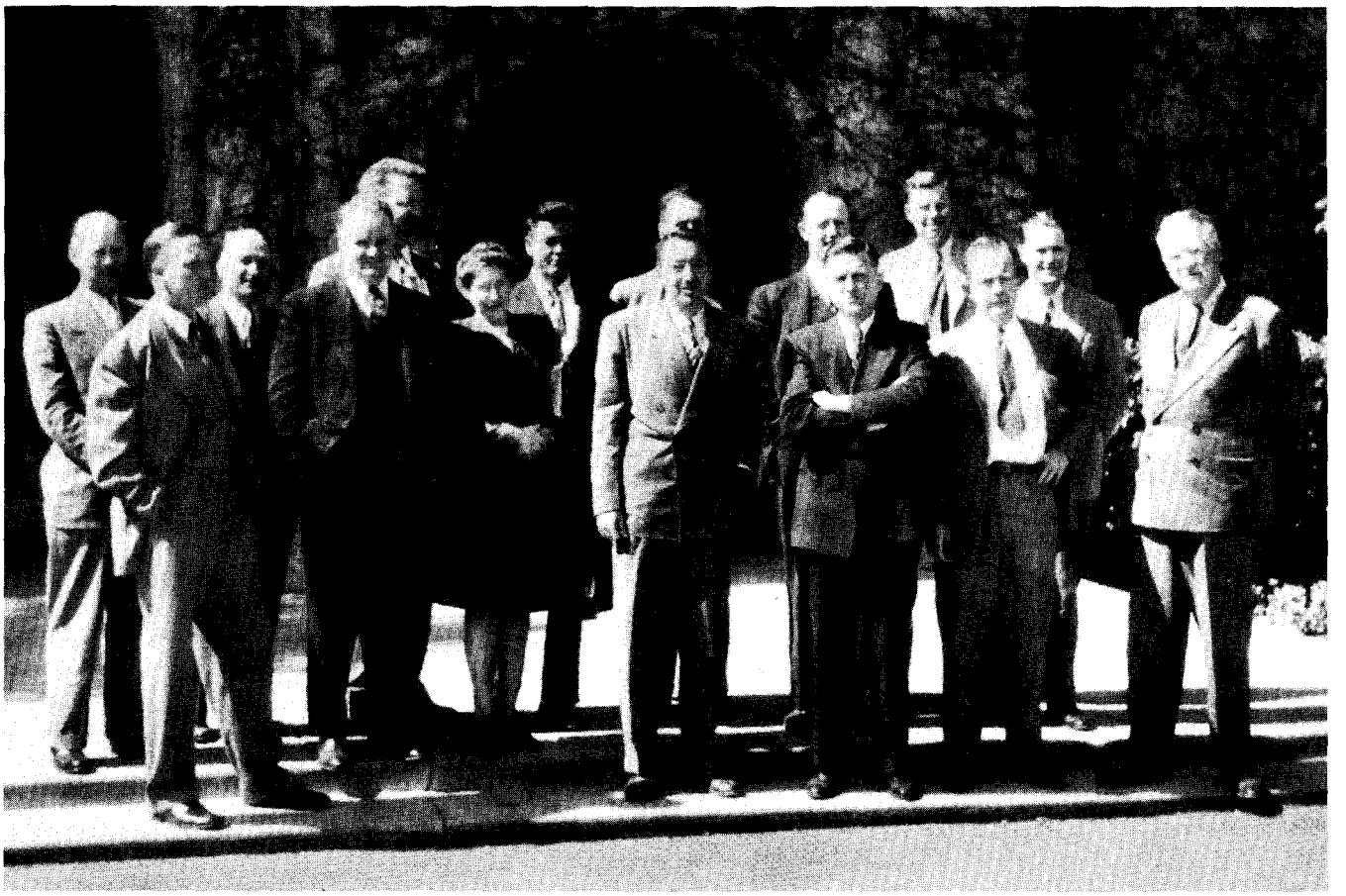

Figure 1. Photograph taken on March 14, 1947, at Stanford University at a meeting of representatives from the *California Department of Fish and Game* (1), the California Academy of Sciences (2), the South Pacific Investigations of the U.S. Fish and Wildlife Service (3), and the Scripps Institution of 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).

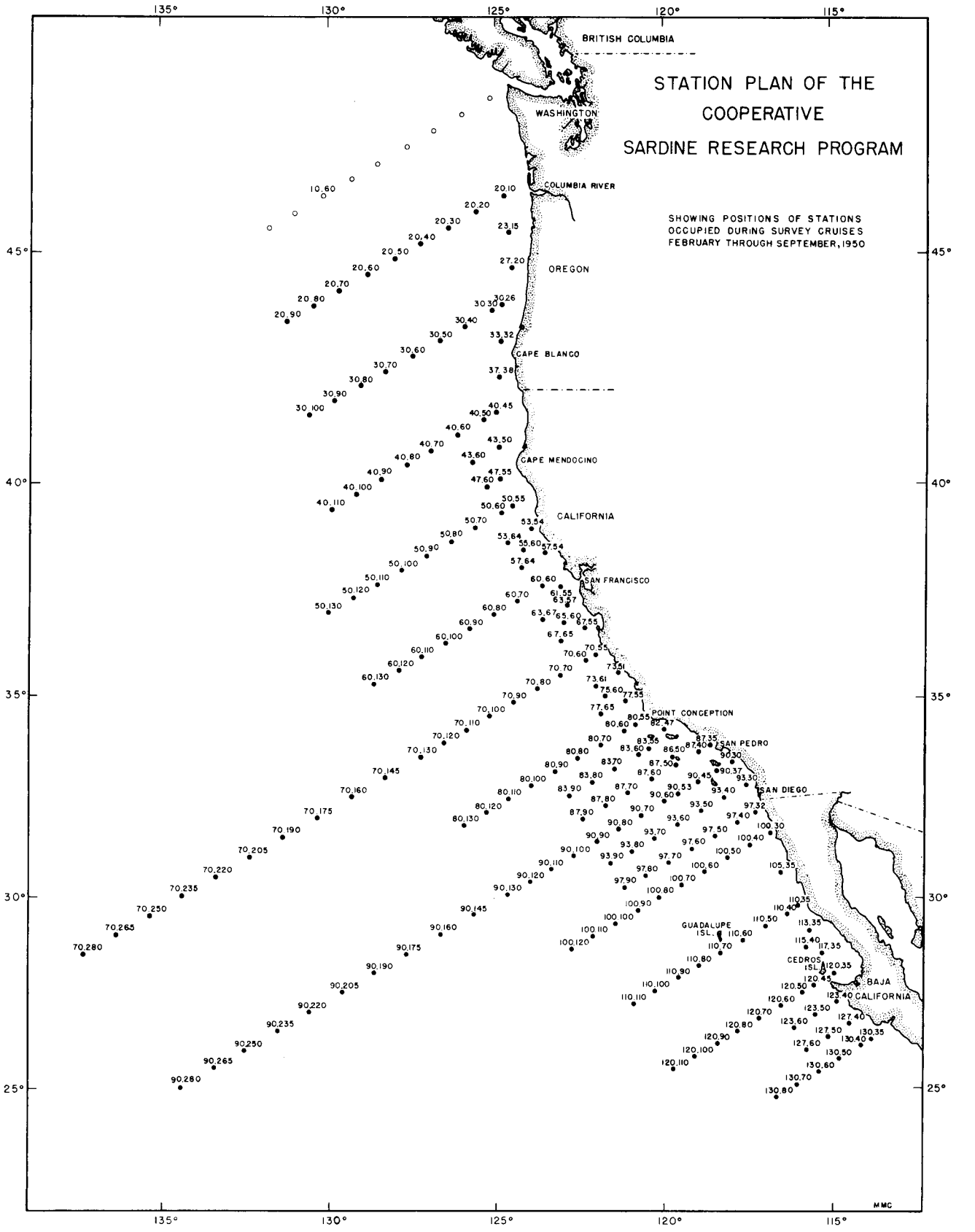

Figure 2. The CalCOFI station plan used for cruises in 1950. The numbering system was planned so that the station lines were 120 miles apart, and individual stations were 40 miles apart. Extra stations were added in regions of particular interest for sardine work. (See inside back cover for basic plan.)

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.

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

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.

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 an-

chovy 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.

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