The Application of Ecological Research to Aquiculture and the Fisheries[1]

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THERE IS A WIDESPREAD BELIEF that the era of a successful aquiculture is just around the corner; that farming the nation's ponds and lakes and the estuarine waters along the continental shores is quite as practical as the age-old practice of farming the soil. Without disparaging the invaluable aquicultural results that have accrued during the past decade or so, I think it must be stated frankly that major obstacles are still ahead of us. Lest we be discouraged, it may be well to remember that soil farmers are not altogether happy in their present situation, despite the highly specialized scientific agriculture that has evolved since the turn of the century. In 1939, of the six million or so farm families in the nation, 65 per cent grossed less than $1,000 cash annually. More than one million had a gross annual cash income of $250 or less (last available census figures). Competent agricultural opinion is that the relative picture today is probably similar to that of 1939.

I refer to the soil farmer, not only because aquatic biologists can learn much from his experience, but also because there is often a decided interdependence between him and the fisherman. Shortsighted farm practices have not only depleted soils of their natural productiveness, but they have silted up numerous rivers, lakes, streams, and bays throughout the nation, causing deterioration of the aquatic environment for economically important species. Watershed protection is probably the greatest single all-embracing need of the farmer and the fisherman alike. Natural spawning grounds for anadromous species in important West Coast streams have been destroyed or greatly reduced. Once-permanent streams are now seasonal. The imprint of pollution is accentuated with changing flows. On the Pacific Coast, perhaps the most striking example of wanton destruction by man-made erosion and pollution is San Francisco Bay. Here an oyster fishery has been extinguished, and the fin fisheries seriously reduced. On the Atlantic Coast, the productivity of the Chesapeake oyster grounds has likewise undergone irreparable damage. But the case of the soil farmer, historically, is quite different from that of the water farmer. The soil farmer *owns* his land, and his success is largely dependent on his diligence and ingenuity. Water, on the other hand, is usually publicly owned; most progress is being made where it is not. Witness the great strides made in the field of private pond culture in Alabama, and in oyster culture in Connecticut.

In large bodies of water, whether they be lakes, rivers, or oceans, there is the very real problem of multiple ownership *without* multiple responsibility. Interest rests primarily in today's catch with little thought of what remains for tomorrow. And this situation brings us to the crux of our discussion—namely, the application of ecological knowledge to human affairs. I am reminded of the introductory sentence in a paper entitled "Fish Management, Looking Forward," presented in 1937 by Carl Hubbs before the American Fisheries Society. It stated, "The need for thorough, soundly planned, and adequately administered fish management is coming to be generally appreciated." Referring to the past record of achievement, he went on to say, "It is being realized that the well-meaning efforts of the past have been to a large degree haphazard, incomplete, imperfect, uncoordinated, nontechnical, and untested." I believe the desideratum is here clearly indicated. Since 1937 important progress has been made in the fish management field. Technological staffs have been implemented, experimental research has become intensified, population analysis has been perfected, and new educational programs have been inaugurated.

The picture for fresh-water fish management is perhaps more promising than for salt-water management. I suppose the shortage of protein during the second world conflict played a major part in the impetus given oceanic fishery investigations since 1945. In any event, studies on a scale heretofore undreamed-of are in progress on the Pacific Coast of North America. One of these centers on the pilchard or sardine fishery, and I have elected to review events in the development of this fishery, first, because a crisis in it has offered a major challenge to administrators, biologists, and fishermen alike; second, because it is intrinsically an ecological problem; and third, because it is now presenting, although a little late, a classical example of a unified, balanced, cooperative effort in scientific management of a great marine resource. Let us hope that, when the full story is written, it will prove to be an analog of the halibut fishery of the North Pacific Coast, developed so skillfully under the leadership of W. F. Thompson.

A great deal has been written about the sardine since it became a major fishery during the first world war. Mention should be made of papers by Elmer Higgins, W. L. and E. C. Scofield, Frances N. Clark, R. B. Tibby, R. P. Silliman, O. E. Sette, L. A. Wal-

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ford, E. H. Ahlstrom, and their associates, among the American workers that have done so much to assure possibilities for a permanent and profitable future for this industry.

I shall review briefly certain of the high lights in the utilization of this natural resource and the factors largely responsible for its disastrous decline from a peak California production of 726,000 tons in 1936–37 to a low of about 110,000 tons in the 1946–47 season. Also, I shall refer to certain ecological data that explain some fluctuations in the population.

During the 1918–19 season, the sardine canning industry yielded 75,000 tons. By 1936–37, it had become an oil business, and the tonnage was up to 726,000 tons. Profit in oil was responsible for the increase, and despite California policy that the state fish crop should be used as food for man, reduction plants multiplied, legislative efforts and those of a minority of the producers notwithstanding. Scofield points out that in the early thirties leading producers were supporting the constructive efforts of the State Division of Fish and Game, and it appeared that disaster would be averted and that a rational management of the sardine harvest would result. But this was not to be. Reduction ships started to operate on the high seas outside state jurisdiction, and the economic balance was again upset. Understandably, the state legislature would not curtail the established shore industry employing nearly 10,000 cannery workers while unregulated competition from reduction ships threatened to run the canners out of business.

Biologists at the Terminal Island Laboratory were quite well aware that a major catastrophe was imminent because the *catch per unit of effort* was on the decline, even though total catches continued to increase. Canneries that once handled 80 tons per day can now take 80 tons or more per hour. Numerous warnings were issued by the Division of Fish and Game as early as 1930 but, seemingly, research evidence was in itself insufficient proof.

The decline started in 1937–38 and continued with alarming consistency until the 1946–47 season, when the California cannery catch reached a low of approximately 110,000 tons. The monthly catch of an average vessel in 1946–47 was 50 per cent below that of the previous season, and in 1947–48 the catch again dropped 50 per cent below the 1946–47 take. Practically all the fish were used for canning. Prices of raw fish were high, whereas oil and meal prices were down. Studies up to 1948 pointed to two main reasons for the decline—a drop in abundance caused by extremely heavy fishing, and a simultaneous succession of many seasons of very poor spawning. This information had been available for several years and was repeatedly used as a scientific basis on which to warn the industry of its impending doom. Most of the industry had ignored the warning signals.

In the crisis, industry, state and federal administrators, and biologists took a decisive step. Industry appointed a Sardine Industry Advisory Committee to work with the State Bureau of Marine Fisheries. Two objectives were agreed upon: first, a temporary program of legislation embodying a size limit and a limitation in the number of fishing days and, second, a long-range legislative program of conservation.

Even at this crucial stage industry failed to go all out in the gamble for survival. It did support some very important and highly constructive recommendations of the State Bureau of Marine Fisheries. The summer canning industry, which draws upon immature fish, was eliminated by industry-supported legislation. Also, the seasons were shortened by one month. Extremely important was the aid given by industry to legislative financial support of a truly great scientific research program, now operating with an annual budget totaling more than half a million dollars.

What about the present status of the population? The cannery and reduction plant catch in 1946–47 was 110,000 tons, in 1947–48 it was 160,000 tons, and in 1948–49 the total was 335,000. The last catch would have been greater but for poor market conditions. Richard Croker, chief of the California Bureau of Marine Fisheries, has stated that, following 1939, there was not a really good spawning season until 1947, although there was some spawning in 1946. The 1947 year class grew exceptionally fast. Together with the 1946 year class, it was, according to Croker, responsible for about 80 per cent of the 335,000-ton catch in 1948–49. Some real benefit has also resulted from the restrictions on the season, as well as on the operation of the reduction plants at San Pedro and Monterey, and in the San Francisco Bay area.

As I have pointed out, this major economic crisis in the largest fishery in the Western Hemisphere resulted in what is destined to be one of the greatest battles, if not the greatest battle, on record for the cause of scientific fishery management and conservation. O. E. Sette, as early as 1943, proposed an admirable cooperative research program designed to determine how fishing affects the sardine resource. I think it is worthy of emphasis that no fewer than eight groups, including industry, one federal and three state agencies, two educational institutions, and the Dominion of Canada, are pulling together toward a common purpose. We have in this instance, then, a striking example of the application of biological knowledge to an economic problem. Thousands of plant workers and boat crews, from Lower California to British Columbia, are dependent on the industry, and the nation's supply of proteins and fats is influenced by the fishery.

Knowledge of how the ecological factors of the marine environment influence the size and composition of the catch is quite as important as an understanding of how fishing intensity affects the population. At present, there are two primary biological considerations in the sardine problem. One of these is the size of the year classes, which are now known to vary enormously. According to Croker,[2] the time between

[2] I am indebted to Richard Croker, and to J. L. McHugh, of the Scripps Institution of Oceanography, for reading this manuscript and providing valuable comments.

good or poor classes varies from one year to several. That brings us to the all-important question concerning the factors that cause the success or failure of a spawning. To answer this query the extensive oceanographic and biologic investigations now in progress were initiated.

In the interest of brevity, I shall refer only to the temperature and food factors in their relation to sardine production. One can scarcely avoid mention of the temperature factor in view of the enlightening conclusions reached by Hubbs in his analysis of temperature as a determinant in the distribution of marine fishes along the Pacific Coast of North America. His analysis of the changes in fauna with changes in ocean temperatures forces a consideration of how temperature affects the sardine population. Walford and Mosher, in their study of the age of adult sardines by scales and the effect of environment on first year's growth, correlated deviations from the temperature norm with anomalies in the calculated first year's growth. They pointed out that the northwest winds prevailing along the coast of California during spring and summer produce upwelling of colder waters, which bring nutrients to the surface, thus making them available to the plankton on which sardines feed. Yearly deviations from normal wind force were found to be correlated positively with anomalies in the first year's growth of the sardine. In general, the correlations indicated that growth during the first year is favored by a sustained presence of diatoms at optimum abundance which, in turn, is favored by upwelling that brings low temperature and high surface salinity.

In 1939 a striking inconsistency occurred. There was a lack of correlation between growth, on the one hand, and diatom persistence and surface salinity, on the other, as well as between surface temperature and surface salinity. Although the surface water at La Jolla was abnormally warm in 1939, it was also abnormally saline. The diatom count was high, but growth of 1939 fish was subnormal. The explanation seems to be that oceanographic conditions were exceptional, resulting in a northward extension of environmental influences favoring spawning. In that year sardine eggs and larvae were collected as far north as the Oregon coast, where growth is slower. Thus, the northern extension of the range of the 1939 year class may account for its abnormally low first year's growth in spite of the apparently favorable food supply in Southern California.

I am reminded of a parallel instance in the Bay of Fundy during 1930, when I was studying the growth of *Mya arenaria*. Studies of the effects of temperature and diatom abundance on the growth of the soft-shelled clam had indicated that growth usually reached a standstill by November. However, in 1930, it continued into December. The explanation seemed to lie in a northward extension of oceanographic conditions that normally stopped at Cape Cod, with the result that an unseasonal diatom population prevailed throughout the fall season and into early winter, as shown by the work of Viola Davidson. Hence, it seemed reasonable to believe that the food factor accounted for the unusually late growth.

Clearly, the formation of the annual ring of the *Mya* shell and the sardine scale ring is determined by a delicate, but nonetheless real, balance. The usefulness of this biological ring in commercial fishery investigations is well known. It represents one scientific contribution that has received wide acceptance by the nonscientist in the field of administration. In fact, during the past ten years there has been copious evidence of change from the one-time aversion of administrators to biological interpretations. Respect for scientific facts has manifestly grown in recent years, and I think the fish industry has gained by the costly lesson of the sardine episode. Certainly, the contributions of the California biologists are no longer in danger of frontal assault or even skeptical indifference. Concrete, incontrovertible observations and experimental evidence are insured against repudiation, whereas theoretical abstractions fail to convince. To effect closer cooperation among industry, administration, and science, effort should be directed toward more effective organization of research facilities. Such organization is now taking place, thanks to large governmental expenditures. We find ourselves thinking in terms of complex population problems, reducing biotic community interactions to mathematical expressions; and even more encouraging is the fact that industry, in not-too-isolated instances, is using some of the very same terms. This is a sign of progress. Committee reports to the Congress are, I believe, progressively more technical and, I might say, convincing. Witness the new annual appropriation of about one million dollars for Pacific Ocean tuna investigations. This marks the beginning of a new era in fishery research, made possible because biologists are at last concerning themselves with the practical problem of production.

The time for this realistic approach is long overdue. Natural populations on land and in the sea are fast disappearing. We seek a normal population, a normal biotic community, a normal factorial pattern as a base for understanding population balances. Applied biology may furnish an explanation for years of plenty, as well as for years of scarcity. But what is plenty? Too often a year of profitable catch is considered to be a year of plenty. To the sardine industry, 1936–37 was a year of plenty. But we know now that catch per unit of fishing effort, rather than total take, is the critical index of abundance.

I have referred with some optimism to the gains registered in the field of scientific fishery management within the past decade. Perhaps the primary reason for encouragement is the progress toward a particular goal, namely, teamwork. It is working in the sardine industry. It can be made to work in other fishing industries. No greater proof of the imperative need for teamwork in scientific affairs is the wastage of our heritage in natural resources by that monstrous spectacle, soil erosion. Once scarcely associated with fish production, today its prevention is the hope of thousands of fishermen; in fact, the prevention of soil erosion could be their salvation.

In concluding these remarks, I wish to emphasize the role played today by applied ecology in sport and commercial fisheries. We meet it on every hand. The primary problems in fresh-water fisheries today concern interrelations between species, and population balance. In the ocean, interest centers on the effect environment exerts on population numbers. If, through careful study, the uncertainties can be eliminated, at least in large part, then the road is clear to measure the influence of the fishery on the maintenance of a high economic level of production.

All this investigational subject matter goes by a variety of names, but I can think of none more appropriate than *applied ecology*. The time has come to crystallize our thinking in regard to these particular categories of knowledge, to encourage a uniform terminology, and to utilize an old and well-established concept—namely, the application of science to human affairs.

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Technical Papers

Ultracentrifugal Studies of γ-Globulins Prepared by Electrophoresis-Convection

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The development in recent years of chemical and physical methods for the fractionation of human and animal blood plasma and serum has made possible the separation of certain components in a state of high purity and others in varying degrees of purity. The physical chemical characterization of the various plasma proteins is important from both a theoretical and a practical point of view. First, it yields information as to the molecular characteristics of these materials, thus giving insight into their reactions and physiological activity and an understanding of those properties that must be employed in the further development of fractionation procedures. It is also indispensable in following the fractionation and assuring uniformity of the products. Electrophoretic analysis has proved well suited as a criterion of purity, and ultracentrifuge studies have been most useful in detecting subtle changes in protein fractions both during preparation and during storage.

Of particular interest to the immunologist and immunochemist is γ-globulin, since this electrophoretic component of serum contains antibodies against a variety of pathogenic agents and other antigens. A number of ultracentrifugal studies of human γ-globulin, prepared by the low-temperature ethanol fractionation procedure, have been reported. In 1944 Williams and his co-workers (*1*) reported studies on 35 preparations of Fraction II of plasma. Electrophoretically these materials analyzed about 87%, and in some cases to 95%, γ-globulin, the main impurities being β-globulin and albumin. Ultracentrifugal studies of these preparations revealed about 5% slow-moving component with sedimentation constant of $S = 4$–5 Svedberg units, 75% normal component with $S = 7$, and 20% fast-moving component with $S = 8$–18. The slow-moving component presumably represented albumin, at least in part. The amount of fast-moving component, in large part γ-globulin, was quite uniform in nearly all preparations. Since that time Deutsch and his co-workers (*2–4*) have studied a number of γ-globulin fractions with different electrophoretic mobilities and purities of 95–99% as judged by electroproresis. In the ultracentrifuge at least 3 components with average sedimentation constants of $S = 7$, $S = 8$–12, and $S = 18$–20 are discernible. It has been found that as the mobility of these fractions increases from -0.97×10^{-5} cm² sec⁻¹ v⁻¹ to -2.63×10^{-5} at pH 8.6, the percentage of $S = 7$ component decreases from about 89 to 57%, with a corresponding increase in the heavier components. Nichol and Deutsch (*5*) have reported that 93% of one preparation of γ-globulin sedimented with $S = 7$.

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