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THE CASE FOR STUDYING NORMAL PATTERNS IN FISHERY BIOLOGY

By

LIONEL A. WALFORD

U. S. Fish and Wildlife Service

It seems a reasonable enough idea that a modern government is fulfilling one of its proper functions when it provides adequately for researches about its natural resources. Surely it is no less than a common sense¹ attitude toward our possessions, needing no justification, that we should seek to know all about them—what they are, how we can use them, how much of them we can use, and what affects them. Applied to the sea fisheries, biological research should be comprehensive enough to cover these resources as a whole, extending *all* along the Atlantic, Gulf and Pacific coasts of the United States, as well as Alaska and the island possessions. It should be a continuing program, designed to determine normal patterns of marine ecology, and to provide useful and necessary information for the distant future as well as for the present. It should be flexible enough to take care of anomalies and yet stable enough to resist being completely diverted by them when they occur.

Unfortunately, it is the anomalies, and not the humdrum norms, which attract the most public interest to provide funds for research. Thus, it seems to be generally true that such understanding as we have about our fishery populations results not so much from research pursued systematically to learn normal patterns as it does from investigations of anomalous or otherwise unsatisfactory conditions needing relief.

A species may appear to be in danger of extermination through over-fishing; or fishermen operating two types of gear may dispute as to whether one of them is unduly destructive; a population of shellfish may suddenly vanish without trace; an epidemic may suddenly break out or masses of dead fishes may wash up on the beaches, the stench of their rotting bodies driving tourists away. But whatever the occasion, it is generally the disturbing condition, preferably a disaster, that arouses people's interest. They hope the Government will solve the

¹ Webster's New International Dictionary gives a definition of this phrase that is most pertinent to the argument: "Good, sound, ordinary sense; specif., good judgment or prudence in estimating or managing affairs, especially as free from emotional bias or intellectual subtlety . . ."

problem quickly by acting, say, to stop dumping ammunition at sea, or by passing legislation providing a size limit, or by enacting a law to stop all commercial fishing for a species, or to abolish purse seining, or to abate pollution. The Government usually holds hearings over such questions, which usually bring out such diverse and conflicting opinions on the issues that it becomes necessary to gather some pertinent, objectively gathered facts before reaching a decision. Thus the Government starts an investigation of a species, or a fishery, or a particular situation.

The first problem ever taken up by the United States Commissioner of Fisheries, in 1871, is an archetypal example of this pattern. In the 1860's people had been fearing that the stocks of fishes in the sea off the coast of New England were diminishing, some of them even to the point of being in danger of extermination. They said that the country was growing, the construction of railroads and the use of ice for packing were making it easier to transport fish long distances; the use of fish for the extraction of oil increased the demand for species like menhaden far beyond anything ever before experienced; and all these things made the danger of depleting the supply all the worse. Some people urged that the simplest way to protect the fish was to abolish traps or pounds, since they were obviously such efficient and unselective gear. Others argued that the evidence against the traps was too flimsy to warrant so drastic a prohibition. Some even asserted there was no sign of diminution at all. This confusing question became the center of local political storms. First, state legislative committees fought over the question without finding an answer to please everyone. Then the United States Congress took it up and in 1871 set up the Commission of Fish and Fisheries to investigate it impartially.²

I am going to quote here what Spencer Baird, the first Commissioner, wrote in his first report on this problem, because it shows how clearly he saw the intricate interrelations that he must understand in order to answer the problem intelligently. He said,

. . . it was a matter worthy of serious inquiry whether so positive a measure as absolute prohibition was expedient or necessary, and whether by limiting the time during which the use of nets is allowed, the interests of both parties may not be reconciled, by giving to the fish the opportunity of spawning undisturbed, and also by regulating the size of the mesh, so as to catch only the oldest and largest fish. All this, however, was only to be ascertained by a careful study of the habits of the fish, so as to determine the nature of their food, the growth of their spawn, and other circumstances bearing upon the solution of the problem in question.

The plan adopted for the inquiry was determined upon after careful deliberation. The great contrariety of opinion developed in the State investigations as to what

² As passed, the resolution extended the subject of inquiry to the Great Lakes in order to investigate the diminution of white fish and other species.

should have been the best-known facts in the life-history of the fishes and their associates in the sea, made it necessary to study the natural history of these species as thoroughly as possible, . . .

As the history of the fishes themselves would not be complete without a thorough knowledge of their associates in the sea, especially such as prey upon them or in turn constitute their food, it was considered necessary to prosecute searching inquiries on these points, especially as one supposed cause of the diminution of the fishes was the alleged decrease or displacement of the objects upon which they subsist. . . .

Furthermore, it was thought likely that peculiarities in the temperature of the water at different depths, its chemical constitution, the percentage of carbonic-acid gas and of ordinary air, its currents, etc., might all bear an important part in the general sum of influences upon the fisheries; and the inquiry, therefore, ultimately resolved itself into an investigation of the chemical and physical character of the water, and of the natural history of its inhabitants, whether animal or vegetable. It was considered expedient to omit nothing, however trivial or obscure, that might tend to throw light upon the subject of inquiry, especially as without such exhaustive investigation it would be impossible to determine what were the agencies which exercised the predominant influences upon the economy of the fisheries.

What seems to me to be most interesting about this passage, written three quarters of a century ago, is not quaintness of expression, nor even the general similarity between it and what any fishery researcher might say today in planning his progream, but rather the measure it gives of the short distance we have progressed in furthering for the country as a whole the desirable aims which Baird implied must be reached to solve such problems. He started with little. There was no great literature about American fishes to guide or hinder him, no systematic meteorological or hydrographic records (except surface temperatures of the sea water in the Gulf of Maine) to associate with past biological observations, even if there had been any; and no dependable catch statistics. That these were desirable, if not necessary data, he recognized at once, and the lack of them must have exasperated him in his efforts to solve his problem objectively. But since he did not have them, he had to get along as best he could, by circulating a questionnaire and otherwise taking testimony from informed or at least opinioned people, and by making collections and surveys of the fauna and of the environment. Within a year, he submitted his recommendations.

This is the pattern which has been repeated in various localities to solve special problems during the whole history of our biological fishery research. There develops an anomalous condition (in this case diminution of the fish stocks below a level which people remembered as having once been much higher); an interested special group of people request that the condition be investigated; after due legislative procedure, scientists are assigned to the problem; to understand the cause of the undesirable condition, the scientists first try to learn

about the time when the condition was satisfactory (*i. e.*, the normal pattern), but because systematic past records are nearly always fragmentary or lacking, this effort usually proves fruitless. Then, because they are expected to devise a remedy for the condition in a reasonable time, they make deductions and recommendations from what data they can assemble. Such an investigation may not be conducive to learning much about the normal, because it is bound by too many limitations, for the anomalous condition is usually sharply delimited in scope. It is limited in time to the memory of the current generation, often even to such a short period as a season or two. It is limited ecologically to the affected species which are of most economic value.

The net effect of our preoccupation with problems of this kind is that we neither cover enough ground in our research programs or make fast enough progress toward the ideal goal set forth in the beginning of this paper. Since 1871, we have arrived this far: Of the 70 species of sea fish populations that now yield catches of two million or more pounds a year, we still know virtually nothing about more than half of them. Occasional brief studies have given us a smattering of knowledge about a third of them; and intensive, long range studies have taught us a good deal, though not all the vital things we need to know about the remainder. Furthermore, fishery studies have been very unevenly distributed geographically, so that there are long stretches of coastline which have been relatively neglected. This is too small an accomplishment for 75 years of sea fishery research in a country like the United States. What can we do to speed up the rate of progress? We can be sure that the most dramatic events, the undesirable situations, the anomalies, will continue to generate public support for special investigations and we will have to continue to conduct them. But at the same time, we must try by all means to get better support for the systematic, less spectacular studies of normal conditions, which in the long run will give us more efficiently what we need to know about the anomalous situations.

Meanwhile, there are signs of a reawakening, among biologists, in the appreciation of the cosmic nature of marine fishery problems. For example, the fishery conservation agencies of the states of Washington, Oregon and California, in a recent report to the Pacific Marine Fisheries Commission said,³ "To collect adequate information for an intelligent management of Pacific Coast fisheries, it will eventually be necessary to set up a program for the collection of data on changes in physical and chemical oceanography. Such environmental factors

³ Quoted in *Pacific Fisherman*, August 1948: 30.

have direct bearing on the availability of fish on the fishing grounds."

For its August 1948 issue, the *Pacific Fisherman* asked several biologists the question, "What does research need?" Some of the replies, anonymously quoted, were as follows:

Applied research, the study of special problems of fishery biology, needs a background of basic research—the kind that doesn't pay. We lack basic oceanography in the Pacific, where we know little of the upwellings of the deep, nutrient waters that feed the 'feed' which the fish follow, and on which they live. . . .

Research needs adequacy. As is true in many fields of endeavor, efforts of less than a certain magnitude are often ineffectual efforts.

Freedon of action for the trained intelligence. . . .

And another, succinctly expressing the view which has for so long prevailed in this country, said, "Research needs to be concentrated for best accomplishment. The dangerous situations must be attacked with all possible energy. Research should not become diffuse and superficial. World-wide skimming of the surface may allow damage to develop undetected. . . ."

This is still a matter of opinion, of course. My view is that the choice is not between handling dangerous situations and being diffuse or superficial. It is rather between studying the normal mechanism of the marine organism as a whole and studying the abnormal of the separate parts, one part after another. In a few years we will have some experience which should take this question out of the realm of dialectics, for several large-scale fishery studies, not tied to anomalous situations or to particular species, are being started. The most spectacular of these is the Pacific oceanic fisheries investigations which the Fish and Wildlife Service is beginning this year (1948) under the terms of the Farrington Act. The purpose of this work will be to explore, investigate and develop the fisheries of the territories and island possessions of the United States in the tropical and subtropical Pacific.

And in another part of the Western Hemisphere, the new program of the recently reactivated Woods Hole Laboratory is devoted to a study of the ecology of the fishery populations of the New England Banks. Here we come around full circle to the same point where Spencer Baird started his work in 1871, and to pretty much the same point of view that he had. Let us now see what progress we can make with this fresh start.