OVERFISHING OR POLLUTION? CASE HISTORY OF A CONTROVERSY ON THE GREAT LAKES

Great Lakes Fishery Commission

TECHNICAL REPORT No. 41

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OVERFISHING OR POLLUTION? CASE HISTORY OF A CONTROVERSY ON THE GREAT LAKES

by

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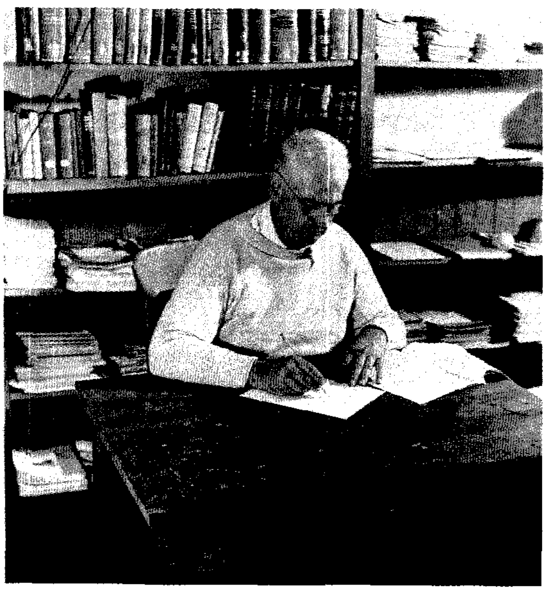
Fred Westerman, John Van Oosten, and Frank Hoard examining a smelt in Frank's ice shanty at Crystal Lake, Michigan on February 29, 1940.



Thomas (Hux) Langlois working at his desk at the Stone Laboratory at Put-In-Bay, Lake Erie (undated).

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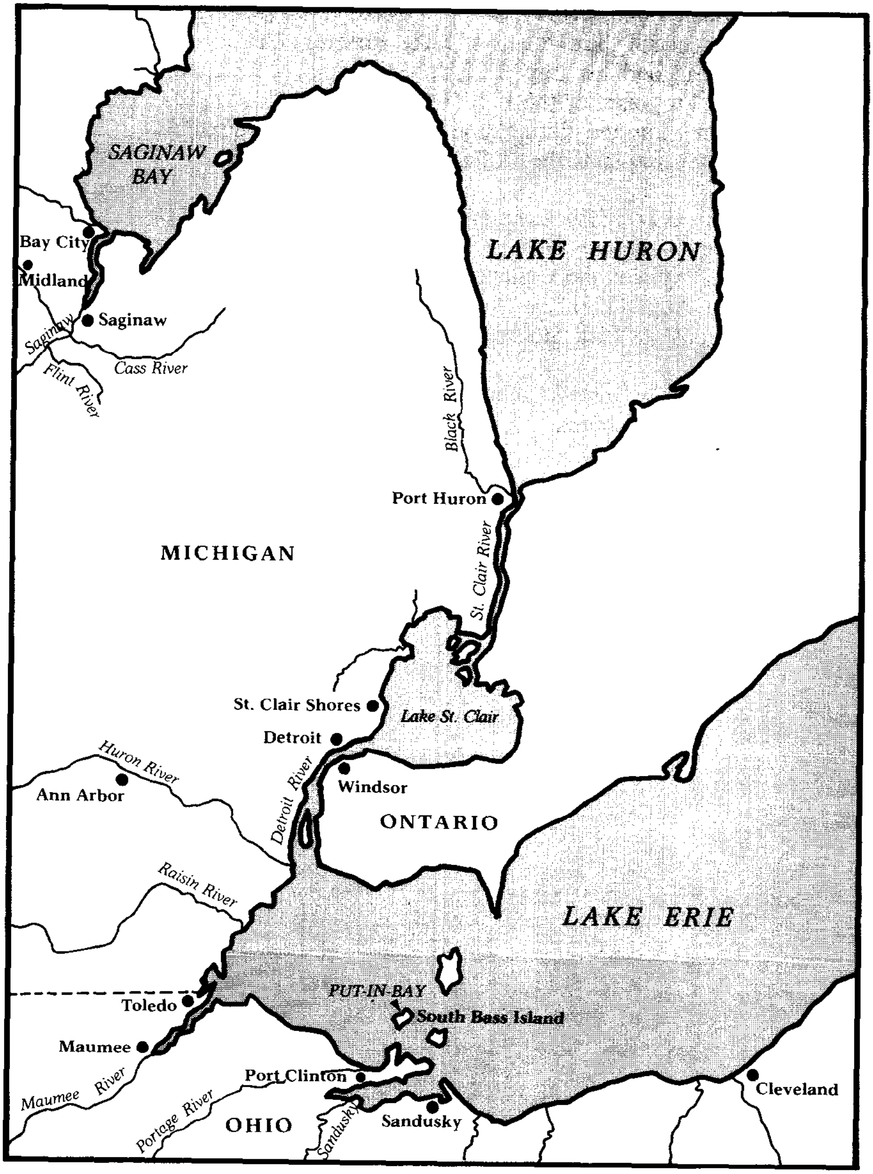

SAGINAW BAY
LAKE HURON
Bay City
Midland
Saginaw
Saginaw
Cass River
Flint River
Black River
Port Huron
St. Clair River
MICHIGAN
St. Clair Shores
Lake St. Clair
Detroit
Huron River
Ann Arbor
Windsor
Detroit River
ONTARIO
Raisin River
LAKE ERIE
PUT-IN-BAY
South Bass Island
Toledo
Maumee
Maumee River
Port Clinton
Portage River
OHIO
Sandusky
Sandusky
Cleveland

INTRODUCTION

In the 20th century society has turned increasingly to science for advice on the management of natural resources. At times this may be a simple matter, with questions limited in scope and answers easily produced. More often, however, obtaining the answers has turned out to be more complex than anyone imagined and at greater cost than anticipated. Frequently, when it realizes that the answers it desires are not already available in the scientific literature, our society, in one way or another, supports the research of scientists who attempt to provide the answers. Even so, the research sometimes takes longer than society is willing to wait before acting. It must then act on the basis of a partial understanding of the situation in question.

A good example of this dilemma is the predicament of a government that is responsible for managing a declining commercial fishery. If it is very conservative in restricting the catch, the fishermen will suffer economically and may attempt to evade what appear to them as excessive regulation. On the other hand, if it is very lax, the fishery may disappear and the government then is blamed for irresponsibility. When government turns to science for advice, it probably learns that ascertaining the answers requires lengthy research. Even if willing to pay for such research, it may want interim advice before science fully understands the situation. That predicament can be compounded when different scientists seek answers in different ways and their different experiences lead them to offer contradictory advice. Presumably, science ultimately achieves a profound enough understanding of the situation to resolve the interim paradox, but the resource in question might have seriously diminished or disappeared by then unless the interim action produced positive results. Since this kind of predicament is not likely to go away (Hartman 1980: 144-145), it seems desirable for society to increase its sophistication in handling future cases by learning from past ones.

Historians of science have only just begun to discuss such issues, and there are not yet enough cases described to provide the basis for a general understanding (Egerton 1983, 1985). Anyone familiar with modem science is bound to be impressed by its complexity, both substantively and socially. Can one gain significant insights from case histories set in a time when science was surely much simpler? Actually, one can probably gain far more insights from several simple cases than from one complex one. Two classic examples may illustrate the point: students of international relations have never ceased learning from Thucydides's history of the Peloponnesian War, nor students of the history of science from the phlogiston-oxidation debate between Priestley and Lavoisier. The Langlois-Van Oosten controversy is just such a classic case, which sheds a surprising amount of light on the difficulties of trying to bring ecological principles to bear upon practical problems of resource management.

In this case, from the 1920s into the 1950s, a relatively simple situation existed, involving mainly the research of only two laboratories. Scientists in both laboratories were interested in explaining the cause or causes of the decline of the fisheries of Lake Erie, and suggesting management policies. The laboratories involved are a Federal fisheries laboratory at Ann Arbor, Michigan, and an Ohio State University biology laboratory at Put-in-Bay. Our principals are the directors of these laboratories: John Van Oosten (1891-1966) at Ann Arbor and Thomas Huxley Langlois (1898-1968) at Put-in-Bay. They had much in common, Both were sons of immigrant fathers, both grew up in Michigan cities, both first went to small colleges for 2 years and then transferred to the University of Michigan and studied zoology. Neither seems to have been fully committed to fishery research until it became clear that that was the field where their best opportunities lay. Both were capable, energetic, conscientious, assertive, outspoken, happily married, and well-suited for their job. Both served as president of the American Fisheries Society. For several months in the fall of 1928 Langlois worked as Van Oosten's assistant. In spite of what they had in common, however, they were protagonists, defending opposing explanations for the causes of the decline in the preferred commercial fisheries of Lake Erie; Van Oosten believed the basic cause was overfishing and Langlois believed it was water pollution. Their controversy exposed a fundamental dilemma of fishery biology-the difficulty of evaluating environmental influences vs. fishing pressure upon the welfare of fish populations.

Because I primarily discuss two scientists and their respective institutions, the question might arise whether the case history given here is not an oversimplification of the real developments. What about the Canadians? They own the northern half of four of the five Great Lakes and their concern for their fisheries seems at least as great as that of the Americans for theirs. The Fisheries Research Board of Canada opened a Georgian Bay Biological Station in 1901, and its research there was of a high caliber. From its start, however, the funds available there were very modest and after 19 13 the Board felt even those modest funds would be better spent on its other stations. After closing that station, the Board had no research laboratory on the Great Lakes until after 1950. Furthermore, although Lake Erie was the most productive of these lakes, Canada lacked cities on its shore, and its fishermen could only sell a small amount of fish locally. When they carried their catch to more distant Canadian or American markets they had to compete with local fishermen who did not have the same transportation expenses. Today, transportation costs are less significant, and the Canadian fishermen on Lake Erie harvest about twice as much as the Americans, whereas earlier in this century they harvested only about half as much as the Americans.

Nevertheless, on two early occasions capable Canadian fish scientists studied the cisco in Lake Erie: Wilbert Amie Clemens studied its habits and food in the early 1920s (Clemens 1922, Clemens and Bigelow 1922), and some three decades later William B. Scott wrote his doctoral dissertation on the probable causes of the collapse of its cisco fishery (Scott 1951). Because of Canada's slight institutional commitment to research on Great Lakes fisheries during this time (Huntsman 1943, Johnstone 1977, Dymond 1964), its scientists exerted little influence on this case history. Significantly, Scott did not cite among his references the articles by his fellow Canadians, Clemens and Bigelow. Upon completing his degree, Scott went to work for the Royal Ontario Museum and the University of Toronto. Although his subsequent work encompassed the fishes of the Great Lakes (see, for example, Scott and Crossman 1973, with its fine account of the cisco: 236-243), it was not mainly focused upon them.

Although I am unaware of any historical bibliographies pertinent to this case history, there are good bibliographies on scientific studies relating to the Great Lakes, including fisheries. Most comprehensive is one compiled on cards by the Great Lakes Fishery Commission; although there are copies at several universities, it is unpublished. Among the published ones, most relevant to the present study are Hile's annotated bibliographies of publications from the Ann Arbor laboratory (Hile 1952, 1966) and Abrams and Taft's (197 1) bibliography of publications from the Put-in-Bay laboratory. Van Oosten did not limit his arguments, or the literature he cited, to Lake Erie, but Langlois refused to discuss the situation at the other Great Lakes, thus limiting the controversy to the lake he knew best. For that reason Hartman's *Effects of Exploitation, Environmental Changes, and New Species on the Fish Habitats and Resources* ***of*** *Lake Erie* (1973) provides a valuable recent assessment of the ecological history of that lake and citations to more recent literature on the subject than is discussed in the present paper. To properly evaluate the Van Oosten-Langlois controversy, one also needs some understanding of the broader picture of researches relating to the Great Lakes. For that, there are Van Oosten's own *Great Lakes Fauna, Flora and Their Environment* (1957), which includes 19th as well as 20th century studies; the Ann Arbor laboratory's mimeographed 48-page bibliography of its publications from 1927 to 1980; and two excellent topical bibliographies which are limited to Lake Erie (Herdendorf et al. 1974; Prantner et al. 1974).

THE U.S. FISHERY LABORATORY AT ANN ARBOR

Background-Before 1927

When one visits the U.S. Great Lakes Fishery Laboratory at Ann Arbor, one gets the impression of a very professional and efficient government operation. The two-story brick building is attractive and spacious. But mighty oaks grow from little acorns. The present building has housed the laboratory only since May 1966. Until then it had existed in space contributed by the University of Michigan-in the Museum of Zoology during Van Oosten's directorship, and then in a 12-room frame house on Washington Street. Like Moses, Van Oosten was never to enter the promised land; having retired in 1961, he died in January 1966, before the Laboratory's present building was completed.

The U.S. Fish Commission was established in 1871 at the initiative of Spencer Fullerton Baird, and from time to time it supported research on the Great Lakes fisheries (Allard 1978). The earliest such research of interest in the present context was that of Professor Jacob E. Reighard in the 1890s on the limnology of Lake St. Clair, which lies between Lakes Huron and Erie. The purpose of Reighard's survey was to clarify the causes of decline in the commercial catch of whitefish *(Coregonus clupeaformis).* His highly-regarded report (Reighard 1894) surveys the aquatic life of that lake. He was a professor of zoology at the University of Michigan and headed the zoology department from 1892 until his retirement in 1925. Because of the bureau's satisfaction with his survey of Lake St, Clair, he seems to have become the Commission's main advisor concerning support of research on the Great Lakes.

Although the total weight of the commercial catch of fish in all U.S. waters rose steadily throughout the first half of this century, the total weight of commercial catch in the Great Lakes peaked around 1900 and then began to decline (Baldwin et al. 1979: 186-187; U.S. Bureau of the Census 1960, Series L, cols. 112, 122). This decline did not at first lead the U.S. Fish Commission to make a permanent commitment to research on the Great Lakes fisheries because the decline in overall productivity of the Great Lakes fisheries was gradual, and the Great Lakes catch was less than 10% of the total for the country. The Commission probably believed that it got more important results from its limited research funds from research on the Atlantic fisheries than it did from Great Lakes research. (Early in this century the Pacific fisheries did not yet appear to be in trouble.) However, with the end of World War I, the situation on the Great Lakes seemed important enough for the Commission to increase its research commitment there, small though its research budget was. The fisheries of Lake Ontario had already declined seriously (Christie 1973), and those of Lake Erie seemed headed in the same direction.

R. E. Coker, head of research for the Bureau, contacted Professor Reighard and arranged to support the research of a graduate student. It was common knowledge that the first step in the management of any wild species is gaining an ability to identify the species to be managed. The great American authority on fish systematics, David Starr Jordan (1866-1936), had, alone or with associates, published repeatedly on the fishes of the Great Lakes. Yet, the salmonids (trout and salmon) and coregonines (whitefish and ciscoes), whether viewed as one family or two, remained difficult to categorize. The coregonines included some of the most commercially valuable species in the Great Lakes, and Coker hoped that the different species might be distinguished by species-specific scale patterns. Reighard assigned this problem to Walter N. Koelz (born 1895) for his doctoral dissertation research. Koelz was a graduate of Olivet College who had come to the University of Michigan in 1915 as Reighard's teaching assistant, and he obtained his M. A. in zoology and botany in 1917.

Koelz found that scale patterns are diagnostically useless for distinguishing coregonine species and that the species determinations for the Great Lakes region made by Jordan and his associates were often inadequate. Koelz collected much larger samples of fish to study than they had. His doctoral dissertation, completed in 1920, became merely a progress report in his preparation of a lengthy monograph on the "Coregonid Fishes of the Great Lakes" (Koelz 1929). Completing it did not take the entire decade, however, for he accompanied the MacMillan Expedition to the Arctic in 1925. As his coregonine project neared completion, he had the opportunity to continue this type of research into an adjacent region, and later he also published "The Coregonid Fishes of Northeast-em America" (1931). After publishing his second monograph, Koelz left the field of fisheries. He became concerned with Tibetan art, ornithology, and collecting Asiatic plants for various organizations, principally the University of Michigan and the U.S. Department of Agriculture.

However, during his decade as a fishery biologist he also found time to survey the fishing industry of the Great Lakes for the U.S. Commissioner of Fishes (Koelz 1926). In his valuable report he commented on the decline of various fisheries, such as that of the bloater *(Coregonus hoyi)* in Lake Ontario (p. 606):

> The first fishery for bloaters was carried on out of Oswego about 1875. A fisherman operating out of that port found a few individuals in the outer ends of his whitefish gangs and conceived the idea that it might be profitable to fish them. The fish were sold fresh and were so much in demand that at one time there were several boats engaged exclusively in bloater fishing out of that port. The industry gradually spread to the westward, and by 1890 bloaters were being taken out of Wilson. At first they were extremely abundant and it was never necessary in American waters to use a net of smaller mesh than 3 inches [stretched mesh], and usually the mesh employed was 3% inches, but before 1900 the bloater was commercially exterminated, and efforts to revive the industry since then have met with absolute failure. Repeated efforts to locate these fish, made by me in the summers of 1921 and 1923, failed, and not a single specimen was found, so that it appears likely that the species is extinct. No cause for its extermination suggests itself. At no time were any but the largest examples of the species taken, and so far as known it had no important vertebrate enemies.

When he went to Lake Erie, he thought that, "considering the immense quantity of netting employed in so small an area as Lake Erie, it is surprising that any fish are left" (p. 594).

Van Oosten and the Growth of Lake HerRing

With reports like these coming in, the U.S. Bureau of Fisheries believed that it could afford to support still more research on the Great Lakes. The second student whom Reighard recommended was John Van Oosten. Van Oosten had tried his hand at being a teaching and laboratory assistant to support himself and his wife while in graduate school in 1918-21, and he disliked it. The government offered him $100 a month to do his dissertation research, and he was delighted (personal communication, Lillian Sherman Van Oosten, June 1983). His dissertation topic, like Koelz's, seems esoteric from the standpoint of management, but here again the purely scientific knowledge had its well-attested practical uses. Koelz had surveyed the relations of all the coregonine species in all the Great Lakes; Van Oosten was content to study just one of them in just one lake, in his "Life History of the Lake Herring (Leucichthys *artedi* Le Sueur) of Lake Huron as Revealed by Its Scales, with a Critique of the Scale Method" (Ph.D. dissertation, 1926; Van Oosten 1929a).

The life history which he wrote on the lake herring is not what would be expected by someone familiar with the life histories published on birds and mammals, where the focus is primarily on the observed behavior and interactions of individuals or groups. One can, of course, observe Great Lakes fishes in tanks in laboratories, but it is difficult to observe them in nature except at the moment of capture. Van Oosten's goal was not to describe how lake herring behave, but merely how well they were surviving. Simple techniques of statistical sampling which might suffice for indicating the number of sunfish in a pond or the number of quail per hectare on a tract of land could not work for lake herring in Lake Huron, because it was known that few if any species of fish are uniformly distributed in such a large lake; these fish move around in undetermined paths. Therefore, no particular sample could be assumed to represent some definite proportion of the whole population of the species in the lake. However, it was possible to gain some insight into the relative abundance of a species from year to year, if the age of the individuals within a sample could be determined. Hence, Van Oosten's interest in the scales of the species.

Leeuwenhoek had first examined the microscopic structure of a fish scale (Egerton 1968: 8), and he guessed immediately that the lines which he saw were indicative of annual growth (letter of 25 July 1684; Leeuwenhoek 1685: 893-895). He had reported earlier (letter of 12 Jan 1680) that the rings visible in the cross-section of a tree represented the annual growth pattern, and he must have assumed (correctly) that he was seeing a similar growth pattern in the lines of the scales. The reliability of this method of determining fish age was apparently discussed rather infrequently in the 18th century biological literature, but more frequently during the 19th century. (Van Oosten 1929a gives a brief survey of the sources: 276-278.) The slow pace of development of knowledge on the subject seems correlated with the slow development of fishery biology. The techniques of discovery were generally available, but real progress did not occur until the 1890s, when a clear use for accurate aging techniques became evident.

This kind of knowledge first became important to zoologists concerned about monitoring the commercial fisheries of the North and Baltic seas. According to Susan Schlee (1973: 221), it was when one of them, Freidrich Heincke, realized that monitoring the age composition of the the annual catch would allow him to know whether the species were stable, increasing, or decreasing in numbers, that age determination techniques became important. He published his findings in his "Naturgeschichte der Herings" (1898), and in 1904 he emphasized the importance of his approach for monitoring fish populations before a meeting of the International Council for the Exploration of the Sea. Johan Hjort was impressed and soon followed Heincke's lead. Other zoologists soon published a spate of papers on the reliability of scale lines for determining the age of different species of fish. Van Oosten did not consult Heincke's monograph, but he was familiar with Hjort's papers. By the time Van Oosten took up the technique, one might think that he could have taken its general reliability for granted, though he would still have to ascertain if it applied to the species he studied. He took rather little for granted, however, and conducted his own lengthy researches to verify the reliability of this age-determining technique.

Having satisfied himself on its reliability, he turned to a statistical study of the lake herrings which he, Koelz, or a Mr. Kavanaugh had collected in the late 1910s and early 1920s. Hjort (19 14) had made the striking discovery in his study of the marine herring that one year class would remain dominant in the commercial catch for several years. He interpreted that situation to mean that the environmental conditions within which that year-class had hatched were unusually favorable, and that therefore an unusually large percentage of the ones that hatched had survived, and continued to be caught for several years. Van Oosten did not find a comparable situation among the lake herring samples that he had. Rather, he found (Van Oosten 1929a: 355) that most of the fish caught every year were 4-year-olds, and that the next most abundant cohort were 5-year-olds, which were about one-third as abundant as the 4-year-olds. His conclusion was it is only in their 4th year that the lake herring are large enough to be readily caught in the commercial fishing nets, and that the fishing intensity was so great that practically all of them not caught in their 4th year were caught in their 5th year. He did not believe that they were dying of old age after their 4th or 5th year, because the species only reached sexual maturity in Lake Huron in their 4th year, and some few individuals were known to have reached 11 years, and he believed they could live even longer, if not fished.

Although he went on to study the growth rates of this species in different parts of Lake Huron, and in the same part of the lake from year to year, the above conclusion, that the statistics indicated a high intensity of commercial fishing pressure, was clearly his most important conclusion. It was a conclusion for which he would continue to find additional evidence for the rest of his life. However, from the standpoint of the controversy which is described below, it is noteworthy that he also observed some effects of pollution on the growth rate of lake herring.

Pollution at Saginaw Bay

One of Van Oosten's main sites for collecting lake herring, especially for observing early growth, was Saginaw Bay. His data from the Bay indicated that "conditions relatively unfavorable to the growth of fish of the first three years [of life] apparently were present during the years 1915 to 19 18, inclusive, and conditions became favorable during the years 1919 to 1922 (1923?), inclusive" (Van Oosten 1929a: 393). Any of four plausible factors might explain the variable growth rate: "temperature, light, fishing intensity, and the chemical pollution of Saginaw Bay by the Dow Chemical Co. of Midland, Mich" (Van Oosten 1929a: 393). However, temperature and light seemed unlikely as significant variables, because he had specimens from the same years collected from elsewhere in Lake Huron, and those fish had grown at a normal rate, Furthermore, there were continuous weather records starting before the years in question and coming down to the time of his writing, and he found no unusual weather records for the years 1915-18. Differential fishing intensity between Saginaw Bay and Lake Huron was a better possibility, but there was no indication in the commercial catch statistics for the years 19 15 to 19 18 indicating that an unusual number of lake herring were harvested in either the Bay or the Lake.

There was, however, considerable evidence favoring pollution as the inhibiting factor. During World War 1 the Saginaw fishermen received many complaints about the bad odor and taste of their fish. The residents of Bay City also complained that their drinking water had acquired a bad taste. The fishermen asked a biochemist, Prof. Herbert W. Emerson, director of the Pasteur Institute at the University of Michigan, to investigate the bad odor and taste of the fish, and Bay City asked its municipal chemist, Louis P. Harrison, to investigate the bad taste of its drinking water. Both scientists traced the source of their problems to Dow Chemical Company, located at Midland, 40 miles upstream on the Saginaw River. They discovered (Van Oosten 1929a: 403)

> that the company was dumping its chemical wastes directly into the river and that the objectionable taste and odor of the fish and water were due to the presence of dichlorobenzol, a heavy, clear, oily liquid.
>
> According to Mr. Dow, the marked pollution was due to an explosion in one of his chemical plants whereby a large amount of paradichlorobenzol, a useless by-product at that time, was suddenly dumped into the river.

Van Oosten quoted from a pharmacological textbook that "the benzene hydrocarbons have a paralyzing action on the motor nerves and a more noteworthy action on the brain and [spinal] cord, causing lethargy . . . chlorobenzene acts in the same way" (May 1921: 19). Harrison found that perch (Perca flavescens) could not live in the river where Dow Chemical's concentrated effluent entered, but after dilution in the Saginaw River and Bay, apparently it only stunted the growth of fish.

As a result of these investigations, the Michigan attorney general issued an injunction against Dow Chemical Co. in April 1917. The company responded by building a settling basin for its waste. The overflow still entered the river, and Harrison found that 10 drops of it in 15 gallons [56.81] of water were enough to kill perch within 24 hours. However, since Dow Chemical Co. had military contracts, the several governments having jurisdiction were not inclined to be more restrictive toward it during the war. Furthermore, in November 1917 Herbert H. Dow claimed he was discontinuing the manufacture of chlorobenzol at Midland. The effects of the pollution were still evident in Saginaw Bay in the summer of 1918, but signs of it disappeared by the end of that year. Van Oosten happened to have excellent statistics on the Saginaw Bay fishery, and he went so far as to compute the loss to the fishermen caused by the stunting of the growth of the fish, which was 4,450,224 pounds [2,018,587 kg], worth $135,753. He did not calculate the losses to fishermen from the decline in price of their stinking fish.

The pollution threat seemed to Van Oosten to be local and temporary. Yet it remained a potential factor in the decline in abundance of fish, and it illustrated the necessity of maintaining a broad perspective concerning fluctuations of the Great Lakes fisheries. The U.S. Bureau of Fisheries sponsored a conference at Cleveland on 6 February 1928 to develop a concensus on what the important problems were and to coordinate research on them. Although the conference was concerned only with Lake Erie fisheries, the three areas of research which it identified as essential for managing the commercially important fish of that lake also are valid for the other Great Lakes-indeed, for the commercial fisheries of any lake. These are, first, “an examination of the yield of the fishery and an evaluation of the intensity of fishing for the purpose of determining the relative abundance of the fish stock of each species.” Second, “a biological study of the fish, their life history, migrations, racial segregation, food, etc.,” and, third, “limnology, a study of the chemical, physical and biological features of the environment and the ecology of the larval fishes while they are a part of the plankton” (Higgins 1928: 304).

Although these areas of research were already being developed, it undoubtedly served a useful purpose to identify them so explicitly. In the previous year the Bureau had established a permanent laboratory at Ann Arbor under Van Oosten, and the research which he supervised fell neatly into the three categories identified in the Higgins report. In 1927 he hired Stillman Wright (born 1898, B.S. Beloit Col. 1921, Ph.D. U. Wis. 1928), who had almost completed his doctoral work, and in the next year he hired Hilary J. Deason (born 1903, A.B . U. Mich. 1927, A.M. 1928, Ph.D. 1936). In 1930 the laboratory hired Ralph Hile (1904-82, A.B. Ind. Central College 1924, Ph.D. Ind. U. 1930) and Frank W. Jobes (1903-69, A.B. Southwestern Col. 1926, M.S. Kansas State Col. 1927, Ph.D. U. Mich. 1940), and in 1931 Harry A. Hanson was transferred to Ann Arbor from elsewhere within the Bureau. Van Oosten and these five were all versatile fishery biologists who could undertake research on problems of either a purely scientific or a practical nature. Hanson did not remain long with the laboratory, and, of the five, only Hile outlasted Van Oosten there. He too wrote his dissertation on the lake herring (a significant fact to which we shall return); he would become the assistant director and also president of the American Fisheries Society. Judging from its bibliography, the Ann Arbor laboratory maintained a scientific staff of about five biologists throughout the period of Van Oosten’s directorship. They were not all stationed at all times in Ann Arbor, however, and at times some of them were part-time employees, who were also graduate students at the University of Michigan.

Since the Higgins report was programatically comprehensive and since Van Oosten assigned investigators to work in all three of its suggested categories of research, there was some possibility that the conflict that developed between him and Langlois might have been avoided. That is, if the investigator whom Van Oosten assigned to study pollution (the category in which controversy arose) had come to the same conclusions as Langlois, then Van Oosten would have faced within his laboratory the conflict which Langlois forced him to face within the broader community of fishery and aquatic scientists. I shall explain why that possibility did not materialize.

The investigator whom Van Oosten assigned to study pollution, Stillman Wright, was assigned to the western end of Lake Erie as part of the Federal Government's role in a cooperative limnological survey of that lake. A number of papers from that survey were published in the *Bulletin of the Buffalo Society of Natural* Sciences (vol. 14, 1929), but Wright waited to publish until he had had a chance to do a few more years of research. Meanwhile, at the annual meeting of the American Fisheries Society in 1929 Van Oosten informed the members of the researches being pursued at the Ann Arbor laboratory. His report on the limnology of the western end of Lake Erie elicited the most comments from the audience. The findings which he offered, based on field work of the previous summer, were never to be significantly modified after he had more extensive data. First, chemical and bacteriological tests of bottom deposits showed that "contamination of the waters due to trade and domestic wastes was restricted to local areas and in no case extended far from the source of pollution." Second, chemical analysis of the water showed that, "with the exception of the polluted areas, the chemical conditions of the lake are entirely satisfactory to the normal existence of organisms." Third, the plankton survey showed that "the plants and animals that form the ultimate source of food for fishes, occurred in very great abundance" (Van Oosten 1929b: 74). Van Oosten then concluded that the decline of the Lake Erie fisheries had not been caused by pollution.

One response from his audience in 1929 is of interest here, because it represents a kind of skeptical feedback that Van Oosten would not encounter again so directly until Langlois spoke before the Sixth North American Wildlife Conference in 1941. Dr. C. M. McCay, a chemist from New York state, expressed concern that the current capability for detecting pollution in lakes might be inadequate. He thought it would take hundreds of chemists many years to properly analyze such a huge body of water as Lake Erie, and even then their methods might not be refined enough to really indicate whether the water was satisfactory for all organisms: "We measure a few things in parts per million, but we are far behind in getting parts per billion, and it is parts per billion and parts per trillion that we must ultimately measure if we are to determine accurately the effects of chemical influences upon biological phenomena" (Van Oosten 1929b: 82). Van Oosten responded that it was already possible to measure the oxygen content of water and the concentrations of bacteria, plankton, and fish, and that these four measurements seemed adequate for indicating whether there is a dangerous level of pollution.

Wright's "Summary of Limnological Investigations in Western Lake Erie in 1929 and 1930" appeared in 1933, co-authored by Wilbur M. Tidd (born 1903, A.B., Ohio State U., 1925, M.S., 1929, Ph.D., 1937), who was then working for the Ohio Division of Conservation. Their paper was the most authoritative study ever made on the western end of Lake Erie, and it was also a fine example of cooperative research between Federal and State governments. Their findings actually represented 5 years of research, but since the data from all 5 years were very similar, they only discussed 2 years's data. Although the western end of Lake Erie receives water from the Detroit River that had passed through Detroit and the Maumee River that had passed through Toledo, they found no dangerous levels of industrial pollution in the lake. The polluted area at its western end was about 100 square miles, which represented only 7.7% of the total water area there, and only about one-tenth of those 100 square miles were judged as heavily polluted. This heavy pollution came from urban sewage and from rural erosion of cropland nutrients. The main liability from this pollution was depletion of some oxygen from the polluted waters, but that seemed offset to some extent by the nutrients which the pollution contributed to the lake. They believed that the oxygen depletion could be remedied by a modest investment in sewage treatment at the urban sites.

The western end of Lake Erie was generally believed to be the worst polluted region in the Great Lakes, yet Wright and Tidd had found the situation there not nearly as bad as many had assumed, and presumably within reach of practical remedy. Most members of the American Fisheries Society to whom they presented their findings were delighted to hear that the alarms had been overstated. Only a few members wondered if they had considered all aspects of the situation as thoroughly as necessary to evaluate adequately the significance of the pollution for the fisheries. Their complete report, the publication of which was delayed by the depression, and probably by World War II, shows no significant deviation in judgement from that which they had presented to the American Fisheries Society in 1933 (Wright et al. 1955). In both the preliminary and final reports they emphasized that what they had measured was the extent of the pollution, not the impact of that pollution upon the fisheries.

The Depression and Elimination of Limnological Research

It is an important function of a laboratory director to evaluate the problems and decide how best to allocate his investigators's time. Van Oosten saw that Wright and Tidd's findings did not indicate that pollution was a likely cause for the collapse of the U.S. lake herring fishery in Lake Erie in 1925. When the Depression led to a cut in the Ann Arbor laboratory's budget from $26,420 in 1932 to $14,950 in 1933, to the all-time low of $11,010 in 1934 (Hile 1952: 3), Van Oosten had to decide which of his operations could be dropped. The research on pollution seemed less important than that in the other two areas, and furthermore, Wright and his colleagues had completed their monograph on the limnology of western Lake Erie. (That is, Wright's findings destroyed his own job. Fortunately, he obtained another job within his field of fishery biology, advising the Brazilian government on the management of Brazilian fisheries. The original 6-month assignment stretched into more than 4 years, from August 1933 to the end of December 1937.) Evidently Van Oosten never again assigned any of his full-time scientists the task of studying water quality or pollution during his tenure as director (through 1949), because the Ann Arbor laboratory's bibliography gives no evidence for such studies, and when he attacked Langlois's claim that the Lake Erie fisheries were declining from pollution (Van Oosten 1948), the only evidence cited from his own laboratory's research was Wright and Tidd's 1933 summary paper. Furthermore, Stanford H. Smith, a fishery biologist who later worked at the Ann Arbor laboratory, mentions no other limnological work done there between 1933 and 1949 in his "Limnological Surveys of the Great Lakes-Early and Recent" (1957). He explains that "these were lean years, and limnological work is very expensive unless your lab is located right on the water, as it was for Langlois, and you can use small boats with investigators to operate them. Van Oosten would have had to have had large vessels and crews, vessel bases, and extra vessel biologists. All this would have cost far more than his small budget" would have allowed (personal communication, 22 September 1983).

Since Van Oosten abandoned one of the three categories of research identified in the Higgins report as important for fisheries, it is fair to ask what research the Ann Arbor laboratory pursued which was important enough to justify this narrowing of the scope of its activities. Studies on the life histories of the fish species are essential for the ultimate goal of a scientifically-based management policy, though there is a long time-lag from the beginning of such studies until the time when a sophisticated management policy can be formulated. Van Oosten's doctoral research and resulting publication (1929a) served as a model for similar studies made on other species (e.g., Deason 1933; Hile 1936; Deason & Hile 1947; Van Oosten & Hile 1949; Smith 1956). These and other studies in fishery biology by Van Oosten and his staff seem to be of a very high caliber, and the same can undoubtedly be said for their work on other fishery matters. Another major concern at that laboratory was to eliminate the accidental destruction of young fish caught in nets intended only for adults. The states of Michigan and Wisconsin and four manufacturers of nets provided the Ann Arbor laboratory with financial and other support to study new designs and uses for nets that would minimize destruction of non-target fish, while maximizing the take of the target fish (Hile 1952: 403-5).

There appear to be no grounds for complaining about either the quality or the quantity of the research done at the Ann Arbor laboratory during Van Oosten's directorship. The only criticism that could have been expressed was whether the laboratory should have cut out limnological research altogether after Wright left for Brazil. It seems unlikely that this question would have bothered anyone for the remainder of the 1930s. Wright's own findings provided the basis for Van Oosten's decision to terminate this research, and the doubts expressed orally at the meeting of the American Fisheries Society about the definitiveness of Wright's findings had not been followed up by any published attempts to discredit them or Van Oosten's conclusions based upon them.

Van Oosten Explains the Collapse of a Fishery

What then, if not pollution, caused the decline of the Lake Erie fisheries? One must next examine Van Oosten's explanation of the collapse of the lake herring or cisco fishery from the American waters of Lake Erie, because that explanation more than anything else became the focus of Langlois's challenge. This is the same species that Van Oosten had studied on Lake Huron for several years for his dissertation and his first published monograph. He surely knew its characteristics better than anyone else. To discover why, after being one of the most important Lake Erie fisheries, the cisco disappeared from the American waters of Lake Erie in 1925, he interviewed American fishermen on that Lake and also studied their catch statistics going back to 1913. Both sources of information indicated that in 1923 the cisco became scarce at the western end of the lake and unusually abundant at the eastern end. The captain of one fishing vessel kept a daily log that recorded persistent gale-force winds in March and April, and Van Oosten deduced that the gales had so stirred up the shallow waters at the western end of the lake that the cisco living there had been driven to the deep waters near its eastern end. Many of them were caught in 1923 and those not caught remained at that end, where they were mostly caught in 1924. The wind, an uncontrollable environmental factor, had aggravated the situation, but the collapse of this fishery had been due to over-fishing (Van Oosten 1930). Although the title of the paper in which he published these findings indicated that they were preliminary, once more-as with his preliminary report of Wright's findings-he would never find reason to alter them.

The fact that the Canadian cisco fishery in Lake Erie collapsed with the U.S. fishery added support to Van Oosten's interpretation, and later when the Canadian ichthyologist William B. Scott looked into the situation, he quoted Van Oosten's conclusions without expressing any objections (Scott 1951: 21-22).

THE FRANZ THEODORE STONE LABORATORY AT PUT-IN-BAY

BACKGROUND - BEFORE 1937

Although Van Oosten was the key local figure in establishing the U.S. fisheries laboratory at Ann Arbor, Ohio State University's biology laboratory existed long before Langlois became its director. Prof. David S. Kellicott, Chairman of the Department of Zoology and Entomology, established it at Sandusky in 1896, in the State fish hatchery building. In 1903 the University laboratory constructed its own building on Sandusky Bay. The hatchery later moved to Put-in-Bay, and the Ohio State laboratory followed, in 1918. Throughout its history this laboratory has served as a base for summer teaching and research for Ohio State's biology faculty and students at the senior and graduate level. The legacy of their efforts is seen in the laboratory's bibliography of 580 scientific papers for the period 1895 to 1968 (Abrams and Taft 1971). In 1925 the laboratory was named in honor of the father of the industrialist Julius F. Stone in recognition of the latter's generous donations of property and money. In 1934 the University appointed an advisory committee to study the Laboratory's "history, accomplishments, functions, place in the University, methods of operation, personnel, scope of instruction and research, and plans for future development" (Langlois 1949: 11-12). The recommendations from the advisory committee were excellent, providing there was a significantly larger budget than the Laboratory had previously enjoyed. Julius Stone was a member of that advisory committee and also the board of trustees for the university, and he undoubtedly was prepared to assist in realizing that larger budget.

One of the recommendations was that the Laboratory hire a full-time director, and it was only when news of that prospect reached Langlois that he enters the Laboratory's history. After receiving his M. S. degree in zoology from the University of Michigan in 1925 he had become an Assistant Fishery Biologist, and later the State Fish Pathologist, for the Michigan Department of Conservation. In 1930 he moved to Ohio to become Chief of the Fish Section of that State's Division of Conservation and Natural Resources. In that capacity he was in charge of the fish hatchery that had long been a neighbor of Ohio State University's Stone Laboratory. He had also decided to earn a doctorate degree at Ohio State, using some research from his job as a dissertation topic. (His dissertation was *A Study of the Small-Mouth Bass,* Micropterus dolomieu *(Lacepede) in Rearing Ponds in Ohio.)* He received his Ph.D. in June 1935 and learned in October that the Stone Laboratory would hire a full-time director. He applied, intending to retain his position with the Division of Conservation and Natural Resources while serving as director of Stone Laboratory. However, the search committee offered the directorship to its entomologist, Professor Dwight M. DeLong (born 1892) and only offered Langlois the assistant directorship. He accepted, beginning in the fall of 1936, and on 25 February 1938 he replaced DeLong as Director. Langlois was to hold the directorship until 1956 and also his position with the Division of Conservation and Natural Resources until 1946.

His laboratory was not of the same sort as Van Oosten's. The Advisory Committee had recommended that the Stone Laboratory work more closely with the State Fish Hatchery than it had before, and with Langlois as its Director, that was bound to happen. However, the faculty members from Ohio State who came up to the Laboratory each summer to teach and do research frequently had research commitments to their academic disciplines. They or their students might be receptive to research suggestions from Langlois, but he lacked the comparable power to direct their researches as Van Oosten could with his staff. That situation would have been disadvantageous had Langlois aspired to compete directly with Van Oosten, but he did not. As it turns out, Langlois was able to fashion an asset for himself out of the situation at the Stone Laboratory. Although the researches conducted there were academically diffuse, they were geographically focused on the western end of Lake Erie. Perhaps a majority of the researches conducted there-before, during, and after Langlois's directorship-have some relevance for fishery ecology, and he was wise enough to realize that as the director of such a laboratory, and of a fish hatchery, he should focus his attention upon making that relevance explicit.

LANGLOIS EXPLAINS THE COLLAPSE OF A FISHERY

The earliest evidence I have found for Langlois's new perspective is his paper on "Two Processes Operating for the Reduction in Abundance or Elimination of Fish Species from Certain Types of Water Areas" (Langlois 1942), which he read before the Sixth North American Wildlife Conference in 1941. It begins with a summary of Milton B. Trautman's "The Effects of Man-Made Modifications on the Fish Fauna in Lost and Gordon Creeks, Ohio, Between 1887-1938" (Trautman 1939). In that pioneering paper Trautman had correlated human modification of the environment with changes in abundance and variety of fishes found in the streams. Langlois realized that what was true for two creeks emptying into the Maumee River Basin was probably also true for the western end of Lake Erie into which the Maumee River Basin empties. The decline in the Lake Erie fisheries should be attributed, therefore, not to overfishing, as Van Oosten claimed, but to environmental degradation of the breeding grounds.

When one examines this opening salvo in the controversy it is disconcerting to find that nowhere in his paper did Langlois clearly state what the two processes are that he alluded to in the title. It is like trying to find Newton's law of universal gravitation in his *Principia Mathematics,* though it is a much shorter search through Langlois's paper. The ambiguity arises in that his only listing of processes contains four, not two: seasonal fluctuations in the lake level, changes in the texture of the bottom, changes in vegetation, and decrease in oxygen (Langlois 1942: 194-195). One's confusion here is increased by the realization that he apparently considered suspended particles as important as bottom siltation, but suspended particles are not included on his list. The two processes which he thought he was discussing probably are siltation (suspended and deposited) and oxygen depletion, both of which he attributed to pollution, and primarily to erosion from farm land.

An important point Langlois made in this paper is that the collapse of the cisco fishery and the decline of some of the others in Lake Erie had not been followed by a decline in the absolute weight of fish removed annually from the lake. However, the species caught by 1940 tended to be those Langlois identified as having the greatest tolerance of pollution. In support of his arguments he did not cite any contributions from the Stone Laboratory; perhaps he had not yet read much of the earlier literature produced there. He did, however, cite two scientific reports from the Federal laboratory at Ann Arbor, one being Van Oosten's published paper on "The Age and growth of the Lake Erie Sheepshead, *Aplodinotus grunniens* Rafinesque" (1938), in which Van Oosten found (p. 660) that fishing was not depleting this species. The other was Hilary Deason's unpublished "Morphometric and Life History Studies of the Pike-perches *(Stizostedion)* of Lake Erie" (1936) which also indicated that the sauger, walleye, and blue pike-perch were not being depleted (p. 89). (Deason had published a "Preliminary Report on the Growth Rate, Dominance, and Maturity of the Pike-perches *(Stizostedion)* of Lake Erie" [Deason 1933], but the full report was never published. It is unlikely that Van Oosten suppressed it because Deason's conclusions were more useful to Langlois than to himself, since the delay in publication occurred before Langlois challenged Van Oosten's conclusions in 1941. The reason was probably a shortage of funds during the Depression. However, if Deason's conclusions had seemed more useful for Van Oosten's case, perhaps the latter would have found funds to publish it later.) On the other hand, Langlois was selective in what he cited from the Federal laboratory; he made no mention of Wright and Tidd's (1933) "Summary of Limnological Investigations in Western Lake Erie in 1929 and 1930," which discounted pollution as a serious threat to the fisheries.

Aside from data gleaned from the Federal laboratory, what evidence did Langlois have to back up his claim that the Lake Erie fisheries were suffering from pollution? Surprisingly little, and what there was could only be judged as suggestive. He cited Victor E. Shelford's studies on the succession of fish species in lakes near Chicago (Shelford 1911, 1913). Two pieces of Langlois's own evidence will indicate the extent and value of it. First, the relation of water quality to breeding success (Langlois 1942: 191):

> The specific factor that may be held responsible for changing Lake Erie from a suitable place for the cisco, whitefish, and perch [Perca flavescens] is the increased turbidity of the waters in the western part of the lake. The ciscoes and whitefishes spawned over the clean hard bottom around the islands, and these bottoms are no longer clean. The average of 40 parts per million of suspended matter in the water there has been found to change quickly to more than 200 parts per million with a strong wind.

Second, the shift in abundance from species of low pollution tolerance to species of high pollution tolerance (Langlois 1942: 192):

> The fishermen around the island know that they catch pickerel [Stizostedion vitreum vitreum] only in the relatively clear water, and when a southwest blow stirs up the bottom the pickerel move out. They also know that saugers [S. canadense] show up in their nets when the roily water comes, and we have recently learned that young saugers thrive best in roily water. Three years after a spring when roily water occurs in the shallows during the sauger spawning period, the saugers reach legal length and the catch is heavy.

As time goes by, the quality and quantity of evidence which science finds acceptable tends to improve. Today, no fishery biologist would think of publishing a scientific paper based upon this kind of evidence. Langlois must, therefore, be judged here by 1941 standards.

RESEARCH PROGRAM FOR THE STONE LABORATORY

The quality of evidence in Langlois's paper does not compare favorably with that found in the scientific papers coming out of the Federal laboratory at Ann Arbor. Nevertheless, there are different categories of scientific papers, and Langlois's was obviously no monograph of the sort that Van Oosten and his staff were producing, and no fishery biologist would mistake it for one. It was simply a programmatic challenge, and neither Langlois nor his audience would possibly have imagined that it could be the last word on the subject. He acknolwedged as much in these statements (Langlois 1942: 192):

> The staff members of the Stone Laboratory have concluded that they need to begin with the fundamentals. Studies are being conducted on the influence of sunlight and wind upon the production of primary food organisms in the lake. Sunlight must penetrate the water of the lake or there will be no primary plant crops and secondary crops of fish food organisms, and sunlight cannot penetrate waters thick with silt.
>
> Investigations of this type, together with studies of the interrelationships of all fish species in the lake, particularly with such predator-prey relationships as that of the blue pike [S. *vitreum glaucum]* and lake shiner *[Notropis atherinoides* acutus], must supply the basis for an effective action program.

Nor were these statements merely pious hopes. Kenneth H. Doan (born 1915 in Toronto, B.A., Univ. Toronto, 1937, M.A., 1938) was just finishing his doctoral dissertation under Langlois's direction on *Some Meteorological and Limnological Conditions as Factors in the Abundance of Certain Fishes in Lake*

Erie (Doan 1942). In the coming years, however, as in the past, the investigators from Stone Laboratory would produce far more limnological than fishery biology studies, just as the Federal laboratory at Ann Arbor would continue to produce far more fishery biology than limnological studies.

IMPLICATIONS FOR FISH STOCKING PROGRAMS

Langlois was still Chief of the Fish Section of the Ohio Division of Conservation and Natural Resources, and policy decisions had to be made every 1 or 2 years. In 1942 Ohio spent $10,793 from its commercial fishing license revenues on fish stocking and the remaining $12,396 on enforcement of the fishing regulations. Yet, Hile, at the federal laboratory in Ann Arbor, had reported to the American Fisheries Society in 1936 that the stocking of yellow pike-perch in lakes Huron and Michigan had had no demonstrable value (Hile 1937). Hile's conclusion reinforced Langlois's belief that fishery abundance was a function of environmental quality more than anything else. Therefore, in 1943 Langlois printed and privately distributed a five-page memorandum, "Methods of Maintaining the Fish Population of Lake Erie at a High Level of Abundance," in which he urged the abandonment of fish stocking and the enforcement of catch restrictions, considering them a waste of money. He imagined that it was impossible to overfish, referring to well-known data that one female cisco produces over 30,000 eggs per year, one female whitefish over 34,000, one female carp *(Cyprinus carpio)* over 1½ million, and so on. (Leeuwenhoek had first estimated the numbers of eggs which several species lay; Egerton 1968: 5-6; Langlois probably took his figure from Stone 1937 or Brown and Moffett 1942.) Langlois thought it unnecessary to stock fish because the eggs laid naturally are abundant enough to take care of the fishing needs providing the environment permits the new generation to grow and survive. He proposed to use the money instead for habitat improvement of the rivers and streams emptying into Lake Erie.

THE LANGLOIS - VAN OOSTEN CONTROVERSY

Langlois's (1942) "Two Processes . . . " paper jolted Van Oosten. His evaluation of minor details had been challenged from time to time; every scientist expects and accepts that. However, he had become the leading authority on the Great Lakes fisheries, and no scientist had ever presumed to challenge his fundamental perspective. It is not clear whether Van Oosten had heard Langlois's oral presentation or was yet unaware of the article when they met in Madison on 10 March 1942 at a hearing of the Wisconsin Conservation Department. The hearing was to help the Conservation Commission decide on **new fishing regulations to cope with the declining chub fishery in Lake** Michigan. Van Oosten was clearly the star witness, and his testimony elaborated upon the perspective which he had been developing over the past 15 or more years. When Langlois spoke he was brief and even offered faint praise for what Van Oosten had said. He was not going to take this forum to press the challenge which he had delivered only a few months earlier. He knew little about Lake Michigan's fisheries and said so, and only offered his new perspective in a very qualified and low-keyed manner (Wis. Conservation Dept. 1942).

Van Oosten's Rebuttal

Sometime later (probably still in 1942) Van Oosten wrote, but never published, a rebuttal to Langlois's "Two Processes . . . " paper, entitled: "The Factors of Abundance of the Fishes of Lake Erie" (12 pp.). It is interesting as being his earliest response and also for being comprehensive. Probably it remained unpublished because he decided that it covered too much ground too briefly to be effective. Eventually he decided to concentrate on the single factor he thought most important in Langlois's "Two Processes . " paper: turbidity in Lake Erie. This was a good choice; not only was it the most important issue, but it also was where Langlois did little more than extend Trautman's arguments from the two creeks to the lake. Van Oosten could cite extensive data supporting his belief that siltation was neither widespread nor a severe problem for the fish in the western end of Lake Erie (Van Oosten 1948: 284-298). Most of the data upon which Van Oosten drew had been published before 1941, and were therefore accessible to Langlois when he wrote his paper, though Langlois had not cited any of it.

Meanwhile, the Stone Laboratory had been busy collecting its own limnological data. In charge of this work was David C. Chandler (born 1906 in Minnesota; B.A. Greenville Col., Ill. 1929; M.A. 1930 and Ph.D. 1934 Univ. Mich). He joined the faculty of Ohio State University and Stone Laboratory in 1938, about when Langlois was promoted to Director of the laboratory. Chandler developed his new research plans in consultation with Langlois, and his five research papers which he published in as many years (1940, 1942a, 1942b, 1944, 1945 [the last coauthored by Owen B. Weeks]) would be cited by Langlois to support the claims of his "Two Processes . . . " paper. Van Oosten, however, was not convinced that Chandler's findings added significantly to Langlois's case. Chandler, like Wright and Tidd (1933), could not relate his data directly to changes in the population of different species of fish. Nor would Van Oosten concede Chandler's assumption that the silt suspended in the water and settling to the bottom around South Bass Island was from the Huron, Raisin, Maumee, Portage, and Sandusky rivers (Van Oosten 1948: 299-301). These five rivers contributed only 2% or 3% of the water entering the western end of Lake Erie. The other 97% or 98% came from the Detroit River, which carried very little sediments.

But if the sediments from the five rivers did not travel far into the lake (as Wright and Tidd [1933] had claimed), where did the silt around South Bass Island originate? And why did it matter? "Unquestionably wave action induced by wind is the dominant agency in erosion on Lake Erie as well as on the other Great Lakes. It is common knowledge that the shore lines of the Great Lakes have been and still are subjected to severe erosion by wave action and other agencies" (Van Oosten 1948: 303). The reason the source of the silt was important is that if it came from the five rivers, then one could reasonably assume, with Langlois, that its tonnage had increased steadily during the 20th century as the watersheds of those rivers were steadily stripped of natural vegetation and cultivated. Since the western end of the lake was the most important breeding ground in Lake Erie, the increase of the silt would be inversely proportionate to the decrease of the fish species requiring the purest waters for reproduction and growth of fingerlings. On the other hand, if Van Oosten were right in interpreting his data, and the silt came from the shore all around the lake, then silting was fairly constant and fishing pressure would seem to be the most plausible cause of the decline in the desirable fisheries.

In addition to his arguments based directly upon scientific data, Van Oosten had two deductive arguments. First, although Lake Erie was the most turbid of the Great Lakes, it was also the most productive for commercial fisheries, and that its turbid western end still supported the greatest mix of species in the lake. Second, the chub and ciscowet trout *(Salvelinus namaycush siscowet)* fisheries in Lakes Huron and Michigan were in decline; yet, since these species live at depths of 90 and more meters, where the environment was extremely stable, no one could imagine that silt was inhibiting their reproduction and growth. Since both of these arguments seem important, one wonders why he did not emphasize them more than he did. Probably in part because, although both points tend to discredit Langlois's arguments, neither explained the decline in the commercial fisheries of Lake Erie. Also, Van Oosten (1935) had published a paper entitled: "Logically Justified Deductions Concerning the Great Lakes Fisheries Exploded by Scientific Research." He knew that, although logic might guide research, it is not a safe substitute for scientific research.

Which one was right may have been important for their egos and reputations, but more was at stake than that. Van Oosten expressed his perception of the stakes in his turbidity paper (Van Oosten 1948: 283): "The Langlois turbidity theory, if true, would carry with it the most sweeping implications. Specifically, it would mean that all efforts at the rehabilitation and conservation of our declining Great Lakes fisheries through the improved control of the intensity, time, and methods of fishing or by other management procedures are foredoomed to failure and might as well be abandoned." Langlois would have agreed that his theory carried the most sweeping implications, but would have disagreed that those implications foredoomed management. He had a rehabilitation and conservation program in mind, and he explained them in papers which he read at the 9th and 10th North American Wildlife Conferences (Langlois 1944, 1945). Basically, his program would regulate the farmers and loggers who damaged the watersheds of rivers rather than the fishermen whom Van Oosten wanted to regulate.

Persistent Debate

It is not unusual for scientific controversies to lead to acrimonious exchanges in speeches or in print. In this case, however, neither Van Oosten nor Langlois let that happen. Van Oosten explained the reason for his lengthy rebuttal of Langlois's claims: "I feel that a thorough discussion of the entire subject in a single paper is desirable, since involvement in an extended controversy would serve the interests neither of science nor conservation" (Van Oosten 1948: 284). Nevertheless, Langlois felt that Van Oosten had published a polemic, and he said so in his unpublished "Rebuttal to Van Oosten's Attack on the 'Langlois Turbidity Theory' " (1948?). Langlois also thought, however, that controversy is counterproductive, and he therefore announced to his colleagues that, rather than engage in one, he and they would "continue to publish our evidence as fast as we get it in shape . . . "

One may suspect that in this quoted phrase Langlois intended to put more emphasis on "fast" than on "shape." In 1946 he published a 3½-page essay, "The Herring Fishery of Lake Erie," in *Inland* Seas, a journal devoted to publicizing technical and scientific findings about the Great Lakes. It was hardly the place to publish new evidence in his controversy with Van Oosten. The reason Langlois chose to do so was probably because the new evidence came from Chandler's research, not his own, and because it could only be indirectly related to the controversy. Chandler had found that the abundance of phytoplankton during the spring at Put-in-Bay varied considerably from year to year, depending on the clearness of the water-which depended, in turn, upon whether sediments were kept suspended by turbulent waters or were allowed to precipitate by quiet waters. Since ciscoes eat plankton all their lives, their abundance must be determined by the abundance of the plankton. In this essay, sediments are detrimental to the cisco not just when spawning, but also for affecting its food supply (Langlois 1946).

Two years later, in his unpublished rebuttal to Van Oosten's turbidity paper, he discussed eight points, seven of which argued that Van Oosten had either neglected or minimized aspects of the pollution argument. His 4th point, however, was a partial concession to Van Oosten which seems to indicate that someone whom Langlois respected (Chandler?) had pointed out to him that tying cisco abundance to phytoplankton abundance, as he had done in 1946, was a bit risky (Langlois 1948?):

> The weak spot in our evidence supporting the turbidity theory which Van Oosten put his finger on is the fact that our data cover principally the phytoplankton while the young of most species of fish feed on zooplankton. We have assumed that the dependence of the highest forms on lesser forms (such as the dependence of the zooplankters on the phytoplankters) was such an obvious step in the energy cycle that the interruptions of the phytoplankton step was good evidence for interference of the whole energy cycle. Studies of such invertebrate animals as Bryozoa, Porifera, Annelids, Rotifera, and certain Insect larvae, Crustaceans, Molluscs, and Worms have shown that these forms are abundant in or near aquatic vegetation, and scarce in waters lacking vegetation. **A** few kinds of turbidity-tolerant forms of bottom organisms are still present in abundance while many organisms have been eliminated or reduced to negligible numbers in the western end of Lake Erie during the last half century.

The public debate was free of acrimony, but it did continue as long as Van Oosten and Langlois lived. One can find some reference to it in a number of Van Oosten's subsequent publications, but two of them (Van Oosten 1949a, b) addressed the issue most directly. In "A Definition of Depletion of Fish Stocks" he discussed 12 situations which could be confused with depletion. These included unsatisfactory fishing methods and the reduction of stock by such factors as "pollution, dams, dredging, silting, storms, 'winterkill,' epidemics, predators, competitors . . . " (1949a: 286). He then defined depletion as "a reduction, through overfishing, in the level of abundance of the exploitable segment of a stock that prevents the realization of the maximum productive capacity. Overfishing is a fishing rate that produces depletion; it is the only causative factor in depletion" (1949a: 288). The point of the paper is to reduce any controversy arising from conceptual or linguistic confusion.

In "The Present Status of the United States Commercial Fisheries of the Great Lakes" he discussed the overall decline of the preferred fisheries in the Great Lakes and some decline in the total tonnage taken per year. Although his point of view was clear, his general conclusions certainly left the door open for further improvement in knowledge (1949b: 329):

> Much of the reduced abundance in modem fishery must be attributed to overfishing or unwise fishing (cisco, whitefish, lake trout, chubs). Part of it we believe was caused by an infectious disease as was true for the smelt; part of it by the parasitic predator, the sea lamprey. Perhaps increased competition for space or food such as might have been brought about by the smelt in Lakes Huron and Michigan or the alewives *[Alosa pseudoharengus]* in Lake Ontario may have played a role. Pollution, too, may have taken its toll. Often we have no better explanation to offer than to state that some unknown change in the environment was responsible.

After delivering this paper before the 14th North American Wildlife Conference he had to admit, in answering a question, that he was unable to evaluate the seriousness of the pollution of Lake Erie going on at Buffalo (1949b: 329).

In 1949 Van Oosten was promoted to Senior Scientist, which meant relinquishing the directorship of the laboratory. In the 21 years of his directorship the laboratory's budget only rose from $15,180 in 1928 to $31,581 in 1949. That was a rate of growth that Van Oosten could comfortably manage. However, in 1950 the laboratory's budget jumped to $286,554 (Hile 1952: 3), and his superiors wanted a younger director to handle the larger operation (Stanford H. Smith, personal communication 31 May 83). The stimulus for the increased budget was not a tardy recognition that the Federal laboratory had neglected limnology far too long, but rather a response to the disaster which the sea lamprey was creating in Lakes Huron, Michigan, and Superior. However, Federal limnological research would benefit from the larger budget, because a research vessel was built with some of the funds, and it was used in part for limnological researches (Stanford H. Smith, personal communication 22 September 83).

Did Van Oosten feel bitter at losing the directorship just when funds were again becoming available for limnological research for the first time since 1933? Apparently not. Mrs. Van Oosten remembers that he was glad to relinquish the directorship because he was ready to return to research which had long remained unfinished, in order to prepare his findings for publication (personal communication, 2 June 83). Although this explanation could have been an excuse told to hide disappointment from his wife, there is reason to consider her perception as accurate. Any bitterness he may have felt did not last long, because he left his large personal library to the U.S. Great Lakes Fishery Laboratory at Ann Arbor, which is not the act of a bitter man. (In appreciation, the Laboratory named its library after Van Oosten, with a wall placque summarizing his career and explaining his gift.)

Langlois also continued to champion his perspective in various writings, culminating in his monograph, *The Western End of Lake Erie and Its Ecology* (Langlois 1954). Since his case for pollution as the cause of the decline in the preferred fisheries of Lake Erie was an environmental issue, he could only claim to have proven his point if he could give an adequate account of the entire aquatic ecology of the western end of Lake Erie. Only then could he argue that he had evaluated all the likely factors involved in the decline. The challenge of producing a reasonably complete ecological account of the western end of the lake was formidable, but he had never lost his general interest in nature during his years as a fishery biologist, and once he had settled in at Put-in-Bay in 1936, he began to redevelop those interests. Although one might argue that Francois Alphonse Forel had done even more than Langlois in his great *Le Léman: Monographie limnologique (*3 vols., 1892-1904), Lake Erie is considerably larger than Lake Geneva, and Forel’s goal of a complete understanding of his lake would have been impractical for Lake Erie. Langlois was not as thorough as Forel, but in many respects his book compares well with that portion of Forel’s of similar scope (Egerton 1962).

The account of the decline of the cisco fishery in this book was to be Langlois’s most definitive. Not surprisingly, he had found no reason to yield ground to Van Oosten. Unfortunately, he apparently was unaware of Scott’s (195 1) “Fluctuations in Abundance of the Lake Erie Cisco *(Leucichthys artedi)* Population," which had been published by the Royal Ontario Museum of Zoology. Scott’s evaluation seems favorable to Van Oosten’s interpretation, but then Scott seemed unaware of Langlois’s counter-arguments. (Their ignorance of each other’s published research points to the need for Van Oosten’s (1957) *Great Lakes: Fauna, Flora and Their Environment: A Bibliography,* which lists both their publications,) In this book Langlois repeated his sediments-and-plankton argument within a scientific publication (Langlois 1954: 271-273), with some new details gleaned from the findings of other biologists since he had published his *Inland Seas* essay (1946). (He referred back to that essay as providing further details, a tactic that might reassure a casual reader but not a skeptical one.)

The new data included an explanation for why the sediments suspended in the western end of Lake Erie varied so widely from year to year: it appeared to be correlated with the degree of flooding of those rivers in the spring (Langlois 1954: 294). Another new point came from Chandler’s study of year-round climatic and other environmental data. Chandler “had found so much difference in the physical, chemical, and biological characters of western Lake Erie *from year to year, during the winter and spring months,* and with summer conditions reflecting the conditions earlier in each year, that he considered *summer data only* practically without value” (p. 292). Langlois added emphasis here because he felt that Van Oosten had relied upon only summer data in his explanation of the collapse of the cisco fishery. Finally, Langlois cited in support of his views Hile’s doctoral dissertation, which compared the ciscoes in four northern Wisconsin lakes. Hile (1936) had found that the ciscoes flourished best in the most oligotrophic lake and least in the most eutrophic lake, which indicated to Langlois that a progressive eutrophication of western Lake Erie by agricultural erosion could only worsen the environment there for the cisco (pp. 299-301).

LESSONS FROM HISTORY

PURE VS. APPLIED SCIENCE

Karl Popper has argued that science progresses by conjecturing hypotheses that may be subsequently refuted; and that no hypothesis can be completely established as right, but that defective hypotheses may be falsified (Popper 1962). This is an abstract model which we may apply to particular historical examples to see how well it fits reality. If applied to examples from pure science, we find that the time lapse between conjecture and refutation is widely variable in different instances. In some, refutation comes within a few days or weeks or months; in others, the conjecture may have occurred in antiquity, but the refutation came only in modern times (as with the aether theory). In pure science the time lag between conjecture and refutation is usually inconsequential, because one presumes that the phenomenon in question is constant and can be observed at any time later just as it was at the time of the original conjecture. This last assumption is considerably less likely to be true in applied science.

If, as in the present case history, we ask why the cisco fishery of Lake Erie collapsed in 1925, we cannot now go there and expect to find the cisco under the same conditions as it was then. It is not there in substantial numbers, and even if it were, it would no longer be possible to reproduce the same fishing pressure and pollution load which the ciscoes encountered in 1925. The best we could do would be to do research on the sensitivity of this species to various kinds of pollution and also to various rates of removal from some body of water. For example, Swenson and Matson (1976) studied the "Influence of Turbidity on Survival, Growth, and Distribution of Larval Lake Herring. " They found only a slight effect, but Langlois, had he had the chance, would have retorted that they did not study the full range of factors which he thought were relevant. We can never know for sure whether Van Oosten or Langlois was right, or, which was more right than the other. It seems unlikely that one was entirely right and the other entirely wrong (Regier et al. 1969: 42). As so often happens in scientific controversies, both scientists had a portion of the truth. If not, the mistaken contender would likely have been rather quickly discredited by the other.

Van Oosten was not so dogmatic as to say that pollution was never a serious threat to a fishery. He had found pollution inhibiting cisco growth in Saginaw Bay during World War I, and in 1949 he was again prepared to admit that pollution could be a factor there in the decline of the fisheries (1949b: 325, 329). In his opinion, it was not a question of whether pollution might ever be a factor, but determining when it, rather than over-fishing, was the serious threat. Langlois may be accused of being more dogmatic than Van Oosten, as one would expect from the challenger of an accepted explanation, but his position was not absolute either. His most dogmatic statement is this: "The cry of overfishing is a bugaboo which is seldom or very rarely justifiable. Underfishing is a much more common cause of dissatisfaction, and a very difficult one to correct" (Langlois 1944: 198). Since Langlois seems to have stuck his neck out further than did Van Oosten, it is good to know that he also eventually pulled it back a notch. One of his last students, James T. Addis, reports (personal communication, 17 August

83) that a few years after Van Oosten had died Langlois was willing to admit that there was some merit to Van Oosten's claims, though he never admitted the merit equal to his own.

The value of a scientific controversy is not the entertainment which it might afford to spectators, but the more precise knowledge which it should stimulate the participants to ascertain. Are we now the beneficiaries of a more precise understanding of the cause of decline of fisheries as a result of this controversy? To some extent, yes, though the controversy did not proceed as one might imagine, and the outcome was correspondingly less clear-cut than one would like. Van Oosten published his rebuttals to Langlois in three papers in 1948 and 1949, discussed above, and then he began publishing on other aspects of Great Lakes fishery biology and limnology. Langlois continued on course for a few years longer than did Van Oosten, until he had published his book, *The Western End of Lake Erie and Its Ecology* (1954), which contains his best evidence in support of his claims. However, it neither refuted Van Oosten's hypotheses nor clearly established his own as stronger. He then turned to a variety of other subjects for the rest of his scientific career. The fact that neither biologist devoted his remaining years to destroying the other's arguments shows that neither was fanatically aroused by the conflict. It probably also shows that each realized that there was no simple way to demonstrate who was right and who was wrong.

If they could not resolve this controversy, did they inspire others to do so? Van Oosten's colleagues and successors at the Ann Arbor laboratory believed that they had gone too long without limnological investigations and were therefore unable to defend Van Oosten's position, since they could not adequately evaluate the pollution arguments. Hile even suspected that Langlois might have had the stronger argument, though he never said so in print (personal communication Stanford H. Smith 31 May 1983). Langlois's successors at the Stone Laboratory did indeed continue to research the limnological problems which Langlois encouraged, but these were about the same problems that the laboratory had always studied anyway. There might be a stronger concern now for the relevance of their findings for fishery biology than there was before Langlois became director, but it would be difficult to say that this stronger concern would not have developed if Langlois had never been there.

The failure of Van Oosten, Langlois, and their associates to resolve this controversy probably was not due to the subsequent research policies which these laboratories followed. One might imagine that the appearance of the sea-lamprey in the upper three Great Lakes was a stroke of bad luck that prevented science from following its normal course, but to do so would be to confuse the "normal course" of applied science with that of pure science. The understanding of a topic in pure science can remain static for any length of time, and then be picked up again. In applied science, it is probably unusual for research on any subject to proceed along for decades without being disrupted by some changes in either the natural or the social world or both; if not a war or economic crisis, then perhaps a climatic change or an invasion of an exotic species. But even if the situation under study does remain static, it may be impractical to achieve the desired precision in knowledge. Fishery biologists now know that either overfishing or pollution, alone or together, can depreciate a fishery. Yet, as useful as a precise determination of these factors would be, it is not always practical to collect enough data to achieve it. Consequently, the Langlois-Van Oosten debate has continued, with different participants and different fisheries in question, from the 1940s down to the present (James F. Kitchell, personal communication).

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