SEACHANGE:
AN ENVIRONMENTAL HISTORY OF THE PACIFIC HALIBUT FISHERY
1878 – 1960

by

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Abstract

Seachange is a study of shifting emphases in Pacific halibut conservation policy, and a story about the changing relationship between people and nature. North American fishery conservation policy cut its teeth on the Pacific halibut fishery. It has been cooperatively managed by Canada and the United States since 1923, and has been at the center of what may prove to be two of the most important debates over fishery policy in the last century. What follows, however, is less a study of specific policies than it is a study of shifting *emphases* in North American fishery management. Although people have been fishing halibut for hundreds of years, the idea that their activities need to be managed is relatively new. But the idea of "management" begs two further questions: *managed how and to what end?* These are the questions to which *Seachange* seeks answers.

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Prologue: People, Policy and Nature

Seachange is a study of shifting emphases in Pacific halibut conservation policy, and a story about the changing relationship between people and nature. The hero, *Hippoglossus stenolepis*, is one of the largest fish in the sea. Most halibut are between thirty and forty pounds, but five-hundred pound specimens are not unheard of, and the historical record is replete with references to two, three and four-hundred pound fish. Like other demersal species halibut live on or near the ocean floor. Both eyes are on the right side of the head, but its mouth is vertical and gives its torpedo-shaped body an odd, twisted appearance. Its topside is usually olive-green with dark brown blotches, making it less visible to predators (of which there are very few) and prey (of which there are very many), and contrasts starkly with its icy-white underside.

Halibut can be found on the continental shelf of the North Pacific Ocean and Bering Sea from Santa Barbara, California to Nome, Alaska, and along the Asiatic Coast from the Gulf of Anadyr, Russia to Hokkaido, Japan. But the largest concentrations, and hence the largest commercial fisheries, occur in discrete pockets, where sandy, pebbly bottoms afford ample food and protection from all but human predation along the northwest coast of British Columbia and the southeast coast of Alaska. The most prolific banks – those near present-day Goose Island in southern Hecate Strait (600 sq. miles), the Flats in northern Hecate Strait (1200 sq. miles), Portlock Bank (6800 sq. miles) and Albatross Bank (3700 sq. miles) – are but a fraction of the total continental shelf.

Halibut spend much of the year in relatively shallow water where they feed on just about anything that fits into their large, toothy mouths – clams, crabs, sablefish,

sculpin, Pollock, turbot, octopus, and occasionally other Pacific halibut. Each year, usually around October, adults begin migrating into deeper water at the edge of the continental shelf where spawning takes place through April. Spawning occurs in many areas but the largest concentrations of mature fish are found in discrete pockets near Cape St. James, Langara Island and Frederick Island off the coast of British Columbia, and near Yakutat, Cape Yakataga, and Chirikoff Island in the Gulf of Alaska.

Like most marine fish, halibut are highly fecund. In a single season, a typical fifty- pound female can produce 500,000 eggs; a one-hundred pound female might yield up to 2,000,000 eggs and a two-hundred and fifty pound female up to 4,000,000 eggs. But the egg that survives to become an adult fish is the exception rather than the rule. Fertilization takes place externally near the ocean floor, and because the eggs are heavier than the water above them, they remain near the bottom, all the while drifting passively in deepwater currents where they are fed on by other marine animals Twelve to twenty days later they hatch and the larvae, which become lighter as they develop, begin to rise toward the surface. Initially the larvae derive sustenance from a large yolk-sac, but once this is fully absorbed they begin feeding on plankton. The larvae continue their ascent in the water column until finally they are swept up by the warm Alaska Current and carried west for many hundreds of miles along the Alaska Peninsula before being deposited in deep water near the outer Aleutian Islands.

By this point a remarkable transformation is already underway. A post-larval halibut looks much like most other marine fish. It swims upright, has one eye on each side of its head, and has characteristic piscatorial pigmentation. But very soon after hatching, the halibut's left eye begins to migrate over its snout, until it comes to rest on

the right side of the head. At the same time pigmentation fades until the left side of the fish is completely white. At six-months-old the metamorphosis is complete. The young halibut has taken on its characteristic flatfish form and begins settling to the bottom. No longer at the mercy of the currents, it begins moving into shallower water and what are believed to be nursery areas just off the coast of central Alaska. There, the young halibut feed heavily on small fish and shrimp-like organisms before spreading out over the continental shelf, usually by age two or three. Of those that survive, many will be caught by commercial fisheries after their eighth year. Those that are not on someone's dinner plate by age 12 are ready to return to the spawning grounds as mature adults, where the cycle begins again.

North American fishery conservation cut its teeth on the Pacific halibut fishery, which thus provides plenty of grist for the mills of critical, environmental-historical scholarship. This fishery has been cooperatively managed by Canada and the United States since 1923, and has been at the center of what may be two of the most important debates over fishery policy in the last century. What follows, however, is less a study of specific policies than a study of shifting emphases in North American fishery management. The idea that ocean fisheries need to be managed is unique to the late nineteenth and early twentieth centuries, and so it is relatively new. But the idea of management begs two further questions: *managed how and to what end?* These are the questions to which *Seachange* seeks answers. The study is divided into three chapters and a short epilogue. Chapter 1 charts both the rise and fall of the halibut fishery between 1878 and 1923 and considers the relationship between science, commercial interest and conservation policy. Chapter 2 focuses on the International Fisheries Commission (IFC)

created under the 1923 Canadian-American Halibut Treaty, and shows how one scientist, William Thompson, attempted to confront biological complexity and scientific uncertainty with an "experimental" approach to halibut conservation policy. Chapter 3, which also focuses on the IFC, considers how debates over halibut conservation policy in the late 1940s and early 1950s prompted a profound rethinking of the goals of North American fishery management. I conclude with a brief review of developments in halibut science and management since 1960.

Chapter 1
Conservation and Commercial Interest in the Pacific Halibut Fishery, 1878-1923

The Rise of Commercial Fishing

Commercial production of Pacific halibut began in the early 1870s in Puget Sound and the Strait of Georgia, with fishing on near-shore banks for local markets, mainly in Victoria, BC and Port Townsend, WA. By 1878 small quantities of halibut were being packed in ice chipped from Alaska glaciers and shipped to San Francisco, where because of its "rarity" it reportedly retailed for fifty cents a pound, high by 1879 standards.[1] Data on the early years of the fishery are sparse but by all indications operations were not extensive. Halibut, because of its high water content, could not be canned and spoiled quickly unless salted or packed in ice, both of which were relatively rare in the mild Pacific Northwest. In consequence, the catch of west coast halibut fishermen was confined to local and regional markets which, in the 1870s and early 1880s, were simply too small to support a commercial fishery on the same scale as that for salmon.

In the late 1880s and 1890s, halibut markets were enhanced by the arrival of transcontinental railroads. Local and regional markets expanded with settlement and for the first time it was possible to ship Pacific halibut east to Montreal, Boston, New York and Chicago, where Atlantic halibut had already established a market. The first shipments of American halibut were carried east to New York in 1888 over the recently completed Northern Pacific Railroad. Four years later the Canadian Pacific Railway (CPR) carried halibut to Montreal. With new railways and expanded markets production increased dramatically. In 1878, west coast halibut producers marketed 500,000 pounds, more or

less, and most of it was consumed locally; small amounts being shipped to San Francisco.[2] Ten years later, three New England schooners fishing on Puget Sound, and dozen or so small Canadian sloops fishing in the Strait of Georgia landed just over a million and a half pounds. Most of the catch was shipped to eastern markets.[3]

Neither fishery proved particularly profitable. Freight rates were high (1.40 per cwt. in 1888) and ice was prohibitively expensive ($15.00/ton in 1888).[4] Ice could be obtained from one of the few ice dealers on the coast, but more often it was collected by the halibut fishermen themselves, from Alaska glaciers 1200 miles and more away from the principle fishing grounds. Poor shipping and handling across the continent further exacerbated the problems of getting the fish to market. Looking back on the early days of the halibut fishery, Samuel Chesebro, an east coast fish dealer, recalled that "In or about the year 1889, Benjamin and West received a carload of West Coast halibut, the first ever to cross the continent. They were packed in all sorts of packages, dry goods boxes, shoe boxes, soap boxes, even cigar boxes for in those days fishermen found pretty crude conditions on the West Coast and they had to use whatever packages they could get their hands on."[5] In fact, repeated failures to place Pacific halibut on eastern markets in anything like edible condition drove the three largest schooners – the *Mollie Adams*, the *Edward E. Webster*, and the *Oscar and Hattie* – out of the fishery and into more profitable pelagic sealing by the end of 1889. "At the close of 1889 the outlook for the continuance of the Pacific halibut fishery as an industry of any considerable importance, was decidedly unfavorable" noted one US federal fisheries authority in early 1890. "There was every prospect that it would be abandoned or at least reduced to a scale only sufficient to supply the limited local demand."[6]

Others were less pessimistic. Although early attempts to tap into eastern markets failed miserably, a few small sailing sloops and schooners of six to thirty tons capacity continued to supply local, regional and to a lesser extent national fish markets with fresh halibut. One way around the spoilage problem was to supply national markets during the winter months, when cooler temperatures kept fish from spoiling, which most fishermen did after 1890. This had the added benefit of placing Pacific halibut on eastern markets at a time when prices were highest, after the Atlantic fishery had shut down for the winter, and helped offset high shipping charges. Another way around the spoilage problem was simply to reduce the amount of time between fishing and shipping. In practice this meant concentrating the fishery on Puget Sound, and in the Straits of Georgia and Juan de Fuca, or as close as possible to the railhead ports of Seattle and Vancouver. Profit margins were still narrow but by the early 1890s the fishery was beginning to pay.[7]

Conditions improved considerably with the development and diffusion of cold-storage technology. Small cold storage plants opened in Tacoma, Washington in 1891 and in New Westminster, British Columbia and Seattle, Washington in 1892. By 1903 there were at least four more plants operating in Washington State and by 1905 there were at least two more in British Columbia.[8] Such facilities provided much needed ice and herring bait at affordable prices, served as way stations for halibut awaiting shipment east, and allowed halibut fishers to extend their operations away from Puget Sound and the Strait of Georgia and into Hecate Strait where halibut were especially abundant. Refrigeration technology also revolutionized rail transport. In 1895 the CPR began introducing refrigerator cars designed especially for the halibut fishery and agreed to waive shipping charges if the fish spoiled en route to eastern markets; two years later the

CPR was running no fewer than six refrigerator cars and the number increased steadily thereafter.[9] Soon after, similar cars were operated out of Seattle and Tacoma.[10]

Meanwhile, capital and labor continued to move into the fishery. The New England-based International Fishing Company (IFCO) established an office and cold storage facility in Seattle in 1892. Two years later, IFCO's parent company, the New England Fish Company (NEFCO), constructed a cold-storage plant in Vancouver and began shipping halibut east over the CPR. The fleet grew accordingly. In 1896 there were forty-eight small sloops, ten larger schooners, and three steamships working out of American ports, twice the number active in the fishery just four years before.[11] Similar, though less spectacular, growth was apparent on the Canadian side. In 1896, for example, 2 steamers and a dozen or so small sailing sloops and schooners fished out of Vancouver, considerably more than in 1890 when just a few fishermen engaged in the commercial fishery.[12]

With this growth came a considerable increase in output. In 1895 Pacific coast halibut producers marketed over 4 million pounds, or more than three times landings in 1890. By 1899 aggregate annual landings were nearly nine million pounds – making a six-fold increase in a decade.[13] The bulk of the catch was shipped to the eastern United States. Even Gloucester fishermen were beginning to complain about western competition. "The arrival of west coast halibut on the eastern markets has caused great commotion among New England fishermen" reported the *Boston Globe* in 1898.[14]

The 1890s also saw the first glimmerings of a Pacific halibut industry defined along national, market, vessel-ownership and technological lines. After 1900, American fishermen were responsible for between 75 and 80 percent of the total catch. Most of this

was marketed in New York, Boston, and Chicago; the balance went to Tacoma, Seattle, and (to a lesser extent) San Francisco. Canadian fishermen landed the rest of the catch. Like their American competitors they marketed most of their fish in the eastern United States; smaller amounts were consumed in Victoria, Vancouver and eastern Canada.[15]

Two-thirds of the catch was brought in by a small fleet of large capacity, company-owned, steamers, each of which carried up to fourteen dories and from twenty to forty-five crewmen. A larger fleet of smaller, independently-owned sloops and schooners, carrying from two to six dories and eight to fourteen crewmen, took the remaining third.[16] Whether the catch came from steamers or schooners or sloops, fishermen worked from dories and used one type of gear, the simple and effective long line.[17] A single unit or "skate of gear" consisted of an 1800 foot "ground line" to which were attached shorter lines or "gangings" and up to 265 hooks at intervals of 9, 13 or 18 feet depending on the size and depth of the banks being fished. Usually from two to twenty skates were joined to form a "string" thus enabling fishermen to cover large areas. Flagged buoys marked their location. Dory fishing would be abandoned by the early 1920s but halibut fishermen still use long-lines to procure their product.

By all indications Pacific halibut was fast becoming a fishery of national importance. Yet federal fisheries officials remained skeptical about the future of the halibut fishery. Surveys of the resource carried out in the early 1890s by the United States Fish Commission (USFC) research vessel *Albatross* indicated that Pacific halibut was less abundant than its Atlantic counterpart, and already in 1899 there were signs of depletion on near-shore banks. "That this fishery will ever approach the Atlantic fishery seems improbable," explained the USFC in 1900, "as the grounds in the Gulf of Georgia,

Puget Sound and those off Cape Flattery have all together a relatively small capacity, which has already been overtaxed."[18]

Later studies confirmed that near-shore stocks were declining around 1900 as the USFC suggested. But whether the *Albatross* surveys, (which lasted less than two months, did not go beyond Hecate Strait and were directed equally at discovering other deep-sea fishing opportunities), were representative of the entire halibut stock is questionable. Fishermen, for example, often spoke of large northern and western halibut stocks.[19] If the halibut fishery was nearing its limits to growth it was because east coast halibut fishermen still controlled the major markets. "When we get to New York" complained a Washington State fishermen, "we are boycotted and can't sell our fish."[20] This was one reason why the heaviest halibut fisheries took place in the fall and winter months – competition from Atlantic halibut was at its lowest. Thus the Pacific fishery in 1900 was limited more by market conditions than by stock abundance.

But the tide was beginning turn against Atlantic halibut fishermen. As early as 1875 a "scarcity of fish" had forced them off local and near shore banks and into deeper water, and by 1890 they were working as far north as Davis Strait on the west coast of Greenland.[21] This was crucial because in 1895 a Seattle steamer could travel to Hecate Strait, obtain a cargo of halibut and have it on eastern markets before Atlantic fishermen even reached Davis Strait. The USFC made an effort to enhance the Atlantic fishery by way of artificial propagation (or fish culture) but, as was so often the case with early attempts to propagate deep sea fishes, it proved impractical and failed. By 1900, the once dominant Atlantic halibut fishery was on the brink of commercial extinction. Annual

landings at Gloucester, the principle halibut port on the east coast, dropped from nearly fifteen million pounds in 1879 to just four million by 1902.[22]

The collapse of the Atlantic halibut fishery was a boon for the west coast industry. The flow of east coast capital and labor into the Pacific fishery continued after 1900, and with it the size and character of the halibut fishing fleet changed dramatically. In 1895 there were just three steamers operating in the fishery. Ten years later there were fourteen.[23] Since steamers typically took half or more of the total catch, aggregate annual landings soared. In 1907 Pacific coast fish dealers marketed some fifty million pounds of halibut, or ten times more than that marketed at the turn of the century, and double that marketed in 1905. Almost overnight Pacific coast halibut producers became the world's largest suppliers of fresh and frozen halibut. They shipped their product to Boston, Chicago, New York, and by 1910, some of it reached London, England.[24]

Changing market conditions, the spread of cold storage technology, and declining catch-rates in Puget Sound and the Straight of Georgia prompted a reorganisation of fishing effort in the fishery after 1900. As market demand increased and stock-abundance (not to mention space) on local and near-shore banks decreased, the fishery spread north and west into deeper water, reaching Cape Spencer, Alaska in 1911, Unimak Pass at the tip of the Alaska Peninsula in 1915, and the outer Aleutian Islands by the end of the decade.[25]

Northward expansion was accompanied and made possible by changes in technology and economy.[26] Everything from the adoption of internal combustion engines to powered winches to simple electric lights allowed fishermen to greatly extend and expand their operations. Gasoline powered engines, introduced in 1905, greatly extended

the range and mobility of the small boat fleet. In 1912 fishermen began using a powered "gurdy" or winch to haul their long-lines. This allowed them to set and reset their gear much more quickly and led, within a decade or so, to the abandonment of dory fishing since fishing could now be done directly from the schooner or steamer. In 1913 electric lights were introduced to the fishery. These were safer than the oil-lamps they replaced and extended the working day in western Gulf of Alaska where the days were shorter than further south. Fishing costs increased but these were more than offset by continued high prices in eastern markets and by the efficiencies associated with these new technologies.

Expansion was also aided by the further spread of cold storage facilities and by improvements in transcontinental transportation. The New England Fishing Company constructed large capacity facilities at Ketchikan, Alaska in 1909 and Prince Rupert, BC in 1911.[27] The latter could hold up to 14 million pounds and was the largest cold storage facility in world when it was built. By 1912 there were fourteen cold storage facilities, of various sizes, in operation along the coasts of British Columbia and Alaska.[28] Southern ports such as Seattle and Vancouver continued to play an important role in the fishery, but quickly lost ground to their northern competitors after 1914, following the construction of a transcontinental railroad to Prince Rupert, BC. By the end of the decade there were only a few small-scale fisheries serving local markets on Puget Sound and in the Straits of Georgia and Juan de Fuca. The prolific Cape Flattery banks still supported a reasonably large fishery in the summer when catch rates were higher (owing to the migration of halibut into shallow water for feeding). But the heaviest fisheries were now in international waters, off the Queen Charlotte Islands in Northern British

Columbia and in the Gulf of Alaska. Lesser fisheries occurred on near shore banks, where smaller boats served local markets. Still, the halibut industry seemed to move, almost en masse, onto the northern banks and into deeper water after 1900.[29]

The first attempts to establish a paying halibut fishery on the Pacific coast failed miserably, but from such humble beginnings a commercial fishery, aided by changes in economy and technology, was established. At the close of 1914, nearly two hundred boats and twelve hundred fishermen engaged in the halibut fishery.[30] Together they took enormous quantities of halibut from the Pacific and shipped it by rail across the continent to the eastern United States and by steamship across the Atlantic to the United Kingdom. Second only to salmon on the Pacific coast, with total landings worth approximately $6,000,000 in 1914, this was the largest halibut fishery the world had ever seen.[31]

Conservation and the Closed Season

Rapid market expansion after 1895 proved extremely profitable for many Pacific coast halibut producers, but it also raised concerns about the possibility of overfishing. As early as 1899 Richard Rathbun, an ichthyologist with the USFC, warned that near shore banks were showing signs of depletion, and recommended that a limit be placed on the fishery. "The rapidly growing halibut trade has caused a heavy drain on the grounds in the Gulf of Georgia, Puget Sound and the Strait of Fuca," explained Rathbun. "A remedy will be difficult to find owing to the indefinite character of the fishery, but some restriction should undoubtedly be placed upon the quantity of fish taken."[32] By the early 1900s similar reports of depletion on the northern halibut banks between Cape Scott at the north end of Vancouver Island and Rose Spit at Dixon Entrance near the Alaska-British

Columbia boundary, began appearing in regional trade journals and in the local press. In 1906 *The Pacific Fisherman,* a regional trade journal published in Seattle, wrote that "a recent close inspection of the northern halibut banks has revealed the fact that many of the grounds have been depleted. Banks which half a dozen years ago were bountiful in their yield of halibut were found as free of fish as a billiard ball is of hair, while others known to have been fine fishing grounds in the past where large fish were numerous were found to carry nothing but the smallest of fish."[33] The *Victoria Daily Colonist* reported an "Alarming Decline on the Northern Halibut Banks."[34]

Opinions differed on the cause of the decline, and whether there had even been one. In British Columbia, where reports of American poaching received almost continuous coverage in the local press, arguments about overfishing often took on nationalistic overtones. "Americans are a menace to the halibut fishery," railed one BC fisherman in 1906. "They have cleaned up all the big fish on the Hecate Strait and Dixon's Entrance banks."[35] Such arguments seem to have had considerable justification. The historical record is replete with complaints about American vessels fishing inside Canadian territory.[36] Some of these were undoubtedly true. On the other hand many BC fishermen were resentful of the Canadian government for allowing NEFCO (an American-owned but Vancouver-based company) to use American crews and vessels to avoid paying a 1 cent per pound duty on all "Canadian" halibut entering the United States. This kind of preferential treatment, they argued, was precisely why American fishermen took 80 percent of the catch while Canadians took just 20 percent. Whether Canadian fishermen truly feared for the halibut's existence or simply for their own is difficult to say. Probably they feared for both.

Almost as often, arguments about overfishing pitted small-scale, independent producers against large-scale, capital intensive producers (like NEFCO), and thus reflected commercial rivalries within the halibut fleet. In both countries, independent fishermen working local and near shore banks from sail and later gasoline-powered schooners deprecated the "wastefulness" of the company-owned steamer fleet, and with considerable justification.[37] In 1910, for example, 14 Canadian and American steamers or about 10 percent of the total fleet accounted for almost half of the total catch. Moreover it was alleged that they threw back almost as many halibut as they kept either because they were too small or too large for freezing, or because they were simply discoloured – a process known as high-grading. Probably all fishermen high-graded their catch to some extent, since dock side dealers paid more per pound more for medium sized fish than they did for either large "whales" (more than 70 pounds) or "baby chickens" (less than 5 pounds).[38]

Yet not everyone agreed that there had been a decline in the fishery. Some large-scale producers maintained that the scarcity of fish on inshore banks probably had more to do with the fishing practices of the small-scale producers than it did a decline in the halibut stock per se. Thus it was claimed that by the time the inshore fishery began, usually in early spring, many of the fish were already moving into deeper water either for feeding or for breeding or both and that this reduced catches accordingly. Of course, it was clearly in the interest of large-scale producers, who for the moment were having no problems procuring their product, to deny that there had been a decline in the fishery. But to claim that inshore fishermen, some of whom had been working the halibut banks since the 1890s, simply had no idea where and when one could catch halibut was absurd. A

1907 editorial in the *Pacific Fisherman* would have none of this: "It has been argued that at certain seasons of the year the halibut migrates, alternating between deep and shallow water, according the breeding and feeding possibilities, and this line of reasoning is advanced to account for the scarcity of fish on the well-known, old time banks. But this is nothing but a fallacy. The Canadian banks are being rendered valueless largely through the activity of the steamers." This counter-argument was probably correct, but as we will see weighing the actual evidence for overfishing was far from straightforward.

In 1908 a Royal Commission into the state of British Columbia's fisheries acknowledged that the decline in the halibut fishery was both real and serious. "It is generally agreed amongst experienced fishermen that the British Columbia banks have seriously deteriorated during the last ten or twelve years," wrote Commission chairman Edward E. Prince, "and it is absolutely essential that some measures be adopted to save the halibut supply from exhaustion, a fate which has befallen the Atlantic shores of Canada."[39] Yet despite the fact that two-thirds of its seven page report on halibut was devoted to the "remarkable" harvesting capacity of the steamer fleet and to the politically "contentious" issue of American poaching, the Commission carefully avoided passing judgment on both.[40] Perhaps it recognized that Canadian steamers, though fewer in number, were every bit as effective as their American counterparts. Perhaps too the Commissioners recognized that the smaller Canadian halibut industry depended almost entirely on American Companies such as NEFCO and were therefore reluctant to declare against them. Whatever their precise reasoning, the Commissioners urged that the interests of the fishery would be best served by a four month winter closed-season on halibut. However with no clear evidence that there had been a decline in the population,

or that halibut spawned in winter, the closed season stood little chance of being agreed to by the Americans. The commissioners had heard from disgruntled fishermen whose national and commercial affiliations were obvious. "Close our bays and harbors to Americans" suggested one halibut fisherman. "I'd almost guarantee in two years to put them out of business."[41] Nothing became of the closed-season and halibut conservation fell by the wayside.

Then in 1911, the Seattle-based steamer *Independent* discovered what was believed to be a major offshore halibut spawning ground near Yakutat Spit in the Gulf of Alaska.[42] The immediate effects of the discovery were twofold. First it gave new life to the closed-season/conservation movement. Second, and related, it aggravated existing divisions within the industry, since only the largest company-owned steamers dared to venture into the Gulf of Alaska during the stormy winter months, when high seas and high winds kept smaller boats at bay. More than one company steamer would eventually succumb to heavy seas, but dependent as they were on producing fish in volume, the potential for quick catches of larger-sized spawning fish seemed worth the risk. Inshore fishermen, partly motivated by conservation and partly by commercial interest, once again urged protection for the halibut. Company-owned steamers had long dominated the fishery but by taking spawning fish they seemed poised to destroy it for everyone else.

By 1912, and in the wake of the Yakutat discovery, the clamor for conservation from small-scale fishers had become so loud that federal fisheries officials in both countries ordered scientific surveys of the resource. Both, however, concluded that such fears were misplaced. The first survey, carried out during the summer of 1912 by "fishery expert" A.B. Alexander of the USFC, concluded that far too much had been made of

overfishing. "The investigation as a whole points to opportunity for development of the Pacific halibut fishery much beyond its present limits" insisted Alexander. "The phenomenal catches landed in the last few years suggest no stringency of supply on the grounds now fished."[43] Whether Alexander, who had little formal biological training, was at all qualified to draw such conclusions about the state of the halibut stock is questionable. But a second survey carried out a year later by zoologist Arthur Wiley for the Canadian Department of Fisheries, arrived at much the same conclusion. "Recommendations to curtail the fishery are easily made," Wiley pointed out, " but they would be entirely ineffective unless there happened to be a clear case for the immediate enforcement of rigid restrictions. The fact is that there is no such pressing call for drastic action."[44]

Yet by 1912 there was ample, if indirect, evidence that stock abundance was falling especially on the older banks between Cape Flattery, Washington and Hecate Strait off the north-west coast of BC. Consider, for example, the following statistics taken from *The Pacific Fisherman.*[45] First, although the total catch was still increasing, the catch per boat was clearly decreasing. In 1904, 39 small sloops and schooners of from ten to forty tons working on Puget Sound harvested roughly 119,000 pounds of halibut or 3050 pounds per boat. Ten years later, 62 vessels barely topped 150,000 pounds or approximately 2420 pounds per boat. The total catch increased but it did so at a lower rate than the growth of the fleet. Second, trips to the banks were becoming noticeably longer. In 1911, for example, inshore fishermen averaged 10 – 18 days fishing per trip, a considerable increase over the 6 – 10 days it took to obtain a full cargo just six years earlier. Third, there was a clear change in the location of the summer fishery after 1911.

Historically halibut fishers had spent the winter months fishing the near shore banks of Southeast Alaska before shifting to the famous Cape Flattery banks near Neah Bay for the summer fishery. But in 1911 no such shift occurred. Instead the majority of fishermen returned to northern British Columbia and to Southeast Alaska. Very little fishing occurred off Cape Flattery after May although this was usually the best time of year for fishing there. Neither Alexander nor Wiley paid any attention to such changes in the fishery, but focused instead on the fact that aggregate annual landings were increasing not decreasing. In consequence they saw no cause for concern.

John Pease Babcock, Assistant Commissioner of Fisheries for the Province of British Columbia, disagreed. "Fishermen and dealers know that the best known halibut banks are becoming depleted," he told D.N. McIntyre in 1914. "Our banks in Hecate Strait, which formerly yielded the greatest return to our fishermen, are no longer productive. It is beyond question that if this important food supply is not to be seriously reduced some protection must be extended to the species."[46] But with federal officials in both countries convinced that reports of overfishing had been exaggerated, such protection was simply not forthcoming. Babcock needed compelling evidence that the decline in abundance was as real and as serious as fishermen had made it out to be. In the spring of 1914 he hired William F. Thompson to further study the fishery.

At 26 Thompson was already an accomplished, if relatively unknown, scientist. He held degrees from the University of Washington and Stanford University in zoology and ichthyology respectively. Between 1910 and 1913 he co-authored no fewer than ten life-history studies of various fish and shellfish with David Starr Jordan, his graduate advisor and probably the foremost North American ichthyologist at the time. In 1911

Thompson studied the clam, abalone and oyster resources of northern California for the California Department of Fish and Game. A year later he conducted a similar survey for the BC Provincial Fisheries Department. An ichthyologist by training, Thompson was also one of a small group of mostly European biologists interested in the dynamics of exploited fish populations. Thus he was able to bring a range of techniques, from life-history analysis to stock assessment, to bear on the halibut problem.[47]

By December of 1914 Thompson's first paper from his BC Research, "A Preliminary Report on the Life History of Pacific Halibut," was ready for publication.[48] In it Thompson argued that halibut were especially vulnerable to overfishing and provided some compelling, if inconclusive, evidence that the stock's capacity to reproduce was in fact being undermined by too intense a fishery. First, Thompson pointed out that halibut's distribution was both confined to and erratic along the narrow Pacific continental shelf. Unlike the pelagic or open ocean fishes – tuna and sardine for example – the demersal or bottom dwelling halibut stayed in relatively shallow water, so there were no roving deep- sea stocks to draw on. Moreover, of the 80,000 or so square miles of continental shelf from Cape Flattery in the south to the tip of the Alaska Peninsula in the north, the largest concentrations of halibut seemed to occur in fairly discrete pockets along the northwest coast of British Columbia and southeast coast Alaska. These were important findings because they suggested that the fishery, which in 1914 was already pushing into the western Gulf of Alaska, was approaching its geographic and hence biological limits to growth.

Second, and related, there appeared to be "no extensive interchange of fish from different areas."[49] Time and budget constraints prevented Thompson from tagging

(attaching small bone or metal discs to individual fish in order to track their movements – see chapter 2) but there seemed to be ample, if indirect, evidence that halibut rarely ventured off the banks on which they were probably born. Thompson noted, for example, that growth rates were consistent within an area or bank but varied considerably between them. An 18-pound male from Kodiak Island in the western Gulf of Alaska was approximately the same age as a 30-pound male from Frederick Island, and a 45-pound fish from Hecate Strait. He also found that halibut from different areas were physically distinct from one another. Halibut from Hecate Strait, for example, appeared to have smaller heads, more compressed bodies, and fewer fin rays than those taken in the vicinity of Kodiak and Frederick Islands. According to Darwin's theory of evolution, this could only happen if a species reproduced in relative isolation. Thompson recognized that his sample size was too small to draw firm conclusions about stock composition, but he seemed fairly convinced that the halibut population was composed of numerous, non-migratory sub-populations or "races." This was important because it meant that once found a stock could be fished to a very low level of abundance, or "played out" as halibut fisherman put it. But it also implied a particular approach to conservation policy. "Were we to find a high degree of localization," Thompson told Babcock, "the logical basis for protection of limited areas would be laid."[50]

Third, and perhaps most important, was the fact that halibut matured very slowly. For example, only half of the females examined were mature at age twelve and there were still immature fish at age 15. Males matured more quickly, usually by 10 years of age, but immature 12-year-olds were not uncommon in Thompson's sample. Although it was well known that longer-lived species (like plaice and halibut) matured more slowly

than short lived species (like sardine and anchovy), Thompson's findings were "somewhat surprising" because they put maturation figures anywhere from three to four years later than Atlantic Halibut, and from five to nine years later than North Sea plaice, both believed to be similar species of flatfish.[51] The implications were obvious to Thompson: an intense fishery could easily exceed the stock's capacity to reproduce. Indeed, already there were signs that this was happening, particularly in Hecate Strait where commercial fishers had been pursuing halibut for fifteen years or so, about the time it takes a female halibut to reach maturity. Only 14 percent of the female fish from Hecate Strait had reached 12 years of age at the time of capture and only 5 percent had reached 16 years of age. Less grievously, but almost equally startling, only 31 percent of the females from the Kodiak Island grounds in the Gulf of Alaska had reached their twelfth year and only 12 percent had reached or surpassed their sixteenth. Most fish examined were, in fact, eight-year-olds. Such statistics were self-explanatory and terribly unsettling to Thompson: too many adults were being taken and too few juvenile fish were reaching maturity in the first place, most likely because of the fishery. But owing to relatively small sample sizes the always-cautious Thompson hinted at, but carefully avoided drawing, such stark conclusions. He needed more proof.

Independent fishermen, by contrast, felt that they had all the proof they needed and continued to press local fisheries officials for an internationally sanctioned winter closed- season on halibut. Only now they did so with the full support of a politically powerful company-owned fleet facing rising costs and looking to eliminate expensive winter fishing. In a letter to the BC Department of Fisheries dated 17 November 1915, Canadian Fishing Company President Alvah Hagar argued that there were good reasons

to eliminate winter fishing.[52] First it would prevent the capture of mature spawning fish "It is our observation that halibut taken during these months have large overgrown heads, thin bodies, condition of flesh in poor shape, and unusually large pokes completely filled with ripe spawn," wrote Hagar. "These fish should be left undisturbed as they are at best only a # 2 article and the taking of these Halibut in this condition means the very rapid depletion of the Halibut." Second, and related, it would prevent the continued capture of mature fish and the destruction of the spawning grounds by lost gear. "During the months of December, January and February the vessels operate at a financial loss as large quantities of gear are lost, which also means the 'fouling' of the grounds, which condition is very bad, as no kind of fish will inhabit waters where gear, offal or other refuse is deposited." Notice that these are essentially economic arguments. The main reason these fish should be left alone was that they were of poorer quality - #2 as opposed to #1 halibut. And the "fouling of the grounds" was surely incidental to the increased costs associated with lost gear.

Thompson too believed that Hagar's proposal was motivated more by changing economic conditions made worse by the decline in the halibut stock than by conservation concerns. Commenting on Hagar's proposal in a letter to the BC Fisheries Department's D.N McIntyre, he pointed out that persistent labor problems culminating in general strikes in 1913 and 1914, and a sharp drop in retail sales of cold storage fish brought on by heavy landings of fresh fish during the unusually mild winters of 1914 and 1915, were already eating at the industry's bottom line when the stocks began to show clear signs of overfishing.[53] Hardest hit by changing conditions in the fishery were the largest firms – New England Fish and The Canadian Fishing Company for example – whose higher

operating costs and volume-oriented approach to the fishery were proving financially ruinous at lower stock densities. Indeed the number of steamers in the fishery was already declining in 1915. In 1913 there had been 18 steamers; in 1915 there were 15. Whether a winter closed-season would materially improve the halibut stock was open to argument, Thompson wrote, since there was every chance that it would simply result in a more intense open season fishery, particularly on the already badly depleted British Columbia banks.

> The establishment of a winter closed-season would immediately have the effect, as the Canadian Fishing Company suggests, of placing each vessel on a paying basis throughout the whole period of it operation. This would [allow] a smaller catch per diem than is at present the case, in other words would allow of the fishing of the banks much nearer to depletion, with profit. The consequence of this would be the temporary cessation of trips to the Alaskan banks and the more complete depletion of those in British Columbia. A closed season would surely aid in the disposal of the cold storage stocks, but on the other hand, just as surely heighten the demand for storage fish, especially of young immature individuals which are most prevalent in British Columbia and are most favored for freezing.[54]

Thus the closed season was not the best conservation measure available to fisheries officials. But it would make the largest fishing firms more profitable and hence better able to compete with the growing number of independent vessels whose lower operating costs and smaller but more frequent catches were, perhaps, better suited to changing environmental conditions in the fishery.

By early 1916 Hagar and the halibut industry seemed close to achieving their goal. American politicians had approved a Congressional Bill providing for a winter closed season on halibut. Now it was up to Canadian politicians to pass similar legislation. However Canadian politicians firmly opposed a piecemeal or single species

approach to international fishery policy. Instead, they wanted a single, comprehensive agreement covering Pacific halibut and other fisheries, including and especially those for cod on the Grand Banks, Atlantic salmon and whitefish on the Great Lakes, and Fraser River sockeye.[55] Moreover, they wanted such an agreement to go beyond conservation and deal with a whole host of fishery-related issues including tariffs and port privileges. In this context the idea of a winter closed-season on halibut, necessarily international in character but clearly tied to more politically contentious fishery matters, seemed dead in the water by mid-1916.

In December, the Provincial Fisheries Department published another of Thompson's papers, this one a statistical analysis of the halibut population showing that it was indeed being heavily overfished. "The most immediately important conclusion reached in this paper is the fact of depletion," Thompson confidently declared. "The intense fishery has, it is evident, made its influence felt throughout the whole biological appearance of the species and in doing so has rendered precarious the future of the banks. The numbers still found on them are so small, and the percentage of mature fish in this population has fallen so low that it appears imminent that the halibut in the Pacific will drop to a minor position among the food-fishes."[56] In other words Alexander and Wiley had been wrong. Halibut was being overfished and Thompson believed he had the evidence to prove it.

To support his argument Thompson relied on data collected from the logs of five company-owned steamers. Though "scattered" and "frequently fragmented" they were the best data he had ever seen, so good in fact that from them he was able to construct a simple population history for Pacific halibut dating to the early 1900s.[57] Unlike

Alexander and Wiley, Thompson understood that aggregate annual landings data were often poor indicators of stock health because they did not take into account numerous factors that could obscure changes in the productivity of the fishing grounds.[58] Strong market demand and more efficient harvesting technologies, for example, could conspire to keep fishermen fishing long after stocks started to decline. By the time overfishing became apparent in aggregate annual landings it might be too late to intervene. The best available measure of stock abundance was catch-per-unit-of-effort (CPUE) which, as the name suggests, relates the quantity of fish caught (usually in pounds but sometimes in numbers) to the amount of fishing effort used (usually expressed in terms of a standardized unit of fishing gear).[59] Calculating CPUE for any given year was simply a matter of dividing the total catch by the total units of gear used in a fishery, provided of course one had data and a standard unit of gear to work with. Fortunately for Thompson the steamer captains kept logs and used long-lines that he assumed had not changed much since the late 1890s, thus making year to year comparisons of CPUE more reliable.

The CPUE measure of abundance was a useful starting point, but by itself it could provide no proof of overfishing. After all, a fishery cannot help but reduce abundance somewhat (since by definition fisheries remove fish) and as most tend to remove the largest adults first, a fall in CPUE is simply unavoidable. The phenomena is not unlike the so-called "fall-down effect" in forestry – the inevitable decline in timber volume that occurs as loggers remove the larger, old-growth trees and begin shifting to smaller second-growth trees. Furthermore, there are any number of reasons why stock abundance (hence CPUE) might change, none of which necessarily have anything to do with

overfishing: changes in food availability, changes in water temperature, disease, parasites, increased predation, the list is almost endless.

Thompson therefore relied on a simple model of overfishing developed by C.G.J. Peterson, a Danish biologist who had been studying the effects of fishing in the North Sea.[60] A sharp decline in catch-per-unit-effort (CPUE) was indicative of what Peterson called "stock overfishing" and showed that rate of removal was exceeding the rate of replacement. A decrease in the average size of individual fish or what Peterson called "growth overfishing" occurred when the rate of removal exceeded the rate of growth. Thompson found evidence of both (and hence of overfishing as defined by Peterson) in the halibut fishery. Thus, for example, CPUE in the fishery fell from 280 pounds in 1902 (the first year for which there were complete data) to just 124 pounds by 1914.[61] Similarly the average size of individual halibut had declined from just over 34 pounds in 1902 to a mere 12 pounds by 1914. The fact that aggregate annual landings were still climbing now seemed disturbing and unsettling. Pushed to the absolute limit the halibut population could collapse all at once, leaving both fish and fisherman alike in ruins.

But Thompson did not stop there. By comparing catch rates from different areas he was able to show how uneven the process of depletion had been. Not only were the older banks south of Dixon Entrance much more depleted than the recently exploited banks in the Gulf of Alaska, but they were themselves unevenly depleted. The Cape Flattery banks were relatively more depleted than the banks in Hecate Strait, as indicated by a far steeper drop in CPUE on the former. This was important for two reasons. First it supported Thompson's contention that the halibut population was, in fact, composed of numerous, non-migratory sub-populations. Otherwise the decline in abundance or falling

CPUE would have been more or less consistent across Thompson's sample. Second, and related, it implied that an area-based approach to conservation policy was better suited to the halibut's habits and to the varied condition of the banks than, for example, the now strongly supported closed season which seemed incompatible with both. Yet Hagar and the halibut industry, now armed with scientific proof that halibut was being heavily overfished and with confirmation that halibut did spawn during the winter, continued to press federal fisheries officials for a winter closed season.

Convinced more than ever that a winter closed season would simply encourage a more intense open-season fishery, and probably frustrated with the fishing industry's selective use of his findings, Thompson published one final paper on halibut late in 1917.[62] He began by setting the record straight on the spawning period. Halibut did spawn during December, January and February, but many continued to do so through April and some fish spawned in early May. Moreover there was no evidence at all that halibut congregated in the eastern Gulf of Alaska to spawn. On the contrary spawning appeared to occur throughout the halibut's range and largely within the confines of each bank. Therefore a three-month winter closed season would only protect part of the spawning population, for part of the spawning period.

But even a five month-closed season covering the entire spawning period seemed inadequate to Thompson. The advantage of allowing breeding fish freedom from capture during the spawning period, he argued, lay mainly in the fact that in species that lived, say, no more than three years after maturity, the value of the first breeding season was disproportionately great. This was due to the progressive destruction of individual fish, by far the greater number dying before completion of the full term. In such cases

protection during the spawning season could be crucial if it ensured that every fish reproduced at least once before dying. But in the case of long-lived species like halibut, which might breed for twelve or more years before dying, the value of first spawning period was not so much more important than say its third or fourth or tenth for that matter. In such cases removal at any time means the complete loss of all future spawning periods. What really mattered, then, was maintaining a stable ratio of mature to immature fish in the population, something Thompson felt could only be achieved by conservation measures aimed at all year-classes in the population as opposed to just a few.

Thompson also challenged the popular assumption, based in large part on the tremendous publicity given to the 1911 Yakutat discovery, that the winter fishery preyed on spawning fish in particular and that these were in dire need of protection. On the contrary – and following Peterson's definition of stock overfishing – Thompson argued that the greater number of mature fish in the winter catch simply indicated that the northern banks were depleted less than those in the south and needed less, not more, protection. "The so-called spawning grounds are those less depleted than others because less accessible or because it pays to resort to them only during the winter seasons," Thompson insisted. "At one time the banks now characterized by small immature fish had a population of large, mature fish, and their absence is due to the effects of overfishing. We therefore come to the anomalous conclusion that protection is proposed for banks which need it least, as they have a more nearly adequate supply of breeding fish."[63]

Thompson believed that an area-based approach to halibut conservation was better suited to the fish's biology and to the varied condition of the banks. Moreover it

would prevent fishermen from concentrating their efforts on the already badly depleted British Columbia banks. The first step was to divide the coast into 6 areas corresponding to the conditions found on the halibut banks. Areas 1, 5 and 6, for example (south of Cape Flattery Washington, Cape Cleare, Alaska to Unimak Pass, and the Bering Sea respectively), were depleted least, while areas 2, 3 and 4 (coastal BC, Dixon Entrance to Icy Strait, and Icy Strait to Cape Cleare respectively) were depleted most. The next step was to alternately open and close fishing areas:

> Areas 2 and 3 could be alternately closed and opened, 2 closed for five years, then 3 for the next five years and so on alternately. Areas 1, 4, 5 and 6 could be closed at the same time as either 2 or 3, their closure being subject to the discretion of conferees appointed by the two Governments: provided that, unless otherwise agreed upon by these conferees, Areas 1, 3, and 5 would be closed together, and Areas 2, 4, and 6. Each area would thus be closed five out of every ten years. This arrangement would allow sufficient latitude of time to overcome any differences in the productive powers of the areas, and at the same time obviate any danger of placing any particular port under a disadvantage.[64]

Thompson's plan clearly corresponded with the best scientific information available at the time. If indeed the halibut population was composed of numerous, non-migratory sub-populations reproducing in relative isolation, and if the banks were as unevenly depleted as the data suggested, then an area-based approach to conservation would seem to be most appropriate. Whether closing large areas for extended periods of time was politically and economically feasible was open to argument. But unlike the popular closed season, which was entirely at odds with the best science available, Thompson's area-based approach promised to protect those fish that needed it most. Now it was up to policy makers to choose.

Conclusion: The Politics of Conservation

1917 turned out to be a terrible year for the halibut fishery. Total landings, some 48 million pounds, were down considerably from the 70 or so million pounds landed just two years earlier.[65] The struggling steamer fleet, already "pushed to the wall" by rising costs and falling fish stocks, continued its slow exit from the fishery. At the beginning of 1918 eleven steamers were outfitted for the fishery, three fewer than the year before and five fewer than the year before that. Fishermen, many of whom were old enough to remember the collapse of the Atlantic halibut fishery – and some of whom actually participated in it – surely wondered whether there would be a Pacific fishery ten years down the road, or five years, or less.

At last Canadian and American politicians were ready to act. The setting was a 1918 International Fisheries Conference, chaired by U.S. Secretary of State, William C. Redfield and Chief Justice for the Dominion of Canada, John D. Hazen.[66] At a public hearing held in New Westminster, Hazen and Redfield heard Alvah Hagar's plea for a closed season, but they also took time to review Thompson's area-based proposal and John Babcock, who was also present at the hearings, remained hopeful that it would be approved. "Thompson revealed enough of the halibut's life history to have his plan adopted" he told Henry O'Malley following the hearing.[67]

Hazen and Redfield, however, were more concerned with the political and economic implications of Thompson's plan than with its biological merit. First, they argued that if implemented it would prove incredibly costly. Funds for ongoing research and for extensive patrolling to prevent poaching in closed areas were absolutely necessary if the plan was to work. With World War I exerting an enormous drain on the

financial resources of both countries, they would be hard pressed to provide such funding or to spare the necessary patrol boats.

Second, Hazen and Redfield insisted that "halibut fishing would become so centralized and concentrated on the open areas that the good effects of the close time would be more than offset."[68] This was a peculiar argument for them to make, since the whole point of Thompson's plan was to prevent spatial and temporal concentration in the fishery, particularly on the British Columbia banks where the decline in the halibut stock was most serious and where the fishery was already heaviest during the summer months.

Third, they argued that "the end in view would not be achieved unless all fishing were prevented in an area, and this would very seriously retard the development of fishing for other species of fish."[69] Hazen and Redfield provided no further argument or explanation on this point, but they were probably alluding to the problem of by-catch or the capture of non-target species in a commercial fishery. Long-line gear, similar to that being used in the halibut fishery, was also being used to catch black cod, greyfish, and several species of Pacific flounder, all of which were beginning to find markets in the late 1910s. What was to stop fishermen from pursuing the more valuable halibut under the guise of, say black cod or flounder fishing? Aside from banning long-line gear – which Hazen and Redfield were in any case unwilling to do since it would stall development of other fisheries – not much.

Fourth, Hazen and Redfield argued that an area-based approach would force small-boat operators out of business. This point was exceedingly important given that the steamer fleet in 1918 was clearly yielding to a fleet of smaller, more efficient, but less mobile fishing boats (most could not operate profitably or safely beyond a radius of 150 –

200 miles). Closing area 2 – coastal British Columbia – for five years would surely drive all but the largest boats out of the fishery, something Hazen and Redfield were loath to assist in since they believed "the greatest promise for development in the fisheries on the Pacific coast of both countries [lay] in the growth of this small-boat fishery operating out of local ports."[70]

Hazen and Redfield were probably right to reject Thompson's plan as politically and economically impractical. But there was a third option. They might have rejected both plans and sent everyone back to the drawing board. Instead, when they drafted a preliminary Treaty for the Preservation of the Pacific Halibut fishery in late 1919 they opted for a winter closed season, on the grounds that spawning fish had to be protected, and called for more research.

Three years later, in spring of 1923, the Treaty was signed. The first-ever agreement on joint management of a high seas fishery, it provided for a winter closed-season and for the creation of an International Fisheries Commission to further study halibut biology. Reaction to the signing, and to the close-season in particular, was overwhelmingly positive. One local newspaper called the treaty "a landmark for fishery conservation."[71] Another applauded Canadian and American diplomats for finally coming together to a save a fishery "quickly going the way of the salmon."[72] Yet no one was more pleased than Alvah Hagar. "The news that a closed season for halibut is now assured is very gratifying to every person directly interested in the industry," Hagar declared triumphantly. "It means that the vast schools of these splendid fish will no longer be depleted and threatened with extinction through continued fishing."[73] Nothing could be further from the truth, but that seemed to matter little now. The first closed-

season came into effect the following November, just as the newly created International Fisheries Commission was holding its first meeting at Seattle.

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Chapter 2
Experimental Management: The International Fisheries Commission, 1924-1930

Towards a Conservation Program for Pacific Halibut: The IFC

The International Fisheries Commission (IFC) was itself a kind of experiment. Unlike earlier Canadian-American fishery commissions the IFC's mandate was manageable.[1] It would examine the economic and ecological effects of the closed season, study the life history of the halibut, and submit "recommendations as to the regulation of the halibut fishery which may seem desirable for its preservation and development."[2] Unlike the much-heralded International Council for the Exploration of the Seas (ICES) – its closest institutional counterpart – which comprised dozens of biologists from many countries and that undertook sweeping scientific studies of the marine environment, the IFC took its direction from a single scientist concerned with a single species of fish. And unlike ICES the IFC was on a tight schedule.[3] It had three years from the beginning of the first closed season to complete its work and report back to federal fishery officials with recommendations for future halibut conservation policy.

The IFC's organizational structure was well suited to the tasks at hand. There were three layers, each with a specific role to play. It comprised four commissioners (two from each country), a scientific staff led by a director of investigations, and an external scientific advisory board. Both countries chose one federal and one regional representative to sit on the Commission. Canada appointed John Pease Babcock from the BC Department of Fisheries, and recently appointed Assistant Deputy Minister of Fisheries for the Dominion of Canada, William Found. The United States on the other

hand chose U.S. Commissioner of Fisheries Henry O'Malley and the publisher of *The Pacific Fisherman,* Miller Freeman, to act as Commissioners. Their role was largely organizational and administrative. The Commissioner's responsibilities included appointing a Director of Investigations, hiring office staff and securing funds for scientific research. They also organized and, over time, implemented an extensive public relations campaign that included newspaper interviews, public hearings in all the major fishing ports, and frequent progress reports in Miller Freeman's widely read trade journal. Finally, they maintained links with state, provincial and federal politicians, keeping them abreast of the IFC's work, while at the same time fostering crucial support for any changes the IFC might recommend for the future of halibut conservation policy.

The IFC chose William Thompson to be its Director of Investigations. And who better? His 1916 study for the Province of BC was considered by many biologists to be the first unequivocal demonstration that overfishing could occur in a strictly marine fishery. "Scotch, Swedes, English and American authorities all commended Thompson's work most highly" wrote Babcock to Found.[4] In 1917 Thompson joined the California Fish and Game Commission (CFGC). There he founded the State Fisheries Laboratory at San Pedro, developed a comprehensive research program for important sardine and albacore tuna fisheries, and together with biologist Norman B. Scofield helped bring the CFGC to the fore of U.S. fishery research.[5] Already regarded as the "foremost authority" on Pacific halibut, by the early 1920s, Thompson was one of the leading fishery biologists in North America.[6]

As Director, Thompson was responsible for organizing and implementing a scientific research program for Pacific halibut. In principle the Director of Investigations

was subordinate to the Commissioners, but in practice Thompson ran the show, something John Babcock not only recognized but also in fact encouraged. "We are prepared at all times to back your slightest request," he told Thompson early in 1925. "We have unlimited confidence in you and your abilities and will give you all the rope you want. Our job is to finance it."[7] This kind of freedom fit Thompson – who had strong views on the way fishery research should be done – to a tee.

The third and final organizational layer of the Commission was a four member external scientific advisory board. University of British Columbia biologist Charles McLean Fraser and John N. Cobb from the University of Washington's School of Fisheries were the first to be appointed. They were later joined by Norman B. Scofield, Chief Biologist for CFGC, and Wilber A. Clemens of the Fisheries Research Board of Canada. The Board's primary function was to review Thompson's findings, and "to strengthen the hand of the Commission by ensuring effective criticism where such may prove to be necessary."[8] The Board was also intended to serve as a link to the larger fishery research community, and to keep Thompson abreast of the latest developments in fishery science, though more often it was other way around.

Thus the IFC stood in stark contrast to other fishery research institutions. Its "study the fish-regulate-the-fishery" mandate was considerably narrower than, for example, the "study-everything" mandate given to the ICES. Its organizational structure, moreover, was geared to meeting that mandate; each organizational layer had a clearly defined role. The commissioners paid the bills. The advisory board helped ensure that the larger scientific community would accept the IFC's findings. And Thompson did the rest. His first order of business was to construct a working research agenda.

A Research Program for Pacific Halibut

Looking back on his career with the IFC from the late 1950s, William Thompson recalled that "in those days fishery science was not very well developed and had little except vague promises to offer the first planners.[9] Indeed, fishery biologists faced an enormous challenge when, in the first decades of the twentieth century, government and industry looked to them to construct conservation programs for failing marine fisheries. The obvious decline of North Sea plaice in the last two decades of the nineteenth century, and Pacific halibut in the first two decades of the twentieth century convinced many scientists that such resources were not "inexhaustible."[10] But even in the 1920s very little was known about the relationship between fishing and overfishing beyond the simple generalization that too much of the former inevitably led to the latter. As fishery scientist and historian Tim Smith has pointed out, "by the end of the First World War the fledgling field of fishery population dynamics, or stock assessment, had developed most of its basic data collection methods but only the most straightforward data analysis methods."[11] Somewhere out there in a sea of statistics was a sustained-yield fishery but no scientist knew how to find it, or even where to look.

In the 1920s there were two basic schools of thought on the factors governing stock abundance, neither of which was especially useful for people concerned with practical conservation policy. The first of these, associated with the work of Danish biologist C.J.G. Peterson, held that the main factor determining the size of a stock of fish and its growth was the amount of food available.[12] Earlier in the century Peterson found that plaice from Nissum Broad, the westernmost of a series of densely populated, shallow

ocean basins called Limfjord in the North Sea, grew faster when transplanted into two of the inner broads where plaice were known to be naturally less abundant. They did so, he reasoned, because there was more food available to them. For conservationists, Peterson's study indicated that a certain amount of fishing effort was probably a good thing, since by making more food available to those fish left behind it would produce populations of larger fish down the road.

Some biologists soon pointed out that the relationship between food and stock abundance was probably more complicated than it appeared in Peterson's data.[13] Thinning a population of plaice would not necessarily increase the amount of food available for only that fish since other species with similar diets would be just as likely to benefit. And there was still the problem of how to enhance the productivity of already depleted fish stocks such as halibut. Surely stock size could not be increased by further augmenting the ratio of food to fish through thinning since presumably there was already a surplus of food available. More generally, Peterson was unable to say just how much thinning should be allowed to occur. Thus the basic question 'when did fishing become overfishing?' remained largely unresolved.

The second school of thought, associated with the work of Norwegian biologist Johan Hjort, held that the stock abundance depended primarily on prevailing hydrographic and biological conditions during the first year of life.[14] Working with statistics from Norwegian cod and herring fisheries Hjort discovered that more than half of the herring harvested between 1907 and 1913 came from a single, unusually abundant cohort born in 1904. Indeed the 1904 year-class was estimated to be thirty times larger than the 1902 year-class. This kind of "year-class dominance" also seemed to

characterize the cod fisheries as well. Thus did Hjort conclude that "there exists an intimate relation between the fluctuations in the numerical value of a stock of fish and the yield of the great fisheries. At certain intervals, year classes arise which far exceed the average in point of numbers, and during their lifetime, this numerical superiority affects the general character of the stock, both as regards the quantity and quality, thus again exerting a decisive influence upon the yield of the fisheries in both respects."[15]

Serious scientific and policy implications followed from Hjort's findings.[16] First, how could biologists know for sure that a sharp drop in abundance (as indicated by a falling CPUE) in some or other fishery was not caused by an environmentally induced natural fluctuation? Peterson's concepts of "stock" and "growth" overfishing could be helpful since one would not expect to find both a sharp drop in CPUE *and* in the proportion of mature, full-sized adults if natural fluctuations were at work. A series of failed spawning seasons, implying a decrease in the number of immature fish should actually increase the relative proportion of full-sized fish in the commercial catches. But Peterson's approach was far from foolproof and depended very much on having detailed statistical and biological information not easily obtained from an opaque ocean environment. Second, and more generally, if stock abundance was determined more by natural factors than by human activity, as appeared to be the case for cod and herring at least, then the very need for conservation would be open to serious argument.

Thompson, who understood the ins-and-outs of contemporary fishery theory as well as anyone, recognized that the study-the-fish-regulate-the-fishery mandate given him by the politicians who wrote the Halibut Treaty was, in fact, a tall order: at least from a

fishery theory point of view. From the outset he cautioned the IFC Commissioners not to underestimate the difficulties that lay ahead:

> That proposed regulation should be upon recommendation of a Commission informed by a study of the life history, is a direct challenge to the practical nature of the existing biological knowledge of our species of fish. Our goal is the restoration of the halibut fishery to a point where the maximum yield possible without a progressive depletion is obtained. But so diffuse and ill-defined is our knowledge of underlying principles that the commission must expect that one of its primary duties will be to find solid ground to act on both in the framing and application of regulations. In short the basis for practical administration of our fisheries is still to be laid by thought and experiment.[17]

What the IFC really needed was knowledge, some "solid ground to act on" according to Thompson. Thus the first step toward regulating the fishery along scientific lines was to construct a working research agenda.[18]

William Thompson had very strong views about the way in which fishery biology ought to done. He frowned on "abstract" theorization and frequently criticized biologists for working on too broad an environmental canvas when what really mattered was "the detection and prevention of overfishing" – a purely practical problem located squarely and narrowly within the realm of applied science.[19] But to understand Thompson's views one has to consider how his education, his maturation as a biologist and his specific approach to studying halibut reflected broader trends in marine biological research.

At the turn of the century, marine biology was largely an exploratory and descriptive discipline. Its practitioners, almost invariably university professors, dedicated their research careers to discovering and cataloguing new species. However, calls for conservation, issued almost simultaneously with calls for scientific research, shifted emphases within marine biology. A new generation of professional fishery biologists

began to emerge and with them came a new approach to fishery research. Largely gone were the detailed life-history and limnological studies so typical of academic biology. In their place were numbers – growth rates, mortality rates, catch per unit of effort (CPUE), rates of migration and so on. Quantification was seen to be the key to conservation.[20]

Thompson was wedged between these two scientific traditions. An ichthyologist by training, he was nonetheless early convinced that the future of fishery research was in numbers. "Biological statistics are necessary in the same sense that book-keeping is required in a business," Thompson told the IFC in early 1925, "and they should be organized as book-keeping is, to record the essential in an orderly way."[21] Fishery biology was fast becoming an exercise in biological bookkeeping. In discussing his approach with the IFC, Thompson captured perfectly the tectonic shift in emphases that was occurring within the larger marine biology community:

> As one looks through the literature, it would appear that the basic rule of conduct has been the general one that everything connected with fish and it environment, the ocean, is of importance and should be studied. This view is undoubtedly sound but it amounts to saying that everything on land is of importance when studying the production of beef cattle. The statement is too diffuse to be of meaning and has lead to the nearly total neglect of thought upon a really great problem, that of overfishing. Indeed it seems to me that it would have met with general approval on the part of biologists had the present investigation been solely designed to throw light upon the general life-history of the halibut. Research must have a statistical basis and the biology of the species will be principally of importance in guiding the collection of these statistics and interpreting their significance.[22]

The implications of this shift to quantitative fishery biology have been far-reaching. By mid-century population dynamics had firmly taken hold and the search for sustained yields of fish had become the central feature of North American fishery research.[23] Yet

the subsequent collapse of some of the world's most important marine fisheries (California sardine for example) has left some biologists wondering whether the radical shift in emphases early in the twentieth century has been to the longer-term benefit of fishery management.[24] And ironically, calls for a more "inclusive" or "ecosystem approach" – one not so utterly different from the "everything connected with the fish and its environment is of importance and should be studied" approach that Thompson so criticized – are common in much contemporary writing on fishery science and management.[25]

Certainly the first generation of quantitative fishery biologists abandoned a more inclusive approach to fishery research for one significantly narrower in scope. Yet Thompson and other like-minded biologists (Peterson and Hjort for example) saw in the decline of plaice and halibut an entirely new problem, one seemingly well beyond the purview of traditional academic biology.[26] The solution to that problem seemed to be in numbers, not detailed life-histories and wide-ranging oceanographic inquiry.

The more immediate problem for the IFC, though, was to find a suitable staff to assist with the fieldwork. For all its future influence, fishery population dynamics was still at the margins of academic research in the 1910s and early 1920s and would remain there for many years to come.[27] Universities still produced ichthyologists and limnologists, not biological statisticians. Probably no one understood this better than William Thompson. He saw that a critical gap was developing between the skills imparted to university graduates and those increasingly deemed necessary to solve the problems facing fishery administrators. "It would seem that the preoccupation of the universities with other more strictly biological problems has left a wide gap between the

training received by their graduates and that demanded in the actual care of the fisheries," Thompson told the IFC. "Very little knowledge of statistics and very little knowledge of the great problems to be solved are found."[28]

He was right. Even Stanford University, Thompson's alma mater and arguably the hub of American marine biology, tended toward traditional ichthyology and limnology. The University of Washington's School of Fisheries on the other hand focused initially on industrial concerns – canning and processing, harvesting and fish culture. Courses in classification and ichthyology were eventually added to the curriculum, but it was not until the 1930s, interestingly under Thompson's direction, that courses in statistics and stock assessment were added. Similar trends were apparent in Canadian fishery research.[29] Like Stanford, the University of Toronto's Fisheries Research Laboratory emphasized training in limnology and general aquatic biology, as did the University of British Columbia.[30] "There are no available men on this coast," complained Babcock. "This is our most immediate problem. The fact that the Biological Board of Canada had to go to Scotland to find a suitable man to place in charge of its Prince Rupert Station accentuates the difficulty we will have in getting such a force as we require."[31] Babcock recognized that this was more than a question of intellectual ability though. Fishery biology was still a field-oriented science, and as he correctly pointed out, "in addition to ability the men must have stamina – the guts – to work under conditions on halibut fishing boats."[32]

Finally, in late May, Thompson found three "young, able and energetic" researchers to assist with the fieldwork.[33] Henry Adam Dunlop, who studied zoology at the University of British Columbia before moving to the University of Toronto for his

graduate work in salmonid embryology, was appointed Assistant Director of Investigations. He was joined by F. Heward Bell, a young University of British Columbia biologist who, in addition to teaching, had spent time tagging salmon on the West Coast of Vancouver Island for the Fisheries Research Board of Canada. Roger Chute, an undergraduate biology student from the University of Washington, was also hired for summer research. Over the next two years, under Thompson's supervision, they collected pages and pages of data both on the ecology of halibut and on the economy of the fishery. With the fieldwork well underway by 1927 discussions within the IFC increasingly turned to the difficult question of regulations.

Embracing Uncertainty: An Argument for Experimental Management

"How to provide practical regulations for a depleted deep-sea fishery interferes with my sleep," yawned Thompson in early 1927. "But we stay with it."[34] Fishery theory was really just the beginning of the IFC's problems. The challenges they faced were further complicated by uncertainties and ambiguities in the data Thompson and his staff had collected. As discussed briefly in chapter one and in more detail above, Thompson's approach to fishery research was rooted in statistical analysis. In 1914 he discovered that crucial statistical information was readily available in logbooks kept by captains on company-owned steamers. However, changing environmental conditions in the fishery made it increasingly difficult to turn a profit in the fishery so that by the mid-1920s the company-owned streamers that dominated the early fishery had given way almost entirely to a new fleet of smaller, independently-owned gasoline-powered vessels better equipped to operate under less remunerative conditions. In 1925 just one steamer, the

New England, was engaged in the halibut fishery. Unfortunately for Thompson, the independent boats typically did not keep records. This was a "serious problem" since so much of his program depended on the availability of adequate statistics.[35] Some records were available but not many, and those that did exist were often poorly kept.

By the end of the first summer of fieldwork in 1925 it had become clear that the IFC would have to convince small boat owners to keep accurate records. In the fall they prepared and distributed specially made log books to vessel owners, and over time the data began pouring in. But there were subtle inconsistencies in the logs that raised concerns about the accuracy of the data recorded. For example, Thompson was especially interested in obtaining information on CPUE by geographic area. Information of this sort gathered during his initial work on halibut indicated how uneven the process of depletion had been, and suggested to him that an area-based approach to conservation might be in order. However a survey of halibut fishermen undertaken in 1925 and 1926 suggested that up to forty-percent of them intentionally misreported the areas from which their catches originated.[36] They did so for market reasons. Dealers simply paid more for fish from certain areas, a problem that would continue to undermine the IFC's effort to gather scientific information on the fish and the fishery. This was a fairly straightforward problem, one that could be accounted for now that the IFC had some sense of the possible errors in their data. On the other hand it introduced a degree of uncertainty into the scientists' work and raised serious questions regarding the reliability of the data they had been collecting. Were there similar problems with other data sets?

Serious methodological problems also undermined the IFC's efforts to better understand halibut biology. Many of the techniques Thompson used to study halibut were

developed by European biologists working with plaice, cod and herring in the North Sea, and it was not at all clear to what extent they could be used to study halibut in the Northeast Pacific.[37] The methodological fit was so poor, in fact, that Thompson spent all of 1925 and part of 1926 simply adapting data collection techniques to halibut and to the working conditions met with in the Pacific. But this was just part of the problem. As time went on, interpreting the data became more and more difficult. "The developments have been very interesting indeed and have rather revolutionized what I would have said a month ago." Thompson told the IFC in 1927. "We find the collecting of our results somewhat similar to reading a novel since the aspect of things is constantly changing and full of interest. The work is at a stage when new things come to light fast enough to render anything I might write at one moment possibly out of date the next."[38] What kinds of policies could be crafted from such uncertainty?

The best example of this kind of "methodological uncertainty" came from Thompson's fish tagging and "racial" studies. In the 1870s biologists began using "tags" to study the migratory habits of marine fish and so-called "racial characters" to identify sub-populations.[39] Tagging was based on the simple idea that if fish caught, tagged, and released along the coast of England were later recaptured along the coast of Norway, then the fish must have migrated there. Thompson's early work on halibut suggested that it might be a non-migratory species, but lacking direct evidence he could not be sure. Fish tagging could provide the proof he needed.

The measurement of "racial characters" was designed to detect what biologists called "races" or sub-populations, but it also provided indirect data on the migratory habits of marine fish. Darwinian in origin, and developed largely by biologist Frederick

Hiencke to study North Sea herring, it was based on the idea that relative geographic isolation within a species produced sometimes subtle but noticeable differences in the physical appearance of fish. Statistically significant differences in the number of vertebrae, fin rays, in growth rates, head size and shape and so on, were all thought to reflect the extent to which fish from one area were related to, or 'intermingled' with fish of the same species from another. Thompson's early work on halibut suggested that the population was composed of numerous sub-populations. A detailed study of racial characters, it was hoped, would determine whether this was in fact the case.

Thompson presented his preliminary findings to the IFC in mid-July, 1927.[40] Tagging and racial studies combined with physical inspection of the commercial catches suggested that Pacific halibut stocks were composed of two major populations, one north and one south of Cape Spencer Alaska. These in turn were composed of numerous, sub-populations. But unlike southern fish, which appeared to be non-migratory, the northern fish were considered highly migratory, at least within their range. Again an area-based approach to conservation seemed to be in order. "We can propose regulation of definite areas and we can treat each separate area more or less independently," Thompson insisted. "Now that is an essential point. We can take those depleted banks and protect those fish area by area, if we chose, up until the time they reach maturity."[41] What exactly to do once they reached maturity was a problem, but not the most important one.

In truth, Thompson had very little reliable data upon which to base his claims. For example, having spent much of the summer testing various tags he noted that "none of the tags in the literature was totally suitable for halibut."[42] The so-called "disc tags " used by Petersen to study plaice and cod caused "large sores," and "many simply fell out."[43]

Also, the rate of return varied depending on the type of tag used, and in some cases a single tag produced different rates of return in different areas. The rate of return for "tag A" was 40 percent for fish marked off Cape Chacon, but just 1 percent for fish marked off west coast the Queen Charlotte Islands.[44] Given that the fisheries in these areas were of roughly equal magnitude, fishermen were unlikely to catch tagged fish in one more than the other. So the discrepancy was difficult to explain. Were some stocks more migratory than others? "Tag C" on the other hand gave high returns because it was designed to be "more visible" to fishermen, but it appeared "injurious" to the fish and many were close to falling out at the time of capture.[45] Did the tags, some of which were clearly hurting the fish, interfere with their natural movements? These early results were difficult to interpret and Thompson urged that "in view of these disparities" they be used with "caution."[46] In fact he would ultimately test four or more tags during 1925 and 1926 before settling on the "strap tag" used by the United States Bureau of Fisheries to study Atlantic cod.

Still the resulting data were difficult to interpret. By the end of the second season of tagging, it appeared that halibut was a relatively non-migratory species of fish as Thompson had earlier suspected. He noted however that "a scattering of fish have traveled considerable distances" but that "since our examination of the fish has been substituted for a simple return of tags the number of long migrations has dropped."[47] Maybe 'long migrations' were more common than the tagging studies suggested but simply fell out the data because fishermen felt returning the whole fish was not worth the effort?

Thompson also wondered whether the tags reportedly from one area actually came from that area. Fish dealers collected tags on behalf of the IFC, paid fishermen $1.00 per tag, and recorded where the fish were recovered. The problem with this method of collection was that the dockside dealers paid more for fish from certain areas. Fishermen naturally wanted the best possible price for their catch. Did they intentionally misreport the origin of their catches? Thompson seemed to think so: "It is the most questionable feature of our tagging experiments that doubt enters in as to the accuracy of the returns of the fishermen."[48]

There were other concerns as well. Norman B. Scofield, the CFGC biologist appointed to the IFC's scientific advisory board, wondered whether the fish tagged were representative of the population as a whole. Although Thompson intended to tag fish on the southern and the northern fishing banks, persistent funding problems kept him from doing so. The majority of the tagging experiments were conducted during the summer months and on the banks south of Cape Spencer only. Were there unknown seasonal migrations that could not be identified in the data? For that matter, were there really two separate populations? The Advisory Board accepted Thompson's conclusions but they did so with some reservation. "The work done gives us able conclusions regarding the southern or older depleted banks and for the young fish but the problem of the mature fish on the western or newer banks has been but barely gotten under way," they noted. "We feel this should be prosecuted vigorously while the effect of regulation on the older grounds is being tested."[49]

But even if the rates of return were accurate and the fish tagged reasonably representative of the species as a whole, there was still the more general problem as to

whether the data reflected the migratory habits of the fish or simply reflected those of the fishery, or both. Thompson noted during his early work on halibut for the BC Fisheries Department that halibut fishermen tended to move quickly from bank to bank in search of fish. Having found a productive area the fleet would usually concentrate there until catch rates fell, at which time they would move on. This was a serious problem since tagged fish could only be recovered where there were active fisheries. Thus their data had charted not just the migratory habit of the fish, but those of the fishery as well. "Recovered tags reflect to a large extent the distribution and intensity of the fishing in the regions to which the halibut penetrate" Thompson wrote in 1927." Were the intensity of the fishing but slightly variable it might be possible to ignore its effects but this is far from true of the halibut fishery. The consequence is that the halibut seem to have migrated in the direction of the locality in which the intense fishery took place."[50]

Thompson clearly anticipated some, though by no means all, of these problems. After all, the study of racial characters was intended to produce additional, indirect evidence that could be brought to bear on the question of sub-populations and migrations. But here too there were similar kinds of problems. As with tagging, methods for studying racial characters had to be adapted to suit Pacific halibut. "Our work will first be directed toward discovering the possibilities of research in this field and toward outlining the methods necessary."[51] Thompson explained. "So far as we know racial measurements have never before been carried out on such large fish and under such conditions as we meet."[52] Research on the racial characters of fish involved first building an apparatus to hold the halibut in place while Thompson and the others took their measurements. Equipment of this sort was particularly important since work on racial characters required

a degree of precision not easily obtained on slimy-decked halibut vessels bobbing up and down in the Northeast Pacific.

Building adequate equipment was just the beginning. Since this was the first detailed study of racial characters in Pacific halibut, Thompson's staff had to establish biological criteria upon which to base their judgements. In the North Sea, biologists depended on vertebrae counts to draw conclusion about races of plaice. Although plaice and halibut are closely related, Thompson was sure that the number of vertebrae in halibut probably did not vary enough to draw any firm conclusions on the existence of sub-populations. Countless halibut were eventually examined before he and his staff finally decided that head length and shape, body thickness, number of fin rays, and growth rates were useful biological categories to study.[53] All of this took considerable time, something the IFC did not have much of in the first place. Indeed, as late as August 1926 Thompson told Babcock that it was "still too early to say much" about the existence of local races, further noting that "the instruments and methods used are as yet inadequate."[54] Thompson believed that there were races of halibut but, in fact, he had very little data upon which to base his claim.

Taken together the many theoretical and methodological problems associated with the IFC's scientific program left Thompson in the unenviable but largely unavoidable position of having to make regulatory recommendations in the face of profound uncertainty. What would he do? More research might help. But could the IFC really afford to waffle about, waiting for some answers on the relationship between fishing effort and stock productivity, or pondering its tagging studies, with the halibut stock

seemingly on the verge of collapse? No. Not making recommendations was not an option. Thompson's solution was simple, pragmatic, and downright ingenious:

> The time will soon come when the Commission must decide upon what recommendations it will make for the future of the halibut work. In their nature these must either be specific and fixed in character, designed each to produce a definite effect by methods supposed to be adequate; or they must be so framed as to provide for a flexible system of observation and control capable of meeting competently circumstances or results at present unknown and unforeseeable and changes in them. To me there appears to be absolutely no question but that the second of these alternatives is the only feasible one. Whatever this flexible system of control may be it must provide for the proper framing of measures which we must acknowledge to ourselves to be experimental. There can be nothing more fatal than a refusal to face the facts. If a definite effect is promised but actually cannot be attained, and if the methods of regulation are adopted blindly and ignorantly, disaster lies before us, and international regulation will meet discredit.[55]

Thompson used three years of data on the economy and ecology of the closed season to further support his argument that the IFC adopt an explicitly experimental approach to managing the fishery. In a report appropriately titled "Conservation of Pacific Halibut" Thompson suggested that the closed-season could be thought of as a kind of experiment, an ill-conceived, uninspired and scientifically skewed experiment, but an experiment nonetheless.[56] What were the results of that first experiment? First by eliminating expensive winter fishing it made the fishery more profitable, though not so profitable that Hagar's steamers could keep fishing, but more profitable nevertheless. Second, and related, it encouraged a more intense open season fishery, thus exacerbating the decline in the halibut population. In 1927 the halibut fishermen set 844,101 units of gear, a considerable increase over the 580, 403 units of gear used in 1922 before the closed season came into effect. Aggregate annual landings also increased after the first closed

season from 51,000,000 pounds in 1923 to a projected 57,000,000 pounds in 1928. This could not have been due to an increase in the halibut stock because CPUE – 49 lbs. and 87 lbs. on the southern and northern banks respectively in 1927 – was still falling. Fishermen were simply fishing harder than ever, catching more fish in less time.

Ironically Thompson, who predicted in 1917 that the closed season would further undermine the halibut's capacity to reproduce, used the example of the closed-season to show how the effects of management could be unpredictable. In principle, though, he was proposing what resource managers now call "adaptive management."[57] Contemporary fishery biologists often argue that scientific uncertainty coupled with biological complexity simply precludes a rigid, predictive approach to management. Resource policies, they argue, must be thought of as experiments, the results of which are unforeseeable in principle. In sum they regard uncertainty and complexity to be more like occupational hazards one has to learn to live with than as problems to be solved by more scientific knowledge. But this cannot be said to be an entirely new idea. Thompson, too, stressed that science was uncertain, that marine ecosystems were complex, that resource policies should be thought of as experiments, and that a responsive resource management agency is crucial to conserving stocks of fish. His argument that management be adaptive and experimental was his way of embracing uncertainty and coping with complexity in both natural and human systems.

Thompson Proposes an Experiment

The closed season was clearly failing to conserve halibut stocks. But what would be the IFC's first experiment in adaptive management? The goals of the Commission,

Thompson often noted, were fairly straightforward even if the means of achieving them were not. Ultimately they would seek to restore the halibut population, and to limit the fishery to its theoretical maximum long-term yield – whatever that might be – by carefully balancing fishing effort with stock productivity. This would take time, not to mention much trial and error. The more pressing problem though was simply how to stop the decline in the fishery.

Thompson, a "glutton for work" and "showing it physically" by 1928, had spent much of the past three years poring over the many piles of data he and his staff had collected looking for answers.[58] Finally, he detected a pattern, and in it the possibility of stabilizing the fishery.[59] Since the beginning of commercial fishing in the late 1880s, Thompson observed, the fishery seemed to have passed through three major increases in fishing effort. The first of these began in 1895 following the completion of transcontinental railways linking Pacific coast halibut producers with halibut-hungry markets in the eastern United States. The second occurred between 1910 and 1913 when steam and gasoline powered boats began tapping into rich offshore stocks of halibut. A third expansion occurred between 1918 and 1921 when newer and more cost-efficient vessels equipped with diesel engines, powered winches and electric lights began fishing for halibut. In each case a sharp increase in fishing effort produced an immediate but short-lived increase in overall landings, a decrease in overall abundance as measured by CPUE, and finally, a measure of stability at some lowered stock size. Here Thompson believed he was seeing "the dreaded overfishing" in plain view.[60] But what would happen if these periodic increases in fishing effort were to stop? Some scant evidence, but

enough for Thompson, indicated that the decline in the halibut population might come to a halt:

> As you will remember, the total catch on the older halibut banks has fallen to approximately 40% of what it was in 1910. Thus the catch tends to restrict itself at a very considerable rate and it may be that a regulating Commission would have a hard time keeping ahead of depletion. But turning to the increase in gear used you will find that 2 ½ times as many skates are used now as were used in 1910. This increase is what is injuring the banks. Were the amount of gear to have been held constant, the catch now would have been about 8 millions of pounds from these older banks instead of about 21 million. The reduction from 50 million to 21 million was over-fishing. The Commission would have been hard put to it to make such a restriction. The moral of this is that we must express our restrictions in terms of amount of gear used. The mere holding of this stationary from one year to the next will prevent the most serious factor we have to deal with – the increase in intensity of effort, and might be sufficient to bring the decline to a halt, the first problem of the Commission.[61]

But why not go one step further and reduce the number of units of gear in the fishery? asked John Pease Babcock. "In view of the evidence our position is a weak one. The fishery cannot be maintained without materially decreasing the catch, you have demonstrated this. As I view it, the need of immediately curtailing the catch is so great that we should show our appreciation of the fact and show we had the courage to face it."[62] Thompson could not have agreed more. Yet he was convinced that any attempt to go beyond a modified closed season would meet with serious objection, and with considerable justification. Not long before Alvah Hagar and the halibut industry had thwarted his area-based conservation scheme. "Upon leaving you with my recommendations the thing that worried me most, was the fact that any real restriction would be bitterly opposed, or at least, held to minimum," he replied. "We must keep in mind that the fleet and the trade think in terms of established interests. Were the present

fleet secured against reduction in gear, they would ask nothing more fair. Any decline in the catch would be rightly ascribed to overfishing and not held against the commission."[63]

Thompson's plan was impressively crafty, and demonstrated his growing sense that conservation and commercial interest were of a piece in the fishing industry. By adopting his approach the IFC could begin to regulate the fishery without being seen to regulate it at all. If stock abundance continued to fall, and it might, then a reduction in fishing effort would be in order. But everyone would see the effects of overfishing *as overfishing.* Then they could go the extra mile and reduce fishing effort. The rub was that the IFC had no regulatory authority and so was not in a position to restrict anything. Anything more than a modified closed season would likely require a new treaty.

The IFC submitted a final report outlining its vision for halibut conservation policy in April of 1928.[64] Its major recommendations were that it be given authority to divide the fishing grounds into zones and to regulate the annual catches therein on a bank to bank basis; that two areas believed to be nurseries for juvenile halibut be closed; and that all halibut boats and halibut dealers be licensed for statistical purposes. The IFC recommended keeping the closed season; they even suggested extending it by two weeks at the request of industry. The reason: it was what halibut fisherman wanted, and Thompson understood as well as anyone how important industry support could be.

Reaction to the report was immediate and, much to the IFC's dismay, surprisingly unfavourable. "There is considerable distrust amongst the fishermen," wrote Commissioner William Found late in 1928. "Many are appalled at the idea of placing a sea-fishery in the hands of four men."[65] The politically powerful Fishing Vessel Owners

Association (FVOA), representing more than three-quarters of the American fleet, even challenged the IFC's research program. First they argued the decline in the fishery was not as serious as the CPUE data suggested, because it did not account for changes in long-line gear – wider hook-spacing and fewer hooks – that occurred when vessel fishing replaced dory fishing, and which they alleged reduced overall gear efficiency. Second, they argued that halibut were probably far more migratory than Thompson's limited tagging studies suggested. And third, they insisted that it was simply too early to pass judgement on the closed season, since the spawn so far produced would not be entering the fishery for at least another four years.[66]

Convinced, however, that conservation needed to be taken in new directions, the Commissioners continued to press federal officials for a new treaty granting the IFC the power to regulate the halibut fishery. "If we cannot have the power to say how, when, and where fishing shall be conducted, I can see no point in future existence," argued Commissioner Henry O'Malley early in 1929. "If authorization is not given, we can only continue to tell the world how the depletion of the halibut has occurred, but I would not consider that a very effective conservation program. This may be the first attempt a body has made to regulate sea fisheries, but if we are to have any sea fisheries in the future others besides halibut will need attention and that mighty soon so I feel we might as well make a start now rather than hedge."[67] It was compelling argument, not least because it was already widely acknowledged that shared Pacific salmon stocks were also suffering and in dire need of conservation.[68]

John Pease Babcock, meanwhile, fired back at the FVOA. He insisted that all changes in halibut gear had been adequately accounted for; that the IFC had "conclusive

evidence" showing that halibut stocks south of Cape Spencer were non-migratory; and that the closed-season was inadequate.[69] "These are inescapable facts," Babcock confidently declared.[70] Nothing, of course, could have been further from the truth. But admitting as much would have seriously undermined the IFC's bid for regulatory power, and no one, Thompson included, was willing to risk that.[71] Babcock's counter-attack apparently worked. In 1931 a revised Halibut Treaty empowering the IFC was ratified by Canadian and American politicians. The first regulations came into effect the following February.

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[1] C.f. Kurkpatrick Dorsey, *The Dawn of Conservation Diplomacy U.S.-Canadian Wildlife Protection Treaties in the Progressive Era*, Seattle: University of Washington Press, 1998; Margaret Beattie Bogue *Fishing the Great Lakes: An Environmental History,* Madison: The University of Wisconsin Press, 2000.
[2] F. Heward Bell, "Agreements, Conventions and Treaties between Canada and the United States with Respect to the Pacific Halibut Fishery," *International Pacific Halibut Commission* 1969, Report No. 50, p. 25- 47.
[3] For a discussion of the ICES research program see Tim Smith, *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855 – 1955,* Cambridge: Cambridge University Press, 1998 pp. 110-162; and D.H. Cushing, *The Provident Sea* Cambridge: Cambridge University Press, 1988, pp. 203-212.
[4] British Columbia Archives, GR 0435, BC Department of Fisheries, box 198, file 5, John Pease Babcock to William A. Found 31 October, 1924.
[5] C.f. William F. Thompson, "The Scientific Investigation of Marine Fisheries, as related to the work of the Fish and Game Commission in Southern California," *CFGC Fisheries Bulletin* 1919 (2), p. 3-27; William F. Thompson, "Notes from the State Fisheries Laboratory," *California Fish and Game* 1920 6 (4), p. 177-182. Also see Arthur McEvoy, *The Fisherman's Problem: Ecology and Law in the California Fisheries, 1850 – 1980,* Cambridge: Cambridge University Press, 1986, p.158-163.
[6] John Pease Babcock, "International Commission Starts Work," *Pacific Fisherman* 1924 22(12), p. 8.
[7] British Columbia Archives, GR0435, BC Department of Fisheries, Box 198, File 6, John Pease Babcock to William F. Thompson, 21 March 1925.
[8] British Columbia Archives, GR045, BC Department of Fisheries, box 202, file 12, John Pease Babcock to William A. Found, 7 February 1925.
[9] University of Washington Archives, William Thompson Papers accession no. 2597-3-83-21, box 3, "Fishery Treaties Between the US and Canada," n.d. (probably late 1950s), 7.
[10] In 1883 Thomas Huxley famously argued that "The cod fishery, the herring fishery, the pilchard fishery, the mackerel fishery, and probably all the great sea fisheries, are inexhaustible; that is to say that nothing we do seriously affects the number of fish. And any attempt to regulate these fisheries seems consequently, from the nature of the case, to be useless." Quoted in Michael Graham, *The Fish Gate* London: Faber, 1943.
[11] Tim Smith, "Stock Assessment: The First Fifty Years," in *Fish Population Dynamics* J.A. Gulland Ed. New York: John Wiley and Sons, 1988, p. 13.
[12] C.G.J. Peterson, "On the Biology of our Flatfishes and on the Decrease of Flatfish Fisheries," Report of the Danish Biological Association 1894 no.4 1-37; C.G.J. Peterson, "Increased Fishery by Transplantation of Plaice," *Report of the Danish Biological Association* 1896 no. 6, p. 49-82; C.G.J. Peterson, "The Yearly Immigration of Plaice into the Limfjord from the German Sea," *Report of the Danish Biological*

Association 1896 no.6, p. 1-48; C.G.J. Peterson, "What is Overfishing," *Journal of the Marine Biological Association 1903* no.6, p. 587-594

[13] C.f. Smith, *Scaling Fisheries*, p. 72 – 94.

[14] Johan Hjort, "Report Regarding the Herring," *Rapports, Conseil Permanent International pour l'Exploration de la Mer* 1910 (4), p. 1-24; Johan Hjort, "Fluctuations in the Great Fisheries of Northern Europe," *Rapports, Conseil Permanent International pour l'Exploration de la Mer* 1914 (20), p. 1-83.

[15] Johan Hjort, "Fluctuations in the Great Fisheries of Northern Europe," Rapports et Proces-Verbaux, 1914, vol. XX, p.202.

[16] For a review of the problems posed by fluctuating fish stocks see Thompson, "The Scientific Investigation of Marine Fisheries, as Related to the Work of the Fish and Game Commission of Southern California," Fish Bulletin (2), p. 19-26.

[17] British Columbia Archives, GR0435, BC Fisheries Department, box 202, file 8, William Thompson to IFC, 27 March 1925.

[18] British Columbia Archives, GR0435, BC Department of Fisheries, "Preliminary Statement Outlining Proposed Investigation of Halibut," 17 January 1925.

[19] Thompson, "Scientific Investigation," p. 4.

[20] For an overview of fishery research since 1850 see J.L. McHugh, "Trends in Fishery Research," *in A Century of Fisheries in North America* Norman G. Benson Ed. American Fisheries Society Special Publication No. 7: 25-56; and Smith, *Scaling Fisheries*.

[21] British Columbia Archives, GR0435, BC Fisheries Department, box 202, file 10, William Thompson to John Pease Babcock, 20 January, 1925.

[22] Ibid.

[23] C.f. Peter A. Larkin, "An Epitaph for the Concept of Maximum Sustainable Yield," *Transactions of the American Fisheries Society* 1977 106(1): 1-11; Peter A. Larkin, "Fishery Management: An Essay for Ecologists," *Annual Review of Ecology and Systematics*, 1978 (9), p. 57-73.

[24] C.f. Larkin, "Fishery Management."

[25] C.f. A.J. Constable, W.K. De La Mare, D.J. Agnew, I Everson, and D. Miller, "Managing Fisheries to Conserve the Antarctic Marine Ecosystem," *ICES Journal of Marine Science* 2000 57(3), p.778-791; P. Mace, "A New Role for MSY in Single Species and Ecosystem Approaches to Fisheries Stock Assessment and Management," *Fish and Fisheries* 2001 2(1), p. 2-32; S. Mayfield and G.M. Branch, "Interrelations among rock lobsters, sea urchins and juvenile abalone: implications for community Management," *Canadian Journal of Fisheries and Aquatic Sciences* 2000 57(3), p.778-791; P. Olsson and C. Folke, "Local Ecological Knowledge and Institutional Dynamics for Ecosystem Management," *Ecosystems* 2001 4(2), p.85-104; D. Witherell, C. Pautzke and D. Fluharty, "An Ecosystem-Based Approach for Alaska Groundfish Fisheries," *ICES Journal of Marine Science* 2000 57(3), p.771-777.

[26] Historian of Science Theodore Porter has written about the role of quantitative methods in the social sciences in his *Trust in Numbers: the pursuit of objectivity in science and public life*, New Jersey: Princeton University Press, 1995.

[27] Kenneth Carlander, "Fishery Education and Training," in A *Century of Fisheries in North America* Norman G. Benson Ed. American Fisheries Society Special Publication No. 7, p. 57-70.

[28] British Columbia Archives, GR0435, BC Fisheries Department, box 202, file 9, William Thompson to John Pease Babcock, n.d. (probably spring 1925).

[29] Carlander, *Fishery Education.*

[30] Ibid.

[31] British Columbia Archives, GR0435, BC Fisheries Department, box 198, file 10, John Pease Babcock to William A. Found, 26 March 1925.

[32] British Columbia Archives, GR0435, BC Department of Fisheries, box 198, file 7, John Pease Babcock to William A. Found, 1 April, 1925.

[33] Babcock, International Commission Starts Work," 8.

[34] British Columbia Archives, GR0435, BC Department of Fisheries, box 204, file 4, William Thompson to John Pease Babcock, 27 March 1927.

[35] British Columbia Archives, GR0435, BC Department of Fisheries, box 202, file 3, William Thompson to John Pease Babcock, 17 September, 1925.

[36] British Columbia Archives, GR0435, BC Department of Fisheries, box 204, file 5, "Meeting of the International Fisheries Commission, 24 June 1927," 14-22.

[37] For a discussion of the European fishery research see Smith, *Scaling Fisheries* p. 51-61; 70-162.
[38] British Columbia Archives, GR0435, BC Department of Fisheries, box 204, file 11, William Thompson to John Pease Babcock, 16 May, 1927.
[39] Smith, "Stock Assessment."
[40] British Columbia Archives, "Meeting of the International Fisheries Commission."
[41] Ibid., 11.
[42] British Columbia Archives, GR0435, BC Department of Fisheries, box 202, file 7, William Thompson to John Pease Babcock 13 July 1925.
[43] British Columbia Archives, GR0435, BC Department of Fisheries, box 203, file 5, William Thompson to John Pease Babcock 29 May, 1926.
[44] British Columbia Archives, GR0435, BC Department of Fisheries, box 202, file 10, William Thompson to the IFC 12 January, 1926.
[45] Ibid.
[46] British Columbia Archives, GR0435, BC Department of Fisheries, box 202, file 12, William Thompson to John Pease Babcock, 29 May, 1925.
[47] British Columbia Archives, GR0435, BC Department of Fisheries, box 203, file 12, William Thompson to the IFC 23 September, 1926.
[48] Ibid.
[49] British Columbia Archives, GR0435, BC Department of Fisheries, box 205, file 4, Scientific Advisory Board to the IFC 18 November 1927.
[50] William Thompson, "The Function of Tagging in the Pacific Halibut Investigation," *Pacific Fisherman* 1925 24(1): 10-11.
[51] British Columbia Archives, GR 0435, BC Department of Fisheries, box 202, file 4, William Thompson to John Pease Babcock, 17 September 1925.
[52] Ibid.
[53] Ibid.
[54] British Columbia Archives, GR 0435, BC Department of Fisheries, box 203, file 14, William Thompson to IFC 23 September 1926.
[55] W.F. Thompson to IFC n. d. GR0435 B. C. Department of Fisheries BCARS plea for experimental approach no date
[56] "Conservation of Pacific Halibut," British Columbia Archives, GR0435, BC Department of Fisheries, box 204, file 7.
[57] C.f. C. S. Holling, *Adaptive Environmental Management* Laxenberg: International Institute for Applied Systems Analysis Laxenberg, 1978; K.N. Lee, *Compass and Gyroscope: Integrating Science and Politics for the Environment*, Washington: The Island Press: 1987; C.L. Smith, "Sailing the Shoals of Adaptive Management: The Case of Salmon in the Pacific Northwest," *Environmental Management* 1998 22(5), p, 671-681; Carl J. Walters and C.S. Holling, "Large-Scale Management Experiments and Learning by Doing," *Ecology* 1990 71(6), p. 2060-2068.
[58] British Columbia Archives, GR 0435, BC Department of Canada, box 198, file 3, John Pease Babcock to Henry O'Malley 20 December, 1927.
[59] British Columbia Archives, GR 0435, BC Department of Fisheries, "The Scientific Investigation of Marine Fisheries," box 204, file 3, (n.d. probably January 1928.)
[60] Ibid.
[61] British Columbia Archives, GR 0435, BC Department of Fisheries, box 205, file 5, William Thompson to John Pease Babcock 1 March 1928.
[62] British Columbia Archives, GR0435, BC Department of Fisheries, box 205, file 5, John Pease Babcock to William Thompson, 12 March 1928.
[63] British Columbia Archives, GR 0435, BC Department of Fisheries, box 205, file William Thompson to John Babcock (n.d probably mid-to-late March, 1928).
[64] British Columbia Archives, GR0435, BC Department of Fisheries, "Report of the International Fisheries Commission," box 205, file 6.
[65] British Columbia Archives, GR 0435, BC Department of Fisheries, box 205, file 8, William Found to John Babcock, 12 November, 1928.
[66] "Halibut Vessel Owners Object to Proposed Regulations," *Pacific Fisherman* 1929 27(1), 15-16.

[67] British Columbia Archives, GR 0435, BC Department of Fisheries, box 202, file 11, Henry O'Malley to John Pease Babcock 11 February, 1929

[68] The IFC commissioners often wrote that a new halibut treaty might finally clear the way for a salmon treaty.

[69] John Pease Babcock, "Halibut Commission Justifies Regulatory Proposals," *Pacific Fisherman* 1929 27(3), 13-15.

[70] Ibid.

[71] This is not at all to say that the FVOA analysis was correct, but rather that facts of the sort described by Babcock were hard to come by.

Chapter 3
Troubled Waters:
The International Fisheries Commission,
1932-1960

Introduction: Commission Control

The first set of IFC regulations came into effect on 15 February 1932.[1] The winter closed-season, which prevented the capture of mature spawning fish in the Gulf Alaska, was continued as industry had requested. In addition two areas, one near Masset in the Queen Charlotte Islands and another near Timbered Islet, believed to be "nurseries" for "baby" or juvenile halibut were closed indefinitely. Finally, four broad administrative zones spanning the range of the fish and approximating the division of labor within the fishery were established. No restrictions were placed on fisheries in Area 1, south of Willapa Harbor, Washington which yielded less than one million pounds annually, or Area 4 in the Bering sea, where little or no halibut had been landed to date. The identification of this area probably reflected the IFC's sense that at some point in the not-to-distant future commercial fisheries might take hold there. It is clear that the IFC was mainly concerned with those fisheries taking place in Area 2, the older southern grounds between Willapa Harbor, Washington and Cape Spencer, Alaska, and those taking place in Area 3, the newer northern grounds between Cape Spencer and the Aleutian Islands.

Since the IFC's immediate concern was to prevent any further increases in fishing effort, setting quotas for each area was simply a matter of multiplying the number of units of gear being used in the fishery in the previous year by the average catch per skate in that year. Fishermen were forced off the banks once the legal limit had been reached. Thus, for example, the Area 2 quota for 1932 was set at 22,500,000 pounds, or roughly

548,130 (number of units in Area 2 in 1931) multiplied by 41.0 (catch per unit of gear in Area 2 in 1931 in pounds). The same calculation was used to set Area 3 quotas which rounded out at 21,000,000 pounds, but due to greater productivity and better overall biological conditions on the northern banks the IFC allowed for an additional three and a half million pounds, thus raising the quota for the northern grounds to 23,500,000 pounds.[2]

However it is important to note that considerable fishing effort had moved out of the fishery owing to poor economic conditions beginning in 1929, and that the 1931 data used to calculate the first quotas were not at all representative of the fishery during most of the 1920s. Between 1929 and 1931 the number of skates used on the southern grounds fell from 653,085 to 548, 130. On the northern grounds the fall was from 416,000 to 290,000.[3] Thus, while the combined total allowable catch of 46 million pounds set for 1932 was about 3 million pounds higher than that landed in 1931, a depression year characterized by strikes and prolonged voluntary lay-ups, it was considerably lower than landings in any one year between 1920 and 1930.[4]

Because Thompson's analysis of historic trends in abundance indicated that the decline in the fishery might have stopped had fishing effort been maintained at its 1928 level, there was now good reason to believe that the sizeable decrease in effort brought on by the Depression might be sufficient to raise the level of abundance somewhat. A less intense fishery, Thompson reasoned, not only meant that the average size of individual fish should increase since large numbers would survive to grow, but also implied more fish in absolute terms since far more individuals should reach spawning age. Thompson, cautiously optimistic at the best of times, continued to stress that no one really knew how

the population would respond. Abundance could continue to fall, he warned, and further reductions in fishing effort might be in order. For the moment all they could do was wait, and see.

More Fish, More Problems

They did not have to wait long. On the older and more depleted southern banks CPUE rose from an all time low of 35 pounds in 1930 to 52 pounds in 1933, an increase of nearly fifty percent. A similar, though less spectacular, trend was apparent on the northern banks where CPUE rose 30 percent from 64 pounds in 1930 to 84 pounds in 1933.[5] Word that stocks were improving spread quickly and industry, which was beginning to recover somewhat from the economic crisis of 1931, asked for a larger quota for 1934. Reluctant to put the needs of industry above those of the fish the IFC refused, cautioning fishermen that it was simply "too early" to tell whether the population was truly on the road to recovery.[6] This initial increase was probably due to the fact that individual fish were becoming older and hence larger on average due to the less intense fishery, as predicted. Nevertheless, conditions seemed to be improving and as early as September 1933 an unusually confident Thompson was able to assure readers of *Pacific Fisherman* that the IFC was "on the way to final mastery of its problem."[7]

Thompson, meanwhile, took a much needed break from fieldwork and focused his efforts on preparing scientific reports for publication and on fulfilling his duties as Chair of the University of Washington's School of Fisheries. He also took time to delve more deeply into fishery theory, in particular the work of Canadian biologist A.G. Huntsman and Russian biologist F.E. Baronov, both of whom were interested in how fishing-

induced mortality changed the biological composition – age, length and weight – of fish stocks. In 1934 Thompson and F. Heward Bell published what biologists now refer to as an "age-structured" model which linked yield and stock abundance to the intensity of fishing. More specifically, they developed a procedure for assessing the effects of various fishing intensities in which the survival of a given year-class from one year to the next was calculated arithmetically by applying a percentage mortality rate, the yield being computed simply by multiplying the number of fish at each age by the average weight of fish at that age.[8]

Their approach yielded two major insights. First, it suggested that stock abundance was inversely proportional to fishing intensity. Thus, for example, a 15 percent decrease in fishing intensity would produce a 15 percent increase in abundance and vice versa. This was crucial since it seemed to explain the early decline in the fishery in terms of simple overfishing and the remarkably quick recovery of the halibut fishery under restrictions: "It was expected, largely because of ideas regarding other fisheries, that the process of depletion and of replenishment would be a slow and time consuming process," Thompson told *Pacific Fisherman* in 1934. "The reverse however seems to be the case, the abundance varying almost inversely to the intensity of the fishery; the result is a simple and direct one."[9]

Second, it described mathematically how fishers could, over time, obtain "more fish with less fishing."[10] This was simply because individual fish would live longer. Not only would they become larger on average but more importantly, older individuals implied an increasingly larger stock of mature spawning fish and hence a larger population down the road. Thompson and Bell's message was clear: A less intense

fishery not only made good economic sense since profits would be spread over fewer units of gear, but also made good ecological sense since more and more fish would reach sexual maturity. More fish with less fishing – it seemed conservationists could have their fish and eat them too.

Like any model Thompson and Bell's was based on a number of assumptions, some of which were questionable, and three of which would haunt the halibut commission for many years to come. First, on their account the fishery took place in a kind of environmental vacuum where things such as oceanographic conditions, reproduction and recruitment, disease and parasites, and predator-prey relationships, never varied significantly from year to year and therefore never disturbed what Thompson called the "internal balance" between mature and immature fish in the population.[11] In essence this was a local expression of the more general "balance of nature" thesis which some historians and ecologists believe to be at the heart of modern science.[12] From there it was a small step to the argument that the halibut population was an entirely malleable entity that responded only to changes in fishing pressure – the one factor that did, and more importantly could be made to vary from year to year. The problem, of course, is that fisheries do not take place in isolation. Ecological conditions can and do vary significantly both in space and time.

Second, like so many of their colleagues, Thompson and Bell assumed that changes in catch-per-unit-of –effort were commensurate with changes in abundance. Thus, for example, a sixty- percent drop in CPUE was taken to indicate a sixty-percent decline in abundance. But there are any number of reasons why CPUE might rise or fall, none of which necessarily have anything to do with changes in abundance. Subtle

changes in technology, especially fishing gear, which often go unnoticed in an unregulated fishery may make fishing more or less efficient over time. Changes in hook–spacing on long-line gear, for example, may increase or decrease catch rates independent of any change in abundance. Subtle changes in ecology, in particular growth rates since CPUE is always given in pounds, can lead biologists to conclude that there are more or fewer fish now than in the past when in fact there may or may not be. In fact a sharp increase in growth rates (and hence CPUE) could occur while the number of fish in a population is declining. Conversely a sharp drop in growth rates (and hence CPUE) could occur while the number of fish in a population is increasing.

Third, Thompson and Bell used tagging studies to approximate fishing induced mortality, as did most if not all fishery biologists in the 1930s. If, for example, fishers returned ten percent of the tags released (which they did on the northern grounds) then that implied a fishing induced mortality rate of about ten- percent. But as shown in chapter 2 tagging studies yield results that are often equivocal at best, especially when it comes to the rate of return. Different tags yield different rates of return; the same tags yield different rates of return in different areas; and the migratory habits of the fishermen themselves can often indicate a much higher or lower rate of return than would have been obtained had the experiment been carried out using, for example, research vessels tagging and recovering fish at random. In short, ambiguities abound.

For all that Thompson and Bell's model seemed to explain the major changes that were clearly occurring in the halibut population, and was well received in the international fishery science community. It was so well received, in fact, that in 1936 Thompson was able to assure the IFC that "the Commission may well be proud of its

scientific results to date" adding that "any abandonment of these principles will receive the same criticism that an engineer meets who abandons sound principles in building a bridge."[13]

The 1930s also saw major changes within the IFC. Miller Freeman left the commission in 1932, and Henry O'Malley resigned a year later. They were replaced by Seattle lawyer Edward W. Allen and United States Bureau of Fisheries Commissioner Frank T. Bell. There were new members on the Canadian side as well. In 1936 William Found resigned, and John Pease Babcock retired bringing to a close a career in fishery administration spanning some forty-five years. They were replaced by George Alexander, recently appointed Commissioner of Fisheries for the Province of British Columbia, and the new Deputy Minister of Fisheries for the Dominion of Canada, A.J. Whitmore. Finally, William Thompson handed the halibut investigations over to Henry Dunlop, and in 1938 he left the IFC to become Director of Investigations for the new International Pacific Salmon Fisheries Commission (IPSFC).

Still, the IFC continued its policy of restricting fishing effort, and the halibut stock continued to grow. In 1942, just ten years after the first regulations came into effect, Dunlop and Bell were able to report a seventy-seven percent increase in CPUE on the southern grounds and an eighty-nine percent rise in CPUE on the northern grounds.[14] Catch per unit of effort continued to climb through the 1940s and by the end of the decade had reached levels not seen since the 1910s.[15] The recovery of halibut stocks was truly remarkable and was widely regarded as such.

The post-regulation period, however, also witnessed some less desirable changes in the fishery. On the one hand there was a dramatic increase in the number of halibut

vessels. By 1948 there were over 800 boats plying the halibut banks, a considerable increase over the 384 vessels fishing in 1933.[16] On the other hand there was an equally dramatic decrease in the length of the fishing season. Thus the Area 2 fishery lasted nearly 7 months in 1938 but by 1948 had shrunk to a mere 26 days. The same trend was apparent in Area 3 where the fishing season declined from 6 months in 1933 to just 60 days by 1948.[17]

Biologists F. Heward Bell and Henry Dunlop clearly recognized the ecological problems associated with the shortened fishing season. First, a temporally restricted fishery, it was pointed out, necessarily involved a geographically restricted fishery since fishermen could only travel so far in a given number of days. Being a largely non-migratory species of fish, this meant that fewer and fewer stocks of halibut were being forced to bear the brunt of the whole fishery. Second, the IFC found it increasingly difficult to account for halibut caught incidentally during the increasingly longer closed-season by fishermen fishing for black cod and other demersal species vulnerable to long-line gear.[18]

But there were also serious economic problems associated with the shorter fishing season, something about which Edward Allen, the Seattle lawyer on the IFC, was acutely aware. First, a shorter fishing season necessarily disrupted markets and price structures. The price paid for fresh halibut did not fall, Allen noted, but more and more fish was ending up on the frozen market, which had always been less lucrative. Second, the increasingly short fishing season implied a tremendous waste of capital, since halibut boats were spending more and more time sitting at the docks. The fishery needed to be

spread more evenly throughout the year, something Allen believed could be achieved by regulating economic aspects of the industry, in particular the rate of fishing.

Faced with a serious social and ecological problem Allen, who believed the short season was "unhealthy from every point of view, social, economic and otherwise," called for a more inclusive approach to halibut policy.[19] A new treaty signed in 1937 enabled the IFC to monitor more closely halibut caught incidentally during the much longer closed season but failed to address economic concerns.[20] Another Treaty was signed in 1953, this time authorizing the IFC to create multiple fishing seasons to combat "underfishing" in some areas, a phenomenon caused by the short season and believed to be undermining the halibut commission's commitment to "maximum sustainable yield."[21] Like the 1937 Treaty, its successor failed to address directly social and economic concerns.

Why did Allen's call for social and economic regulation fail? First, since its inception the IFC tended to draw a sharp distinction between conservation policy on the one hand and social policy on the other. "The primary function of the Halibut Commission is conservation of the halibut of the North Pacific," Henry O'Malley declared. "The Commission in making regulations should not subordinate itself to economic matters which have as their sole objective the stabilization of the industry."[22] Similarly John Babcock noted that "the International Fisheries Commission cannot and should not concern itself with economic matters. To attempt economic control of a great industry has plainly not been contemplated by the Governments of either country, and there is good reason to believe that had such economic control been proposed it would

have been decisively refused as being dangerous in character."[23] Social policy simply was not part of the IFC's mandate, and many obviously felt that it should not be.

Second, and related, Allen's call for economic regulation lacked support in the larger fishery conservation community, particularly among biologists who insisted that social matters were simply beyond the purview of conservation proper. In the early 1940s, for example, William C. Herrington and Roger Nesbit debated whether the State of Maryland ought to adopt a policy of limited entry to combat excessive competition in its fisheries, something many people felt was undermining fishermen's incomes.[24] Both authors drew examples from the Pacific halibut fishery to make their respective cases.

Like Allen, Roger Nesbit believed that in addition to meeting biological goals, conservation policy also needed to be about meeting social goals. "In the past," wrote Nesbit, "we conservationists have had a ready answer to the question 'what will this proposal do for the fish?' but we have never been able to give a satisfactory answer to the fisherman's natural question 'what will it do for me?'"[25] The IFC, he argued, was a case in point: it saved fish but sacrificed fishermen's incomes in the process by refusing to restrict the number of participants in the fishery. Thus, over time, more and more fishermen came to compete for a portion of a quota that had not increased significantly since 1933. Nesbit believed the solution involved adopting a more inclusive "fishery management" model, one that included explicit social and economic policies.

Herrington disagreed. In fact he felt that conservation defined primarily in terms of social objectives wasn't really conservation at all. "Our primary object is conservation" Herrington responded. "Improved social and economic conditions would be very important, but secondary objectives. If you wish to reverse this order of

importance then the problem is primarily for the economist or the sociologist instead of the biologist or conservationist."[26] Herrington also took issue with Nesbit's interpretation of events in the halibut fishery, finding it impossible to reconcile his argument that the fishery was becoming unprofitable with the fact that more and more vessels were joining the fleet each year. This of course was exactly Nesbit's point: the fishery was no longer profitable precisely because more and more vessels were entering the fishery. In any case, Herrington believed that conservation policy had little or nothing to do with social and economic policy – save perhaps in a negative way by identifying biological limits to growth – and therefore rejected Nesbit's "fishery management" model.

Third, even biologists who seemed to agree with Allen and with Nesbit's fishery management model were reluctant to embrace it because they feared that successful biological programs, such as that for Pacific halibut, would collapse under the political weight of contentious social and economic policy. "The [halibut commission's] method of regulation does not necessarily make for more profitable fishing and certainly puts no effective brake on waste of effort since an unlimited number of boats is free to join the fleet and compete during the short period that fishing is open," wrote well known Canadian biologist William Ricker. "However the stock is protected, and yield approximates to a maximum if quotas are wisely set; as biologists, perhaps we are not required to think any further. Some claim that any mixing into the economics of the matter might prejudice the desirable biological consequences of regulation by quotas."[27] The message from fishery biologists was simple and direct: we know how to manage fish stocks, they argued, but nobody knows how to manage people. The mere consideration of economic objectives could undermine all the biological good that had been done.

Fourth and finally, Allen's call for economic regulation lacked support among federal politicians in both countries and indeed within the IFC itself since it seemed to involve restricting access to the fishery, something some were loath to do. As William Found put it: "Not only are there inherent difficulties in deciding how much fish a particular boat might be allowed to land but the regulation must really have the effect of determining at the beginning of each season how many people may engage in halibut fishing, which is a public right."[28] More than a public right, open access in the fisheries was a matter of public policy in both countries.

In this context it was hardly surprising that Allen's bid for a more inclusive approach to conservation policy failed. The IFC was largely against it, federal politicians were against it, and many biologists were against it. For the moment the IFC rested easy knowing that "the fishery [was] both biologically and economically far improved over what it was prior to regulation and over what it would have been had it been allowed to run its uncontrolled course."[29] Both of these interpretations would be seriously challenged beginning in the late 1940s.

The Thompson –Burkenroad Debate

Many biologists, but not all of them, accepted Thompson's argument that overfishing was at the heart of the halibut problem, and that the fishery's remarkable recovery through the 1930s and 1940s was due to catch restrictions imposed by the IFC. In a 1934 essay reviewing the history of fishery biology Elmer Higgins, Chief Biologist for the United States Bureau of Fisheries, applauded the IFC's effort and predicted that "their achievement will rank high in the annals of natural science."[30] In 1942 E.J. Russell, a

noted British fishery biologist, concluded a series of lectures on "The Overfishing Problem" by declaring that "all fishery workers [were] grateful to the Commission and to its devoted staff under the leadership of Dr. W.F. Thompson for this impressive object lesson in fishery regulation."[31] And in a 1946 essay on the production and utilization of fish populations, William Ricker cited approvingly the halibut commission's work noting that "following the limitation of fishing effort [by the Commission] there has been an increase in the availability, the average size, the spawn produced, and hence possibility of the rate of recruitment of this long-lived and late maturing fish."[32] By all indications the IFC's conservation program had worked.

Then in 1947, Martin Burkenroad, Chief Biologist for the North Carolina Commission of Fisheries, argued that both the early decline in the halibut fishery and its later recovery were caused by a natural fluctuation in abundance probably brought on by a regular cycle of environmental change in the Northeast Pacific.[33] In arguing thus he not only called into question what many regarded as an almost unqualified biological success story, but in some sense he also questioned the very feasibility of fishery conservation.

Martin Burkenroad was an eclectic individual. In addition to being a formidable fishery biologist, he also published in astrophysics and anthropology, and was an accomplished poet. However he harbored a deep disdain for formal education and was openly critical of political and institutional interference in scientific research.[34] At least one of his colleagues considered him a "lone wolf" and he seems to have spent much of his career at the margins of the international fishery biology community.[35] Although Burkenroad made important contributions to the study of marine shrimp, some of which

have since become "classic," he is best known for his "radical views" on the ecological history Pacific halibut stocks.[36]

The dispute between Thompson and Burkenroad began during the spring of 1946, while Burkenroad was carrying out a scientific survey of the fishery resources of the State of North Carolina. Earlier that year he completed a study linking long-term changes in starfish abundance along the coast of New England to possible changes in environmental conditions.[37] Burkenroad suspected that such oscillations were probably more common than his single-species study suggested but went unnoticed by scientists because there was often little or no data to work with. Whether this led him to take up a similar study of halibut, for which there were ample data dating to the early years of the fishery, is unclear. But he was certain that natural fluctuations posed a particular problem for fishery biologists charged with conservation. How could they be sure that overfishing was in fact overfishing, and not simply a decline in abundance independent of human activity?

By June 1946 Burkenroad's "Changes in the Population of Pacific Halibut and their Significance" was ready for publication, but because of the "possibly controversial nature of the paper" his supervisor, Harden F. Taylor, forwarded an advance copy to Thompson for review.[38] "I find many things in it which are wrong," Thompson replied. "I am not impressed with the paper."[39] Convinced, however, that he was in fact on to something, Burkenroad pressed on. In October and December he forwarded additional drafts to Thompson, who had just returned from Alaska where he had been busy investigating Bristol Bay salmon fisheries as part of his work with the Fisheries Research Institute at the University of Washington. "I believe you will find that many of the

criticisms brought against my first draft will be inapplicable to the second," wrote Burkenroad, "and unless my reasoning from the data which you have published can be shown to be false, my actions ought not to be regarded as unjustified or illegitimate."[40]

Thompson disagreed, not so much on scientific principle, but on more or less ethical grounds. "The approach to these problems must be constructive and positive," he replied. "After all, the conduct of a major conservation project is being considered and neither you nor I have the right to take any hasty action that might tend to hinder a project of this sort, unless we are sure of ourselves. It means millions of dollars to the industry and great expenditures by the Governments."[41] Clearly much more than scientific principle was at stake and Thompson asked that the paper not be published, if not "for the good of the halibut fishery" then for the good of conservation generally, since "rationally developed, [the halibut program] may be of great importance to conservation of other like fisheries and other like species elsewhere."[42] Undaunted, Burkenroad went public with his analysis. The setting was a 1947 "Symposium on Fish Populations" organized by the University of Toronto.[43] For three days in January some of North America's leading biologists presented their views on the state of fishery biology at mid-century. Among them were William Ricker, A.G. Huntsman, R.E. Foerster, Daniel Merriman, and William Herrington a former IFC scientist who co-authored papers with Thompson in the early 1930s dealing with halibut tagging studies. Thompson was asked to participate but declined.[44]

Burkenroad's analysis of the early decline in the halibut population was based on a comparison of changes in CPUE with actual withdrawals by the fishery between 1915 and 1927 in Area 3, the northern grounds between Cape Spencer, Alaska and the Aleutian

Islands. Assuming that returns from tagging approximated fishing induced mortality, Burkenroad calculated a stock size of 300 million pounds in 1927, or 30 million pounds (landings in 1927) divided by 0.10 (10 percent fishing mortality rate for the northern fishery). Assuming also that changes in CPUE were commensurate with changes in abundance, Burkenroad calculated that the stock in 1915, when the northern fishery began, was 900 million pounds or 3 times that in 1927 since CPUE had fallen by two-thirds over that period. However total landings between 1915 and 1927 were a mere 284,000 million pounds, less than half the apparent decline of 600 million pounds on the northern banks. Thus removals by the fishery could not have been responsible for the early decline in the abundance.[45]

A comparison of changes in CPUE with actual withdrawals by the fishery during the post regulation period revealed a similar disparity. Thus Burkenroad was unable to reconcile the tremendous increase in CPUE (hence abundance) through the 1930s and 1940s with the fact that average landings as a percentage of total population were slightly higher during the recovery years than in the 1910s and 1920s when the population was in decline. How was it, Burkenroad wondered, that a stock averaging 375 million pounds during the period 1932 – 1944 and subject to a fishing induced mortality of seven-percent per year nearly doubled while a stock averaging 488 million pounds during the period 1915 – 1926 subject to a fishing induced mortality of only four-percent declined by two-thirds? It was highly unlikely, Burkenroad concluded, that catch restrictions had any more to do with the recovery of the halibut population than overfishing had to do with its decline.[46]

William Herrington disagreed with Burkenroad's interpretation of the data. He questioned Burkenroad's use of tagging studies to arrive at a fishing induced mortality rate of ten-percent on the northern grounds. "If you had a thousand fish," Herrington asked, "and you got a 100 back and 50 tags fell off, then the mortality would be heavier than that wouldn't it?"[47] This was a crucial question because if fishing mortality was believed to be say, twenty percent, Burkenroad's calculations would produce a stock size of 450,000,000 pounds in 1915 and 150,000,000 pounds in 1927 a difference of 300,000,000 pounds, a figure very close to the 284,000,000 pounds actually taken by the fishery. The problem, of course, was that the IFC used the ten-percent mortality rate derived from tagging studies to prove that the fishery was being severely depleted. Burkenroad was thus able to respond to Herrington's challenge with ease. "You yourself named that ten percent fishing rate," Burkenroad fired back. "If you and Thompson are going to use that in coming to the conclusion that the fishery has been responsible for the decrease in abundance, then it is legitimate for me to use it in an analysis of your conclusions."[48] Herrington was right to point out the problems associated with tagging but in doing so he revealed a major weakness of the Halibut Commission's work.

C.H.D. Clarke, a biologist with the Ontario Department of Lands and Forests, questioned whether changes in CPUE were in fact commensurate with changes in abundance.[49] This was absolutely crucial because this had been the base assumption of all fishery theory since the early 1900s. Burkenroad admitted that he too had doubts about the relationship but simply pointed out that "if this assumption is questioned than it bears equally against Thompson's conclusions. In other words, if catch-per-unit is not a rectilinear index of population, then you can't use it as a rectilinear index of depletion."[50]

Like Herrington, Clarke's concerns were largely to do with assumptions and revealed major weaknesses not only in the IFC's scientific work but in fishery theory generally.

The major shortcoming of Burkenroad's analysis was that it did not address the southern fishery where depletion had been more extensive, and where fishing-induced mortality was believed to be much higher. Indeed similar calculations to those carried out for northern fish stocks failed to reveal any serious discrepancy between changes in abundance and removals by the fishery. But were IFC estimates of fishing mortality on the southern grounds accurate? Burkenroad insisted they were not and in 1951 he extended his analysis to the southern fishery.[51]

Burkenroad accepted that returns from tagging were representative of fishing mortality on the northern banks because the fish there migrated freely throughout their range. Thus the fishery was no more likely to catch tagged fish than regular fish. But on the southern banks, where fish were believed to be non-migratory and where the fishery had been concentrated in a relatively small area, this was not the case. Thus the southern fishery was far more likely to take tagged fish than its northern counterpart, giving the illusion that fishing mortality rates there were far higher than they really were. Indeed Burkenroad believed that fishing mortality on the southern grounds was no more than twenty-percent, considerably lower than the 40 percent suggested by the IFC. Using a twenty-percent fishing mortality rate Burkenroad revealed a similar discrepancy between changes in abundance and withdrawals by the fishery leading him to draw the same conclusions about the effects of fishing on southern stocks.

Thompson and the IFC fired back with a series of papers but they failed to address Burkenroad's specific arguments, opting instead to offer up a forceful reiteration

of the overfishing argument upon which Thompson's career was made and the IFC's very existence was based.[52] Perhaps this was their only real option. They could not dispute Burkenroad's use of tagging studies to arrive at rates of fishing mortality because they had done the very same thing. Nor could they challenge his estimates of stock abundance because they too assumed that changes in CPUE were proportional to changes in abundance. Ironically, the IFC could not refute Burkenroad's work without refuting its own. But as Thompson's early correspondence with Burkenroad clearly shows, the IFC's response was also conditioned by personal and institutional commitments to fishery conservation narrowly defined as the detection and prevention of overfishing. Burkenroad challenged those commitments. For a variety of scientific and nonscientific reasons, the "experimental" IFC of the late 1920s had become a rigid, uncompromising, resource management agency by the mid-1950s.

The scientific community's response to the debate was mixed. Many scientists sided with Burkenroad, many others did not.[53] But even those who were not convinced by Burkenroad were unable to muster up much of a response to his specific arguments. Thus, for example, biologists Raymond Beverton and Sidney J. Holt devoted fully one third of the *Introduction* to their classic 1957 text *On the Dynamics of Exploited Fish Populations* to Burkenroad's arguments – itself an indication of their impact – but could only conclude that "the temporal coincidence of fishery limitation and a sudden change in the stocks in a favorable direction must be regarded, a priori, as unlikely."[54] However the recovery of halibut stocks did not, and in fact could not, have coincided with regulations. Because halibut are on average 8-10 years old before they become vulnerable to the fishery, the dramatic increase in abundance that occurred during the 1930s and early

1940s must have been caused by fish born in the 1920s, while the stocks were supposedly declining and long before the first IFC regulations came into effect. Second, because halibut are slow to mature, any benefits that might have derived from IFC regulations would not have been visible until at least the early 1940s. At first glance the recovery of halibut does seem to coincide with the first regulations, but a closer look reveals otherwise.

Oddly, the easiest and most obvious solution to the challenge thrown down by Burkenroad was overlooked by almost everyone participating in the debate. It is the recognition that both human *and* natural factors are at the heart of the environmental history of the fishery. Instead the debate became highly polarized. Thompson insisted that the changes in the halibut stock could be explained entirely in terms of human activity, hardly a reasonable argument even if fishing pressure was primarily to blame, since fishery biologists have long known that environmental conditions influence fish populations. But Burkenroad's analysis was equally problematic in that it denied that years of aggressive fishing had had at least some effect on the stock of fish. This was also an entirely unrealistic argument, even if natural factors were primarily to blame.

By the late 1950s the debate was beginning to wane. Burkenroad ended his assault with one final paper in 1953 before moving on to a brief stint as chief fishery biologist for the United Nations.[55] F. Heward Bell and Alonzo Pruter published one final rebuttal on behalf of the IFC in 1956.[56] Some biologists have since argued that the Thompson emerged victorious, but a closer examination of the historical record suggests that although the IFC got in the last shot, this fish-fight ended with a technical knockout for Martin Burkenroad.

The 'Common Property' Problem

Many biologists, but not all of them, also accepted the IFC claim that halibut fishermen were more prosperous in the 1940s than at any time in recent memory, and its argument that the shorter fishing season was caused by increased abundance on the halibut banks. Reflecting on the halibut fishery's remarkable recovery E.J. Russell noted in 1942 that "so great was the general increase in the stock that the fishermen were able to catch the permitted total in five months instead of nine."[57] Similarly Russell's colleague at the Lowestoft Laboratory in England, Michael Graham (well known for his Great Law of Fishing which held that unregulated fishing becomes unprofitable), applauded the work of the IFC adding that "the result has been that it now takes only five months to catch the quantity of halibut that formerly needed nine. This of course has meant profit, where there was none before."[58] Finally, William C. Herrington, the former IFC scientist who had since become Chief Biologist for the American Fish and Wildlife Service, simply noted that "The [halibut] program has been a success. Before management started, fishermen's incomes were so low that many were turning to other occupations. Now there are more fish on the banks and income has improved to a point where new men are coming into the fishery."[59] But early in the 1950s these interpretations were seriously challenged, not so much by biologists as by a new generation of fishery economists led by H. Scott Gordon, whose work would eventually exert an enormous influence over fisheries policy in North America and beyond.

The economic critique began taking shape in the summer of 1951 when Gordon, then Professor of Economics at Ottawa's Carleton University in Ottawa, accepted a

summer research position with the Canadian Department of Fisheries. It was there that he developed what has since become perhaps the most influential argument in fisheries management – that overfishing is not so much a biological problem as it is a social problem rooted in the economic organization of the fishing industry, specifically the common property nature of most marine fishery resources. Gordon's most influential paper, "The Economic Theory of a Common Property Resource: The Fishery," was published in the *Journal of Political Economy* in April 1954.[60]

Gordon's paper was largely an exercise in economic theory intended for economists, but it was also directed at policy makers and did not shy away from drawing conclusions about the wisdom of basing conservation policy solely on biological criteria. Drawing on the inexhaustibility debates of the 19th century, on the great proliferation of conservation programs in the 1920s and 1930s, and, crucially, upon the Thompson – Burkenroad debate which was still raging, Gordon noted that, in fact, "general opinion among fisheries biologists seems to have had something of a cyclical pattern...and the Huxleyian faith in the inexhaustibility of the sea has once again begun to find advocates."[61] The underlying message of Gordon's paper was that biologists still had much to learn about fishery biology.

He was even more convinced that biologists knew less about fishery economics, as evidenced by their insistence on making maximum biological yield the goal of fishery policy. "Focusing attention on the maximization of the catch," Gordon complained, "neglects entirely the inputs of other factors of production which are used up in fishing and must be accounted for as costs."[62] The only reasonable fishery policy, he believed, was one that focused on maximizing net economic yield which, as it turned out, occurred

at a lower and hence safer level of harvesting. This was because biological productivity declines as a fishery approaches MSY whereas costs increase linearly with fishing effort. Fisheries remain profitable up to MSY but because they do so at a diminishing rate and any rent the resource was capable of generating slowly disappears, is used up in additional costs. In Gordon's model, therefore, the marginal return from fishing is greatest at MEY, is zero at MSY, and is less than zero at any point thereafter.

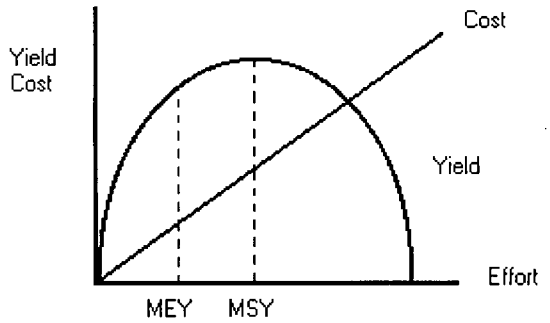

Figure 1. Gordon-Schaefer Model[63]

Ideally, Gordon noted, fishermen would tie up their boats or move on to other fisheries or other occupations at precisely the point where the gap between total costs and total income was greatest, or MEY in figure one. But they typically did not, in part because they were often tied "educationally and romantically to the sea," in part because there were still fish to be had and profits to be made up to MSY, but more importantly because of the common property nature of most marine fishery resources:

> We now come to a point that is of greatest theoretical importance....In the sea fisheries the natural resource is not private property; hence the rent it may yield is not capable of being appropriated by anyone. This is why fishermen are not wealthy despite the fact that the resources of the sea are the richest and most indestructible to man. By and large the only

> fishermen who becomes rich is the one who makes a lucky catch or who participates in a fishery that is put under some form of social control that turns the open resource into property rights.[64]

Provided there were any profits to be made and so long as entry remained unlimited, effort would continue to pour into a fishery. Indeed, under conditions of open access it would be useless if not irrational, wrote Gordon, for an individual fisher to act unilaterally in the interests of either profitability or conservation and withdraw from a fishery, since there was no assurance that he or she would reap any benefits that might result from such action: Thus, concluded Gordon:

> There appears to be some truth in the conservative dictum that everybody's property is nobody's property. Wealth that is free for all is valued by none because he who is foolhardy enough to wait for its proper time of use will only find that it has been used by another. The fish in the sea are valueless to the fisherman because there is no assurance that they will be there for him tomorrow if they are left behind today.[65]

Gordon believed the only way around this economic nightmare, was to remove such resources from the commons, by "mak[ing] them private property or public (government) property, in either case subject to a unified directing power."[66] This done, fishers and fishery administrators alike could rest easy knowing that the fish in the sea today would be there tomorrow and as such were as a good as money in the bank.

Gordon believed his argument was "applicable generally to all cases where natural resources are owned in common and exploited under conditions of individualistic competition" but he relied on examples from the fishing industry, especially the Pacific halibut fishery, to make his case.[67] Contrary to popular opinion Gordon believed that the

increasingly shorter fishing season, which had shrunk to a mere 26 days in Area 2 and 60 days in Area 3 by 1952, had little or nothing to do with increased abundance on the halibut banks but was caused instead by the IFC's policies in combination with the common property nature of the fishery. "Since the method of control was to halt fishing when the limit had been reached, this created a great incentive on the part of each fisherman to get the fish before his competitors [and] during the last twenty years fishermen have invested in more, larger, and faster boats in a competitive race for fish."[68] Moreover, he insisted there was no clear evidence whatsoever that halibut fishermen were more prosperous now than they would have been had the fishery remained unregulated. On the contrary they were probably less so, wrote Gordon, since "what has been happening is a rise in the average cost of fishing effort, allowing no gap between average production and average cost to occur."[69]

Had it ended there the IFC would have emerged largely unscathed since there was still strong support for the IFC's program in fishing and biological circles. But it did not and much to IFC's chagrin Gordon's largely theoretical paper was merely the tip of an enormous economic iceberg in natural resources management. At the fore of the movement was University of Washington economist James Crutchfield.[70]

In Crutchfield's analysis, the Pacific halibut fishery was used as a case in point and the IFC, whose policies were seen to be both irrational and inept once again bore the brunt of this latest economic assault. Crutchfield applauded Gordon's analysis and he agreed that the common-property problem in combination with the quota system of regulation was at the bottom of the halibut and indeed the entire fishing industry's economic woes. But he insisted that the "repercussions go far beyond those sketched in

Gordon's simplified model" adding that "the readjustment of the entire marketing operation to an artificial shortened season is an inevitable result of the present technique of control."[71]

For Crutchfield the most serious problem associated with the shorter halibut season (a mere 23 days in Area 2 and 55 days in Area 3 by 1954) was excess-capacity, which he believed tended to have ripple effects throughout the entire fishing industry. So Crutchfield argued that since the first quotas came into effect the number of full time halibut vessels had increased 122 percent from 384 in 1933 to 854 in 1950, and the number of part-time halibut vessels had increased over 400 percent during the same period. But crucially, total landings had increased only slightly from 46,000,000 pounds in 1932 to 54 million pounds in 1950, or roughly 23 percent. For Crutchfield there were far too many boats in the water. He also discovered that many full time halibut fishermen equipped with new combination vessels had moved into other fisheries in order to avoid having their boats tied up for the better part of the year. Not only were these boats more expensive but – he claimed – they also led to much duplication of fishing gear and reduced efficiency across the board, because they were less effective than vessels designed to catch a single species.[72]

Next Crutchfield noted that the ever shorter fishing season was forcing more fish onto the less lucrative frozen market, an enduring problem for fishermen and fish dealers alike since it implied lower incomes for the former and higher handling and marketing costs for the latter. Thus nearly 22 million pounds of halibut or roughly forty-five percent of total landings found its way onto the frozen market in 1933, whereas 48 million pounds or approximately eighty-three percent of total landings was frozen in 1950.[73]

Finally, Crutchfield insisted that quite aside from the ecological problems associated with the short season, the temporally and geographically restricted halibut fishery was causing major economic problems for the increasingly fewer ports where halibut were landed. Since 1933 the entire geography of the industry had undergone a radical reorganization as changes in public resource policy interacted with changes in ecology and economy. So landings at Seattle, perhaps the principle port for much of the halibut industry's history, declined from forty-three percent of the total catch in 1934 to just seventeen-percent in 1950. Prince Rupert on the other hand saw a major increase from fifteen percent of the total catch in 1934 to forty-four percent by 1950. Likewise, landings in Alaskan ports jumped from fifteen-percent of the total catch in 1934 to forty-four percent in 1950. Crutchfield insisted that this great concentration of landings in just a few ports by the 1950s imposed serious costs on local freezing, cold storage and other port facilities since the halibut fishery now coincided with other major ground fisheries occurring along the northwest coast of British Columbia, Alaska and into the Bering Sea.[74]

Crutchfield was reluctant, however, to declare against maximum-sustainable-yield as a goal of fishery policy. Biologists, after all, were charter members of the conservation movement and would not be displaced easily, even if policy makers agreed that MSY was economically irrational. Moreover, he clearly recognised that Gordon's solution was hardly a solution at all, since it implied that biology had no role to play in public policy formation when clearly it did. It mattered whether halibut spawned at 2 or 10 years of age, or whether they were migratory, or spent half of their lives on land. Biology was important, even if everyone agreed that social goals were to take precedence

during the policy making process. "We may move more rapidly toward the goal of more economical use of the sea fisheries if our specific policies fly less vigorously in the face the biologist," Crutchfield wrote. "And a programme limiting output to the level of maximum sustained physical yield might not deviate too far from the social optimum if entry could be curbed. Such a programme is at least within the realm of possibility in the halibut fishery."[75] In other words, there was a middle ground where biology and economics could merge into a more inclusive fishery management model. But this could only occur if the IFC admitted that under quota management the halibut fishery had become an economic monstrosity – a bitter pill, indeed, to swallow.

Conclusion

In his 1981 book *Pacific Halibut: The Resource and the Fishery F.* Heward Bell dismissed entirely the work of Gordon and Crutchfield and with it that of Burkenroad as well:

> During this rather critical period a number of economists developed an interest in fisheries. With no research upon which to base their findings, they expressed the belief that the halibut management program had been an 'economic failure' and of 'doubtful biological advantage.' They were concerned with the common property character of fisheries generally and of the Pacific halibut fishery in particular. They espoused limited entry and felt that biologists did not function in the real world. Very few facts were adduced and most of their conclusions seem to have been based on hearsay.[76]

Bell was wrong. Burkenroad used IFC data, and while Gordon's largely theoretical piece lacked an empirical base, Crutchfield's simply did not. Arguments to the contrary, simply fail to stand up to the facts. But Bell was surely on to something when he identified the

late 1940s and early 1950s as "critical period" for the IFC and indeed for fishery management generally. If the rise and fall of fish stocks was a purely natural phenomenon, then scientists simply had no business dictating the content of conservation policy, especially when their actions could be shown to be having disastrous effects on the economy of the industry. Fortunately for fish and fishermen few people drew such stark conclusions. And yet there was no going back either. " There are a number of questions of pure theory that must be carefully examined," wrote Gordon in 1958, "[but] in the realm of practice the domination of the natural scientist in the field of conservation policy making is beginning to break down; economists are being admitted, not, I must add, without resistance to what were once the preserves of experts in the fields of biology, mineralogy and the like."[77] Arguably, the two have been at odds ever since.

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[1] International Fisheries Commission, "Pacific Halibut Regulations for 1932," *Pacific Fisherman Yearbook for 1930.*
[2] Ibid.
[3] British Columbia Archives, GR 0435 BC Department of Fisheries, box 5, file 12, William F. Thompson to International Fisheries Commission 8 December 1931
[4] Jozo Tomasevich, *International agreements on Conservation of Marine Resources, with special reference to the North Pacific* Stanford: Food and Research Institute, 1943.
[5] William F. Thompson "Steady Improvement Noted in Condition of Halibut Fishery," *Pacific Fisherman Yearbook* 1936 24(8), p.41-42.
[6] Ibid.
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[48] Ibid, 124.
[49] Ibid., 128.
[50] Ibid., 128.
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[60] H Scott Gordon, "Economic Theory of a Common Property Resource," *Journal of Political Economy* 1954 62(2): 124-142; also see H. Scott Gordon, "An Economic Approach to the Optimum Utilization of Fishery Resources," *Jounral of the Fisheries Research Board of Canada* 1953 10(7), p. 442-457; H. Scott Gordon, "The Trawler Question in the United Kingdom and Canada," *The Dalhousie Review* 1951 31(2), p. 117-128; H.Scott Gordon, Economics and the Conservation Question," *Journal of Law and Economics* 1958 10(2):110-121; Gordon's work inspired an entire field of inquiry. See for example Anthony Scott, "The Fishery: The Objectives of Sole Ownership," *The Journal of Political Economy* 1955 63(2), p. 116-124.
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[65] Ibid., p.135.
[66] Ibid.
[67] Ibid., p.124.
[68] Ibid., p.132.
[69] Ibid.
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[74] Ibid., p. 297.
[75] Crutchfield, "Common Property," p. 300.
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Epilogue: Living with Uncertainty

Fishery Management Since 1960

We now know that the "commons" argument turned on a notion of property that is false, and on assumptions about human nature that cannot be wholly sustained.[1] Common property (like other property relationships) is a form of social organization, and implies that potential resource users who are not members of a group of co-equal users are excluded. It is not to be confused, as it is in Gordon's formulation, with open-access. The distinction is important because it enables us to see that the world is replete with reasonably successful common property arrangements, many of which are in any case continually threatened by those who continue to confuse concepts.

As for human nature, critics question the dominant rational-choice perspective implicit in Gordon's analysis, which one summary avers, "assumes that individuals are by disposition egocentric, parsimonious, and atomistic; that it is in the human constitution to maximize individual gain; and that people always relate to each other and to rules and regulations in an instrumental, strategic and cost-benefit manner."[2] Quite aside from there being no human-nature independent of social, cultural and historical circumstance, critics point out that in assuming thus the commons argument denies the very possibility of "collective action on behalf of the resources and habitats upon which people depend."[3] It is, therefore, avowedly and unjustifiably anti-democratic.

For all that, no other idea has had a greater impact on North American fishery policy. Consider, for example, the U.S. Magnuson Fishery Conservation Act and the Canadian Coastal Fisheries Protection Act, which, in 1977, unilaterally extended national

fishery jurisdiction to 200 miles, or the steady proliferation of private property rights in fishery management since the 1980s. These can be traced back through a body of literature on common property resources that began with Scott Gordon. For if, as legal scholar Lawrence Juda has argued, the goal of extended jurisdiction legislation was to remove fishery resources from the 'international commons' and subject them to the unifying power of the nation-state, then surely the goal of private property rights is to remove those very same resources from the 'national commons' and subject them to the unifying power of the market. Both moves have had important consequences for Pacific halibut management.[4]

The immediate effect of extended jurisdiction legislation was to bring an end to Russian and Japanese high seas trawl fisheries at a time when the halibut fishermen were producing historic low catches (just over 21 million pounds in 1974, down considerably from the 70 million or so pounds caught in 1962). Since trawl fisheries were known to take considerable by-catch of halibut (an estimated 21 million pounds in 1965), fishermen and IPHC scientists were glad to see them go. But by nationalizing the halibut fishery and returning some decision-making powers to Canada and the United States, extended jurisdiction effectively diminished the IPHC's regulatory authority. All international treaties had to be renegotiated to reflect the new powers of the nation-state. The IPHC retained the right to set annual quotas in national waters, but Canadian Department of Fisheries and Oceans (DFO), and the newly created U.S. North Pacific Fishery Management Council (NPFMC) could now manage their respective fishing fleets and allocate the catch however they saw fit.

Canadian fisheries officials wasted little time pondering their new powers, deciding in 1979 to placed a cap on the number of vessels entering the fishery. The goal was to raise incomes and spread the fishing season over a longer period of time, but the measure failed. In 1980 the Canadian fleet harvested 5.7 million pounds of halibut over sixty-five days. In 1990 the same fleet, aided by improvements in harvesting technology and a sharp upturn in stock abundance, took its 8.5 million pound quota in just six days. The following year Canada combined limited entry with a much more aggressive Individual Vessel Quota or IVQ program. Under the new program, which continues today, each vessel is guaranteed a share of the total Canadian quota set by the IPHC. The United States followed a slightly different path, initially opting not to limit entry into the fishery, but ended up in much the same place as Canada. The fleet and the halibut stock expanded so much through the 1980s and early 1990s that the fishing season was reduced to mere hours. In 1980 American halibut fishermen harvested about 15 million pounds over a two-month period. Incredibly, in 1994 they harvested roughly 30 million pounds in just less than two days (24 hours in the Gulf of Alaska, 12 hours in the Bering Sea, and just 10 hours on the Washington and Oregon coast). To curb competition in the fishery in 1995 the NPFMC implemented an Individual Quota or IQ program, which ensured fishermen a definite share of the catch. To the extent that the IVQ and IQ programs have eliminated excessive competition and its negative side effects (short seasons, depressed prices, uneven harvesting of stocks etc.) they seem to be working. What effect the continued spread of private property rights will have on fishermen and fishing communities remains to be seen.[5]

Fishery Science since 1960

Many scientists now agree that Martin Burkenroad was right all along. The Burkenroad-revival began in the mid-1970s when Bernard Skud, a Halibut Commission scientist, discovered that changes in hook-spacing on long-line gear had biased recent estimates of stock abundance based on CPUE.[6] Beginning in the late 1950s halibut fishermen increased the spacing between hooks from 13 feet to 18, 21, and 26-foot intervals. This caused no immediate concern because Halibut Commission scientists assumed that catch was proportionate only to the number of hooks and not to the space between them. It turned out that they were wrong. Catch-per-unit-of-effort increased with increased hook-spacing thus indicating a larger population than actually existed.

Skud knew that similar changes in hook-spacing occurred in the 1910s and 1920s as fishing from vessels replaced dory fishing, and he wondered what affect this might have had on Thompson's early stock assessments. Not surprisingly, he found that Thompson, who also assumed hook-spacing had no affect on catch rates, had under-estimated total fishing effort and therefore over-estimated CPUE during the early years of the fishery. This meant that the decline in abundance prior to 1930 and its subsequent increase, though significant, was nowhere near as sharp as Thompson made it out to be. Skud, who believed both scientists had overstated their arguments, stopped well short of resolving the debate. But his analysis left considerable room for the idea that natural factors had been at least as important as human factors in the history of the fishery. "Burkenroad rightly questioned Thompson's interpretation of the early data." Skud acknowledged. "However the exact role of the effects of fishing and environmental factors cannot be determined until there is a better understanding of the population

parameters and the interrelation of stocks. Until unknowns, particularly growth and recruitment, are determined, one cannot properly credit the increase in abundance either the management program or to fishery induced changes or to environmental effects."[7]

Spurred on by Skud's findings halibut commission scientists continued to carry out retrospective stock assessments and in the early 1980s made an astonishing discovery. It appeared that cyclic high and low values of adult abundance tended to lag similar cycles of juvenile abundance. This to say that the smallest spawning stocks produced the largest year-classes of halibut over the past century. Biologists Robert Deriso, Stephen Hoag and Donald McCaughran proposed two hypotheses to account for the observation: juvenile production is either (a) density-dependent and hence a function of fishing effort or alternatively (b) density-independent and hence a function of exogenous environmental factors. They found good evidence for both. "Our study shows that even after another forty years of data we still do not know whether environmental factors are the primary cause of changes in the natural production rates of the young [since] the additional hypothesis of density-dependent production is also consistent with our estimates."[8]

Deriso, Hoag and McCraughn recognised that their findings had important policy implications. Should stocks be kept low in hopes that large recruits will be consistently produced (in keeping with the density dependent hypothesis), or should stocks be managed so as to buffer against cyclic declines in juvenile survival rates (in keeping with the density-independent hypothesis)? They decided to work on the assumption that fishing-induced mortality and natural fluctuations affected stock productivity. In practice this meant abandoning the Maximum Sustainable Yield or MSY concept for the more

cautious Constant Exploitation Yield or CEY concept. It works like this. A biological target level for removals (the CEY) is calculated by applying a fixed harvest rate – presently 20% – to an estimate of total exploitable biomass. Commercial quotas are then calculated by subtracting all other removals – by-catch, sport fishing, personal use, waste etc. – from the total CEY. The percentage is the same from year to year, but the catches increase or decrease depending on longer-term trends in stock abundance.[9] The rub is that accurate estimates of total exploitable biomass are notoriously difficult to obtain. The implications are profoundly important. Setting a quota of 40 million pounds on the assumption that total exploitable biomass is 200 million pounds could be disastrous if in fact total exploitable biomass is only 100 million pounds.[10]

More recently Stephen Hare, who joined the Halibut Commission in the early 1990s, linked fluctuations in salmon and halibut populations to a pattern of climate variability called the Pacific Decadal Oscillation or PDO.[11] The PDO as been described as an El Nino-like phenomenon but with notable differences. Both apparently exert an enormous influence over ocean productivity.[12] But unlike El Nino conditions which last anywhere from six to eighteen months and whose effects are more visible in the tropics, PDO "regimes" – alternating cool and warm phases – can last for decades and are most pronounced in the North Pacific. Hare found that halibut, like salmon, sablefish and several other fish species, thrive during warm phases but recruit poorly during cool phases. This is where things get really interesting. Scientists now believe that cool PDO regimes prevailed from 1890-1924 and again from 1946-1976, while warm PDO regimes prevailed from 1925-1945 and from 1977 into the mid-1990s. Notice how closely these dates follow the broad contours of halibut conservation history.

The PDO concept sheds new light on old debates, but scientists are no closer to resolving the Thompson-Burkenroad dispute. As biologists Carl Walters and Ray Hilborn have put it "despite what is arguably the best fisheries data set in the world, one can explain the history of the Pacific halibut stock equally well as changes due to environment or changes due to fishing."[13]

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[1] C.f. S.V Ciriacy-Wantrup and Richard C. Bishop, " 'Common Property' as a Concept in Natural Resources Policy," *Natural Resources Journal* 1975 vol. 15 p.713 – 727, Patricia Marchak, "What Happens when Common Property Becomes Uncommon," *BC Studies*, 1988-89 no. 80, 3-23, and David Bromley, *Environment and Economy: Property Rights and Public Policy* Cambridge: Blackwell, 1991 for summaries.

[2] Svien Jentoft, Bonnie McCay and Douglas C. Wilson, "Social Theory and Fisheries Co-Management," *Marine Policy* 1998 22, 4-5, p. 423.

[3] Bonnie J. McCay, "The Ocean Commons and Community," *Dalhousie Review* 1994-1995, p. 74.

[4] Lawrence Juda, *International Law and Ocean Use Management* New York: Routledge, 1996.

[5] International Pacific Halibut Commission. Annual Reports. 1960-1999.

[6] Benard Skud, "Revised Estimates of Halibut Abundance and the Thompson-Burkenroad Debate," *International Fisheries Commission Scientific Report* no. 56 1975, p.5-35.

[7] Ibid., p.31.

[8] Robert Deriso, Stephen Hoag and Donald McCaughran, "Two Hypotheses About Factors Controlling Production of Pacific Halibut," International North Pacific Fisheries Commission Bulletin 1986, 47, p.165.

[9] International Pacific Halibut Commission. *Annual Report*. 1999.

[10] This, of course, is exactly what happened in the Atlantic cod fishery. C.f. Christopher Finalyson, *Fishing for Truth: A Sociological Analysis of Northern Cod Stock Assessments from 1977-1990*, St. John's: Institute for Social and Economic Research, 1994.

[11] Robert B. Francis and Steven R. Hare, "Decadal-scale regime shifts in the large marine ecosystems of the North-east Pacific: a case for historical science," *Fisheries Oceanography* 1994 3(4), 279-291.

[12] Ibid.

[13] Carl Walters and Ray Hilborn, *Quantitative Fisheries Stock Assessment: Choice, Dynamics and Uncertainty* New York, Chapman and Hall, 1995, p. 57.

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