https://doi.org/10.17895/ices.pub.8886

ICES Marine Science Symposia, 215: 453–463. 2002

Reflections on 100 years of fisheries research

Raymond J. H. Beverton† and Emory D. Anderson

Beverton, R. J. H., and Anderson, E. D. 2002. Reflections on 100 years of fisheries research. – ICES Marine Science Symposia, 215: 453–463.

The origins of fisheries research as an organized and coherent scientific discipline in its own right can be traced back to the mid-1890s. Some of the highlights of progress, a few of which are well known and a few perhaps less well known, are recalled by the senior author (RJHB), a major researcher during the second half of this first century of fisheries science. The reflections are grouped into five time periods, with the major personalities and their contributions within the ICES arena noted for each. Of special interest are RJHB's recollections of his days at Lowestoft working with Sidney Holt on their famous *magnum opus* published in 1957. Lastly, RJHB offers his opinion on a range of issues including deterioration of fisheries statistics; communication between scientists, administrators, and fishermen; management philosophy; uncertainty estimates and assessing risk; economics and politics of fisheries management; and multispecies assessment and management.

Keywords: assessment, Cambridge, fisheries, ICES, Lowestoft, management, multispecies, yield.

R. J. H. Beverton: late of the School of Pure and Applied Biology, Cardiff University, Cardiff, Wales, UK. E. D. Anderson: NOAA, National Marine Fisheries Service, Office of Science and Technology, 1315 East-West Highway, Silver Spring, Maryland 20910, USA; tel: +1 301 713 2435; fax: +1 301 713 0799; e-mail: emory.anderson @noaa.gov.

Introduction

This paper is based on a lecture given by the senior author (RJHB) at the Woods Hole Laboratory of the National Marine Fisheries Service (NMFS) on 3 May 1994, 15 months before his death[1], and taken from a transcription of a video/audio tape of the actual lecture. It was one of three presentations given over a two-day period by RJHB in Woods Hole (Anderson, 2002), his first stop on a nation-wide speaking tour of selected NMFS facilities. The reflections are entirely his; many of the words and quotes used in the lecture have been preserved to enhance authenticity. He noted that these recollections were derived from having met a few of "the old sages" from pre-World War I days. As a result, he felt able to provide a bridge from the past to the present.

Fisheries science, in an accepted sense, came together roughly in the mid-1890s. There had, of course, been important developments before that through hatchery work in the United States and Europe, big expeditions such as the HMS "Challenger", and people like Sir John Murray. Worries had already begun, in the North Sea in particular, about the effects of fishing, which increased very rapidly from the middle of the 19th century. In terms of serious, organized, recognizable fisheries science as known today, it probably began about 100 years ago. RJHB was a major practitioner in the discipline during the second half of that period. His reflections are grouped into five time periods beginning with the pre-1914 era and ending with the 1980s–1990s period. The paper concludes with his views on issues ranging from deterioration of fisheries statistics to multispecies assessment and management.

[1] Deceased 23 July 1995.

Pre-1914

Prior to 1900, several countries had already begun to realize that a better scientific base was required to address fisheries issues, primarily those in the North Sea. In Britain, there was much concern about the effects of fishing. Ernest W. H. Holt was commissioned by the Marine Biological Association (MBA) to sample fish at the Grimsby fish market and to establish whether or not the worries of the fishing industry, reported to several Royal commissions during the latter part of the 19th century, about the decline in fish stocks were genuine. In Scotland, scientists such as Thomas Fulton had, in fact, taken action even sooner.

In Denmark, the great C. G. J. Petersen, who developed the Petersen tag for flatfish, was concerned with transplantation into and out of the Limfjord as an en-

Figure 1. The staff at Lowestoft in 1907. Front row (l-r): W. Wallace, W. Garstang, J. O. Borley. Middle row (l-r): James, A. E. Hefford, Rosa M. Lee, R. A. Todd, G. T. Atkinson, Dykes. Back row (l-r): Potter, Arrowsmith, Walton, Ansell.

hancement program. Enhancement was very much one of the driving forces in the early days of fisheries science.

In Germany, a very strong trawling country in those days with fishing interests similar to those of the UK, there was Friedrich Heincke, Director of the Helgoland Biological Station. Even before the turn of the century, Johannes Reibisch at Kiel had been the first to age plaice from otoliths. It was well into the 1903–1904 period before William Wallace at Lowestoft took plaice ageing further.

In the Scandinavian countries, the Norwegians had been active, with Johan Hjort, G. O. Sars, and others who were very much concerned by the fluctuation of their big cod and herring fisheries, and with strong physical oceanographic support from leaders like Fridtjof Nansen.

It was the realization that all of these efforts should be coordinated internationally, instead of being attempted by individual countries, that led to the establishment of ICES in 1902.

In Britain, the MBA in Plymouth, commissioned in 1900 to do the fish survey work by a special Treasury grant, was the only organization available to do it. There was the realization of the need for a more established base for fisheries research. Consequently, the Lowestoft Lab was established in 1902.

A fascinating history of the Lab and its precursors was written by Arthur Lee (1992). The first recognized photo of the Lowestoft staff is from 1907 (Figure 1). Walter Garstang, the Director, was a very considerable figure, the leading fisheries scientist in those days in the UK. Other staff members whose names survive in terms of folklore and publications were William Wallace, who pioneered work on plaice, Rosa Lee of "Lee's phenomenon" fame (back-calculation of growth rates), and George Atkinson, who went off to the Arctic as a young man to work on Barents Sea plaice. Atkinson was fisheries inspector at Lowestoft until the outbreak of World War II when the Lab closed and everybody went off to war. However, Atkinson remained in contact with the

Figure 2. Deck of the RV "Huxley", showing the crew with a catch of primarily flatfish.

Lab even after retirement and was remembered as a wonderful man who got along well with everyone, was respected, and was looked to for advice because of his enormous knowledge. The first ship used by the Lowestoft Lab was the RV "Huxley" (Figure 2). She did her first station in November 1902 southwest of Dogger Bank, and there are logbooks surviving from that work.

Garstang was in charge of the early investigation at Lowestoft into what had happened with the North Sea plaice fisheries over the latter part of the 19th century. His paper (Garstang, 1900), published in the *Journal of the Marine Biological Association* because it was the only appropriate journal at that time, followed the catch per unit effort (cpue) of four specific sailing trawlers from 1867 to 1891. In spite of the clear decline in plaice cpue (Figure 3), British authorities were not prepared to accept that as very strong evidence of a decrease in abundance. In retrospect, however, it was the first tangible evidence of a decline in cpue almost certainly attributed correctly to the effect of fishing.

Not surprisingly, when ICES established its three committees in 1902, one of them was called the Committee on Migration of Food Fishes. Johan Hjort of Norway chaired the committee, but was more concerned about fluctuations than migrations, and his classic report (Hjort, 1914) was about fluctuations of the fisheries. From that report came his famous hypothesis of the first critical phase, first feeding, and so forth.

Garstang was a member of the Committee on Migration of Food Fishes, but chaired the Committee on Over-fishing which included, amongst others, C. G. J. Petersen and Thomas Wemyss Fulton from Aberdeen. The latter committee became much more concerned with plaice and continued the work begun earlier by Garstang.

Garstang did not remain at Lowestoft long enough to report on his committee's work because, by 1907, there were considerable difficulties. Only England and Germany were providing length compositions, which every country had been asked to submit, and Garstang was getting more and more disillusioned with trying to make progress. In addition, he had difficulties as Director stemming from the ongoing question within Britain relative to the source of funding. He eventually moved to the Chair of Zoology at Leeds, and later Oxford, and his daughter married Sir Alister Hardy, who was a much later occupant of the same Chair at Oxford.

Harry Kyle was a great help in the first few years after the establishment of the Lowestoft Lab, but after he left for ICES headquarters in 1906 to become the Biological Assistant to the Council, no one took over his work on plaice.

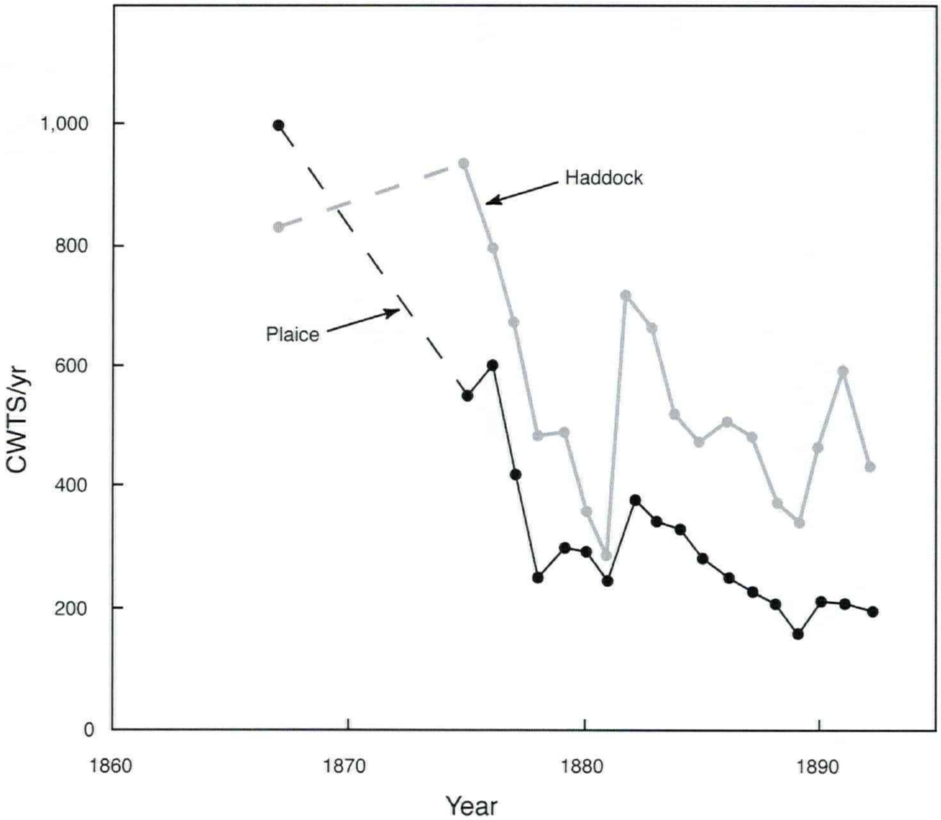

Figure 3. The cpue (cwts/yr) of haddock and plaice recorded by four Grimsby sailing trawlers, 1867–1890 (redrawn from Garstang, 1900).

One of the little gems of those early days, not widely known, was a note by Thomas Edser (1908), the statistician in London assigned to organizing the massive five million length measurements collected in Lowestoft in 1905–1907. Heincke, in his classic 1913 report on plaice, had observed that a plot of the logs of the length compositions produced a very nearly straight line on the right-hand side. To Heincke, that meant log N = A + B times L and, if that were a general rule, which he said he did not believe, then there was a means of constructing a life table. In fact, it was Edser who had made that observation in his 1908 paper, in which he wrote, "It appears that, within certain limits, the number of plaice at any length is directly related to that length by a formula of the type, log y = A + bx where y is the number and x is the length..." Edser had produced, logged, and plotted the length composition (Figure 4) which constituted a nice little piece of population demography, later to be known as a catch curve and used to estimate survival and mortality. Heincke referenced Edser in his report, essentially the first ICES assessment working group report, which recommended that the plaice size limit should be increased from almost nothing – they were catching an average size of around 17–18 cm – up to 21 or 22 cm. Such a recommendation sounds fairly modest today. It carried very little weight as there was no official arrangement then for providing ICES advice; it was just a case of ICES writing to its Member Governments. However, the recommendation failed to come into effect because of World War I.

1918–1939

The post-World War I period began with one of the great contributions published in 1918 by Fiodor Ilyich Baranov (in Russian), a paper not seen in the Western world until the latter part of the 1930s, and not by RJHB until 1947 when E. S. Russell, then Director of Lowestoft, obtained a copy. The paper contained a length-based yield biomass equation with a linear growth function. The paper had no effect because it had disappeared into the limbo following World War I and the Russian Revolution. Baranov, an engineer, was assigned other work and disappeared from Western view.

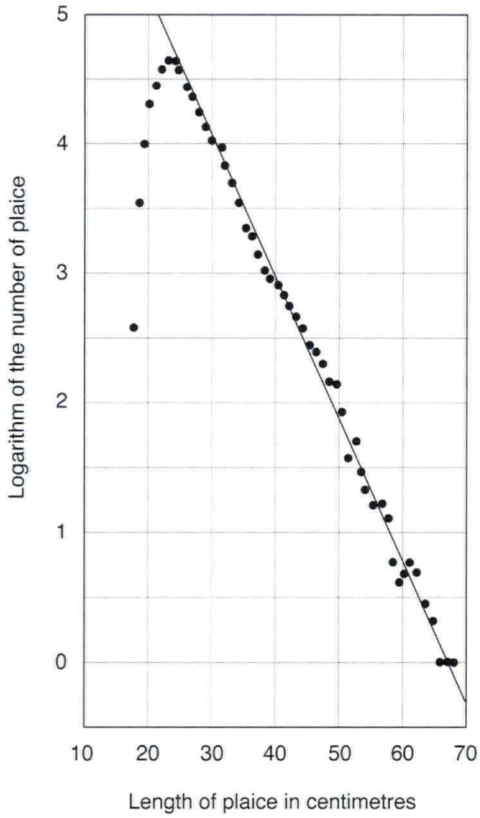

Figure 4. Reprinted plot from Edser (1908) of the logarithm of numbers vs length for plaice taken from the southern North Sea in 1906.

The sigmoid or logistic curve, applied by Raymond Pearl to human demography (Pearl, 1925), was first employed in fisheries science in the 1930s. The Norwegians (Ottestad, 1933), from the point of view of whaling, first realized this could possibly be the basis for population and yield assessment. Michael Graham, Director of the Lowestoft Lab after the war, realized that this was a potential way of coping with the problem of assessment and rather cleverly used changes in abundance during World War I to try to estimate the natural rate of increase and develop the idea of getting a maximum yield. In fact, it was more luck than anything else, but he was not far from the truth back at that time. Graham did not put formal mathematics into it, but left it essentially in the form shown in Figure 5. This was not done until 1954 when Benny Schaefer in the United States adopted the logistic production model and applied it systematically to tunas (Schaefer, 1954).

Fisheries scientists were not just concerned in those days with the question of population assessment. E. S. "Bill" Russell, Director at Lowestoft during 1921–1945, who was on the interview committee when RJHB first went for a job at Lowestoft in 1945, had developed his equation (Russell, 1931). It was a simple statement of what comes in must go out if you ever intend to get it balanced, which itself has had a profound influence. At the same time, the other major thrust forward was by Bill Thompson in the United States on Pacific halibut (Thompson and Bell, 1934), doing arithmetic yield equation calculations, basically the same sort of thing fundamental to working out the whole population age composition.

A special ICES meeting held in 1936 on "Comparative Studies of the Fluctuations in the Stocks of Fish in the Seas of North and West Europe" included papers which looked at year-class strength based on sampling a number of species. In a paper on European cod stocks by Oscar Sund, a Norwegian cod expert, a synchrony of good year classes (1904, 1917, 1922) was evident (Sund, 1936). Adolf Bückmann, the German flatfish expert of that period, did the same thing for plaice, picking out the 1922 and 1928 year classes (Bückmann, 1936). In the case of haddock, strong year classes were rather less clear-cut, but still evident, as determined by Robert Clark (1936), who was then Director of the Aberdeen Lab. In a later ICNAF publication, Wilf Templeman (1972) again traced the synchrony of good cod and haddock year classes across the whole of the North Atlantic.

1945–1960

RJHB did his first two years at Cambridge in 1940–1942, enrolling initially to be a chemist and taking courses in physics, math, and chemistry, but not biology. During the war, he left Cambridge and worked at the Operational Research Group developing products used for radar. Upon returning to Cambridge after the war, he was required to take a third major subject and took zoology. His professor at Cambridge, James Gray, told him, "Why don't you spend a few months at Lowestoft before you come back to do your final year? They'll have to rebuild, they have quite a few people, and they need some people with some reasonably good quantitative skills. That's the way it's going." So RJHB went to Lowestoft in the autumn of 1945 and was immediately sent by Michael Graham to sea in the Arctic on a filthy, dirty, smelly, rust-covered commercial trawler that had just been decommissioned from wartime minesweeping. Graham had said, "We have to have an Arctic programme after the war and we'll have to have a research vessel if we want to find out what's going on." RJHB went on that trip, but wrote up his resignation three times because of the severe weather. Even though it was a very bad experience, he did not submit his resignation. By the time he had finished his last year at Cambridge, he knew that fisheries research was what he wanted to do.

Before RJHB returned to Cambridge in the spring of 1946 for his final year, Sidney Holt came to Lowestoft.

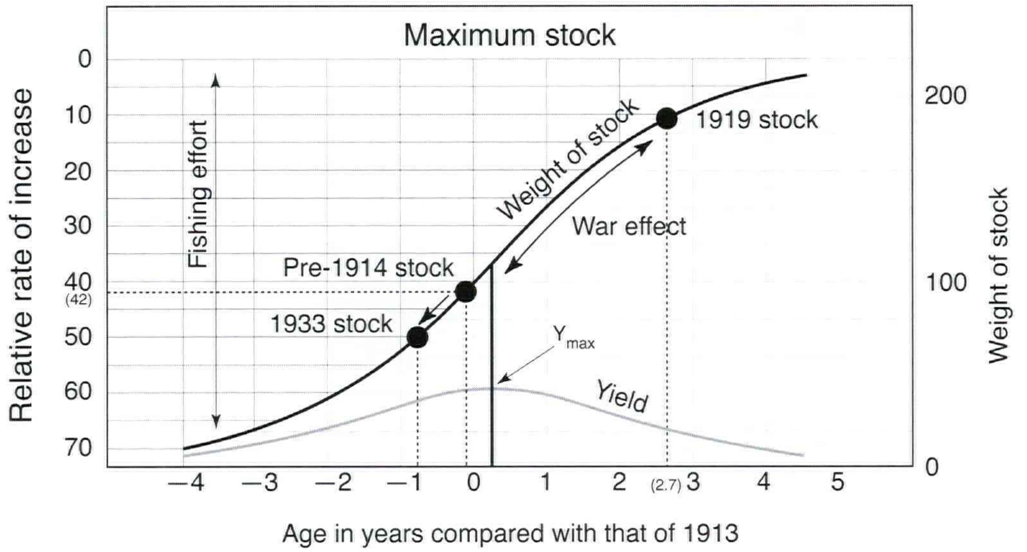

Figure 5. Redrawing of Michael Graham's (1935) use of the simple logistic production curve and the effects of the partial cessation of fishing in the North Sea during World War I to estimate requirements for maximum yield.

He had just finished at Reading University where Basil Parrish had graduated four years earlier. Michael Graham said, "I want the two of you to really see if you can't put the whole of this fish population stuff on a more substantial basis. We've had a go at it – the sigmoid curve stuff, Thompson is doing arithmetic over in Seattle, and it really needs a more systematic approach."

When RJHB came back to the Lab in 1947, he remembered Graham saying to Holt and him, "Well, I'll give you four years. We'll leave you alone for four years to your own devices. I can't tell you how to do it. I'm satisfied you know more than I can tell you about it. It's up to you. If you don't succeed at the end of four years, I can't protect you any longer. You'll have to take a chance after that, but for those four years, I will."

A chap named Henry Hulme, who was Michael Graham's aide in Operational Research during World War II, worked on ballistics and the theory of convoys, i.e., how to place and do convoy work in relation to the U-boats. Graham said to Hulme after the war, "Come down to the Lab and talk to my two young boys." Out of that visit came a paper (Hulme, Beverton, and Holt, 1947), written while RJHB was still at Cambridge, which was really what Baranov put in an age-specific form instead of a length-specific one and still with a linear growth rate.

By the time RJHB returned to Lowestoft in the summer of 1947, Holt had discovered von Bertalanffy (1938) because of two very good friends at Reading: Bill Thomas, a mathematician, and Peter Jewel, a physiologist. Jewel had first seen the paper, because von Bertalanffy was a human physiologist, and introduced it to Holt, who immediately realized it was what they needed in their yield equation. They successfully used the equation, and the algebra came out quite neatly except for a few places. Although the original von Bertalanffy formula ($dy/dt = \eta yn - xy^m$) did not specify it, the two powers (n and m) were rates of anabolism and catabolism, and RJHB and Holt often found the need to go back to that formulation in terms of weight rather than worrying about the length version. Other people also did, such as Andersen and Ursin (1977) when they developed the multispecies version of it, and Jan Beyer and others who picked up the whole question of length-based methods and tried to build in and simulate food, growth, and so on. Getting the von Bertalanffy equation off the ground gave RJHB and Holt the lead they wanted. As long as they could adjust for density-dependent growth, a whole lot of things became possible.

RJHB and Holt worked together for four years, 1947–1951. They were left to their own devices and had a room for themselves in a house adjacent to the Lab, deliberately to put them away from the rest of the staff. They got on wonderfully together, never had an argument or a cross word, and had that sort of partnership that does not often happen (Figure 6).

The work on Beverton and Holt (1957) was finished by 1951. Holt, however, was unhappy at Lowestoft and particularly did not like going to sea. When Geoff Kesteven came to Lowestoft in the early 1950s head-

Figure 6. Photo of Ray Beverton (left) and Sidney Holt (right) at work on their *magnum opus* in 1949 in the Fisheries Laboratory, Lowestoft. Ray can be seen working next to a 3-dimensional cardboard model of a yield isopleth diagram, while Sidney can be seen operating a hand-Brunsviga calculating machine.

hunting for the new Fisheries Division of FAO, Holt left. RJHB did the writing from 1952 to 1954. They exchanged letters and would have all-night sessions every time Holt returned, which was quite a few times each year, continually working on new ideas and ways of putting things. The manuscript was finished by 1954 and Michael Graham took it to the Stationery Office. When they refused to have anything to do with it, he threatened to resign as Director. Although they did not believe him, he actually was serious, and within a day or two they capitulated. It took three years to publish it because it was so large and complex and required detailed typesetting. It was expensive to produce, but the Stationery Office actually did very well and had very few typos. However, it was difficult persuading them to print 1500 copies; they thought they would be lucky to sell 100.

In the meantime, in 1954, out came Bill Ricker's classic on stock and recruitment. Ricker had spotted a paper on insects by Paul Moran (1950), an Oxford statistician, which demonstrated that if stronger feedback occurs in a difference equation from one term back to the next, some very funny things happen. Ricker produced a stock-recruitment curve with a replacement line. When RJHB and Holt saw Ricker's paper, they thought, "Oh, God, this has driven the coach and horses through it." With a sigh of relief, however, they realized that Ricker had not actually tackled it the way they had, but had gone for a totally different formulation. Consequently, the two approaches still survive as the two basic ways of looking at stock and recruitment. It was all a question of time-lag. In the Beverton and Holt equation, it was instantaneous with the density at that moment determining the mortality rate. But in Ricker's equation, the density-dependent part was back-dated to the initial numbers, with contemporary abundance having no effect.

RJHB and Holt, together with Basil Parrish and Rodney Jones from the Aberdeen Lab, taught two population courses at Lowestoft during the 1950s. These

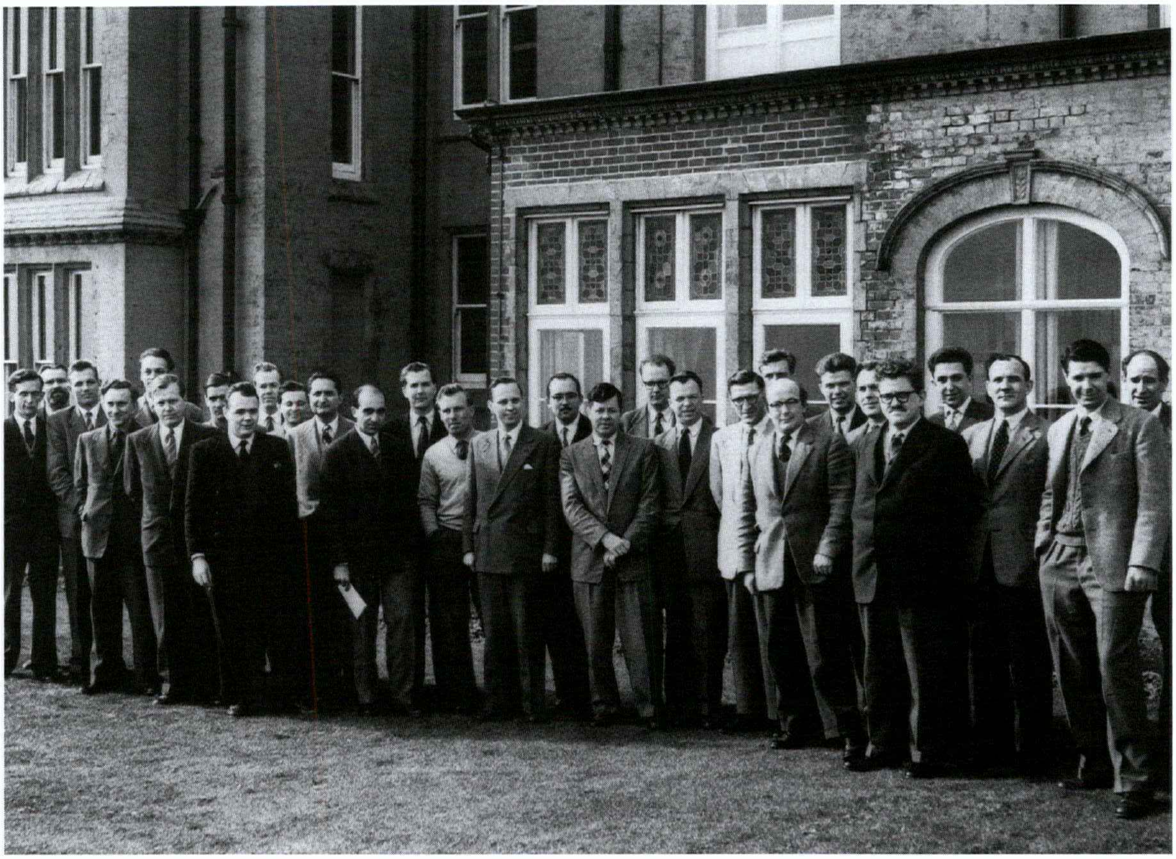

Figure 7. Participants (l-r) at the "Fish Population" course given at Lowestoft February 20–March 7, 1957: George Bolster, Robert Clarke, Ole Johan Østvedt, Alec Gibson, Luit Boerema, Aage Jonsgaard, Torolf Lindström, Albert Percier, Don Hancock, Rodney Jones, Richard Vibert, Rui Monteiro, Dick Laws, Sidney Holt, Dietrich Sahrhage, Manuel Larrañeta, Jón Jónsson, Arvid Hylen, Erling Bratberg, Vincent Hodder, Olav Aasen, John Gulland, Olav Dragesund, Ronald Keir, Knud Peter Andersen, Gotthilf Hempel, Basil Parrish, Ray Beverton, and Dick Baird.

courses had been encouraged by ICES and were an excellent way to transfer knowledge of methods as well as become better acquainted with the scientists. This proved to be extremely valuable in later years when the ICES assessment working groups were established and began to function. In addition, the personal contacts established from these courses proved to be enormously important in later years. Many who participated in the second of these two-week courses held in February–March 1957 (Figure 7) subsequently became well known and held important positions in fisheries.

1960s and 1970s

One of the big discoveries in the 1960s was virtual population analysis (VPA). Normally attributed to John Gulland (1965) in an appendix to an ICES Arctic Fisheries Working Group report, it was, in fact, discovered simultaneously by Garth Murphy from California (Murphy, 1965). Gulland and Murphy very likely had no knowledge of each other's efforts, but the idea of back-calculating instead of forward-calculating obviously dawned on several people at the same time. The earliest record of this approach, however, was by Rodney Jones in an appendix to a paper on calculation of mesh increase (Jones, 1961). VPA was a very reluctant discovery; it always appeared in an appendix with something else. Jones said, "Look, if you work this catch equation business backwards, lo and behold, it doesn't matter what F you start with; it will converge." He later included this notion in a paper (Jones, 1964) presented at the 1963 Symposium on "The Measurement of Abundance of Fish Stocks", one of the landmark ICES Symposia. VPA was not readily accepted and caused considerable concern until scientists realized just how complicated it was and the implications that can arise when applied improperly. However, there is no doubt that VPA has become a very powerful assessment tool.

Every now and again, an ICES Symposium really hits the right moment when everything comes together. There were a number of these where this happened, including "Fish Stocks and Recruitment" in Aarhus in 1970, "The Biological Basis of Pelagic Fish Stock Management" in Aberdeen in 1978, and "Early Life History of Fish" in Woods Hole in 1979.

The use of yield-per-recruit and other equilibrium models diminished with the advent of discrete time models such as VPA, but growth rates and other basic biological information again took on importance in later years.

The International Commission for the Northwest Atlantic Fisheries (ICNAF) was well under way during this period. RJHB had some wonderful experiences with ICNAF and its Assessments Subcommittee, as well as with ICES.

By the late 1960s and early 1970s, however, collapses of major fish stocks began to influence changes in fisheries science. Daily rings were discovered in 1974, and the computer came into use. Computers were not available when RJHB and Holt did their major work. All of their calculations were done by a hand-Brunsviga machine, and it took them about eight months to complete the density-dependent, stock and recruitment modelling of North Sea haddock alone. The only way they could solve the four simultaneous equations, growth, and everything else was by taking batteries of things and plotting until they obtained the answers.

1980s and 1990s

RJHB considered it presumptive to comment authoritatively about developments in the 1980s and 1990s because he was not actively involved in fisheries assessment science between 1965 and the early 1980s and after that only on the "sidelines" (e.g., several academic positions and Editor of the *ICES Journal of Marine Science*). However, this did not deter him from expressing his opinion on a variety of topics and present-day problems in fisheries science.

Deterioration of fisheries statistics

Although fisheries science has advanced considerably in terms of computing and modelling, and tools are available to do more in two minutes than RJHB could do in eight months, some of the basic ingredients for these models are being lost. One of the most serious causes for the deterioration in databases used to assess fish stocks (e.g., catch statistics, length frequencies) is the attempt to manage with total allowable catches (TACs).

Since most of the fisheries for which this is the case are in a heavily overfished condition, there is considerable pressure to circumvent regulations. If somehow a happier state could be reached devoid of such pressure, some of the incentives to avoid accurate reporting of landings or size compositions would disappear.

One consequence of deteriorating fisheries-dependent data is a greater reliance on abundance indices, length compositions, and age–length keys from research vessel surveys. Years ago (e.g., during RJHB's days at Lowestoft), it would have been considered wasteful to use valuable research vessel time on surveys. Commercial catch per effort was deemed a valid indicator of abundance. However, it is easy to understand that such surveys are now necessary to obtain unbiased indices of abundance.

Communication between scientists, administrators, and fishermen

Communication by scientists with fishermen and also with administrators is a major problem. Success depends on scientists knowing the fishermen, their attitude, how to convince them what is really happening with the stocks, and what should be done. Scientists need to develop more sophisticated ways, such as modern interactive visual methods, to recast past stock events and demonstrate what would have happened if, for example, stock biomass had been held at a particular level by curtailed fishing rather than reduced by uncontrolled harvesting. Some of the lessons of stock collapses have been learned and will not be repeated, but there are many other situations where this has not been the case.

The answer to those who are waiting for scientists to tell them how to manage the ocean fisheries is that they should have listened 50 years ago, as scientists have been saying it for the last 50 years or more. It is rather worrying that the finger is being pointed at scientists. If scientists fail, it is because they have failed to have their message understood and accepted, rather than not having delivered the message.

Management philosophy

Difficulties with fisheries management in the North Sea during the 1980s and 1990s were due in large part to scientists being pushed into a position of producing absolute measurements and predictions of absolute yield. Although following from the maximum sustainable yield principle, this has presented real difficulties because scientists are not in a position to predict yields accurately for subsequent years. Management by TAC in the North Sea has stemmed from catch or landings, accurate or otherwise, being the only common "currency" among the countries which share those resources.

The real objective of fisheries management is the regulation of harvesting or fishing mortality rate, not the catch itself. Michael Graham once said, "We should stay at home and not fish too hard." His principles of "The Great Law of Fishing" (Graham, 1943) say that unlimited fishing sooner or later drives the fishery down to a more-or-less break-even, zero profit. He said this 50 years ago, and it is just as true now as then. The ultimate aim must be to keep the harvesting rate down to a reasonable level. It does not matter after that, within a certain amount of variation, what level of catch is taken because that will be governed by the size of the year classes produced. Within limits, both good and poor year classes should be harvested at about the same rate, unless an overfished stock is being rebuilt.

The first question in fisheries management should be where you are aiming, and the second question should be how to get there. It will be hard and will cost money because the present capitalization of fishing effort is greatly in excess of what is needed. It will be difficult to reduce fishing effort to the proper level, but the fishery will be very profitable when a given level of catch is taken with one-half or one-third as much effort.

Uncertainty estimates and assessing risk

The use of uncertainty estimates in scientific advice creates mixed feelings. It is accepted that a degree of reliability must be built in and taken seriously for a given estimate, parameter, or assessment. However, when transmitting scientific information or advice to fishermen or administrators, the risk is not always as clear-cut as it ought to be. First of all, no decisions are made which will apply forever, and managers must be continually responsive to changes or errors in predictions in order to take necessary remedial actions. Simply to provide a statement of whether an estimate is right or wrong within a certain level of precision for an indefinite period of time is not the type of risk assessment that the fisherman or the administrator can use. If scientists portray uncertainty too "honestly", there is a chance they will so undermine those who are looking for any excuse to claim the scientists are wrong as a reason for doing nothing.

Economics and politics of fisheries management

Fisheries scientists have failed to communicate the point adequately that, unlike farming, the only way the future replenishment of a stock can be influenced is by controlling the harvesting rate. Unlike farming, increasing fishing effort does not always result in increased production.

Mike Holden's 1993 book describes the history of the Common Fisheries Policy (CFP) of the European Community (EC) and the "horse dealing" that took place (Holden, 1993). The CFP failed because it was an attempt to have a market-oriented expansion similar to that done in agriculture. It was decided to double the harvest by doubling fishing power through subsidization. Instead of building new ships, however, countries should have been doing the opposite. Decades of advice provided by ICES through the North-East Atlantic Fisheries Commission (NEAFC) were ignored. There were no fisheries scientists on the staff of the Directorate (Fisheries) until Holden was employed in the early 1980s. The net result was a CFP set up on an expansionist policy when the exact opposite should have happened. They should have been reducing harvesting capacity, and it was only in later years that they had to "bite the bullet" and start offering subsidies for removal of a few percent of fishing power. It was a special case of market-oriented and political forces dominating fisheries management and was a terrible and tragic lesson in sensible husbandry of natural resources.

Multispecies assessment and management

The first century of fisheries research was basically a golden age of single-species population dynamics. However, there is now the realization that individual species cannot be managed in isolation and that fisheries cannot be managed just for the sake of the fish. We have seen the height of population dynamics research on a single-species basis and are now entering an era of social and economic analysis of multispecies fisheries.

Two similar species, e.g., plaice and sole in the North Sea, can probably be safely treated as a single species for the purpose of management. In terms of a fishing mortality rate, a first approximation would be something less than F_{max}, but the question remains what that would be in a mixed fishery or interacting species complex. Undoubtedly, it would depend on the amount of fishing effort being directed to each species, as well as the interaction between the two species.

In a multispecies situation, the appropriate level of fishing mortality rate (F) may be uncertain within the zone of "moderate" rates, but not at either very high or very low rates. Fishing at an F of 1 on cod, irrespective of whether in a multispecies situation or not, is almost certainly not going to be very sensible. Conversely, reducing F to extremely low levels would be unwarranted. But there is a wide area in the "moderate" zone for which, given the necessary information, different F rates would be required for the different species.

Marine mammals need to be included in any multispecies equation to ensure getting the total picture right. Whether or not management takes account of it is another thing.

Managing a multispecies situation on the basis of the weakest species or a weak species which has collapsed would be a useful approach. When stocks are rebuilding from the brink of collapse and becoming more abundant, it would be extraordinarily difficult, complex, and expensive to select individual species for separate treatment or management. A level of effort not too excessive should be selected for the fishery. Some species may be temporarily overfished to some extent, depending on market demands and local abundance or shortage, while others may be underfished. In time, fishermen will redirect their effort from overfished to underfished species. If a mixed fishery could be brought to that type of situation and fishermen permitted to fish within some reasonable limits of effort, there would be no need for managers to interfere with their activity. As long as there were not too many fishermen, they would not do dramatic harm.

There comes a point in some of the more complicated fisheries when it becomes unrealistic to have an objective that is too sharply defined and to fine-tune the fishery by management. Trying to do so would make fisheries science less credible. It must be acknowledged that some things cannot be predicted and to do so would be foolish. A difficulty when managing by TAC is that a seriously incorrect prediction will destroy scientific credibility. Only those targets that can be achieved scientifically should be aimed at, and scientists should not pretend that they can be absolutely precise about future catch levels, even for a short time. All of this comes back to not making TACs the dominant idol to be worshiped. Let fishermen go fishing and review the situation every few years to monitor F. Such a system would not be perfect, but it would not be a disaster either, and it may be the best way of gradually edging fishermen into a more sensible husbandry approach.

References

Andersen, K. P., and Ursin, E. 1977. A multispecies extension to the Beverton and Holt theory, with accounts of phosphorus circulation and primary production. Meddelelser fra Danmarks Fiskeri- og Havundersøgelser, New Series, 7: 319–435.

Anderson, E. D. (Ed.). 2002. The Raymond J. H. Beverton Lectures at Woods Hole, Massachusetts. Three Lectures on Fisheries Science given May 3–4, 1994. U.S. Department of Commerce, NOAA Technical Memorandum NMFS-F/SPO-54. 161 pp.

Baranov, F. I. 1918. K voprosy o biologicheskikh osnovaniyahk rybnogo khozyaistva (On the question of the biological basis of fisheries). Nauchnyi Issledovatelskii Ikhtiologicheskii Institut Izvestiya (Report of the Division of Fish Management and Scientific Study of the Fishing Industry), 1: 81–128. (In Russian).

Bertalanffy, L. von. 1938. A quantitative theory of organic growth. Human Biology, 10(2): 181–213.

Beverton, R. J. H., and Holt, S. J. 1957. On the Dynamics of Exploited Fish Populations. Fishery Investigations Series II, 19. HMSO, London. 533 pp.

Bückmann, A. 1936. Scholle (*Pleuronectes platessa*). Die relative Stärke der einzelnen Jahrgänge in verschiedenen Meeresteilen. Rapports et Procès-Verbaux des Réunions du Conseil International pour l'Exploration de la Mer, 101 (3,7): 3–15.

Clark, R. S. 1936. Age composition of haddock in European waters. Rapports et Procès-Verbaux des Réunions du Conseil International pour l'Exploration de la Mer, 101(3,2): 3–7.

Edser, T. 1908. Note on the number of plaice at each length, in certain samples from the southern part of the North Sea, 1906. Journal of the Royal Statistical Society, 71: 686–690.

Garstang, W. 1900. The impoverishment of the sea. Journal of the Marine Biological Association of the United Kingdom, New Series, 6: 1–69.

Graham, M. 1935. Modern theory of exploiting a fishery, and application to North Sea trawling. Journal du Conseil International pour l'Exploration de la Mer, 10: 264–274.

Graham, M. 1943. The Fish Gate. Faber and Faber, Ltd, London. 196 pp.

Gulland, J. A. 1965. Estimation of mortality rates. Annex to Arctic Fisheries Working Group. Report of meeting in Hamburg, 18–23 January 1965. ICES CM 1965/Gadoid Fish Committee: 3. 9 pp.

Heincke, F. 1913. Investigations on the plaice. General report. I. The plaice fishery and protective regulations. Rapports et Procès-Verbaux des Réunions du Conseil International pour l'Exploration de la Mer, 17: 1–153 + appendix.

Hjort, J. 1914. Fluctuations in the great fisheries of northern Europe reviewed in the light of biological research. Rapports et Procès-Verbaux des Réunions du Conseil International pour l'Exploration de la Mer, 20: 1–228.

Holden, M. J. 1993. The Common Fisheries Policy: Origin, Evaluation and Future. Fishing News Books, Oxford. 274 pp.

Hulme, H. R., Beverton, R. J. H., and Holt, S. J. 1947. Population studies in fisheries biology. Nature, 159: 714–715.

Jones, R. 1961. The assessment of the long term effects of changes in gear selectivity and fishing effort. Marine Research (Scotland), 2: 1–19.

Jones, R. 1964. Estimating population size from commercial statistics when fishing mortality varies with age. Rapports et Procès-Verbaux des Réunions du Conseil International pour l'Exploration de la Mer, 155: 210–214.

Lee, A. J. 1992. The Ministry of Agriculture, Fisheries and Food's Directorate of Fisheries Research: its Origins and Development. MAFF, Directorate of Fisheries Research for England and Wales, Lowestoft. 332 pp.

Moran, P. A. P. 1950. Some remarks on animal population dynamics. Biometrics, 6: 250–258.

Murphy, G. I. 1965. A solution of the catch equation. Journal of the Fisheries Research Board of Canada, 22: 191–202.

Ottestad, P. 1933. A mathematical method for the study of growth. Essays on population. Hvalrådets Skrifter, 7: 30–54.

Pearl, R. 1925. The Biology of Population Growth. A. A. Knopf, New York. 260 pp.

Ricker, W. E. 1954. Stock and recruitment. Journal of the Fisheries Research Board of Canada, 11: 559–623.

Russell, E. S. 1931. Some theoretical considerations on the "overfishing" problem. Journal du Conseil International pour l'Exploration de la Mer, 6: 3–20.

Schaefer, M. B. 1954. Some aspects of the dynamics of populations important to the management of the commercial marine fisheries. Inter-America Tropical Tuna Commission Bulletin, 1(2): 26–56.

Sund, O. 1936. The fluctuations in the European stocks of cod. Rapports et Procès-Verbaux des Réunions du Conseil International pour l'Exploration de la Mer, 101(3,1): 2–18.

Templeman, W. 1972. Year-class success in some North Atlantic stocks of cod and haddock. International Commission for the Northwest Atlantic Fisheries, Special Publication, 8: 223–238.

Thompson, W. F., and Bell, F.H. 1934. Biological statistics of the Pacific halibut fishery. 2. Effect of changes in intensity upon total yield and yield per unit of gear. Report of the International Fisheries (Pacific Halibut) Commission, 8. 49 pp.