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Half a Century of Fishery Biology In Europe

MICHAEL GRAHAM

Fisheries Laboratory, Lowestoft, England

At the invitation of the King of Sweden, representatives of several Governments met on 15th June, 1899, in Stockholm. The countries represented were Denmark, Norway, Sweden, Germany, Great Britain, the Netherlands and Russia. They wished to join in studying the seas: the hydrography especially in relation to fisheries, the fisheries themselves, and the biology of the various species of fish.

During the century that was just ending there had been some half dozen pioneers into these subjects. Victor Hensen had conceived the possibility of evaluating the production of the sea by sampling with plankton nets (Hensen, 1911). Frank Buckland had devoted a life's work to the conception of managing fresh water fisheries according to their natural history.* T. W. Fulton (1897) had started to relate hydrographical studies to the facts of the commercial fisheries. E. W. L. Holt (1893) had done the same for biological studies, and had advocated remedial measures for the North Sea plaice. Particularly eminent among these pioneers was C. G. J. Petersen (1894) who had correctly appraised the problem of rational exploitation of a fishery, and had also demonstrated the possibilities of transplantation to areas with a rich bottom fauna. On the hydrographical side, Otto Pettersson had sketched the possibility of relating fluctuations in fisheries to astronomical phenomena through hydrographical effects, and so of foretelling them (1899). Frijof Nansen (Helland-Hansen and Nansen, 1909) had begun the study of water masses, and Martin Knudsen (1899) the chemical and physical characteristics

*See, eg., Graham, 1948

of sea water. These names may serve to indicate the kind of discoveries that had already been made when the conference met in 1899. It is not claimed that everybody important in that period has been mentioned.

The requirements seen by the delegates at the Stockholm conference stand out as three. There was considerable need for improvement and standardization of methods, especially in hydrography. Next, the delegates evidently thought that the isolated workers had not yet given sufficiently full descriptions either of the hydrography or of the biology of fisheries, and they planned a very full program of observations at sea. Thirdly, they considered that the time had come, or was coming very soon, for international regulation of the fisheries in the general interest, and they saw the need of joint consideration and discussion of the best means for bringing this about.

We can relate those requirements to two aims. For the great fisheries, especially those in northern waters, the delegates saw reason to believe that a study of the hydography would advance the efficiency of the fishing. For the hard-fished North Sea and for similar waters, they saw a fair prospect of introducing more profitable fishing by agreement between governments.

At Stockholm, Sir John Murray thought that, "within the last 50 years the civilized nations of the earth have been raised to a higher plane of intellectual progress than that on which humanity before proceeded"; and that civilized people were outgrowing the stage of thought where seas and mountains fortuitously made enemies of nations who otherwise would be friendly and cooperative.

The spirit of the conference, and of the two that followed it, was optimistic. At the inaugural conference of the *Conseil International pour l'Exploration de la Mer* in Copenhagen in 1902, the Danish delegate, Captain C. F. Drechsell, said that he was sure that when the Conseil was constituted there would be results of importance for the fishing industry.

The authority from which the early history of the International Council can most conveniently be gathered is that of the Jubilee Report published in 1928, covering the period 1902-1927. The same volume gives an adequate account of the research effort and equipment made by each country to fulfill its undertakings at the meetings of 1902 and 1903, at the first of which Finland had joined, and at the second of which Belgium was also represented. In 1912, France and the United States also joined, but the United States and Russia did not resume participation after the first World War. Portugal joined in 1922, Poland in 1923, Spain in 1924, and Ireland in 1925.

In the dozen years or so that were allowed to them before the first World War interrupted their activities, the scientists under the Council did a great deal of work. On the hydrographical side their achievements were both necessary and new. They standardized the methods for determining salinity and temperature by which the main water masses could be described. On the biological side, however, the work at the beginning of the century does not, at the present day, appear to represent substantial advances on the conceptions of the pioneers of the previous century. Two exceptions may be made.

The first exception was the study of fluctuations, associated particularly with the name of Johan Hjort and his fellow workers in Norway, which emphasizes the fact that the success or failure of most broods of most species of fish is determined very early in their history, so that a rich or poor brood can be designated as such either before it enters the commercial fishery or just afterwards. This discovery has been the basis of forecasting fisheries in many countries. The demonstration depended on methods of age determination by scales or similar structures, which often had clearly marked on them a number of zones corresponding to the years of life of the fish. The year-rings had been discovered before the Council was established, but the Council helped to finance the Norwegian workers who made the method applicable to herrings and cod (Hjort, 1914).

The second exception that could be made would be in favour of Buchanan-Wollaston's work in applying Hensen's conception of evaluating marine production, including that of the demersal fishes, to the plaice of the southern North Sea (Buchanan-Wollaston, 1923). Hensen and Apstein (1897) had not succeeded in making a satisfactory quantitative estimate for the winter spawning fishes in the North Sea. It remained for Wollaston to devise new ways of treating the data, from an adequate number of cruises using Hensen's net, which he did successfully.

For the rest we must notice the marking of fish on a prodigious scale, which served to confirm and elaborate the knowledge from earlier markings; and detailed descriptions of the hydrography of various regions: in general, a conscientious filling in and confirmation of the discoveries made by men of genius in the previous century. The confirmation of the high recapture rate of marked fish has proved of great propaganda value, and, in general, doubtful minds were sustained by the masses of data that were produced; but it might be argued that in no other way were the aims of relating hydography to fisheries or of understanding and introducing rational fishing brought nearer in 1914 than they were in 1899. Indeed, the emphasis on fluctuations in brood strength, which do not in fact reduce the importance of the other two problems, served as a distraction.

Two recommendations did come from the studies in the period before 1914. The first was that an extensive area should be closed on the continental coast as a nursery for plaice in the North Sea (Holt's proposal, 1893), and the second was that there should be large-scale transplantation of small plaice from the crowded nursery areas to the rich feeding grounds of the Dogger Bank. Neither recommendation has been applied internationally as yet.

In most countries, fishery research ceased during the war of 1914-1918. At the end of that war, however, it was plain for everybody to see that the contention of the scientists was amply justified, that fishing controlled the weight of the stock of demersal fish in areas such as the North Sea. The stock in 1919 was very much heavier than it had been in 1913, manifestly as a result of the reduction of fishing to a small fraction of what had taken place formerly. The question was indeed asked, whether this increase could be due to natural fluctuation in brood strength, and there had, in fact, been a favorable fluctuation of haddock in 1913. It was however forcibly pointed out by "Quibbon," a writer in *The Fish Trades Gazette*, that nobody had previously heard of three species enjoying a great positive fluctuation simultaneously: cod, haddock, and plaice. The evidence after the war of 1939-1945 proved even more telling (Cons. Int. Explor. Mer. Rapp. et Proc. Verb. 132).

The Jubilee Volume of 1928 records research done between 1919 and 1927, some of which is a little more inspiring than what had preceded the first World War, and for which the painstaking studies of the earlier periods may be given some credit, because they provided a background of general and of particular information. For example, the determination of temperature and salinity was standardized in the earlier period; the life-history of several important species became known in sufficient detail; and the larval stages of nearly all species of fish were described so well that they could easily be recognized. As hydrographical methods no longer constituted a problem, E. le Danois was able to extend his imagination to the effect of fluctuations — "transgressions" as he called them — in the North Atlantic water, as they affected the fisheries on the coasts of Europe. Without claiming that his ideas were valid in all respects, we may recognize his as an attempt to reach that understanding of fishery hydrography which was aimed at by the founders of the International Council.

The detailed studies of the larvae of various fish had led Johannes Schmidt to fix his attention on the Leptocephali of eels, and thence to his classical discovery of the main spawning ground for European eels in the region of the Sargasso Sea, from which the larvae drift towards the coasts of Europe, naturally impinging in greatest numbers on the western seaboards. This discovery, that the source of all their eels was far to the westward, led some German workers to the practical idea of transplanting elvers from places where they arrived in enormous numbers, such as Epney on the estuary of the River Severn, to inland waters in north Europe, which the eels did not naturally reach so easily. It can be said that in this curious way the International Council was instrumental for the first time in raising the yield of a fishery. How soon the same result would have been achieved without the existence of the International Council is a question on which it is idle to speculate.

Although the Jubilee Volume of 1928 contains several other matters of technical interest, the reader will find it difficult to discern anything definite, apart from the theory of le Danois and the transplantation of the eels, that could be classed as an important advance either in fishery biology or in its practical application. The first 25 years of the International Council formed, certainly, a period of promise on the original aims of the Council, but there was precious little that an outsider would recognize as performance.

From the Norwegian work on brood strength, the first quarter century did see the establishment of forecasting of fisheries, of which Harold Thompson's work on the haddock (1930) and W. C. Hodgson's on the herring (1932) may be mentioned, although neither receives very much emphasis in the Jubilee Volume. Both have continued successfully until modern times, and the Council must certainly be given credit for discernment in financing the work in Norway on which this method of forecasting was based.

The picture changes in the second quarter century, beginning in 1928, because then the main aims of the Council began to be realized, although in this period too there was necessarily a great deal of work which could be described as promising for the future rather than as of important practical application, or as representing a major advance of knowledge.

Perhaps it will be best to deal with this promising work first and then deal later on with the lines of research that can be said to have arrived.

Part II of the second Jubilee Volume of the International Council is constructed as a review and guide, in eight subjects considered to be of major interest. In addition, we may consider the report on fisheries hydrography published in 1952, making a ninth subject for consideration in the present history. Of the nine subjects, four may be considered as having arrived. Nothing derogatory is intended in this distinction: it refers merely to the original aims of the Council, judging the contributions on the rather strict basis of whether a critical layman could agree that the research work in question had or had not yet contributed to the fulfillment of those aims.

The first subject to consider is called "The Milieu," which is a perhaps not too happy name for general hydrography in a wide sense, but excluding the cycle of nutrient salts. The paper in the Jubilee Volume serves as an informative catalogue to a prodigious bulk of work describing the movements and temperature and salinity of water masses in the North Atlantic and in its dependent seas. In addition, it gives considerable space to the outstanding advance in knowledge and understanding of submarine illumination that had taken place during the period under review.

Quite outstanding also, has been the advance in knowledge of the cycle of nutrient salts, including the carbon dioxide system, which receives full and excellent treatment in another paper of the volume.

The second paper in the volume reviews the progress of knowledge and understanding of the plankton, which has used a great proportion of research effort ever since the start of the Council, and which continues to be judged, probably rightly, as likely to yield understanding of many practical problems in fisheries. Indeed, here and there, knowledge of plankton has nearly or quite arrived at this stage. Cushing (1953) appears to be on the point of making the current fate of patches of *Calanus* a guide to the fishermen for herrings in the North Sea during the summer; and it must not be overlooked that Russell's diagnosis, with the aid of species of *Sagitta*, that the water of the English Channel off Plymouth has in recent years been less productive than formerly, appears to provide a warning to owners of herring drifters not to attempt a revival of the winter herring fishery in that area. The proof of the practicability of this argument will come when *Sagitta elegans* again becomes abundant in the English Channel — if a herring fishery is then successfully revived. There is still a strong belief that in many fisheries the detection of indicator plankton organisms may be of practical importance to fishermen.

It is interesting that the marking of fish, which almost always arouses the interest of fishermen, does not by itself appear to achieve the classification of being already of value to them. Marking plays an important part, but it does so indirectly, because of its information about overfishing, which will be dealt with under the second group of studies. In the meanwhile we must pass by the half million fish that have been marked by the member countries of the International Council in the recent quarter century, noting only how fascinating it is to see charts of migrations of marked herring between northern Iceland and southern Norway, or of marked cod extending via Greenland and Iceland from Newfoundland to the Faroe Islands. Mention too must be made of salmon migrating between Scotland and southern Norway, and between northern Norway and the north coast of Russia.

When we consider the record reviewed so far, it is interesting to speculate on whether some of our fore-runners would have found the slowness of the progress, combined with such a prodigious amount of work, on the whole irritating, or whether they would have been satisfied. Would Buckland, Hensen, Petersen, and Nansen have been pleased? Personally, I am not at all sure that they would. When, however, we turn to the five remaining subjects, I think that we could be more confident that we would receive some praise from the pioneers, although I should think poorly of them if they did not find much to criticize in the halting and hesitating manner in which we have fulfilled some of their original conceptions.

I can imagine, for example, that Frank Buckland would have been pleased at the evidence given in the second jubilee volume of numerous applications here and there of scientific knowledge to the conduct of the fishing industry or to the formation of policy. In a first category are noted investigations of fishing methods, use of aids to find fish, information on the occurrence of fish, including the establishment of new fishing grounds, and forecasts of the yields of various fisheries. On applications to policy, it is noted that this is a slow process, but the Baltic fisheries were the subject of a Convention in 1929, those of the Transition Area (Skagerrak, Kattegat and Belts Seas) of a Convention in 1937, and the crustaceans of the Skagerrak and Kattegat were protected by the Convention of 1951. The discoveries made under the Council had also influenced some actions by individual countries, and had contributed some of the knowledge necessary to the setting up of the International Whaling Commission. The principal activity, however, that might have commended itself to Frank Buckland was that leading to the Conventions that attempt to introduce more rational fishing in the North Sea. The Convention of 1937 introduced regulations of the meshes of fishing nets, along with size limits for several important species of fish, and the Convention of 1946 raised the figures to levels that should, if all goes well, after its introduction in 1954, result in appreciably better fishing of the important species the haddock as well as of hake and sole.

Hensen would surely approve of the extent to which his method of evaluating a fish stock, by estimating the total number of eggs, and thence the number of spawning females, has become established as an important part of the evaluation of the stock of plaice in the southern North Sea. Ordinary observations of length and weight on fish markets can establish the number of fish landed, and the ratio of spawning females to the total catch. Hensen's method, as developed by Buchanan-Wollaston, allows an estimate of the number of spawning females in the sea. It is a small step to estimate the total number of plaice of fishable size in the sea, applying the ratio found on the fish markets. We then have an estimate of the stock of fish, and of the catch, and so of the rate of fishing. This is an invaluable check on independent estimates, made either from marking experiments or from observations under widely different rates of fishing, such as became available owing to the second world war.

I think that we could reasonably expect Nansen to have been pleased with the progress in the study of the water masses in the northern seas and of their effects on the fisheries. The Council meeting in 1951, the year before the Jubilee, had included a paper by Devold (1952) in which he showed, in the wide area of sea between Iceland, Jan Mayen, and Norway, that vast populations of herring were located on the borders of Atlantic and polar water. When this border shifted so did the herring. Further details may be gleaned by study of the original paper. In the report of the same meeting, a paper by A. J. Lee (1952) describes how the English "Arctic team" discovered that the concentrations of cod in May/June, October, November, and December, off Bear Island, tend to be strictly located on the border of Atlantic or Arctic type water. West of Bear Island the cod in those months are found in concentrations along the boundary between the Atlantic water of the West Spitzbergen Current and the mixed water of the Bear Island shelf, which may be designated by the isotherm of 3¾°C. South of Bear Island the concentration tends to be at the boundary of the mixed water and water that is more Arctic in origin, marked by the isotherm of 1¾°C. The Norwegian herring fishery and the English cod fishery are so large in magnitude that these discoveries in fishery hydrography stand out conspicuously as beginning to fulfill one of the original aims of the International Council. The jubilee volume rightly calls attention to earlier discoveries of the same kind, referring especially to whiting in the English Channel, to the herring of the "Smalls," off the south of Ireland, and to the tunny species *Germo alalunga*. Other examples come to mind.

We may pass quickly over two subjects considered in the jubilee volume, important as they are. The first of these is the transplantation of fish, which has been conspicuously successful in Danish waters, and is still somewhat promising for the North Sea, although it has not yet been applied there internationally. The second is rather on one side from the main researches of the Council, namely the detection and recognition of fish by echo-sounding. This is established as a regular aid to several kinds of fishing, and is likely to prove of major importance permanently, both in commercial fishing and in research.

When we turn to the question of whether Petersen would have approved the progress in understanding of the problem of regulating fisheries to the best advantage (fishery management), there is doubt about the answer. In 1894 he had written, "For it cannot well be doubted that the same area of sea would be able to give a quantitatively greater profit as a constancy, when we suffered the stock of fish to be as fully developed, as in the years before the too eager fishing commenced . . . and then took exactly so much as the stock would reproduce by new growth." His participation in the work of the International Council was not very close after the year 1908, and from then onwards until his death in 1928 his name is mainly associated with investigations of bottom fauna, although he did report again on the stock of plaice (Petersen 1921). The words that I have quoted include the phrase "quantitatively greater profit." It is a conspicuous fact that this sixty-year-old enunciation of the main prize obtainable by regulation of a fishery is still a novelty to the vast majority of people who hear about fishing, and, alas, to a substantial number of those who speak about it. Instead, during the remainder of Petersen's lifetime and since his death, discussion about fishing has nearly always concerned itself with conceptions dating from the previous century, or earlier; fear of reduction in numbers of fish, or in the yield of the fishery; to be countered by this or that self-sacrificing method of protection of the fish, for the benefit of future generations of fishermen and consumers.

Petersen's paper of 1894 shows that he had correctly appreciated the relation between fishing and a stock of fish. This is a dynamic relation, with the stock tending to grow heavier all the time, and fishing tending to make it smaller, the net result, when conditions are, on the average of a series of years, stable, being no change in the weight of the stock. But there is a greater profit "as a constancy" at some levels of fishing than at others. Admittedly, many factors enter into the achievement of a profit, but for the individual fisherman the profit depends directly upon the catch per unit effort of fishing, as for example per day's absence of a standard steam trawler. However, catch per unit effort cannot be allowed to run away with policy, because the maximum catch per unit effort is at a very low rate of effort—in the extreme case, one single fishing vessel—and hence a very low yield and total profit. Nor can yield be allowed to run away with the situation, because the maximum yield would be obtained by allowing the fish to grow to a great size, and the fishing them at that size instantaneously, which would require such a prodigous effort that there could be no profit in it. The greatest profit would come at some intermediate level of fishing, and the exact level which would be of greatest benefit to the community as a whole will not depend only on purely biological factors. The problem of fishing is therefore one of making reliable, in the sense of unbiased, estimates of effort and yield, to be expected from changes in the rate of fishing. Reading of Petersen's paper suggests that he perfectly understood the nature of this problem; reading of all other papers published before 1930 suggests that nobody else, except the Russian Baranov (1916) whose paper was overlooked, appreciated the nature of the problem.

The proper task of fishery biology, during the half century under review, can now be seen to have been the conversion into mathematical symbols of Petersen's understanding of the problem in words, and the collection of data strictly applicable to the problem. This task was begun by Russell (1931) when he expressed the tendency of the stock to increase in weight by the two symbols, A, standing for the increment due to the weight of recruits to the fishery, and G, the increment due to growth of fish during the period when they are in the fishery. He pointed out that in a steady state the sum of those must equal, quantitatively, the sum of the two other quantities, C, the weight caught by fishermen, and M, weight lost by natural mortality.

It will be seen that Russell's formulation contained two clarifications of particular importance. The first was the distinction of the stock old enough to be taken by the fishery, which he emphasized by designating a critical length of fish *l*. He pointed out that if one raised *l* by mesh regulation and size limits, one would have heavier fish comprising A and G, and so would expect to be able, other things being equal, to take a greater catch, C. The second clarification was the simple separation of the three unknown factors that required evaluation, A, G, and M.

Russell's formulation was quite self-evident, but when it was considered as an aid to guidance in making great changes in fishing and in stocks, as for example in trying to decide what level of fishing to aim at, the difficulty was immediately seen that his factors, or processes, did not remain constant at different levels of stock, but re-acted with each other. Russell himself laid stress on this difficulty, and for two or three years it seemed to render any estimates that one could make of the processes, of little use, because they could refer only to the current state of the stocks. The way out of this difficulty was fairly easy, and was found by Graham (1935), who saw that Russell's equation could be of use, not in order to decide ultimately how far one should go in regulating a fishery, but to decide the important point of whether to go towards reducing fishing or towards increasing it, compared with the present state, which could be diagnosed from existing data on values of the processes. For this purpose, however, it was necessary to re-state Russell's equation, which had been in the form of increments, in terms of instantaneous rates. It thus became a differential equation. We may note, although it is outside the terms of reference of the present paper, that Thompson and Bell (1934) had already found their own way of using theory, essentially on Russell's lines without being hampered by Russell's difficulty.

Another way of overcoming, in theory at any rate, Russell's difficulty of the interaction of the processes, had been taken by Hjort, Jahn, and Ottestad (1933). Without separating the processes, they advanced a theory for the overall growth of a stock, which would be the net increase, with reproduction and growth positive and with natural mortality negative. Having graphed this total growth against time, the first derivative of their main function would represent what could be taken by fishing, at any given age of stock, without either increasing or decreasing it. Bearing in mind that the catch per unit effort is proportional to the stock of fish on a ground, strictly and practically so in a steady state, we perceive that the yield of a fishery in a given time is the differential coefficient of the catch per unit effort. Unfortunately, determination of the parameters of the equation of Hjort *et al*, requires observation of very great changes in rate of fishing and of stock. The first World War had provided such changes, and Graham (1935) with considerable difficulty, used the observations that happened to result from the war to make a crude determination of the maximum yield of the North Sea fisheries. It will be seen later that, mercifully, Graham's paper of 1935 is now obsolete, but I venture to include it in this review on the ground that at least one fishery biologist had returned to the statement of the problem as correctly seen by Petersen. The Petersen emphasis on profit was re-stated by Graham, "that the benefit of efficient exploitation lies more *in economy of effort* than increase of yield, or preservation of future stocks though both of these purposes may also be served."

The full and precise formulation of the problem of fishing has had to await the reconstitution of fishery scientific staffs after the second world war. In the meanwhile, however, the International Council was not idle in this subject, but was pushing ahead in its own characteristic way. The Conventions of 1937 and 1946 have already been mentioned, and it now falls to be related how they came into being. The aim was to raise the age at first capture by mesh regulations and size limits. The special activity to promote this extended from 1929 to 1934. In the latter year the entire Council, in its committees and as a body, recommended that member governments should put such regulations into force. The idea of mesh and similar regulations was very old—the first English mesh regulations were made in the reign of Edward III.* Nevertheless, the Council had to settle two main difficulties. The first was that small fish, if thinned out, grew faster, and it might therefore be a mistake to spare their lives. This, from its nature, was a very difficult point, but at a meeting in 1932 Russell and Bückman were each able to show quantitatively that this consideration need not deter the Council. The second objection was that, however large the mesh, it is drawn so tightly while towing a trawl that there is no hole through which the fish could escape. Goodchild's experiment, reported at the meeting of 1932 by Davis, disproved this. An essential part of the investigation was Wollaston's theory (1927) of the selective action of a net, which allowed nets of different meshes to be compared correctly.

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* Moore and Moore, 1903, p. 173. In 1376 parliamentary commissioners found that the net complained of had meshes of length and breadth of two thumbs. The first mesh regulation, 2½ inches, was in I Elizabeth, C.17 1558.

Several countries put into force the very moderate mesh regulation of the Convention of 1937, but some did not; and nineteen years after the Council's final recommendation, the more useful regulations agreed in 1946 are not yet in force. But that history is not part of a review of fishery biology.

Instead, fishery biology has been concerned with the dawning realization that mesh regulations, essential as they are for certain species, including haddock and hake, will not cure over-fishing. A mesh regulation should cause the stock to consist of larger fish, thus forming an inducement to harder fishing, which, by increasing the total mortality rate, would tend to reduce again the average size of the fish in the stock. Equally, a regulated fishery in which the mesh was left free could still proceed on a downard course, by successive reductions of mesh. The two kinds of regulation go hand in hand, and existing conventions have done only half the job.

Understanding of the dual nature of the problem grew rather slowly in the 12 years from 1935 to 1947, during seven of which fishery workers mainly had other matters on their minds. By 1947, however, the double aspect was realized, though it is doubtful whether, to this day, many people realize the attraction of substantial profit that Petersen saw in allowing the stock to grow heavier. The old fear motive for conservation is so traditional that it is difficult indeed to substitute encouragement for warning; to announce good news when everyone expects bad.

In the meantime, one advance was made. Bückmann (1939) and Baerends (1947) realized that there must be a different best rate of fishing for each mesh of net: ideally the one should be adapted to the other. This is an important point in the theory, and will be referred to again; but mention of these two workers is a reminder of how many in the Council's orbit have made contributions to the general body of knowledge and understanding, yet have not been mentioned in this review. Most of them, however, are referred to in one or the other of the Council's Jubilee Volumes.

To return to the theme of this paper, the close of the half-century clearly needed a full and precise formulation of the theory of fishing that all the studies had shown necessary. At the Jubilee Meeting, Beverton (1953) was able to sketch in outline his work with S. J. Holt, which unfortunately is not yet published in full. Theirs proved a very great task, especially as the decision was taken that nearly every theoretical point should be illustrated by worked examples from data. The main equation for yield was printed in the second Jubilee Volume, from which it can be seen that it takes account of the following factors: fishing mortality, natural mortality, number of recruits, age of recruitment to the fishable area, age of capture, fishable life-span, parameters of Bertalanffy's law of growth. All these have been realized as bearing on the yield of a fishery, but it has been a major operation to fit them together correctly, each bearing their proper weight.

Without the use of such an equation, no problem of "fishery management" can be said to be completely solved. With such an equation, and with data to fit it, fishery biologists will in future be able to make diagnoses that they, at least, will be able to believe in. Where data are not available, we shall know that we cannot give a full answer, but must proceed, as formerly, by giving the best advice we can on the information available.

This review must end, but a winding-up sentence would be unreal, because the subject of the review is by no means wound up. Fifty is an arbitrary number of years, and a small one.

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50 Years of Progress in Solving Fishery Problems

WILLIAM C. HERRINGTON
The Special Assistant for Fisheries and Wildlife to the Under Secretary of State, Washington, D. C.

The subject under consideration is a broad one, for accomplishments in the field of fishery biology are many. They may be found in the way of general additions to knowledge, contributions to the philosophical or practical solution of problems, benefits to the individual, and doubtless many others. However, this discussion will be confined to accomplishments in solving, or contributing to the solution of problems of fishery conservation and productivity, particularly in the field of commercial fisheries. The term "accomplishments" will be considered in its broadest sense, for fishery biology has contributed to many non-biological accomplishments.

The State of Fishery Knowledge 50 Years Ago

One procedure for checking on accomplishments is to consider the state of knowledge in the field of fishery biology 50 years ago, as a starting point from which to measure progress. Any difference in the situation then and now can be considered as an accomplishment, whether good or bad.

The Proceedings of the Fourth International Fishery Congress held in Washington, D.C., in 1908 provide a good source of information on this subject. Assuming some lag between the development of ideas and the presentation of such ideas in print, it may be reasonable to conclude that the reports at this Fishery Congress in 1908 reflect the general situation in the field of fisheries some 50 years ago.

In discussing international regulations of fisheries on the high seas, Mr. Charles E. Fryer, Superintending Inspector of the Board of Agriculture and Fisheries, London, made the following statement:

"The Fisheries carried on in the high seas are to a large extent concerned with fish of whose habits we know very little. Notwithstanding all that has been done here in this great country — probably more than