Recent Reports of Fishery Authorities.

The Scottish, Newfoundland, and United States Reports.

By

J. T. Cunningham, M.A.

(1) Thirteenth Annual Report of the Fishery Board for Scotland, being for the year 1894. Edinburgh, 1895.

(2) Annual Report of the Newfoundland Department of Fisheries for the year 1894. St. Johns, N.F., 1895.

(3) Report of the United States Commissioner of Fish and Fisheries for the year ending June 30th, 1893. Washington, 1895.

Artificial Hatching of Fish Eggs.—The Scottish Report, whose title is given above, is stated to refer to the year 1894; but as a matter of fact, a great deal of the work recorded in it was carried out during the earlier portion of 1895. This is the case with the operations of the Dunbar Hatchery, described by Mr. Harald Dannevig, the Manager of that establishment. We find that 44,085,000 eggs of plaice were collected last spring, and from these 38,615,000 fry were obtained, and liberated in the sea. This shows a loss of only 12 per cent. in the process of hatching. But large as the numbers appear, it should not be forgotten that the above number of eggs represents the produce of only 220 female fish, reckoning 200,000 eggs to each, which is a low estimate, for it has been proved by Dr. Fulton that the larger female plaice produce each from 300,000 to 500,000 eggs in one season.

It is, I think, interesting to consider, from various points of view, the proportion borne by the artificial hatching operations to the natural propagation of the fish in the sea. We have not at present ascertained approximately the number of females which spawn in the sea in one season, but we have some data concerning the number of mature females taken out of the North Sea, in one year, by the fishermen. According to Mr. Holt's statistics, which were very carefully collected, the number of mature plaice, over 17 in. in length, landed at Grimsby alone in one year is more than 7,000,000; and as there are three females to two males, we may reckon that over 4,000,000 of these are females. We take, then, 4,000,000 of mature female plaice from the North Sea at Grimsby alone, not to speak of the numerous other trawling ports on the east coast of Britain, and in return we hatch the eggs of 220. The proportion here is one spawner in the hatchery for every 19,090 spawners killed at Grimsby. But next we have to take into consideration the superiority of the artificial process. We do not know what is the mortality of the eggs and fry in the period between fertilisation and the absorption of the yolk, under natural conditions. As we have seen, in the hatchery the mortality is only 12 per cent. Let us assume, for the purposes of calculation, that the loss is only 10 per cent. in the hatchery, and is 90 per cent. in the sea. Then we obtain nine times as many fry in the hatchery as in the sea from the same number of fish. It comes to the same thing if we say that one female spawner in the hatchery is equal to nine spawners shedding their eggs, without assistance, in the sea. We may say, therefore, that the work of the hatchery is equivalent to saving 9 females out of every 19,090 landed at Grimsby, or one out of every 2,121, or, in round numbers, one out of every 2,000. The disproportion would be very much greater if we took the total number of female plaice landed on the east coast of Britain. It seems to me beyond question, that if we regard the whole North Sea plaice fishery in this way, not taking the numbers caught by foreign fishermen into account, the results produced by the operations at Dunbar will be quite imperceptible. To diminish the destruction of mature fish even by one in every 2,000 in each year, could not make any appreciable difference in the general abundance of plaice in the North Sea.

It must not be supposed that I have any prejudice against artificial hatching, or that I am unable to appreciate the skill and efficiency with which the Dunbar establishment has been organised and operated. On the contrary, I think that Dr. Fulton and Mr. Harald Dannevig deserve great credit for the energy and ability they have exhibited in the working of the first British hatchery for sea-fish, and for the success they have obtained. No harm, but only good, can result from an honest and strictly accurate calculation of the possible results. The evidence available from other enterprises of the same kind tends to show that quite obvious local results have been produced by the liberation of large numbers of fish-larvæ in the sea, and although, as the above calculations show, we cannot expect to perceive any increase in the general plaice production in the North Sea, in consequence of the work at Dunbar, it may be quite possible to recognise on particular local grounds an increased abundance of marketable plaice, derived, with reasonable probability, from the fry liberated from the hatchery. We cannot, however, admit the correctness of certain calculations contained in the official general statement of this Report. These are, that if one in a hundred of the fry distributed from the hatchery survived, and were worth sixpence each, the resulting value to the fisheries would be about £18,000, and that it would require the survival of only one in a thousand, in value one penny each, to cover the expenses of the work. Fish in the sea have clearly no value, and we cannot hope to catch all of them. It is difficult to say whether a quarter, a half, three-quarters, or what proportion would be caught; but even when they were caught and sold, their value could not be all applied to defray the cost of hatching, because the greater part of it, as usual, would go to defray the cost of catching and marketing. Such calculations would only be applicable to fish that were reared entirely in confinement, like chickens or pigs.

The importance of the working of a marine hatchery at the present time, and on the comparatively small scale of that at Dunbar, may be reasonably held to be, not in the immediate utilitarian result to be derived from it, but in its value as a sufficiently extensive experiment in the open. We have reached a certain point in laboratory research and experimentation. We have discovered enough concerning the life-histories of food fishes, and their place in nature, to obtain glimpses of the possibility of a more scientific and more profitable exploitation of the products of the sea than that which is now practised. To convert these glimpses into comprehensive perception, we require more investigation and experimentation under the open sky, and on a scale commensurate with the extent of the regions to be exploited. Thus the managers of a hatchery ought not be content with proclaiming the millions of fry they have liberated, but should ascertain what ratio these numbers bear to the number of fry naturally present in the region where they are placed, and should make every endeavour to trace their future history. In this Report Dr. Fulton gives the result of some very valuable experiments he has made, as to the effect of the currents on the east coast of Scotland, on bottles floating level with the surface. These results show that buoyant objects at the surface are carried southward and eastward to the neighbouring shore. One or two of these bottles were found ultimately on the German coast, near Heligoland. The fry from the hatchery were liberated at the mouth of the Firth of Forth, and in St. Andrew's Bay, and according to the direction of the drift, ascertained by the experiments just mentioned, the survivors should be found chiefly on the southern shores of the Firth, and further south-east towards Berwick. It will probably turn out, therefore, that the influence of a hatchery is confined for the most part to a comparatively limited neighbourhood, and it ought to be possible, if the necessary data are accurately observed, to ascertain the magnitude of its influence within these limits. Comparisons should be made between the natural propagation of the fish, and the artificial propagation within the limits thus set by natural conditions. Hitherto they have not been made, and the necessary observations have not been carried out. The results would doubtless be more favourable than those of the comparison above made between the operations of one hatchery and the Grimsby fishery.

In the Newfoundland Report for 1894 it is stated that the number of cod eggs treated at the hatchery, on Dildo Island, Trinity Bay, in that year, was 346 millions, from which 221½ million fry were obtained and liberated. This is a survival of 64 per cent., or a loss of 36 per cent. The number of cod fry liberated was, therefore, nearly six times as great as that of the plaice-fry produced at Dunbar. But it must be remembered that the cod normally produces a much larger number of eggs than the plaice. According to Dr. Fulton's calculations, the number in the cod varies roughly between three and six millions. If we take four millions as a moderate average, the above number of eggs is the produce of only eighty-six female cod, so that from this point of view the work of the Dunbar Hatchery on the plaice was really of greater magnitude than that of the Newfoundland Hatchery on the cod. The efficiency of the treatment was considerably greater at Dunbar, that is to say, the loss or mortality during the treatment was much less in the Scottish establishment. But at Dildo Island the number of eggs collected was so large that there was not room for all of them in the hatching apparatus, and the excess was utilised by being placed in linen bags, suspended in wells, in the wharves outside the hatchery. This may to some extent account for the greater percentage of loss.

As evidence of the successful results of the hatching operations in Newfoundland, it is stated that in the beginning of the summer of 1894 there was a great abundance of cod of various sizes and ages in Trinity Bay, and none in the neighbouring Bonavista and Conception Bays, where the season's fishery turned out very poorly. The liberation of fry has been carried out annually since 1890 in Trinity Bay only, and it is maintained that the cod found in large numbers in that bay in 1894 were derived from the fry deposited. It is stated that the cod one year old were most abundant, next to these in numbers were cod of two years, and then the three-year-old fish, with a fair proportion of still older and larger fish. This is in accordance with the continual increase in the number of fry liberated each season since 1890.

In the Newfoundland Report for 1892 it is pointed out by Mr.

Nielsen, who conducts the hatching operations, that the idea that if the fish were not artificially treated they would propagate in the natural way, is a mistake. All the spawners are caught in the neighbourhood of the hatchery, and if there was no artificial hatching just as many fish would be taken, and no living fry would be returned to the sea from them. We here come upon the same question which was indicated above, in reference to the Dunbar Hatchery, namely, what proportion exists between the number of spawners artificially treated, and that of those which spawn naturally in the same district. Evidently Mr. Nielsen's view is that the capture of adult fish is so great that very few are left to shed their spawn, or, at any rate, such a small number that the number of the fry derived from them is small in proportion to the number of fry liberated from the hatchery. We cannot deny that the evidence given of the great increase in the number of adult cod, following directly upon the liberation of millions of fry from the hatchery, gives strong support to Mr. Nielsen's contention. But in this, as in other cases, we ought to be supplied with other important evidence, perhaps the most important being a direct determination of the number of cod eggs actually present in Trinity Bay, during the spawning season of the cod, in order that we may compare this with the number of eggs treated artificially in the hatchery. In Newfoundland, as in Norway, it has been observed that an increase in the supply of cod has followed upon artificial hatching. But in regard to all such evidence the vastness of the numbers put forward, and the absence of accurately observed data for comparison, tempt one strongly to adapt a well-known phrase and say, "c'est magnifique, mais ce n'est pas la science." The operation which is stated to produce such successful results, is that of placing so many million living fry in the sea at the stage at which the yolk has just been all exhausted. Surely it is not impossible, or even difficult, to ascertain how many such fry were in the sea already, without the operation. Until this or similar facts have been ascertained, it cannot be said that the process of artificial propagation has been put on a practical basis.

However, notwithstanding this criticism, it must be admitted that local benefit from artificial propagation appears to have been produced. This leads to some further interesting considerations. It is well-known that the Scottish Fishery Board have closed certain inshore areas to beam-trawling. In these areas there appears to be no kind of fishing carried on which involves the destruction of young plaice or the young of other flat fishes on a large scale. The abundance of the flat fishes in these closed areas has been examined annually since 1886, with great statistical accuracy, by means of experimental trawlings carried out by the Board's steamer *Garland*. Notwithstanding the protection, the number of plaice and other flat fishes has not increased: it has fluctuated from year to year, but never maintained a steady increase. The number per haul of the trawl has also not increased in the open area. * It seems reasonable to infer that the reduction of the number of spawners on the open grounds, by the great extension of the trawling industry, is so great that protection of the young fish is not sufficient to compensate for it. With food-fishes, as with oysters, we are apt to attach so much importance to the number of ova produced by each female, that we overlook the importance of the number of females. It is of course true that if we could save a larger proportion of the progeny of a few parents, we should obtain a large number of fish or oysters. But, on the other hand, it may be, and experience indicates that it would be, more practicable to obtain our object by preserving a larger number of parents. In the case of oysters more success has been secured, as Mr. Bashford Dean has pointed out, by maintaining a very large reserve of parents, than by trying to preserve a larger proportion of the progeny. One method of doing this with sea-fishes would be to create reserved and protected spawning grounds. But there are objections to this method: there is the difficulty of protection, and also the fact that the fish will wander away, and be caught on other grounds. Now it is possible to regard the hatchery, as at present worked, as simply a reserve of spawners. No matter how many spawners may be taken from the sea, those in the hatchery are safe, and will supply their annual quantum of eggs or fry. But to carry out this principle effectively it would be necessary to keep in confinement, not hundreds but thousands or millions of spawners. We should have to maintain a number bearing some significant proportion to the number which now survive to spawn in the sea. It is conceivable that if spawning fish were maintained in confinement all along the coast in sufficient numbers, we might depend for our fish supply almost entirely on the eggs and fry derived from those. It may be that this will be the ultimate solution of the problem of replenishing our exhausted fishing grounds. In the meantime, although it seems wonderful to read of hundreds of millions of fry placed in the sea, as a matter of fact we are dealing, as I have shown, with only a few hundred spawners, while thousands upon thousands are being annually captured.

* The following are the average numbers of fish taken per shot of the trawl, in two periods of four years:—

	Closed Area.		Open Area.	
	Flat fish.	Round fish.	Flat fish.	Round fish.
1886–89 . .	178·5	77·8	91·2	86·4
1891–94 . .	120·9	89·9	77·6	102·5

In the American Commissioner's Report (p. 72), we find a section devoted to the description of "Some Results of Acclimatisation." The most important of these results is the successful introduction of the Atlantic shad (*Clupea sapidissima*) to the waters of the Pacific coast of North America. The supply of shad on that coast, we are told, continues to increase, and is now so great that the retail price of the fish there is actually less than on the Atlantic coast. The shad has within a few years not merely been successfully introduced, but has permanently established itself, and become one of the cheapest fish of the region. It must be noticed, however, that this is not, properly speaking, a success to be placed to the credit of the system of artificial propagation. It is true that the introduction was effected by the transportation of artificially hatched fry from hatcheries on the Atlantic seaboard, and their liberation in Pacific waters. But the abundance of the fish in the Pacific States is due to its own natural multiplication in its new habitat, not to its continued artificial propagation there, and the same success might possibly have been obtained if a sufficient number of adult fish had been placed alive, and in healthy condition, in the rivers of the Pacific slope. The transportation of the minute fry may have been easier than that of the adult fish; perhaps, indeed, the latter operation would not have been possible at all. But even if this were so, the artificial hatching of the fry, in the first instance, was only a detail in the process of transportation and introduction, and artificial propagation has not been carried on subsequently in the new habitat, and therefore has had nothing to do with the subsequent increase in the supply, any more than artificial breeding has had to do with the troublesome multiplication of European rabbits in Australia.

The introduction of shad fry to the rivers of the Pacific States was first attempted in 1871, 24 years ago, when 12,000 of them were liberated in the Sacramento River. From that year until 1886, 609,000 fry were liberated in the Sacramento, 600,000 in the Williamette River, 300,000 in the Columbia River, and 10,000 in Snake River. Nothing is said of any planting of fry after the year 1886. The catch of the fish in 1892 was estimated at 700,000 lbs., having a value to the fishermen of £4000. But, probably in consequence of thinness of the population, the demand for shad in the west seems very slight, the price in 1892 being 4 cents. or 2d. a lb., and the fish being only incidentally taken in nets operated for salmon, or other fish. This fact has, doubtless, an important bearing on the increase in the abundance of the shad. The remarks I have made show how completely illogical, in my opinion, is the argument contained in the following sentence, quoted from the Report under review: "If these far-reaching and no doubt permanent results attend the planting on few occasions of small numbers of fry, in waters to which the fish are not indigenous, is it not permissible to assume that much more striking consequences must follow the planting of enormous quantities of fry year after year, in native waters?"

The history of the introduction of the striped bass (*Roccus lineatus*) to the same region adds strong support to my argument, for this introduction was altogether independent of artificial propagation. In 1879 about 150 specimens, a few inches long, taken in Shrewsbury River, New Jersey, were carried across the Continent and liberated at the mouth of Sacramento River; in 1882, another lot of 300 fish was transported to the same region. As a result of these two small deposits the species became distributed along the entire coast of California, and the catch in 1892 was about 43,000 lbs., for which the fishermen received somewhat more than £1,000.

The operations of the U.S. Commission for the year, in the propagation and distribution of fish, are recorded in the Report in great detail, but only a few points need be mentioned here. The discussion of results is not attempted in this section of the Report. The propagation of marine fishes is still conducted on a rather small scale. At Gloucester Station, Mass., 49 million cod eggs were obtained, and 20 million fry produced and liberated. At Woods Hole cold killed the spawners, and only 2,883,000 cod eggs were obtained, from which 850,500 fry were produced. Lobster eggs were also hatched; and mackerel, sea-bass, and flat fish on a very limited scale. Of shad 31 million fry were hatched at Battery Island Station on the Chesapeake, about 7 million on the Delaware, 5½ million at the Central Station, a total of 43½ millions. Thus, the number of fry obtained was only a little greater than that of the plaice hatched at Dunbar, and little more than one-fifth of the number of cod-fry hatched in Newfoundland. But, on the other hand, the number of eggs per female shad is given as 45,000, and the number of eggs obtained was 74,150,000, so that 1,647 females were stripped, and from this point of view the propagation of shad in the United States, is on a larger scale than that of plaice or cod in Scotland or Newfoundland.

INVESTIGATIONS.—As usual, a considerable amount of research is described in the Scotch Report. Dr. Fulton has added another series of experiments to those which have been carried out on the *Garland*, by his instructions. In this case he has had an equal number of hooks of different sizes fitted on one long line, and the line has been shot, in order to see whether the larger hooks caught fewer small and immature fish. The fish caught in largest numbers were of course haddock, and although the proportion of mature to immature fish was greater with the larger hooks, still this advantage was not sufficient to compensate for the great general reduction in the number of fish caught. Small hooks catch a large proportion of large fish, and large hooks a considerable proportion of small.

Of Prof. McIntosh's additional contributions to the knowledge of eggs and larvæ, perhaps the most interesting is that concerning the turbot. The material in this case was derived from the mature living turbot, collected at the Dunbar Hatchery. In the summer of 1894, these turbot, although gravid, did not spawn. On the 7th September a specimen was examined, and in the centre of the enlarged ovary was a large space filled with mucus and the remains of ripe, but dead, ova. The fish were evidently, it is stated, getting rid of the eggs of the season which had been retained in the ovary, and died there. This is exactly what I described years ago, in this Journal, concerning the sole in the Plymouth tanks. Prof. McIntosh thinks that the egg-bound condition, *i.e.* the refusal to shed the spawn in a normal manner, is voluntary, and that it would soon disappear when the fish grew accustomed to confinement. It is quite probable that the turbot would shed its spawn in confinement after a time, but in the Plymouth Aquarium the soles did not spawn till after five or six years, and the turbot has not spawned there yet. At Dunbar, the soles and turbot collected in 1894 were unfortunately lost from overcrowding, in consequence of the limited capacity of the ponds; and in 1895 the fertilised eggs from other turbot which were obtained, were artificially stripped from the fish.

Mr. Arthur T. Masterman has two papers in the Scottish Report, one on the rate of growth of plaice, and one on hermaphroditism in the cod. The former paper consists largely of comments on Petersen's work and my own; those on the former being complimentary, those on the latter very much the reverse. As he bases his comments largely on theoretical assumptions, I do not think it necessary specially to defend my own work. Mr. Masterman's own contribution to the evidence concerning the growth of plaice consists in the application of Petersen's method of graphic curves to the measurements of plaice taken on the east coast of Scotland by the *Garland*. The curves obtained, especially those of plaice taken in St. Andrew's Bay in 1891, do give successive maxima in the number of individuals at certain sizes, but that these maxima correspond to the broods of successive years seems to me more than doubtful. Thus, according to Masterman, the mid-size of the year old fish in July is 6 in., of the two year old 8½ in., while in November the mid-size of the year old fish is 9½ in., and even in October is 9 in. That is to say, the majority of the year old fish grow 3 in. in length in the three months, July to October, but only

$2\frac{1}{2}$ in. in twelve months. Another objection is that, according to Masterman, the plaice of 13 in. mid-size are in July in their fourth year; although it is known that plaice on the east coast of Scotland are at that size, with few exceptions, immature, while three-year-old plaice are nearly all mature. It is true that Masterman only urges that by the method, with a proper series of observations, valuable results *might* be obtained; and if we could explain away the cusp of the curve for July at 6 in., the two cusps at $8\frac{1}{2}$ and 13 in. would represent the plaice in their second and third years, a result which would agree with my own conclusions. In his second paper Mr. Masterman describes two hermaphrodite specimens of the cod, and discusses their condition in relation to hermaphroditism in general. In the course of his remarks he refers to "Nansen's observation of the protandric hermaphrodite condition of *Myxine*," apparently in ignorance of the fact that Nansen's description of that condition was a confirmation of my previous discovery. This is the second time that my discovery of the hermaphroditism of *Myxine* has been attributed in a Report of the Scottish Fishery Board to Nansen. On the former occasion the error was corrected, not by myself, in the columns of *Nature*. If Mr. Masterman had consulted my paper he would have found that the habits of the hag-fish were more definitely known than he seems to suppose.

It should be mentioned that the Report of the United States Commissioner, whose title is given at the head of this article, is merely the report proper, without the appendices, which were issued previously, and which contain detailed accounts of many of the investigations mentioned in the general report. This general report consists of four parts—the Commissioner's own statement, and three divisional reports, one on the division of investigations by Richard Rathbun, one on the division of statistics and methods, and the third on artificial propagation and distribution. Reference to interesting points in the last two divisions has been made, and it remains to mention the character of the investigations carried on by the Commission in the year 1892–93. In 1892 the *Albatross* was employed by the United States Government in investigations of the seal and seal fisheries of the Behring Sea. From August, 1892, till April, 1893, she was under repair at San Francisco, after which, by direction of the President, she joined the fleet which was employed in patrolling the North Pacific and Behring Sea. The naturalists belonging to the ship remained with her, except when she was under repair, and carried on observations concerning the seals, and the fishes of the places visited, as opportunities occurred.

On December 6th, 1892, an agreement was concluded between the

Governments of Great Britain and the United States, which provided for the appointment of a joint commission of two experts, one on behalf of each Government, to report upon the fisheries in the territorial and contiguous waters of the United States and Canada. The reports were to be presented within two years, and the object in view was the recommendation of practical and administrative measures to be adopted by both authorities. The two Commissioners appointed were Mr. Richard Rathbun and Dr. William Wakeham, and their investigations during the time covered by this report were confined to the mackerel fishery.

Various other investigations, such as the survey of oyster beds in Chesapeake Bay and Galveston Bay, the study of the lobster by Professor Herrick, at Wood's Hole, the discovery that the tile-fish had returned to the Continental slope, south of New England, with the return of warm water to that region in consequence of a change in the interaction of the currents, are mentioned, but the full description of them is to be found in special papers.

The Fourth Report of the Danish Biological Station.

By

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The Plaice in Danish Waters.—The Fourth Report of the Danish Biological Station consists of a lengthy paper by Dr. C. G. J. Petersen "on the Biology of our Flat-fishes and on the Decrease of our Flat-fish Fisheries," which was awarded a prize by *Det Kongelige danske Videnskabernes Selskab*, and which certainly deserves the careful attention of all who are interested in fishery questions. The first chapter gives a fairly complete account of some of the main features in the life history of the plaice in the Danish seas, together with shorter notes on other flat-fishes; the second and third are occupied by a discussion of the reasons for the deterioration of the fisheries, and of the remedial measures by which this evil may in the future be prevented. The paper is supplemented by five appendices, one of which, on the post-larval stages of flat-fishes, is of particular interest. For the full English translation with which we are provided English naturalists can but express their gratitude to the author.

The first question to which our attention is drawn in this paper is that of the variations in size, which plaice from different localities are found to exhibit.* These differences are seen on comparing the average sizes of plaice which have just arrived at maturity, and also the average sizes of mature (grown-up) plaice (*i.e.*, three years old and over) from different localities. Thus while in the Baltic the average size of mature plaice is about 10 inches, it is 11 inches in the Lesser Belt, and 12–13 inches in the Cattegat. Whether this gradual decrease in the average size of the mature fish, as we pass from the Cattegat to the Baltic, is due to a corresponding gradual change in the conditions favourable to growth, or whether it implies a migration of the larger plaice from the Baltic towards the Cattegat, is not certain, but there are reasons for thinking that the plaice of the Baltic do not enter that sea in any numbers till they are one year old, so that to speak of a race of plaice peculiar to the Baltic would be erroneous.

Further, as we pass from the German Ocean to the Baltic, there is a gradual decrease of the size at which plaice become ripe for the first time. If to these differences others (*e.g.*, in the number of fin rays) be added, the existence of separate races is still unproved. For seeing that the eggs and fry of all the plaice are pelagic, and must in consequence all be mixed together, the appearance of one form of plaice in the Baltic and another in the Cattegat, must be due either to the fact that the eggs of one form cannot live when carried into the territory of the other, or that the differences between the two forms are wholly ontogenetic. Of these two alternatives our author is inclined to accept the second.

Perhaps the most interesting part of Petersen's paper is that in which he describes his method of determining the rate of growth of plaice. By fishing at any given time of year in a number of different places, at different depths, and with nets of various kinds, and measuring all the fish caught, Petersen found that the fish were grouped about certain maxima corresponding to the most common lengths of the fishes born in successive years. These groups he calls the "0 group," consisting of fish less than one year old, the "1 group" between one and two years old, the "2 group" between two and three years old, and the "3 group" consists of fish three years old and over.

Leaving for a moment the question of how far this method of determining the rate of growth of the fish, and the probable age of any particular individual, is a sound one, we may pass on to a brief resumé of the life history of the plaice in Danish seas as traced by Petersen. The spawning season lasts from November to April, with a maximum in January and February. The larvæ, so long as they retain their yolk sac, are 6–7 mm. long. "When the yolk sac is absorbed, and the fish have become unsymmetrical and compressed, with their left eye sitting

* Cf. CUNNINGHAM. "North Sea Investigations"—this Journal, vol. iv. nos. 1 and 2; especially no. 1, pp. 23–25, and no. 2, pp. 97–108.

nearly on the edge of the brow, but while they are still transparent pelagic fish, they are 10 to 12 mm. long." The length at which the metamorphosis is complete, appears to vary from 10 mm. to 13 mm. Petersen was unable to find the young plaice of 12 mm. before the month of May, and concludes that the larvæ hatched in November take six months to pass through their pelagic stage. The same does not hold, however, for the turbot, the brill, the flounder, and the sole, in all of which the spawning season begins later than in the case of the plaice.

Further, the young plaice of 12 mm. in length are always found close in on the shores, and never in water of two fathoms and over. From this fact Petersen draws the conclusion that of the pelagic fry in the sea only those which happen to be near the shore at the time when metamorphosis takes place can survive. It is the physical conditions then, and not the presence of enemies, which causes that enormous destruction of larvæ which undoubtedly takes place.

The young fish belonging to the "0 group," which have all reached the length of 12 mm. by the month of May, grow to a length of 2–4 inches by the following autumn. In the succeeding winter they cannot easily be found on the shores, and it is suggested that they bore down deep into the sand where the seine cannot reach them. By the end of their first year the young fish migrate into deeper water, and this migration probably begins in the winter months. An investigation made at Aalbek in July, 1893, showed that the plaice were larger the deeper the water examined. Summarising the results of this investigation it was seen that besides the " 0 group," which were found in water of less than 2 fathoms, there was a "1 group" from 2½–2 inches at 5 fathoms, and a "2 group" from 6¼–10 inches, which began to appear in water of 8 fathoms.

The different groups are not, however, found in all the seas. On the contrary, while as we have already seen, the "0 group" is entirely absent from the Baltic, the plaice in their second and third years are present in considerable numbers. The largest specimens (14 in.) found in the Baltic, probably represent the 3 group, and the size at which the 2 group meets the 3 group, is set down at 8 to 9 inches. On the other hand, fish of 3 years old and over are not found in the Northern Cattegat in any numbers "without much searching"; and it seems clear that owing to persistent over-fishing there has been in recent years a decrease in the size of the plaice caught in the Cattegat. As Petersen remarks in pathetic italics, "*they do not get time in the Cattegat*"—cut off, as it were, by ruthless fishermen before they attain their prime! The fact that the "3 group," which is almost absent from the Cattegat, and but poorly represented in the Baltic, is found in the intervening seas, has suggested the view that an emigration of three-year-old plaice takes place from the Baltic.

Turning now to the question of the accuracy of the method employed for determining the rate of growth of fish, it seems clear that while the existence of the natural groups, each varying about a most common length, correspond to the average size of fish of that age is clearly shown; a reference to Petersen's tables leaves the impression that considerable uncertainty exists as to where one group ends and another begins, and as to the exact position of the most common length for each group. And this is practically admitted, when the remark is made that "besides distinguishing the sexes, we ought also properly . . . . to fish the same number of specimens of each annual series, in order not to efface the boundary lines between them." How can we be sure that this is done?* In cases where fish of all the different ages can be fished "in one draught" the difficulty is no longer present.

It was mentioned above that plaice less than 1 year old were not found in the Baltic. Hensen has, however, shown that the eggs of the plaice are found there; and the absence of the young fish is accounted for by the peculiar hydrographical conditions which obtain in that sea. For experiments made by Petersen, in Copenhagen, on living plaice eggs, proved that the highest specific gravity at which all the eggs sank was 1·0120 10° C., corresponding to a salinity of 1·44 per cent.; and Hensen's investigations prove that "*almost every month* there occurs such a low salinity that the eggs *must sink to the bottom*." If, as is probable, the eggs, on sinking to the bottom, are killed, the absence of the young fish is clearly accounted for.

The same does not, however, hold for the turbot, the brill, or the flounder. The fry of these fish are sometimes met with in multitudes on the shores of the Baltic, while in the Cattegat young flounders are found in company with young plaice. With regard to the turbot and the brill the explanation given is that the pelagic fry of these fish are more hardy than those of the plaice, and so can live in water of a lower salinity.

With regard to the *food* of plaice of different ages a short summary of the main facts is given by our author. Thus, during the pelagic stages the food consists chiefly of copepods; and even when the fish have grown to $1\frac{1}{4}$ to $1\frac{3}{4}$ inches, "*Copepoda, Cladocera, Ostracoda*, and *the larvæ of bivalves*" may form their food.

At a length of 2 in. to 3 in., however, the diet has changed, and now consists of *Idothea, Gammaridæ*, smaller *Annelida*, and the *fry of bivalves*." When the fish have grown to a length of 3 to 5 in., "they

[* Cf. for a more detailed criticism of Petersen's results CUNNINGHAM's paper in this number of the Journal pp. 136–138.]

take in the main the same sort of food as the older plaice," and after the end of their first year there is no change in the character of the food of the plaice.

We may now pass on to our author's discussion of the economic question: how may the plaice fishery be prevented from further deterioration? We may say at once that Petersen is in favour of the imposition of a size limit, but for reasons somewhat different from these commonly given in support of this proposition. He points out that the object to be kept in view is to make the fishery yield as large a profit as possible. It is necessary, therefore, to allow the fish to grow to such a size, that the largest possible weight of fish involving the highest selling price can be obtained.

Now, a plaice of 10 in. weighs less than ½ lb., and one of 14 in. more than twice as much: it follows that, unless in the time that it takes 10 in. plaice to grow to 14 in., the mortality is such as to reduce the population of these plaice to less than half its original number, the total weight of the plaice at 14 in. will be greater than at 10 in. It is not likely that the death-rate is as high as this, because the plaice does in fact grow to a much larger size than 14 in.; further, it would seem that disease is almost unknown among these fish: and their enemies are apparently few. Hence, by allowing the 10 in. plaice to grow to a larger size before capturing them a greater profit will be obtained. There will of course be a limit of size beyond which it will not be profitable to allow the fish to grow, and this limit will depend on the death-rate at each size.

Our author contrasts his view, which he calls the "growth theory," with the "propagation theory" of other writers. Those who hold this latter view insist on the necessity of increasing the *number of individuals*, and, in recommending a size limit, are happy if they can secure that the fish shall be allowed a chance of spawning. Petersen, on the other hand, thinks it of more importance to allow the fish to grow to the size at which they will yield a maximum profit, and holds that ample provision is made in nature for keeping up the numbers. As his point of view is an important one, and as it is stated with great clearness, I do not hesitate to quote him on this point *in extenso*. "It has always been hard for me to believe that there should be any want of eggs of plaice in our seas, partly because Hensen's excellent investigations have shown what immense quantities there are of them, partly because I myself see our seas filled with such eggs. Nor have I ever been able to believe in any want of young plaice. . . . Nay, everything seems to me to indicate that it is not in the *beginning*, but *in the middle and end of the life of the plaice* that we must look for the injury; for it is here that man interferes as a troublesome factor."* And again, "If we fish the plaice while they are small, we do not get so great a profit from them as we might and ought to have. *In this only, so far as I can see, the 'destruction' consists.*" †

It is of interest to notice that the view here set forth appears also in the discussion on the desirability of a close time for crabs and lobsters.‡ And if it is assumed for any species—(1) that the provision made in nature for keeping up the numbers of individuals is more than sufficient, and (2) that owing to *natural* influences, the number of surviving *grown-up* individuals is fixed and relatively small, then it will follow that the continual destruction caused by man must of necessity lead to a decrease in the number of large individuals, and that this decrease cannot be met either by artificial propagation, or by the imposition of a *small* size limit. These two assumptions appear to me to underlie the view which our author puts forward as the "growth theory"; and the first of them implies that there is, under natural conditions, an excessive wasteful production of young fish.

Our author further points out that, in respect of the plaice, both theories will lead to a similar practical conclusion—the imposition of a size limit; though the effective size limit, from the point of view of the "growth theory," will be higher than that which is required by the "propagation theory." This, however, does not hold good in all cases. The eel-fishery, for instance, consists wholly in the capture of eels which have never spawned. "The propagation theory would be obliged to require a size limit of at least 20 inches, in order to protect the stock of eels satisfactorily; *but then the males could not be caught at all.*"§ The growth theory, on the other hand, would be content with a lower limit.

It will be seen that the questions raised by the discussion of these different theories are of great practical importance. They are among the scientific problems which call for settlement, before sure guidance can be given to the legislator.

* *Loc. cit.* pp. 61, 62. (*In all quotations the italics are Petersen's.*)

† *Loc. cit.* p. 57.

‡ *Cf.* p. 186, this number of Journal, answers of fisherman to cross-examination by Messrs. Pannett and Mally.

§ *Loc. cit.* p. 82, footnote.